Speeding, Coordination, and the 55 MPH Limit

By CHARLES A. LAVE*

Do laws coordinate or restrain? A number of recent papers discuss the optimality of the 55 mph national maximum speed limit (NMSL), and evaluate the tradeoff of time-lost vs. lives-saved resulting from the lowered speed (James Jondrow et al., 1983; Dana Kamerud, 1983; Thomas Forester et al., 1984). These papers all implicitly accept the conventional wisdom—speed kills, slower is safer.

This conventional wisdom leads to laws designed as *limits* on behavior, whereas "...the crucial element is often coordination. People need to do the right things at the right time in relation to what others are doing" (Thomas Schelling, 1978, p. 121). There are indeed some traffic laws that establish conventions of expected conduct: we ask that motorists drive to the right, not because driving on the left is evil, but because it is important that the direction of flow be commonly agreed upon. Likewise, traffic lights are best viewed as a coordinating device: allowing free flow to alternating lanes of traffic to reduce the confusion and loss of time in unsignalized intersections.

For peculiar historical reasons, speed laws evolved as *limits* on driver behavior, rather than as signaling devices meant to *coordinate* it. Guided by the limit-rationale, police concentrate on those drivers who exceed the legal speed, and tend to ignore those drivers who disrupt coordination by traveling much slower than the norm.

This paper tests these differing views of the law by examining the current effects of the 55 mph NMSL—should it be viewed as a coordinating mechanism or a limiting mechanism? I measure the effects of limit-defying behavior (speeding), and absence of coordination (speed variance) on the fatality rate. Based on analysis of 1981 and 1982 state cross-section data, I find that there is no statistically discernable relationship between the fatality rate and average speed, though there is a strong relationship to speed variance. When most cars are traveling at about the same speed, whether it is a high speed or a low one, the fatality rate will be low—presumably because the probability of collision will be low. Variance kills, not speed.

I. Data and Methodology

The dependent variable is fatalities per 100 million vehicle miles traveled; data points are state averages. Since fatality rates differ by type of highway, I looked separately at six different types of high-speed roads: rural interstates, arterials, and collectors; and urban freeways, interstates, and arterials.[1] Separate regressions were fitted to data for 1981 and 1982; thus there were twelve distinct equations—six highway types for each of two years. I screened out any data point based on five or fewer fatalities; and Alaska and Hawaii were excluded because of their markedly atypical highways and driving conditions.

Table 1 shows the characteristics of each subset of the data. ("Speed Variance" is a measure of the dispersion of speeds among drivers. The distribution is approximately bell shaped: the "Average Speed" is about at the center of this distribution, and the "85th Percentile Speed" is about one standard de-

*Department of Economics, University of California, Irvine, CA 92717. Amihai Glazer made a major contribution to the conceptualization of the research; he is innocent of any errors in its execution. The work was done while I was serving as a member of the National Research Council's Commission for the Study of Benefits and Costs of the 55 MPH National Maximum Speed Limit; though the conclusions do not necessarily represent those of the staff and members of the Commission.

[1]"Urban freeways" also include urban expressways, hence their average standard is lower than interstates. "Arterials" have improved shoulders and wider lanes than "collectors."

TABLE 1—MEANS OF THE VARIABLES

Highway Type	*Fatality Rate*[a]	*Average Speed* (mph)	*Percent of Cars > 55 mph*	*Percent of Cars > 65 mph*	*85th % Speed* (mph)	*Speed Variance*[b] (85 – Avg)	*Citations per Driver*[c]	*N*
1981								
Rural Interstate	1.81	58.1	68.7	9.31	63.2	5.13	74.0	41
Rural Arterial	4.97	54.1	43.4	4.37	60.0	5.85	72.0	46
Rural Collector	4.11	51.7	33.4	4.42	58.7	6.99	75.3	41
Urban Freeway	3.25	54.9	46.3	2.94	59.9	5.00	58.2	19
Urban Interstate	1.37	55.8	54.1	4.46	61.1	5.33	59.4	26
Urban Arterial	2.67	51.9	31.2	2.20	58.1	6.12	63.7	23
1982								
Rural Interstate	1.50	59.0	73.1	14.2	65.1	6.12	67.0	44
Rural Arterial	4.24	54.4	47.1	6.23	61.2	6.81	64.8	47
Rural Collector	4.32	51.8	35.4	5.54	59.6	7.85	67.3	41
Urban Freeway	1.77	56.2	55.1	6.80	62.5	6.32	53.9	18
Urban Interstate	1.24	56.6	61.8	8.53	63.0	6.41	55.2	27
Urban Arterial	2.33	52.2	35.9	4.02	59.4	7.25	55.7	21

Source: Highway Statistics, U.S. Department of Transportation.

[a] Fatalities per 100 million vehicle miles traveled.

[b] 85th percentile speed minus the average speed: a rough measure of the standard deviation of the distribution of speeds.

[c] Speeding citations (on all highway types) per 100 drivers, per year. Variations in the average, across highway types, occur because state-composition varies across subsamples.

TABLE 2—RANGE OF VARIATION AMONG STATES

Variables	Mean	Lowest Value	Highest Value
Rural Interstates			
Fatality Rate	1.81	0.39	4.79
Average Speed	58.1	54.8	62.5
Percent of Drivers > 55 mph	68.7	40.3	88.8
Percent of Drivers > 65 mph	9.31	1.70	28.6
85th % Speed	63.2	58.8	69.4
85th Percentile – Average Speed	5.13	2.70	9.10
Citations per Driver	74.0	24.1	193.
Urban Freeways			
Fatality Rate	3.25	0.89	15.5
Average Speed	54.9	51.1	57.2
Percent of Drivers > 55 mph	46.3	23.1	64.6
Percent of Drivers > 65 mph	2.94	0.40	8.10
85th % Speed	59.9	57.4	63.8
85th Percentile – Average Speed	5.00	1.80	7.50
Citations per Driver	58.2	24.1	118.

viation to the right. Thus, the measure "85th Percentile – Average Speed" is a proxy for the standard deviation of observed speeds.)

Driving speeds and fatality rates differ considerably among states. Table 2 illustrates this variation for two subsets of the data: interstate rural roads and urban freeways in 1981.

II. A Model of the Fatality Rate

On a priori grounds, we can say that the fatality rate is a function of the probability of a collision, and of the consequence of the collision. Thus we can write:

$$Fatality\ Rate = F(Consequences, Probability).$$

Simple physics indicates that the consequence of a collision is a function of crash speed; and simple logic indicates that the probability of collision is a function of the dispersion of speeds on a given highway—more passing means more chances to collide. Thus:

$$\textit{Fatality Rate} = F(\textit{Speed}, \textit{Speed Variance}, \textit{Other Factors}).$$

Operationalizing *Speed* as the average speed, and operationalizing *Speed Variance* (85th percentile − average speed) where *FR* denotes *Fatality Rate*:

$$(1)\quad FR = a + b_1 Avg + b_2(85th\% - Avg) + e$$

$$= a + b_2 85th\% + (b_1 - b_2) Avg + e$$

$$(2)\quad = a + b_2 85th\% + b_3 Avg + e.$$

Suppose that speed variance has more effect on fatalities than does speed, per se—"coordinating" the traffic flow is more important than "limiting" it. Then in equation (1), b_2 will be larger than b_1. Since b_3, in equation (2), combines the effect of speed and speed variance, b_3 will actually be negative. That is, in equations of the form of (2), we would expect to get oppositely signed pairs of regression coefficients, b_2 positive and b_3 negative.

Table 3 shows the result of estimating equation (2) on the twelve subsets of the data. (In addition to the speed measures, it also includes a measure of access to emergency medical care,[2] and a measure of driver characteristics.)[3] The results confirm the model: 10 out of 12 of the regression coefficients of *Average Speed* are negative, and 10 out of 12 of the regression coefficients of *85th % Speed* are positive. The coordination effect is larger than the limit effect.

A further interesting result in Table 3 is that the pairs of speed coefficients, in a given regression, tend to be of approximately equal magnitude. But, by definition, $b_3 = b_1 - b_2$, so if b_3 and b_2 are approximately equal, then b_1 must be near to zero. Since, from equation (1), b_1 measures the effect of average speed on the fatality rate, then this would imply that speed, per se, has very little or no effect on fatalities. That is, the limit effect is very small.

TABLE 3—THE COMBINED EFFECTS OF SPEED AND SPEED VARIANCE[a]

Road Type and Year	*Average Speed*	*85th % Speed*	R^2
Rural Interstate 1981	−.24	.20	.62
	(1.8)	(2.3)	
Rural Arterial 1981	−.75	.58	.25
	(3.7)	(2.8)	
Rural Collector 1981	.01	−.01	.00
	(0.0)	(0.1)	
Urban Freeway 1981	−1.3	.55	.29
	(1.7)	(0.7)	
Urban Interstate 1981	−.04	.10	.12
	(0.3)	(1.2)	
Urban Arterial 1981	−.58	.50	.15
	(2.4)	(2.2)	
Rural Interstate 1982	−.21	.19	.52
	(2.3)	(2.5)	
Rural Arterial 1982	−.41	.35	.08
	(2.0)	(1.8)	
Rural Collector 1982	−.09	.001	.10
	(0.7)	(0.0)	
Urban Freeway 1982	−.39	.30	.14
	(0.8)	(0.7)	
Urban Interstate 1982	.04	−.01	.13
	(0.3)	(0.1)	
Urban Arterial 1982	−.29	.23	.16
	(1.7)	(1.2)	

[a] *t*-ratios are shown in parentheses; R^2 is corrected for degrees of freedom; and *Hospital Access* and *Driver Characteristics* are also in the equation.

[2] We want to measure both the number of hospitals per square mile, and the uniformity of their distribution. The variable used was *Hospitals/Square Mile* multiplied by the *Proportion of Population Living in Nonmetropolitan Areas*. Several variants of this were examined, and this one proved to be superior.

[3] Speeding *Citations per Driver* is a function of both driver aggressiveness and police conscientiousness. The results in Table 4 show a positive coefficient in 10 of the 12 cases, indicating that *Citations per Driver* is primarily a measure of driver behavior. Partial confirmation of this idea is seen in the negative correlation between citations and average driver age: a high proportion of young, presumably aggressive, drivers leads to a high citation rate.

TABLE 4—FINAL REGRESSION EQUATIONS[a]

Road Type	*Speed Variance*	*Citations per Driver*	*Hospital Access*	R^2	*N*	*Average Speed (if Entered)*[b]
Rural Interstate 1981	.176	.0136	−7.75	.624	41	(−0.5)
	(2.3)	(4.6)	(3.6)			
Rural Interstate 1982	.190	.0071	−5.29	.532	44	(−0.4)
	(2.6)	(2.8)	(3.7)			
Rural Arterial 1981	.677	.0122	.915	.237	46	(−1.3)
	(3.5)	(1.6)	(0.2)			
Rural Arterial 1982	.375	.0116	−.424	.101	47	(−0.5)
	(2.0)	(1.7)	(0.1)			
Rural Collector 1981	.011	.0041	−8.61	.019	41	(−0.1)
	(0.1)	(0.6)	(1.6)			
Rural Collector 1982	.046	.0139	−0.83	.089	41	(−1.2)
	(0.3)	(2.4)	(0.2)			
Urban Freeway 1981	.892	.0634	−.126	.269	19	(−1.2)
	(1.3)	(1.9)	(1.1)			
Urban Freeway 1982	.281	.0410	−2.86	.193	18	(−0.5)
	(0.7)	(2.5)	(0.5)			
Urban Interstate 1981	.103	.0101	.324	.139	26	(0.7)
	(1.2)	(2.0)	(0.2)			
Urban Interstate 1982	−.011	.0106	−.168	.167	27	(0.3)
	(0.2)	(2.8)	(0.1)			
Urban Arterial 1981	.526	−.0187	−1.93	.177	23	(−0.6)
	(2.4)	(1.9)	(0.5)			
Urban Arterial 1982	.304	−.0068	−5.72	.168	21	(−1.0)
	(1.9)	(1.2)	(2.2)			

[a] *t*-ratios are shown in parentheses; R^2 is corrected for degrees of freedom.
[b] Shows the *t*-ratio of the *Average Speed* variable if it were to be added to the equation (its potential significance and sign).

A. *A Direct Measure of the Effect of Speed*

We can test this implication by estimating equation (1): it gives direct coefficient estimates for the separate effects of speed and speed variance. When these regressions were run, *Average Speed* was insignificant in all 12 equations, and actually negative in 10 of them. I also tried replacing *Average Speed* with three other speed measures—percent of cars exceeding 55 mph, percent of cars exceeding 65 mph, 85th percentile speed—but results were no different. Once the effect of variance is held constant, there is no discernable effect of speed on the fatality rate.[4]

[4] This conclusion is not contradicted by the observed drop in fatalities following the imposition of the 55 mph NMSL in 1973, since speed variance fell that year.

Table 4 shows the final regression equations. *Average Speed* has been removed from the equations, but the last column (*Average Speed (if Entered)*) indicates the significance and sign it would have if it were to be included—it is not only insignificant but actually has a perverse sign in 10 of the 12 equations. As expected, *Hospital Access* plays its biggest role on rural interstates, the highways that are far removed from normal medical services. And, as expected, the effect of *Speed Variance* is least on relatively uncongested, multilane highways—the rural and urban interstates.

B. *Supporting Evidence*

These results are not unprecedented in the traffic engineering literature, though they do seem to have been forgotten. Twenty years ago, David Solomon investigated the relation

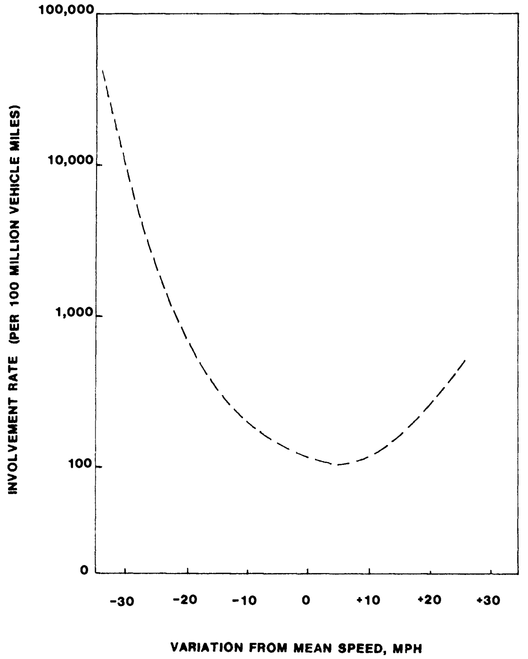

FIGURE 1. DEVIATION FROM AVERAGE SPEED VS. THE COLLISION RATE

between accident rates and variance and plotted the curve in Figure 1. This shows that it is safest to drive at the median speed, and increasingly dangerous to deviate from this speed in either direction; that is, slow drivers are equally responsible for causing accidents. Julie Anna Cirillo (1968) replicated the Solomon curve on interstate highways; and Ezra Hauer (1971) provided a theoretical foundation for the Solomon curve: he derived the number of overtakings expected at various speeds (for example, if I drive at 45 mph, while the median of the pack is 60 mph, how many cars will overtake me per hour, and hence have a chance to collide with me), and showed that his theoretical distribution was nearly identical to the Solomon curve.

III. Discussion

This paper presents evidence that speed laws should be viewed as devices for coordinating speed, not just limiting it. Both the slow driver and the fast one impose negative externalities on the median driver. Apparently, this is a novel conclusion: all current safety campaigns emphasize that "speed kills." They imply that the slower driver is the virtuous one and is helping protect himself and other drivers. It isn't so. To reduce fatalities, it is important that everyone drive at about the same speed. Thus the major consideration in choosing a speed limit is that it be obeyed. And the major consideration for the police is to reduce variance, not speed, because slow drivers are as much a public hazard as fast ones.

Clearly, the 55 mph NMSL ignores these considerations. It focuses on average speed to the exclusion of everything else. Even its compliance mechanism is ill-conceived: any state where more than half the drivers exceed 55 mph is subject to loss of federal highway subsidies. Thus there is as much federal sanction for a 56 mph driver as for a 76 mph driver.[5]

Although I have found no statistically discernible effect from speed, per se, this does not necessarily imply that it is safe to raise the speed limit, for we do not know what effect a higher limit would have on the speed variance. In the twelve data sets examined, there is generally a *negative* correlation between average speed and speed variance (8 negative correlations, 3 positives, and one 0.0); but I take these correlations to be suggestive rather than predictive.

However, the results presented here, and supported by the apparently forgotten observations in the highway engineering literature, do imply that major changes in the National Maximum Speed Limit and police behavior are warranted.

[5] Of course the NMSL was instituted to save energy, not lives, but its energy effects are relatively trivial—approximately a 0.2–1.0 percent reduction in gasoline consumption (Glenn Blomquist, 1984).

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Speeding, Coordination, and the 55-MPH Limit: Comment

By DAVID T. LEVY AND PETER ASCH*

That "speed kills," has been virtually a cliche in discussions of traffic safety for decades. The notion that greater velocity "produces" greater danger and more fatalities, *ceteris paribus*, is both intuitively appealing and empirically supported.[1] Recently, however, Charles A. Lave (1985) has presented evidence showing that the variance of speed rather than speed itself (i.e., mean speed) is the culprit. When most cars travel at about the same speed, Lave argues, fatalities tend to be low. It is the lack of "coordination" implied by dispersion of speeds that implies higher probabilities of collision and increased fatalities.[2]

We have tested Lave's intriguing suggestion using 1985 data. Our main finding is that mean speed and variance of speed are both correlated and interactive. At higher speeds, driving is less coordinated. Further, the effect of coordination (or its absence) on fatality rates is greater, the higher is mean speed. Thus, the hypothesis that speed kills, cannot be discarded.

I. Data and Model

We have examined 1985 data for 50 states (the District of Columbia was excluded).[3] The dependent fatality rate variable is interstate motor vehicle fatalities[4] (obtained from the Fatal Accident Reporting System distributed by the National Traffic Safety Administration), deflated by licensed drivers (from *Highway Statistics*, 1985).[5] Independent variables examined include:[6]

VMTPLD: Interstate vehicle miles traveled per licensed driver (*Highway Statistics*, 1985).
PCY: Per capita income (*Survey of Current Business*, 1985).
PRMALDR: Male drivers as a percentage of all licensed drivers (*Highway Statistics*, 1985).

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[1]See Charles A. Lave (1985) for citations of studies which conclude that speed is an important determinant of fatalities. For recent evidence, see L. Egmose and T. Egmose (1986), and A. Hoskin (1986).

[2]The 55-mile per hour speed limit has been wrongly credited, in Lave's view, with reducing traffic deaths; rather, a reduction in the variance of speeds produced this result.

[3]We estimated equations for 1983 and 1984 as well. The results were generally consistent with those reported below, except that in equation 1 the coefficient on *SPEED* was negative (though insignificant) in some cases and positive and significant in other cases.

We have also estimated separate equations for urban and rural interstate fatalities for one of the years. The results were not substantially different, with degrees of freedom and, pooling equations, an F-test ($F=1.775$) (22,74) failed to indicate significant structural differences between the two equations at the 0.10 level.

[4]We also examined total motor vehicle (including noninterstate) fatalities per driver and obtained consistent results.

[5]The fatality rate used by Lave (1985) was based on vehicle miles traveled. This rate is biased against a speed effect if lowered speeds also reduce vehicle miles traveled by making travel more expensive. We avoid this problem by including vehicle miles traveled as an independent variable, rather than as the deflator of the dependent variable.

[6]The included variables are similar to those commonly employed in cross-sectional studies of fatality rates. See Peter Asch and David Levy (1987).

Following Lave, we included a variable for hospital access, which introduced some significant collinearities but did not substantively affect our results. We also examined trucks as a percentage of motor vehicles, the percentage of the age 18-to-20 population that could drink legally, and average vehicle size (measured as miles driven divided by gallons of gasoline consumed). These measures were statistically insignificant and did not affect our results.

TABLE 1—REGRESSION RESULTS OF FATALITY RATE EQUATIONS-LINEAR FORM

Independent *Variable*	Equation 1 *Coefficient*	Equation 1 *Standard Error*	Equation 2 *Coefficient*	Equation 2 *Standard Error*
Intercept	−0.24	0.178	1.32*	0.59
VMTPLD	0.021*	0.0030	0.020*	0.0028
PCY	0.649	26.9	0.615	25.0
PRMALDR	0.36*	0.168	0.37*	0.15
PRMUN	−0.18	0.170	−0.27	0.16
PRYOUNG	0.084	0.26	0.15	0.24
DRAGE	−0.002	0.005	−0.009	0.049
ALCOHOL	0.0051	0.0043	0.0048	0.0039
SPEED	0.00095	0.0016	−0.027*	0.011
SPVAR	0.0041*	0.0019	−0.21*	0.078
*SPEED*SPVAR*			0.0038*	0.0014
Adjusted R^2	0.67		0.71	
Degrees of Freedom	40		39	

*Indicates statistical significance at the 0.05 level (two-tailed test).

PRMUN: Percentage of a state's highways mileage classified as municipal (*Highway Statistics*, 1985).

PRYOUNG: Percentage of the population aged 15–24 (*Census of Population*).

DRAGE: Legal driving age without driver's education (*1985 Drivers' License Administration, Requirements and Fees*).

ALCOHOL: Apparent alcohol consumption per capita, ages 14 and above, in gallons (*Alcohol Epidemiologic Data System*).[7]

SPEED: Mean driving speed—statewide average for various road types (*Highway Statistics*, 1985).

SPVAR: 85th percentile speed minus mean speed (*SPEED*); a measure of variance of speed.[8]

*SPEED*SPVAR:* The interaction of mean speed and variance of speed (the cross products of *SPEED* and *SPVAR*).

Regression equations are reported in linear form. We also estimated the equations in log form, but found the linear form to be superior.[9] Our particular concern is with the role of *SPEED*, *SPVAR*, and a variable, *SPEED*SPVAR*, interacting *SPEED* and *SPVAR*.

II. Results

Table 1 reports our results.[10] Equation 1 includes only mean speed (*SPEED*) and variance of speed (*SPVAR*). The observed patterns are consistent with those reported by Lave, except that mean speed has a positive though insignificant effect. The effect of variance is positive and significant.

[7]All available data on apparent alcohol consumption suffer from common measurement error because they are based on location of sale rather than actual consumption. The measures are therefore subject to distortion by crossover purchasing among states. Dropping this variable did not substantively affect the results reported below.

[8]Since the distribution of speed is approximately normal, the difference between 85th percentile speed (i.e., the speed that is not exceeded by 85 percent of drivers) and average speed, is about one standard deviation.

[9]Upon transforming the dependent variables so that the *R*-squares were comparable (see G. E. P. Box and D. P. Cox, 1964), the linear equations were found to have greater explanatory power. Further, the log equations yielded "incorrect" and significant signs on several of the variables.

Linear-log and loglinear functional forms also were inferior to the linear form.

[10]We found no evidence of heteroscedasticity due to uneven populations across states. Weighting by numbers of licensed drivers did not affect our results in any meaningful way.

The signs on the non-speed variables were generally as expected (see Asch and Levy (1987) for discussion of expected effects).

The interaction variable *SPEED*SPVAR* is included in equation 2. Its coefficient is positive and significant at the 0.05 level (while the coefficients on both mean speed and variance of speed become negative and significant), indicating that lack of coordination has greater fatality effects at higher speeds. The total effect of mean speed on fatality rates in equation 2 depends on the level of *SPVAR* and is positive and statistically significant at the 5 percent level when *SPVAR* reaches a value of 8.2.[11]

III. Conclusion

Based on our observations above, it appears that Lave's argument concerning variance is incomplete. Lack of coordination does imply greater risk. The degree to which it does so, however, depends on mean speed; and mean speed also contributes to risk at a rate that depends on the variance. Further, unlike Lave's results, our results suggest that enforcement efforts would be better directed at slowing down high speed drivers rather than speeding up slower drivers.[12] Publicists for safe driving campaigns may in good conscience continue to claim that "speed kills."

[11]The total effect of speed on the fatality rate is $-0.027+0.0038*SPVAR$. Significance was determined using an *F*-test. The mean of *SPVAR* is 6.94.

The total effect of variance (lack of coordination) on the fatality rate becomes positive and statistically significant when *SPEED* reaches 63.8, but the mean of *SPEED* is 55.2.

[12]The Pearson correlation coefficient between *SPEED* and *SPVAR* is −0.122 and statistically insignificant at the 0.10 level, which would suggest that speed and variance can be moved independently by policy.

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Speeding, Coordination, and the 55-MPH Limit: Comment

By RICHARD FOWLES AND PETER D. LOEB*

A recent article by Charles Lave (1985) in this *Review* argued that the main impact of vehicle speed on traffic fatalities is not so much the direct effect of speed as the variability of speed.[1] His analysis is conducted using a very simple model devoid of any socioeconomic or driving-related variables generally included in models of this sort.[2] This raises the possibility of omitted variables bias and the related possibility that the effects of average speed and the variance of speed are fragile with respect to model specification. Hence, we consider in this paper a more inclusive model. The model investigates the effect of motor vehicle inspection, as well as other policy-related variables, on vehicle fatalities along with the effects of vehicle speed and the variability of speed. This study is conducted using a traditional econometric modeling approach as well as Bayesian extreme bounds analysis.

I. The Model

The model used in this study is based on aggregate data.[3] The average speed (and the variability of speed) are measured as weighted averages of the speeds of free-moving traffic on rural and urban interstate highways, where the weights are the percentage of vehicle mileage traveled on interstate rural and urban highways.[4] The model is:[5]

$$(1)\quad Fatalities_i = \beta_0 + \beta_1 D_i a \beta_2 WSPEED_i + \beta_3 WVAR_i + \beta_4 AGE_i + \beta_5 BEER_i + \beta_6 MILES_i + \Sigma_j \beta_j X_{ji} + \varepsilon_i,$$

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[1] The effect of additional determinants of motor vehicle fatalities have been investigated by others. Loeb (1985, 1987, 1988), Loeb and Benjamin Gilad (1984), Victor Fuchs and Irving Leveson (1967), and W. Mark Crain (1980) evaluated motor vehicle inspection; Philip Cook and George Tauchen (1984), Peter Asch and David Levy (1987) and Loeb (1987) evaluated the minimum legal drinking age; Loeb (1987, 1988), Paul Sommers (1985), and Thomas Forester, Robert McNown, and Larry Singell (1984) considered the impact of average speed on rural interstate highways, alcohol consumption, and speed limits.

[2] See, for example, Loeb (1985, 1987), Loeb and Gilad, and Crain for a further discussion on this.

[3] Disaggregated data, as used by Lave, provide an accounting of deaths based on the site of the accident, for example, whether it was on a rural interstate, urban interstate, etc. It does not account for fatalities on a particular roadway due to driving conditions on a different type of roadway prior to the accident. More precisely, it does not take into consideration the "speed adaptation effect," where drivers exiting a high-speed roadway tend not to reduce speed so as to match the speed of the slower traffic on the newly entered roadway in a timely manner. (See Insurance Institute for Highway Safety, February 1987). It has been estimated that the "adaptation effect" persists even after several miles of travel on a connecting road. (See Insurance Institute for Highway Safety, January 24, 1987, p. 5). This effect cannot be minimized given that between 1978 and 1980, 24.4 percent of all fatal accidents occurred at intersections. (See National Safety Council, 1981, p. 46). Aggregate data obviously do not suffer from the dilemma of properly assigning a particular fatality to the proper contributing roadway.

In addition, the coefficients associated with speed in Lave's study are always insignificant and they are negative in ten of the twelve specifications. As such, one would expect similar results using aggregate data. This is not the case. Rather, the coefficient associated with speed is generally significant and always positive.

Finally, Yehuda Grunfeld and Zvi Griliches (1960, p.10) indicate, "that aggregation is not necessarily bad if one is interested in aggregates." They indicate that aggregation not only leads to an aggregation error but to an aggregation gain as well. This gain due to aggregation may be related to misspecification of the micro-relation as well as measurement errors in the regressors. Lave's model may be affected by specification errors due to the omission of the socioeconomic and driving-related variables.

[4] A more disaggregated set of data and weights was not available. The models, however, were evaluated

where:

$Fatalities_i$ = the number of traffic fatalities in the ith state.

D_i = a binary variable accounting for the existence or nonexistence of an inspection system in the ith state, that is, $D = 1$ for states having an inspection system in effect in 1979 and $D = 0$ otherwise.

$WSPEED_i$ = weighted average speed in miles per hour in the ith state of free-moving vehicles on rural and urban interstate highways.

$WVAR_i$ = weighted variability of speed in the ith state of free-moving vehicles on rural and urban interstate highways.

AGE_i = minimum legal drinking age for purchasing beer in the ith state.

$BEER_i$ = per capita consumption of malt beverages in the ith state.

$MILES_i$ = 100 million vehicle miles driven in the ith state. This variable serves as a proxy for out-of-state drivers passing through the ith state, along with its role as a scaling variable.[6]

$X_{ji}(j = 7,\ldots, k-1) = k-7$ additional socioeconomic and driving-related independent variables.

$\beta_j(j = 0,\ldots, k-1) = k$ parameters to be estimated.

ε_i = a random error term.

Table 1 provides a list of all the explanatory variables considered in the models along with indication of the expected effect of each variable on the dependent variable.[7] The models are evaluated for the year 1979 using data for all states with the exception of Louisiana, Maryland, Rhode Island, Arkansas, Delaware, the District of Columbia, and Wyoming, where complete data sets were not available for the year in question.[8]

II. Regression Results

Table 2 presents regression results on various specifications of the fatality model where variability of speed is measured as $WVAR_i = WSP85_i - WSPEED_i$. As can be readily seen, both the speed and the variability of speed variables have coefficients which are positive and significant. This is consistent with a priori expectations. The results are exceedingly stable across a rather extensive set of model specifications leading one to believe that the results are indeed plausible.[9]

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using the average speed of rural interstate highways in place of the weighted average of speed on rural and urban interstate highways. The results were similar to those presented here and are available from the authors. In addition, the models were evaluated in terms of fatality rates, as in Lave, with results indicating that both speed and speed variance had non-fragile effects on fatality rates. These results are also available from the authors.

[5]The definitions of weighted speed ($WSPEED$) and weighted variability ($WVAR$) are as follows:

$SPEEDR$ = average speed of free-flowing traffic on interstate rural highways.

$SPEEDU$ = average speed of free-flowing traffic on interstate urban highways.

$VMIR$ = vehicle miles on interstate rural highways.

$VMIU$ = vehicle miles on interstate urban highways.

$SP85R$ = 85th percentile speed on rural interstate highways.

$SP85U$ = 85th percentile speed on urban interstate highways.

$$WSPEED = SPEEDR(VMIR/VMIR + VMIU) + SPEEDU(VMIU/VMIR + VMIU).$$

$$WSP85 = SP85R(VMIR/VMIR + VMIU) + SP85U(VMIU/VMIR + VMIU).$$

$$WVAR = WSP85 - WSPEED.$$

[6]We are indebted to an anonymous referee for this recommendation.

[7]See Barry Jackson, Loeb and Karen Franck (1982) for a further discussion of the variables and their expected effect on the dependent variable.

[8]So as to comply as close as possible to Lave's specification, a hospital access variable was included in the analysis. We define $H80$, hospital access, as per Lave, as (hospitals/square mile) multiplied by the proportion of the population living in nonmetropolitan areas. The nonmetropolitan population component of $H80$ was not available for 1979 but rather for 1978 and 1980. The 1980 data were used given there were no substantial differences in results regardless of whether 1978 or 1980 nonmetropolitan population data were used.

[9]Only a partial set of all results are provided due to space limitations. Additional results, consistent with those reported, are available from the authors. Also, see Edward E. Leamer (1978, 1983) and Loeb (1987, 1988) on the use of fragility analysis in the evaluation of econometric models. Caution should be used when examining the coefficient of population in that population

TABLE 1—SYMBOLS AND DEFINITIONS[a]

Symbol	Definition	Expected Effect on the Dependent Variable
$FATALITY_i$	Traffic fatalities in the ith state	–
AGE_i	minimum legal drinking age in the ith state	<0
$BEER_i$	Per capita consumption of malt beverages (in gallons) in the ith state	>0
$WSPEED_i$	Weighted average speed (in miles per hour) in the ith state of free-moving vehicles on rural and urban interstate highways	>0
$WVAR_i$	Weighted variability of speed in the ith state of free-moving vehicles on rural and urban interstate highways	>0
D_i	Dummy variable for inspection in the ith state	<0
$H80_i$	Hospital access in the ith state	<0
$HIGH_i$	Total highway miles in the ith state	<0
Y_i	Personal income per capita (in dollars) in the ith state	<0
$POPD_i$	Population density (measured as population per square mile) in the ith state	<0
$PPOP1_i$	Percentage of the population between the ages of 18 and 24 in the ith state	>0
POP_i	Population in the ith state	$\gtrless 0$
$MILES_i$	100 million vehicle miles traveled in the ith state	>0
$WEST_i^a$	Dummy variable for western states: AZ, CA, CO, HI, ID, MT, NV, NM, OR, UT, WA, WY	>0

Sources: Data on fatalities, vehicle speed and highway mileage are from U.S. Department of Transportation, *Highway Statistics*, 1979. Data on the inspection variable are from the American Automobile Association, *Digest of Motor Laws*, 1981 and data on personal income per capita, population density, population by age, hospitals, and nonmetropolitan area populations are from U.S. Department of Commerce, *Statistical Abstract*, 1980 and 1981. The data on minimum legal drinking age limits and the states' areas are from *The World Almanac*, 1979 and 1981, respectively.

[a]Included in extreme bounds analysis. In addition, the extreme bounds analysis considered dummies for states in the east, north central, and south.

is highly correlated with miles, having a correlation coefficient of 0.967. It is important to note that the inclusion of population in the model did not affect the results reported for speed and speed variance as demonstrated in Tables 2 and 3. Similar problems were encountered when population density was included in the model. Once again, the robustness of speed and speed variance were not compromised.

These results are in strong disagreement with Lave.

The coefficients associated with beer, inspection, and hospital access conform with our a priori expectations as well as the results reported in Peter Loeb (1985, 1987, 1988), Loeb and Gilad, and Asch and Levy.

TABLE 2—ORDINARY LEAST SQUARES ESTIMATES FOR THE FATALITY MODEL[a]

Model	1	2	3	4	5	6	7	8	9	10
CONSTANT	−4005.41	−4184.20	−2185.27	−3825.24	−3913.24	−2732.19	−2286.07	−3160.82	−1892.17	−1933.99
	(−2.80)	(−3.02)	(−1.45)	(−2.83)	(−3.00)	(−1.80)	(−1.46)	(−2.68)	(−1.64)	(−1.44)
WSPEED	62.24	65.05	43.08	63.90	64.24	41.88	43.16	52.15	30.48	37.58
	(2.64)	(2.85)	(1.81)	(2.82)	(2.84)	(1.72)	(1.79)	(2.57)	(1.51)	(1.77)
WVAR	71.49	60.39	51.25	54.19	63.10	41.90	50.12	52.27	47.00	33.46
	(2.93)	(2.49)	(2.24)	(2.25)	(2.66)	(1.72)	(2.13)	(2.45)	(2.38)	(1.58)
MILES	3.28	3.32	3.36	3.29	3.31	3.42	3.37	4.07	4.03	4.08
	(39.92)	(40.60)	(38.98)	(41.16)	(41.20)	(37.58)	(37.49)	(14.62)	(14.57)	(15.49)
AGE				−4.05		10.02				
				(−0.20)		(0.54)				
D		−109.15	−79.14	−72.14	−102.50	−84.46	−83.69		−66.60	−69.83
		(−1.92)	(−1.46)	(−1.23)	(−1.85)	(−1.45)	(−1.46)		(−1.43)	(−1.44)
BEER				4.05		11.64				8.13
				(0.83)		(2.25)				(1.75)
Y			−0.05			−0.07	−0.05			−0.06
			(−1.72)			(−2.30)	(−1.72)			(−2.08)
H80			−855.59	−633.52			−820.55	−744.13	19.45	−616.37
			(−2.26)	(−1.46)			(−2.03)	(−2.20)	(0.05)	(−1.79)
POP								−0.05	−0.04	−0.05
								(−2.98)	(−2.45)	(−2.71)
HIGH									−1.0 E-4	
									(−0.14)	
POPD									−0.42	
									(−2.73)	
PPOP1	785.05	1807.59				2267.83	828.20			
	(0.27)	(0.63)				(0.81)	(0.28)			
RSQUARE	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.99	0.99
RBARSQR	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.98	0.99	0.98
SEE	161.93	156.51	146.67	153.06	155.25	148.46	148.59	138.61	124.72	130.09

[a] *t*-statistics are provided within parentheses. The robustness of the results in Tables 2 and 3 were not diminished when regional dummies for the East, North Central, South, and West were included. These additional results are available from the authors.

TABLE 3—BOUNDS FOR THE POSTERIOR MEAN FOR THE FATALITY MODEL FOR THREE PRIOR REGIMES

Regime	Free Variables[a]						
Policy Prior	*WSPEED*	*WVAR*	*BEER*[b]	*AGE*	*D*	*CONSTANT*	*MILES*
Upper Bound	82.11	60.71	13.01	18.52	41.62	−913.44	4.27
Lower Bound	26.14	28.11	−0.26	−21.07	−120.14	−5246.30	2.88
Economic Prior	*WSPEED*	*WVAR*	*H80*[b]	*INCOME*	*CONSTANT*	*MILES*	
Upper Bound	55.62	61.97	527.69	−0.02	−948.70	4.25	
Lower Bound	26.85	27.07	−1288.15	−0.09	−3022.99	2.93	
Demographic Prior	*WSPEED*	*WVAR*	*PPOPI*	*WEST*	*CONSTANT*	*MILES*	
Upper Bound	82.21	63.70	732.03	347.33	−935.56	4.23	
Lower Bound	28.39	27.81	−5653.77	27.96	−4925.12	2.91	

[a]The identity prior variance-covariance matrix is used on the set of doubtful variables. The set of doubtful variables includes all variables except *POPD* that are listed in Table 1 when not included as free variables. Results of the extreme bounds analysis remain invariant to those reported when *POPD* is included as a doubtful variable.

[b]When *WEST* is excluded as a doubtful variable H80 and *BEER* are non-fragile with signs consistent with a priori expectations.

Detecting fragility in model specification is efficiently accomplished using Bayesian extreme bounds analysis.[10] Table 3 summarizes extreme bounds values for the posterior mean on important "free" variables. In the analysis, three "prior" regimes, each consisting of different sets of free variables, are investigated. All possible linear combinations of a set of socioeconomic doubtful variables are considered in the evaluation of model fragility in addition to the free variables.[11] It is informative to note the narrow bounds contained in regions conformable with prior expectations for *WSPEED* and *WVAR*. In terms of Bayesian model selection, the strong data on these variables enhance posterior precision. This is especially true when the posterior mean is in agreement with the prior mean.[12] These results reinforce the conventional regression results reported in Table 2. There, estimated coefficients were generally "statistically significant" and always of the "theoretically correct" sign.

III. Conclusion

The empirical results, both classical and Bayesian, are remarkably similar in their findings. Unlike Lave, we find the effect of speed on fatalities to be positive, significant, and most importantly, non-fragile across a large set of reasonable alternative specifications after accounting for the effect of speed variance.[13] As such, government officials might be cautious in developing policy recommendations which would result in higher average speeds, such as raising speed limits, since such recommendations are likely to result in an increase in fatalities.

[10]Extreme bounds analysis is developed in Gary Chamberlain and Leamer (1976) and discussed in Leamer (1978, 1982, 1983). Computations in this paper were performed using *MICRO-EBA* developed by Richard Fowles (1988).

[11]Extreme bounds reflect the maximum and minimum values for minimally specified priors. The prior location for the set of doubtful variables is set to zero; priors are not entertained for the free variables. It is assumed that the prior variance/covariance matrix is positive definite.

[12]See, for example, Leamer (1978) and Arnold Zellner (1971).

[13]One might anticipate this result given the potential positive interaction between *WSPEED* and *WVAR*. The effect of speed, although at variance with that reported by Lave, is consistent with the findings of Asch and Levy and Loeb (1987, 1988).

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Speeding, Coordination, and the 55-MPH Limit: Comment

By DONALD SNYDER*

The conventional wisdom of transportation policymakers is that high absolute speed is a major cause of highway traffic fatalities. Charles Lave (1985) and Thomas Forester, Robert McNown, and Larry Singell (1984) (FMS) suggest the possibility that variation in driving speeds (many fast drivers, many slow drivers) also causes fatalities, creating more vehicle overtakings and thus more opportunities for collision. Both sets of authors suggest that a minimum speed limit might be added to the conventional maximum speed limit in order to reduce speed variation and hence traffic deaths.

This is plausible reasoning. Unfortunately, their statistical analyses utilize measures of speed variability which do not differentiate the fast driver from the slow one. I propose an approach which does make this crucial distinction.

I. Methodology

FMS measure speed variability by the percentage of vehicles traveling between 45 and 60 MPH; Lave measures it by the difference between 85th-percentile speed and mean speed (a proxy for the standard deviation). Neither measure clearly separates fast and slow drivers. An increase in the standard deviation of traffic speeds may occur because some drivers speed up, others slow down, or both. The same reasoning applies to a decrease in the percentage of vehicles traveling between 45 and 60 MPH. Suppose fatalities were to increase in response to greater speed dispersion. Neither of the above measures would indicate whether the faster vehicles, the slower vehicles, or both accounted for the fatality increase.

To distinguish between fast and slow vehicles I use two variables to measure speed dispersion: the difference between 85th-percentile speed and median speed (*85TH-MED*) for the fastest drivers, and median speed minus 15th-percentile speed (*MED-15TH*) for the slowest.

The traffic safety literature suggests other potentially important determinants of fatalities, many of which are difficult to measure and could well be correlated with speed or speed variance. Failure to allow for such variables in the model could introduce omitted-variables bias into the parameter estimates. My approach to this problem is to use panel data and supplement the ordinary least squares (OLS) estimates with estimates obtained from a fixed-effects model which reduces or eliminates the influence of omitted variables.[1] Comparison of fixed-effects estimates with OLS estimates can give an indication of the importance of omitted-variables bias.

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[1]The fixed-effects model is most easily estimated by running an OLS model with dummies for states and/or time periods included. The basic features of the FE model are described in most advanced econometric textbooks, for example, Thomas B. Fomby, R. Carter Hill, and Stanley Johnson (1984, ch. 15). Its usefulness for controlling for the effects of omitted variables, however, may not be widely appreciated; the reader is referred to Cheng Hsiao (1986, ch. 3) for a lucid description of this property of the FE model. Essentially, what happens is that the effects of the omitted variables which fluctuate over time are captured by the time dummies and the effects of those which fluctuate across states are captured by the state dummies, leaving the other model coefficients free of their influence. The model of course requires that panel data (observed across states and over time) be used for estimation.

TABLE 1—OLS AND FIXED-EFFECT ESTIMATES: DETERMINANTS OF RURAL HIGHWAY FATALITIES[a]

	Model	F	R^2	(*MED-15TH*)	(*MED*)	(*85TH-MED*)
(1)	OLS		0.40	0.03 (0.15)	0.37 (3.25)***	0.40 (2.85)**
(2)	OLS		0.41		0.38 (4.18)***	0.42 (4.55)***
(3)	OLS		0.25		0.51 (5.19)***	
(4)	FE, states	9.93***	0.80	0.11 (0.75)	0.43 (3.27)***	0.26 (2.29)**
(5)	FE, years	1.43	0.42	−0.03 (−0.15)	0.35 (2.82)**	0.37 (2.43)**

Note

[a]The dependent variable is *FTRATE*. Standardized regression coefficients (betas) and their *t*-values (in parentheses) are shown. (*), (**), and (***) represent statistical significance at the 0.05, 0.01, and 0.001 level, respectively. R^2 is adjusted for degrees of freedom. The *F*-statistic for the fixed-effects model tests the hypothesis that the fixed effects (measured by state/time dummies) are significant.

II. Model

Consider the following model:

$$FTRATE = \alpha + \beta(MED\text{-}15TH) + \gamma(MED) + \delta(85TH\text{-}MED),$$

where *FTRATE* is highway fatalities per 100 million vehicle miles and (*MED-15TH*), *MED*, and (*85TH-MED*) are as defined above. If speed variation were the sole cause of highway fatalities then β and δ would be positive and γ would equal 0. If high average speed were the sole cause the pattern would be: γ positive, β and δ equal to 0. If both hypotheses were valid, all three coefficients would be positive. If neither hypothesis were valid, all three coefficients would equal 0.

III. Results

I estimated the parameters of this model using data for main rural highways (including interstates) from 26 states for the period 1972–1974.[2] Table 1 presents the principal findings.

Equation (1) is the most important. The large positive coefficient for *MED* supports

[2]The 26 states included were New Jersey, Pennsylvania, Virginia, West Virginia, Georgia, North Carolina, South Carolina, Illinois, Michigan, Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, South Dakota, Arkansas, Texas, Arizona, Colorado, Idaho, Montana, Nevada, Utah, Oregon, and Washington. Most of the remaining states were excluded because of missing speed distribution data; a few were dropped because of outlier problems or having fewer than 5 highway fatalities in any one year. Sources of data are given in the Appendix. The years 1972–1974 were chosen because published speed distribution data by state is available for that period only.

The speed distribution data was collected on "level, straight sections of main rural roads, including the interstate system." I calculated the 15th, 50th, and 85th-percentile speed for each state and year according to the formula in Frank Scalzo and Roland Hughes (1975, p. 75). The fatality rate data is for primary federal-aid rural highways, including interstates. The speed distribution data was not published for any other type of highway: therefore, further research will be necessary to determine the extent to which the findings of this study apply in other traffic situations.

the conventional wisdom: the higher the average traffic speed, the higher the fatality rate.

The small, nonsignificant coefficient for (*MED-15TH*) indicates that unusually slow vehicles are not contributing to traffic fatalities. Unusually fast vehicles do cause fatalities, however: the large positive coefficient for (*85TH-MED*) indicates that the faster the fastest vehicles drive (compared to the average vehicle) the higher the fatality rate. Equations (2) and (3) confirm that this pattern is not an artifact of collinearity among the regressors: dropping (*MED-15TH*) actually leads to a small improvement in model performance, whereas dropping (*85TH-MED*) causes model performance to deteriorate.[3]

Results for the fixed-effects model are consistent with the OLS results. Equation (4) allows for omitted regressors which vary across states. The important difference between equation (4) and equation (1) is that the coefficient for (*85TH-MED*) is smaller and less significant, suggesting that this variable, although important in its own right, also acts as a proxy for omitted variables (speed limit enforcement? alcohol consumption?). Equation (5) allows for omitted regressors which vary across time. The similarity of results between equations (1) and (5) indicates that omitted time-varying regressors are not present or not important.[4]

IV. Conclusions

These results confirm that average traffic speed is an important determinant of highway fatalities, as is widely assumed. They further suggest that speed variance is important for the fastest vehicles only: slow vehicles do not have a statistically detectable influence on fatality rates. The whole situation might be summed up as follows: the faster you drive, relatively or absolutely, the more likely you are to get killed.

Why is it that unusually slow vehicle speed does not contribute to traffic fatalities, whereas unusually fast vehicle speed does? Perhaps the answer lies is the composition of the two groups. Slow vehicles are more likely to be large (buses, tractor-trailers, etc.) and therefore easier to see and avoid. They are more likely to be driven by professionals. Fast vehicles are more likely to be smaller and driven recklessly by nonprofessionals. Perhaps the driver going at average speed finds it easier to avoid a collision when overtaking a large, slow, safely driven vehicle as opposed to being overtaken by a fast and recklessly driven one.

If the necessary data become available and these findings are supported by results from future studies involving more states, other time periods, and other highway types, the implication would be that a minimum speed limit would not be particularly effective in reducing fatalities. As an aside, we may note that recent revisions in national speed laws may be encouraging fatalities on interstates. They mandate different speeds for autos (65 MPH) versus trucks and car-trailers (55 MPH), and this might well have caused an increase in (*85TH-MED*).

APPENDIX: DATA SOURCES

FTRATE: Traffic fatality rate on rural primary highways, including interstates. *Source*: U.S. Department of Transportation, Federal Highway Administration, *Fatal Injury and Accident Rates on Federal Aid and Other Highway Systems*, annual, 1972–1974, table FR-2.

MED: Median vehicle speed in miles per hour on level, straight sections of main rural roads, including interstates. *Source*: computed from data in U.S. Department of Transportation, Federal Highway Administration, *Highway Statistics*, annual, 1972–1974, table VS-2.

85TH-MED: Difference between 85th-percentile speed and median speed. *Source*: same an *MED*.

MED-15TH: Difference between 15th-percentile speed and median speed. *Source*: same as *MED*.

[3]The correlation is 0.59 between (*MED-15TH*) and *MED*, and 0.76 between (*MED-15TH*) and (*85TH-MED*).

[4]It is interesting to note that the switch to the 55-MPH speed limit took place in early 1974. The similarity between equations (1) and (5) suggests that the effect of the lower speed limit on highway fatalities is largely due to its effects on *MED* and (*85TH-MED*). Caution should be exercised in drawing conclusions about the effects of omitted time-varying regressors, however, because the sample data were available for only a three-year period.

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Speeding, Coordination, and the 55-MPH Limit: Reply

By CHARLES LAVE*

When I wrote my article, we were in the midst of an intense campaign concentrating on the dangers of speed. Nothing was said about the dangers of variance. Everyone knew "speed kills," no one knew that slow driving was dangerous. Society was preoccupied with the *limits* function of speed laws (thou shalt not exceed 55 MPH) and was ignoring their *coordination* function—speed laws as information devices to signal expected behavior and coordinate the flow of traffic. For example: (a) All media safety campaigns and most of the law enforcement action concentrated on speed and ignored variance; (b) One of the major insurance companies was running a TV commercial which advised viewers to *ignore the speed of other drivers*, to follow the 55-MPH limit even if it meant going much slower than the rest of the traffic—advice we now know to be akin to urging suicide.

My major finding was the discovery that speed variance was dangerous. All three comments—David Levy and Peter Asch, Richard Fowles and Peter Loeb, and Donald Snyder—confirm this.

My secondary finding was the absence of a statistically significant relationship between the fatality rate and average speed. The comments do find a discernible effect, but I believe their results are simply an artifact of aggregating dissimilar highway types.

I. The Aggregation Issue in General

Highway types differ markedly in their physical characteristics and usage patterns, and in turn, these differences in characteristics cause a 5 : 1 difference in the fatality rate across highway types. The dissimilarity between a local urban street and an interstate highway is obvious. Less obvious are the important differences between seemingly similar roads such as interstate highways: (a) rural interstates are more isolated than urban interstates, the time delay for medical help after an accident is greater, and so ordinary accidents become converted into fatalities; (b) rural interstates carry 2.4 times the proportion of heavy trucks, so there are more car/truck interactions; (c) rural interstates have greater trip lengths, more driver fatigue, and hence 15 percent more accidents where the vehicle simply wanders off the road.

Mindful of such differences, I run separate equations on each type of highway. The three comments do not. They aggregate data from diverse highway types, which leads to two potential problems: First, since the quantitative relationships between the dependent variable and the causal factors will be different for each highway type, coefficient estimates may become muddled. Second, the aggregation process may cause the average speed variable to become statistically significant by creating a spurious correlation between speed and fatalities. How might this occur?

Rural highways have higher fatality rates than urban highways (for the structural reasons cited above), and rural highways have higher average speeds than urban highways. Thus, combining rural and urban data will produce a spurious positive correlation between speed and fatalities.

Later on, in Table 3, I demonstrate that aggregation can make an insignificant average speed effect appear to become significant.

The method of combining data from diverse highway types can also produce a positive bias for the speed coefficient. When data are combined as weighted averages of the separate components, states with a high pro-

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portion of rural travel will show high weighted-speed, and a high weighted-fatality rate.

II. The Levy/Asch Model

Results: David Levy and Peter Asch (L/A) make the interesting extension that average speed and speed-variance (*SPVAR*) interact. It is reassuring to see that my 1981–82 results are similar to their 1985 results (their equation (1)). That is, holding background factors constant, in an equation based on speed and *SPVAR*: the regression coefficient for *SPVAR* is positive and significant and the coefficient of average speed is insignificant ($t = 0.6$). This was also true in an earlier draft where they used 1983 data.

However, speed does become significant when the interaction term is added to the equation. Their equation (2) estimates: $FATALITY\ RATE = -0.0274\ SPEED - 0.208 SPVAR + 0.0379 SPEED{*}SPVAR$. The partial derivative of the *FATALITY RATE* with respect to *SPEED*, evaluated at the mean of *SPVAR* (6.94 MPH) is -0.0011; and the partial with respect to *SPVAR*, evaluated at the mean of *SPEED* (55.2 MPH) is $+0.0012$. The two effects are of opposite sign and equal in magnitude. A 1 MPH

TABLE 1—EXPLORING THE LEVY/ASCH SPECIFICATION

	Dependent Variable = Fatalities / Licensed Driver[j]				Dependent Variable = Fatalities / *VMT*[k]			
Fatality Data Source								
Rural + Urban Interstate[a]	XXX	XXX			XXX	XXX		
Rural Interstate[b]			XXX	XXX			XXX	XXX
Speed Data Source								
Statewide[c]	XXX		XXX		XXX		XXX	
Rural + Urban Interstate[d]		XXX			XXX			
Rural Interstate[e]				XXX				XXX
	"A"	"B"	"C"	"D"	"E"	"F"	"G"	"H"
Average Speed[f]	0.00330	0.00140	0.00227	0.00135	0.00123	0.000267	0.00143	0.000402
	(2.3)	(1.1)	(2.1)	(1.1)	(2.2)	(0.54)	(2.1)	(0.85)
Speed Variance[g]	0.00578	0.00457	0.00492	0.00552	0.00237	0.00205	0.00228	0.00265
	(2.6)	(2.3)	(3.5)	(3.7)	(3.1)	(3.0)	(2.7)	(4.1)
Percent Male[h]	0.277	0.347	0.216	0.317	0.0866	0.112	0.145	0.142
	(1.7)	(2.0)	(1.8)	(1.8)	(1.4)	(1.7)	(1.9)	(2.0)
VMT/Licensed Driver[i]	0.0212	0.0216	0.0219	0.0161	–	–	–	–
	(6.4)	(6.1)	(9.9)	(4.6)				
Constant	−0.389	−0.314	−0.282	−0.300	−0.118	−0.0755	−0.157	−0.101
R^2*df corrected*	0.68	0.62	0.80	0.55	0.29	0.23	0.28	0.36
Number of Cases	41	42	42	43	41	42	42	43

1985 state cross-section data, same year used by Levy/Asch; t-ratios in ( ).

[a] Sum of fatalities on rural and urban interstate highways (DOT-2, p. 30).

[b] Rural interstate fatalities, only (DOT-2, p. 30).

[c] *VMT*-weighted average speed for all high speed highways in the state (DOT-4, p. 176).

[d] *VMT*-weighted average speed for data from rural and urban interstate highways (DOT-3: Attachments 4 and 6).

[e] Rural interstate speed only (DOT-3: Attachments 4 and 6).

[f] Average Speed: uses speed data from row c, or d, or e, depending upon the specification for that column.

[g] Speed Variance: 85th percentile—average speed; uses speed data from row c, or d, or e, depending upon the specification for that column.

[h] Percent Male: total male licensed drivers divided by total licensed drivers in the state (DOT-1, p. 11).

[i] *VMT*/Licensed Driver: total state vehicle miles traveled (*VMT*) divided by total licensed drivers (DOT-1, pp. 11, 28).

[j] Fatalities/LicDriver: number of fatalities (from row a or b, depending upon the specification for that column) divided by the number of licensed drivers in their state (DOT-1, p. 11).

[k] Fatalities/*VMT*: number of fatalities (from row a or b) divided by the relevant associated vehicle miles traveled (DOT-1, p. 28).

increase in average speed would have *decreased* the fatality rate; a 1 MPH increase in speed-variance would have increased it. Of course, these partials would be different at other values of *SPEED* and *SPVAR*.

Specification: Aside from their development of the variance/speed interaction effect, their specification also differs from mine in a number of ways that I do not believe are improvements:

L/A combine together fatalities from rural and urban interstate highways, and run a single equation on this aggregate variable. I run separate equations to allow for different highway characteristics.

L/A use fatalities per licensed driver (*FAT/LD*) as their dependent variable. I use fatalities per vehicle mile traveled (*FAT/VMT*) because the scaling unit which causes a fatality is a mile of driving: that is, the fatality risk is associated with miles driven, not with the possession of a license.

L/A use "Statewide" speed data: a weighted average of speed data from *all* state highways posted at 55 MPH. Thus, they add in data from urban arterials (principal and minor), rural arterials (principal and minor), and major rural collectors. They are using speed data that describe all high speed highways to explain the fatality rate for a subset of the highways—a subset whose characteristics are quite different from the average. I use the speed variables which describe the highway of interest: rural interstate speed data in the equation predicting rural interstate fatalities, and so on.

Table 1 uses 1985 data, as L/A do, to display the consequences of these contrasting specification decisions. The left side of the table shows results from using fatalities per licensed driver as the dependent variable; the right side shows the results from using fatalities per vehicle mile traveled. The four right-hand equations have a parallel structure to the four left-hand equations, and each set of four equations shows the relevant combinations of possible data sources. For example regression "A" is based on fatality data from rural plus urban interstate highways, and uses the Statewide speed data to explain these fatalities. Equation A is similar to the L/A equation (1). Equation H is similar to the equations in my paper: speed and fatality data from the same specific-highway type.

The average speed variable is only significant in the four equations that use the Statewide speed data (A, C, E, G). I believe this is a consequence of the aggregate nature of this data, as explained above. In the two equations which use interstate aggregate speed data (B, F), average speed is not significant. Statewide speed data is based on all high speed roads; the speed variable in equations B/F is based on interstate speed data only.

Equations C/G illustrate an intermediate case, fatality data from rural interstates combined with the Statewide speed data. Since the results are essentially the same as in A/E, then the major determinant is the aggregation of the speed data, rather than the aggregation of the fatality data.

Equations D/H represent my preferred specification. Disaggregate fatality and speed data that are specific to the particular highway type in question, rather than combining data at different levels of aggregation.

Equations based on fatalities per licensed driver have higher R^2 values, but this is an incidental result of using licensed drivers as the scaling variable, not an indication that the overall specification is better. If we use t-ratios as the criterion of interest, then the fatality/*VMT* specification gives better results for the dissaggregate regressions.

(*Note*: There are less than 50 cases because some states did not report speed data by highway type; and Alaska was excluded because its driving patterns and highway conditions are quite atypical).

III. The Fowles/Loeb Model

Results: Richard Fowles and Peter Loeb (F/L) explore the effects of a variety of interesting socioeconomic variables on the fatality rate, using 1979 data. They find the effect of both Speed Variance and Speed to be positive and significant.

Specification: Aside from their development of the socioeconomic variables, their specification also differs from mine in a number of ways that I do not believe are

TABLE 2—EXPLORING THE FOWLES/LOEB SPECIFICATION

	Dependent Variable = Fatalities			Dependent Variable = Fatalities/*VMT*		
	"I"	"J"	"K"	"L"	"M"	"N"
Average Speed	12.4 (0.80)	–	12.4 (0.78)	0.0716 (1.4)	–	0.0684 (1.4)
Speed Variance	–	0.634 (0.03)	−0.189 (0.01)	–	0.112 (1.7)	0.108 (1.6)
Vehicle Miles	2.42 (35.)	2.41 (35.)	2.42 (35.)	–	–	–
Constant	−710	16.6	−709	−1.67	1.78	−2.21
R^2	0.96	0.96	0.96	0.02	0.04	0.05

1985 State cross-section data; (*t*-ratios); R^2 is d.f. corrected; 48 cases Fatalities: total fatalities, on all highway types (DOT-2, p. 6).

Average Speed: weighted average of urban and rural interstate speeds; weighting factor comes from relative *VMT* ratio (DOT-3, Attachment 4).

Speed Variance: 85th percentile speed minus average speed; *VMT*-weighted average of urban and rural interstate data (DOT-3, Attachment 6).

Vehicle Miles: total vehicle miles traveled (*VMT*) on all highways in the state (DOT-1, p. 28).

improvements:

F/L combine together fatalities from *all* the different highway types in the state (high and low speed, rural and urban), and run a single regression on this aggregated fatality variable.

F/L use fatalities as their dependent variable. This automatically produces very high R^2 values since total fatalities and total *VMT* have a high correlation and there is 50:1 range of *VMT* in the sample. (A simple regression of fatalities on *VMT* gives an R^2 of 0.97). But that is not the relationship of interest.

F/L use speed variables based on average interstate speeds. Thus they are trying to explain the fatality rate on *all* highway types by using speed data that come from a subset of the highways—a subset whose physical characteristics and travel usage is quite different from the average. Why would we expect total highway fatalities to be a function of the speed variance on interstate highways?

F/L expect the interstate speed variable to serve as a proxy for the "speed adaptation effect"—drivers accustomed to fast roads will continue to drive at high speed when they move to ordinary roads for some brief transition period. I would argue that average interstate speed is an inappropriate proxy: a set of variables based on speed *differences* are what is needed, for example, a variable based on rural interstate speed minus rural arterial speed; a variable based on urban interstate speed minus urban arterial speed; and so on. Such variables are more congruent with the underlying theory, and are less likely to create spurious significance for an average speed variable. I would also argue that the speed adaptation effect must be small—it only applies to the tiny proportion of total *VMT* at the interface between the two highway types—hence the advantage of correcting for this small effect is not worth the danger of adding in a spurious variable.

Table 2 shows my attempt to replicate the F/L 1979 results using 1985 data. The first three equations (I, J, K) use the F/L dependent variable, total fatalities: none of the speed variables are significant. The next three equations (L, M, N) use my dependent variable, fatalities/*VMT*: the fit of the speed variables improves considerably though they are still not significant. My attempted replication does not include the F/L socioeconomic variables, but I do not believe this is sufficient to account for our different results.

After all, their first reported equation has no socioeconomic variables either, but it still shows a significant speed effect.

IV. The Snyder Model

Results: Snyder poses an interesting question: are the consequences of driving slower than traffic the same as those of driving faster? Using 1972–74 data he finds that fast driving matters but slow driving does not.

Specification: The model is elegant, but the implementation is problematic because of the data he is forced to use. His speed data is for "Main Rural Roads," a grouping that combines interstates, primary arterials, and some secondary arterial roads. Of the 600k miles of road in this category, 30k are of interstate quality, 45k are 4-lane roads, and 2-lane roads. Unfortunately, these are the only published data with detailed speed distributions. An additional complication comes from the time period, 1972–74: the first two years are under one causal regime, but 1974—the year the speed limit was dropped to 55 MPH—was radically different.

I cannot construct an exact example to show the consequences of this aggregation: the detailed speed distributions of 1972–74 are only available in aggregate form; and the years when disaggregated data were published do not give information on the lower tail of the speed distribution. But Table 3 is certainly suggestive. It uses 1985 data to illustrate the consequences of aggregation; fitting the same model to three different data sets: first, rural arterial roads; then rural interstates; and finally on a *VMT*-weighted combination of arterials and interstates. (It only fits two of the three Snyder terms since there are no low speed data available).

Average speed is not significant in column one (arterials), or in column two (interstates), but it is significant in column three (the aggregated data). That is, though average speed is insignificant in both of the disaggregated data sets, it becomes spuriously significant when the model is estimated on a data set where the two kinds of data are combined.

Snyder finds no significant effect from low speed drivers. I believe this is partly the result of using aggregated data and partly the result of combining dissimilar years 1972–73 (pre-55 miles/h limit) with 1974. Though I cannot disaggregate the data, I can disaggregate the years. Table 4 runs Snyder's model on 1972–73 data. The variable that represents the effects of slow-speed drivers is significant, or nearly so, in both equations.[1]

So which tail of the speed distribution is more dangerous? Should public policy worry more about fast drivers or slow ones? Table 4 says that slow drivers create fatalities; fast ones do not. This may be true, but since these results, like Snyder's, result from fitting

TABLE 3—EFFECT OF DATA AGGREGATION ON THE SIGNIFICANCE OF THE "AVERAGE SPEED" VARIABLE

	Arterial Highways	Interstate Highways	Arterial + Interstate
Average Speed	0.705	0.488	1.30
	(0.96)	(0.96)	(2.0)
Speed Variance	1.89	2.85	2.43
	(1.5)	(4.1)	(2.7)
Constant	15.3	34.0	62.2
R^2	0.06	0.31	0.27

Dependent variable is Fatalities/($10^5 VMT$) for 1985 rural highways; t-ratio in ( ); $n = 41$.

TABLE 4—EXPLORING THE SNYDER SPECIFICATION, THE EFFECT OF FAST AND SLOW DRIVERS IN 1972–73

	Fatalities *per VMT*	Fatalities *per VMT*	Fatalities *per VMT*
Slow Drivers	0.268	0.263	–
(Median-15th)	(1.8)	(2.1)	
Median Drivers	0.0494	0.0511	0.0842
	(1.1)	(1.3)	(2.0)
Fast Drivers	−0.00972	–	0.142
(85th-Median)	(0.06)		(1.1)
Constant	0.012	−0.129	−0.965
R^2	0.10	0.10	0.06

Dependent variable is Fatalities/($10^8 VMT$) for rural highways (arterial + interstate); t-ratio in ( ); $n = 67$.

[1] I use 67 observations; Snyder has only 52 available because of the limitations imposed by his fixed-effects model.

the model to basically unsuitable data, it seems more appropriate to ignore them, and go find a better data source. The next section discusses one possible way to do this.

V. Evidence from Detailed Micro-Level Data

In a recent unpublished study Nicholas Garber and Ravi Gadirau (1988) collect detailed data on speed, and speed variance for a number of specific-sampling sites in Virginia, and then use this data to model the local accident rate. Looking at their interstate results only: they find no relationship between accidents and average speed; a strong relationship between accidents and *SPVAR*; and a negative relationship between speed and speed variance—speed variance declines as average speed increases.

The Garber/Gadirau results are not definitive because they rest on a small sample for a single state. But their paper does point the way toward resolving our various questions: high quality, disaggregate data would avoid the problems of multicollinearity and interpretation that plagued my analysis and the analyses of the three comments. Extra hours invested in data improvement are likely to have high payoff at this point.

VI. Conclusion

All three comments strongly support my finding that speed variance is an important determinant of the fatality rate. But there is still dispute about the importance of speeding, per se. I do not claim that speeding is unimportant, but I could find no statistically discernible effect in any regression on disaggregate data. By fitting different specifications than mine, the authors believe they have found evidence for the importance of speeding. I am not sure this is true. The difference between my results and theirs is not due to their new specifications—whose cleverness and contribution I am happy to acknowledge—but rather to their use of aggregate data.

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Statutes Versus Enforcement: The Case of the Optimal Speed Limit

By PHILIP E. GRAVES, DWIGHT R. LEE, AND ROBERT L. SEXTON*

An important literature has evolved which considers the optimal tradeoff between the probability and magnitude of fines (see Gary S. Becker, 1968, A. Mitchell Polinsky, and Steven Shavell, 1979). The conclusion of this literature is that, under risk-neutrality, efficiency dictates that the probability of catching an individual engaging in an externality generating activity should be set as low as possible, while the fine should be as high as possible (limited only by the wealth of the perpetrator).

Similar considerations apply to the setting of statutes versus enforcement activity although we shall return to caveats to this approach in concluding remarks. There have been occasional cases where at least implicit tradeoffs apparently have been made, such as the $1,000 fines for littering (recently raised from $500 in many states, though scarcely ever enforced). Such cases are, however, usually consigned to activities generating externalities that are difficult or impossible to effectively enforce in any event. In the more general case, those setting standards seldom consider enforcement efforts, perhaps tacitly assuming full compliance. We consider here some implications of the separation of statute setting and enforcement efforts for the topical issue of establishing highway speed limits in Section I, while more general social implications close the paper in Section II.

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I. Statutes, Enforcement Effort, and Average Speed

In assessing the benefits from lowering the speed limit, Charles A. Lave (1985) argues that it is not the reduction in average highway speed that generates the major benefit, but the reduction in its variance (see also Thomas Forester et al. (1984) and David T. Levy and Peter Asch, Donald Snyder, and Richard Fowles and Peter D. Loeb (in this *Review*).[1] Recognizing the possible importance of variations in speed on highway safety would seem to push in the direction of reducing the speed limit, although as seen below, this is not necessarily the case.

We begin with a model in which reductions in accident externalities come entirely from reductions in average highway speed, then discuss implications of extending the model to allow accidents to depend on speed variance. The crucial feature of this model comes from the recognition that average highway speed, S, is a function of both the speed limit, L, and the level of policing, P, given by $S(L,P)$. Over relevant ranges of L and P, it is reasonable to assume that S increases at a decreasing rate with respect to L and decreases at a decreasing rate with respect to P:

$$S_1 > 0, S_{11} < 0, S_2 < 0, S_{22} > 0.^2$$

[1]Even the argument regarding average speed may be dubious: after an initial sharp decline in death rates between 1973 (55,000) and 1974 (46,000), the 55 miles-speed limit came to be viewed as a safety measure rather than an energy-saving measure. However, during the 1980s death rates have *continued* to fall (especially on interstates) even though average driving speeds have risen. Other factors are probably at work, obscuring the safety benefits of lower speeds, although in the absence of a controlled experiment, this is speculative.

[2]Obviously the magnitude of these partials will depend upon the severity of the penalties imposed on

The private net benefit realized from the average highway speed is given by the function $B(S)$. Since the purpose of a speed limit is to keep motorists from traveling as fast as they otherwise would, it is assumed that $B'(S) > 0$ over the relevant range of speed, with $B''(S) < 0$. The cost of accident externalities, $C(S)$, is given as a function of average speed only, with $C'(S) > 0$ and $C''(S) > 0$. It is assumed that the marginal and average cost of policing is given by the positive constant θ. Finally, it is assumed that there is some speed $\bar{L}$ below which it is politically impossible to lower the speed limit.[3] The recent legislative activity raising certain speed limits suggests that 55 miles/h may well be at or below $\bar{L}$. In terms of the introductory discussion $\bar{L}$ may alternatively be thought of as the speed limit set by the legislature (perhaps on energy-use grounds) without regard to enforcement.

We are now in a position to express the objective of speed limit policy as solving for the L, P, and λ which maximizes:

$$(1)\quad Z(L,P,\lambda) = B[S(L,P)] - C[S(L,P)] - \theta P + \lambda(L-\bar{L}).^4$$

The Kuhn-Tucker solution to this inequality constrained maximization problem is:[5]

$$(2)\quad \frac{\partial Z}{\partial L} = [B'(S) - C'(S)]S_1 + \lambda \leq 0,$$

$$(3)\quad \frac{\partial Z}{\partial P} = [B'(S) - C'(S)]S_2 - \theta = 0,$$

$$(4)\quad \frac{\partial Z}{\partial \lambda} = L - \bar{L} \geq 0,$$

$$(5)\quad L, P, \lambda, \geq 0,$$

$$(6)\quad ([B'(S) - C'(S)]S_1 + \lambda)L = 0,$$

$$(7)\quad \lambda(L - \bar{L}) = 0.$$

The intuition behind these conditions is straightforward. Condition (3) calls for an increase in policing until its marginal value, $[B' - C']S_2$, is equal to its marginal cost, θ. Since $\theta > 0$ and $S_2 < 0$, it follows from (3) that $B' - C' < 0$. This, along with the fact that $S_1 > 0$, means that (2) holds as a strict equality in light of equation (6); hence the marginal value of increasing the speed limit is negative. This implies that the advantage that could be realized if the speed limit were reduced is being frustrated by the constraint $L > \bar{L}$ and, therefore, $L = \bar{L}$. (Note that λ is strictly positive from (6), hence $L - \bar{L} = 0$ from (7)). It is interesting to note that in this case the optimal speed limit is completely independent of the functions $B(S)$ and $C(S)$, with only the amount of policing being affected by the benefits or costs associated with highway speed.

The solution conditions (2) and (3) are diagrammed in Figure 1. The curve $MV(P;\bar{L})$ represents the marginal value of

those detected in violation of the speed limit. For example, the fine for going 65 miles/h is $5 in Montana (with no points assessed) while in Maryland the fine would be $40, with points (see *Newsweek*, July 21, 1986, p. 15, for information on other states). In order to focus attention on the policy variables L and P, the penalty structure will be assumed fixed throughout the analysis, ignoring regional variations. The sign on the cross partial S_{12} will also be of significance. Since an increase in L will find more motorists obeying the speed limit voluntarily, it is reasonable to assume that increasing L will reduce the negative effect an increase in P has on S, or $S_{21} = S_{12} > 0$.

[3]We let $\bar{L}$ be sufficiently low so that if a speed limit of $\bar{L}$ were perfectly enforced the result would be an average highway speed of $\bar{S}$, $\bar{S} < \bar{L}$, where $B'(\bar{S}) - C'(\bar{S}) > 0$.

[4]In this formulation, we impose the inequality constraint that $L \geq \bar{L}$, but not a corresponding nonnegativity constraint on P. This appears plausible in that if P were not strictly greater than zero, motorists would realize that *any* speed limit is meaningless and would react by returning to the private outcome which ignores external costs. Hence the optimal P value will involve an internal solution.

[5]The sufficient conditions are satisfied by the earlier restrictions.

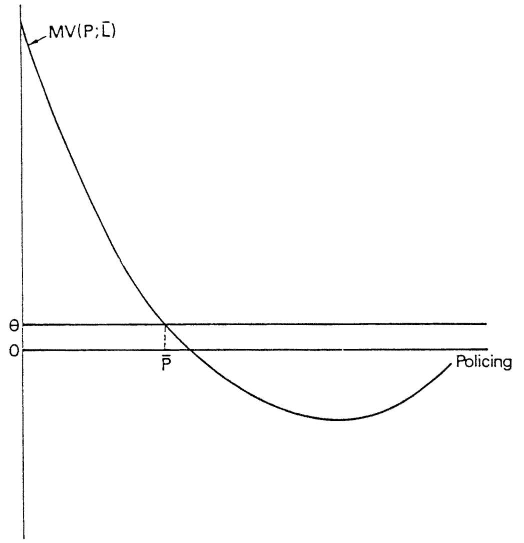

FIGURE 1

policing the speed limit $\bar{L}$, or

$$(8)\quad MV(P;\bar{L}) = \left[B'(S(\bar{L},P)) - C'(S(\bar{L},P))\right] S_2(L,P).$$

This marginal value is positive over some initial range of policing, but since perfect enforcement of speed limit $\bar{L}$ would result in $B'-C'>0$ (see fn. 3), $MV(P;\bar{L})$ will become negative if policing is increased to a sufficiently high level. Continued increase in P will eventually find the $MV(P;\bar{L})$ curve sloping upward and approaching zero asymptotically since one would expect that $S_2 \to 0$ as $P \to \infty$ and full compliance is realized. In accordance with condition (3), the optimal level of policing is given by $\bar{P}$, where $MV(P;\bar{L})$ intersects the horizontal line at θ. It should be noted, since $B'-C'<0$ under the optimal policy, that the average highway speed will be higher than that which satisfies the conditions conventionally thought to determine the optimal average speed, or $B'-C'=0$.

If any attempt to reduce the highway speed limit below 55 miles/h would encounter overwhelming political resistance (as suggested by the successful recent efforts to raise that limit on rural interstates), then it is the case that $\bar{L}=55$ miles/h. Therefore, according to the model just developed, the nationwide speed limit of 55 may well be optimal.

The plausibility of this result, in the context of the present model, comes from recognizing that of the two ways to reduce current average highway speed, lowering the speed limit or raising the level of policing, the former will have, on the margin, lower social costs than the latter. This is akin to substituting harsh penalties for costly detection efforts in the control of crime, as discussed by Becker (1968).

The impact of speed variance, in the present setting which emphasizes enforcement costs, is interesting and potentially perverse. Lave (1985) argues that a reduction in the speed limit makes its largest contribution to highway safety by coordinating (decreasing the variance of) highway speeds. As discussed more formally in Dwight Lee, Philip Graves, and Robert Sexton (1987), incorporating the impacts of speed variance in the present model is capable of implying that the optimal speed limit in the wide-open western states (for example, Montana) may be *lower* than in the more congested eastern states (for example, Maryland).

To clarify, consider the extreme case where speed variance is the sole culprit in lowering highway safety and that, on other grounds (perhaps foreign energy dependence), desired average speed were the same in Montana and Maryland. Stringent enforcement of the speed limit in Maryland—to reduce variance, which matters more there—would have the additional impact of lowering the average speed *below* the optimum. Hence, under these circumstances, the posted speed limit would optimally be set higher in Maryland to offset the impact on the average speed of the optimally greater enforcement to reduce variance! This seemingly perverse result need not, however, occur if the optimal average speed in Montana were enough greater than in Maryland.

The model developed in this section puts a different perspective on the cost-benefit studies that conclude that the 55 miles/h speed limit is too low but that ignore the role of policing costs (see, in addition to those already cited, Gilbert Castle (1976), Charles A. Lave (1979), and James Jondrow et al., 1983). The conclusion of these studies is that the average highway speed is too low under the 55 miles/h limit. This conclusion is consistent with the present model, which calls for an average highway speed even greater than that typically considered to be socially optimal. But in the present context, evidence suggesting that highway speeds are too low under the 55 miles/h limit is not an argument for increasing the speed limit. Rather it argues for reducing the amount of policing used to enforce the speed limit.

II. Conclusions

The tradeoff between statute setting and enforcement efforts is not limited to the choice of statutory speeds. All regulations require enforcement if they are to be effective; nevertheless, most economic analysis proceeds as if somehow regulations are self-enforcing. This can lead to policy conclusions that are questionable. It can also result in a failure to uncover implications that are of interest quite apart from specific policy concerns. Policy approaches to several recent social issues can benefit from the observations here. In the past few years drunken driving has been of increasing concern in this country. An approach to this problem analogous to the setting of speed limits discussed here would be to define "drunk driving" as occurring when blood alcohol levels exceed, say, 0.05 rather than the current 0.10 to 0.15. This redefinition would no doubt reduce the average level of blood alcohol observed among drivers in this country as it has in Scandinavia where, as an extreme, one country does not allow a positive blood alcohol reading. Juvenile crime could similarly be reduced, without the use of additional scarce resources, by redefining "juvenile" to be a lower age than at present. Other examples will no doubt occur to the reader.

Some objections to the approach taken here, and indeed to that of Becker and others, should be raised at this point. First, issues of equity emerge: a lower speed limit with less enforcement to obtain the same average speed would mean that there would be more violators of the posted speed, each having a lower probability of being caught. This might engender considerable sympathy for those arbitrarily singled out for punishment. In the extreme, a speeding or DWI arrest could become viewed as "cruel and unusual punishment." Moreover, a mistaken conviction (while no doubt much less likely than at present) would be received with great resentment. Similarly, relying extensively on this approach could turn a majority of our citizens into law-breakers, perhaps leading to reduced self-policing behavior on their part. There is the related legal presumption, partially questioned here, that laws without enforcement are meaningless, indeed counterproductive if they result in negative externalities to more general law-abiding behavior. Clearly, pure statutory changes are not costless up to the point of overwhelming social opposition. Finally, in the specific context here, the rapid development of enforcement technology (for example, photographic techniques) also softens the policy implication. To the extent that these criticisms are valid, they reduce the quantitative significance of the point being made here. The impact of the current separation of statute-setting from enforcement does, however, leave this point qualitatively unaffected. Indeed, as already indicated, large litter fines are virtually never enforced but probably do contribute to litter reduction, yet much of the preceding criticism could be leveled at litter laws. However such objections might suggest that great caution be exercised in moving to a revised legal system in which the tradeoff between statute stringency and enforcement is fully exploited.

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