Using Mandated Speed Limits to Measure the Value of a Statistical Life

Author(s): Orley Ashenfelter and Michael Greenstone

Source: *Journal of Political Economy*, Vol. 112, No. S1, Papers in Honor of Sherwin Rosen: A Supplement to Volume 112 (February 2004), pp. S226-S267

Published by: The University of Chicago Press

Stable URL: https://www.jstor.org/stable/10.1086/379932

Using Mandated Speed Limits to Measure the Value of a Statistical Life

Orley Ashenfelter

Princeton University and National Bureau of Economic Research

Michael Greenstone

Massachusetts Institute of Technology, National Bureau of Economic Research, and American Bar Foundation

In 1987 the federal government permitted states to raise the speed limit on their rural interstate roads, but not on their urban interstate roads, from 55 mph to 65 mph. Since the states that adopted the higher speed limit must have valued the travel hours they saved more than the fatalities incurred, this institutional change provides an opportunity to estimate an upper bound on the public's willingness to trade off wealth for a change in the probability of death. Our estimates indicate that the adoption of the 65-mph limit increased speeds by approximately 4 percent, or 2.5 mph, and fatality rates by roughly 35 percent. Together, the estimates suggest that about 125,000 hours were saved per lost life. When the time saved is valued at the average hourly wage, the estimates imply that adopting states were willing to accept risks that resulted in a savings of $1.54 million (1997 dollars) per fatality, with a sampling error roughly one-third this value. We set out a simple model of states' decisions to adopt the 65-mph limit that turns on whether their savings exceed their value of a statistical life. The empirical implementation of this model supports the claim that

We are grateful to Steven Levitt and an anonymous referee for especially insightful comments. We also thank Gary Becker, Glenn Blomquist, David Card, Mark Duggan, David Lee, Helen Levy, Alan Manning, Enrico Moretti, Casey Mulligan, Kevin Murphy, Todd Olmstead, Anne Piehl, W. Kip Viscusi, and Will Manning for valuable comments. Numerous seminar participants provided very helpful suggestions. Michael Park and Anand Dash deserve special thanks for superb research assistance. We acknowledge generous financial support from the Hoover Institution, the Industrial Relations Section at Princeton University, and the Robert Wood Johnson Foundation.

[*Journal of Political Economy,* 2004, vol. 112, no. 1, pt. 2]

$1.54 million is an upper bound, but it provides imprecise estimates of the value of a statistical life.

Public choices about safety in a democratic society require estimates of the willingness of people to trade off wealth for a reduction in the probability of death. In this paper we exploit a novel opportunity to measure the revealed preferences for safety risks from public choices about speed limits. The idea is to measure the value of the time saved per incremental fatality that results from the voluntary adoption of an increased speed limit. Since adopters must have valued the time saved by greater speeds more than the fatalities created, this ratio provides a convincing and credible upper bound on the value of a statistical life (VSL).

Although there have been a number of creative attempts designed to estimate the VSL,[1] there have been few opportunities to obtain estimates based on the public's willingness to accept an exogenous and known safety risk. Our analysis exploits the opportunity that the federal government gave the states in 1987 to choose a speed limit for rural interstate highways that was higher than the uniform national maximum speed limit then in existence. This remarkable experiment led 40 of the 47 states that have rural interstate highways to adopt 65–mile per hour (mph) speed limits on them, and the remaining seven states retained 55-mph speed limits.

This institutional change permits us to address several conceptual problems that have plagued previous attempts to estimate the VSL. First, the earliest estimates of the VSL were based on hedonic wage equations that many observers acknowledge suffer from severe omitted variables biases.[2] The 1987 law provides a plausibly exogenous change that may avoid the difficulties inherent in making causal inferences with observational data that reflect individuals' past optimizing decisions. Moreover, our estimates of the trade-offs between the value of time saved and fatalities can be made both from comparisons of rural interstate highways across states that altered their speed limits with those that did not and from comparisons of rural interstates and other highways within states that adopted increased speeds. This statistical design provides

[1] For useful, detailed surveys, see Viscusi (1993) and Blomquist (2001). Also see de Blaeij et al. (2000) for a formal meta-analysis of the VSL from studies of road safety.

[2] See especially Black and Kniesner (2003). In more than half of their specifications, they find that for male (and female) workers, fatality risks are estimated as negatively related to wage rates, implying VSL estimates that are also negative. Also see Hersch (1998), who finds a negative association between injury rates and wages for all male workers. These findings are universally interpreted to result from the difficulty in properly specifying and measuring the key variables that enter wage equations.

many alternative estimates of the actual trade-off between the value of travel time and fatalities and thus provides many tests of the consistency of the estimates.

Second, many questions have been raised about the usefulness of studies of the VSL when the decision makers studied may be poorly informed about the relevant risks. We show that the relevant decision makers (i.e., state governments) were cognizant of the trade-offs associated with a change in speed limits. Although this does not provide conclusive evidence that the participants in the decisions were well informed, it is certainly more plausible than is often the case.

Third, any VSL estimate that is based on the decisions of a third party (e.g., government policies) may not reflect the preferences of the group whose VSL is of interest. For example, federal regulatory agencies, such as the Environmental Protection Agency and Federal Aviation Administration, regularly assess prospective safety projects. Since the benefits and costs of these regulations are borne by entirely different groups, the political process by which they are determined may seriously distort the agency's decisions. It seems likely that the substantial heterogeneity in the cost per life saved in enacted safety projects both across and within agencies shown by Viscusi (2000) reflects these problems. Speed limit regulations, however, provide benefits (reduced travel time) and costs (fatality risk) to precisely the same people, so that appeals to a simple model of the typical voter are far more plausible in this context.

Finally, previous studies of safety risks in the marketplace frequently measure the VSL of a selected group of individuals that place a low valuation on increased risks, since they will be the marginal adopters.[3] These individuals' VSL will rarely be the appropriate one for evaluating policies that affect a broader cross section of the population. In contrast, this paper presents a simple individual-level behavioral model that predicts that the median driver's/voter's preferences determine which states adopt the higher limit. We provide evidence that is consistent with this behavioral interpretation of the results.

Our empirical results indicate that among states that adopted increased speed limits on their rural interstates, average speeds increased by approximately 4 percent (i.e., 2.5 mph) and fatality rates increased by roughly 35 percent on these roads. In the 21 states that raised the speed limit and for which we have complete data, the estimates suggest that there were an additional 45 million hours saved and 360 lives lost annually, which translates into 125,000 hours per life. These two effects

[3] There are examples of studies that estimate the value of safety risks across broad cross sections of individuals. See Atkinson and Halvorsen (1990) and Dreyfus and Viscusi (1995), which estimate hedonic models for motor vehicles in which safety characteristics are a measured feature.

are estimated precisely, and the key inferences are similar across many different specifications.

Valuing the time saved from increased speeds at the average hourly wage implies that adopting states were willing to accept risks that resulted in a savings of \$1.54 million (1997 dollars) per fatality, with a sampling error roughly one-third this value. Since this figure is the value of time saved per marginal fatality among states adopting higher speed limits, it provides an upper bound on the VSL in the adopting states. Consequently, we set out a simple structural model to recover the VSL that is identified by variability across the states in the probability of the adoption of increased speed limits. The empirical implementation of this model supports the claim that \$1.54 million is an upper bound, but it provides estimates of the VSL that are very imprecise.

The paper is organized as follows. Section I sets out the conceptual rationale for a simple econometric model that may be used to estimate the trade-off between risk and wealth. Section II provides a brief history of speed limits, describes how the 1987 law can be used to estimate the VSL, and informally explores the validity of our assumption that the median driver's/voter's preferences determined the decision whether to adopt the 65-mph speed limit. Section III describes the data sources, presents the key descriptive statistics, and reports the unadjusted estimates of the effects of the 65-mph speed limit on fatalities and speeds. Section IV lays out the econometric framework for estimating the value of the time saved per marginal fatality, and Section V presents our estimates of this figure. Section VI implements the structural model and reports the resulting estimates of the VSL. Finally, a discussion of the primary results and some of their major limitations in Section VII is followed by a brief conclusion.

I. Conceptual Framework

In order to see how empirical estimates of the effect of speed limits on speeds and fatalities provide a way to quantify the revealed preferences of the determining driver/voter for safety, it is useful to set up a simple explicit model of behavior.

A. Selecting an Optimal Speed

The first-order effect of traveling at a higher speed is a change in travel times for each mile traveled by each driver and a corresponding change in the likelihood of a fatality. This ignores the altered costs of fuel and other driving costs from changed speeds. These incremental costs, as noted by Ghosh, Lees, and Seal (1975), are very small compared to the time costs.

To provide a dollar measure of the value of life, it is necessary to provide a dollar value to the benefits of travel. To do this, write h for the hours spent traveling m miles so that $h/m = 1/s$ is the average hours required to travel m miles per driver. The term h/m is, of course, the reciprocal of the average speed (s) on the road. If the cost of an hour of time spent traveling is w, then the average cost of a mile of travel time per driver is

$$c = w\left(\frac{h}{m}\right); \tag{1}$$

c is also a measure of the value of a mile spent traveling. After all, if a mile of travel were not worth at least c, it would not be undertaken.

The appropriate way to measure the cost of time can be controversial. For most workers, however, a natural measure of the value (or cost) of their time is their wage rate. In the empirical work reported below, we use the mean wage rate in adopting states as a measure of the value of time; but our primary measurement methods do not depend on this assumption, and other values may be used where appropriate. For some cases, it may be thought that a value less than the wage rate is appropriate (see Lee and Dalvi 1969; Beesley 1973; Domencich and McFadden 1975; Deacon and Sonstelie 1985; Waters 1996). Virtually any measure of the cost of a worker's time, however, will be closely linked to a worker's wage.

Selecting a speed balances the desire to reduce the cost (c) of travel time by increasing speed (s) against the risks of increased fatalities that may exist from greater speeds. The full costs of travel are then

$$g = g\left(c, f\left(\frac{c}{w}\right)\right), \tag{2}$$

where $g_1 > 0$, $g_2 > 0$, and $f = F/m$ is fatalities (F) per mile, and the function $f(c/w) = f(h/m) = f(1/s)$, with $f' < 0$, indicates how fatalities increase with speeds.[4]

[4] This model is based on the assumption that vehicle miles traveled are fixed and independent of speed limits. It is a straightforward matter to extend the model to include the value of miles traveled as a separate component of welfare. However, the available evidence supports our assumption that vehicle miles of travel did not increase in response to the introduction of the higher speed limit in 1987. Greenstone (2002) uses data from 1982–90 and finds that vehicle miles of travel on rural interstates did *not* increase in states that adopted the 65-mph speed limit. We extended this analysis to the length of our sample (1982–93) and reached a similar conclusion (the results are available on request).

The effect of a decrease in travel time on the total costs of travel time per mile is

$$\frac{dg}{d(h/m)} = g_1 w + g_2 f'. \tag{3}$$

At low levels of speed, increases in speed presumably reduce time costs ($g_1 w$) by more than the increased accident costs ($g_2 f'$). Thus a small increase in speed, *ds*, that leads to a decrease in travel time of *dh* and an increase in fatalities of *df* is desirable if $-g_1 w dh > g_2 df$, which is satisfied when

$$-w\left(\frac{dh}{df}\right) > \frac{g_2}{g_1}. \tag{4}$$

The speed that minimizes the full time and accident costs of travel, if it exists, satisfies

$$V \equiv -\frac{w}{f'} = \frac{g_2}{g_1} \equiv V^*. \tag{5}$$

When (5) is satisfied, the monetary value of the extra time saved per marginal fatality, $V \equiv w/f'$, is just equal to the marginal rate of substitution between monetary travel costs and fatalities, $V^* \equiv g_2/g_1$.

The marginal rate of substitution between monetary travel time costs and fatalities, $V^* \equiv g_2/g_1$, is often called the VSL (see esp. Thaler and Rosen 1976; Viscusi 1993). This interpretation is derived from the fact that increases in speeds that decrease the cost of travel time per incremental fatality by more (less) than V^* will decrease (increase) the full costs of travel. A driver who minimizes the full cost of travel would correspondingly increase (or decrease) speeds according to whether the monetary value of time saved per fatality were greater or less than g_2/g_1, the implied monetary value of a life.

B. *Optimal Speed Limits*

The discussion above shows how an individual driver should determine his or her optimal speed, but it provides no rationale for the existence of speed *limits.* In fact, legally enforced speed limits are a result of the externality present because the probability of a fatality depends not only on a driver's own decision about the speed of travel but also on the decisions of other drivers.

It follows that the appropriate specification of equation (2) for individual *i* will depend on the speed limit (L) through its effect on the

ith driver's speed, but also on the risk of a fatality resulting from other drivers' responses to L. This is denoted as

$$g^i = g^i\left(c^i(L), f^i\left(\left(\frac{c}{w}\right)(L)\right)\right), \tag{2'}$$

where $c^i(L)$ indicates the effect of the speed limit on the ith driver's average cost of a mile traveled and $f^i((c/w)(L))$ shows how the ith driver's probability of a fatality depends on the speed limit L (through the vector of speeds $(c/w)(L)$). From the point of view of the ith driver, the optimal speed limit balances the decreased cost of a mile traveled against her increased fatality risk, which is satisfied when

$$V^i \equiv -\frac{w^i}{df^i/dL} = \frac{g_2^i}{g_1^i} \equiv V^{*i}. \tag{5'}$$

The key implication of this analysis of the social decision about speed limits is that the observed result reflects the VSL for the person whose views are reflected in the political process.

C. *The Value of a Statistical Life and Mandatory Speed Limits*

A key point of the previous discussion is that measures of the monetary value of time saved per fatality as a result of a speed increase do not provide a measure of the VSL, V^*. In general, such measures provide only an upper bound to the VSL.

Suppose, for example, that the determining driver/voter is offered the opportunity to increase the speed limit from $\bar{S}$ to $\bar{S}'$ through the political process. Associated with this offer is a decrease in the cost of travel time of $w\Delta h_i$, in location i, and an increase in the fatality rate of Δf_i, so that we may write

$$\begin{aligned} V_i &= -w\left(\frac{\Delta h_i}{\Delta f_i}\right) \\ &= \alpha + \beta Z_i + \epsilon_i, \end{aligned} \tag{6}$$

where Z_i and ϵ_i index observable and unobservable factors that make the effects of a speed limit increase more or less costly per fatality. The left-hand side of equation (6) is a discrete measure of V in equation (5).

We assume that the VSL, V^* in equations (5) and (5′), for the determining driver/voter in state i can be approximated by

$$V_i^* = \alpha' + \beta' X_i + \epsilon_i', \tag{7}$$

where X_i and ϵ_i' index observable and unobservable factors that influence the VSL. From the inequality (4), it follows that a higher speed limit will be adopted if $V_i > V_i^*$, for in this case the time costs saved by the increased speeds that result from the higher speed limit will be greater per fatality than the value of the determining statistical life, V_i^*. The probability that the higher speed limit is adopted is thus

$$\begin{aligned}\Pr(\text{adoption}) &= \Pr(V_i > V_i^*)\\ &= \Pr(\epsilon_i - \epsilon_i' < \alpha - \alpha' + \beta Z_i - \beta' X_i). \qquad (8)\end{aligned}$$

It is apparent that the average value of V among adopters, $E(V|\text{adoption}) = E(V|V > V^*)$, must be at least as great as $E(V^*)$, the unconditional average VSL among both adopters and nonadopters. Thus the measured average value of time costs saved per fatality from the adoption of an increased speed limit is generally greater than the mean VSL and provides an upper bound on that quantity. More generally, because the left-hand side of equation (6) is observed only for adopters, estimation of the parameters of equation (6) may suffer from selection bias.

To make further progress in estimation, we assume that ϵ_i and ϵ_i' are jointly normally distributed, so that (8) can be estimated by the probit function

$$\Pr(\text{adoption}) = F\left(\frac{\alpha - \alpha' + \beta Z_i - \beta' X_i}{\sigma}\right), \qquad (9)$$

where $\sigma = \sigma_{\epsilon-\epsilon'}$ is $[\text{Var}(\epsilon - \epsilon')]^{1/2}$ and $F(\cdot)$ is the cumulative unit normal distribution. It is apparent that even with this functional form assumption, it is possible to obtain only estimates of $(\alpha - \alpha')/\sigma$, β/σ, and β'/σ; the separate parameters in equations (6) and (7) cannot be identified from this probit function alone.

However, since V_i is observable, it is possible to estimate (6) by the usual selection-corrected regression methods (Heckman 1979). In particular,

$$E(V_i|\text{adoption}) = \alpha + \beta Z_i + \rho\sigma_\epsilon \lambda_i, \qquad (10)$$

where ρ is the correlation between ϵ and ϵ', $\lambda_i = \lambda(X_i, Z_i) = f(\mu' W_i)/F(\mu' W_i)$, and μ' consists of the vector $[\alpha - \alpha', \beta, -\beta']'$ and W_i the vector $[1, X_i, Z_i]$. The next subsection outlines how it is possible to obtain estimates of α' and β' and, in turn, V^*, the VSL, through the estimation of (9) and (10).

D. *Implementation of the Model to Obtain Estimates of* V *and* V*

In this subsection, we clarify the relationship between the conceptual framework and the subsequent empirical work. The empirical part of this paper is divided into two parts. In the first part, we estimate the mean value of the V_i's among adopters, which is the mean monetary value of time saved per marginal fatality associated with the decision to adopt a higher speed limit. From equation (6), it is evident that an accurate measure of the average V requires estimates of the mean wage rate and $\Delta h/\Delta f$, which is the derivative of hours spent traveling with respect to fatalities, with miles held constant. The measure of mean wages is straightforward to obtain, and we get it from the Current Population Survey. Section V is devoted to obtaining a reliable estimate of $\Delta h/\Delta f$ and then using it to infer the average value of V among adopters.

Many observers are likely to find the average V an interesting and policy-relevant parameter since the theoretical framework suggests that it can be interpreted as an upper bound to the VSL. However, the second, and more ambitious, goal of this paper is to infer V^*, the VSL. The key insight of our model is that the parameters of equations (9) and (10) can be used to derive an estimate of V^*. The estimation of (9) is straightforward and is done with a probit. The estimation of (10) requires measures of the V_i's, the state-specific estimates of the monetary value of time saved per marginal fatality.

To understand how V^* is obtained, recall that the fitting of equation (9) provides estimates of $(\alpha - \alpha')/\sigma$, β/σ, and β'/σ, and the fitting of (10) yields estimates of α and β. These estimated parameters can then be used to obtain α' and β', which determine V^* (recall eq. [7]). In particular, we use the following three expressions to solve for α' and β' (where the number in the subscripts indicates the equation that the parameter is estimated from):

$$\hat{\sigma} = \frac{\hat{\beta}_{10}}{(\widehat{\beta/\sigma})_9},$$

$$\hat{\beta}' = \hat{\sigma}\left(\widehat{\frac{\beta}{\sigma}}\right)_9,$$

$$\hat{\alpha}' = -\hat{\sigma}\left(\widehat{\frac{\alpha - \alpha'}{\sigma}}\right)_9 + \hat{\alpha}_{10}.$$

The mean VSL is then calculated as $\overline{V}^* = \hat{\alpha}' + \hat{\beta}'\overline{X}$, where $\overline{X}$ is the mean of the 1986 mean hourly wages in adopting states. Section VI reports on the empirical implementation of this approach and the resulting estimates of V^*.

II. Speed Limit Legislation and a New Approach to Estimating the VSL

A. *A Brief History of Speed Limits*

The first laws imposing restrictive speed limits on motor vehicles were passed in 1901 in Connecticut. With the exception of a Second World War emergency limit of 35 mph, the setting of speed limits remained the responsibility of state and local governments until 1974. In that year Congress enacted the Emergency Highway Energy Conservation Act in response to the perceived "energy crisis." This bill, intended as a fuel conservation measure, required, among other things, a national maximum speed limit of 55 mph. This new national speed limit was lower than the existing maximum daytime speed limit in all 50 states.

By 1976 the Federal Highway Administration began to enforce compliance with the national speed limit. Each state was required to measure compliance with the federal limit. States that did not enforce 50 percent compliance with the limit were penalized by a 10 percent reduction in federal highway funding. By 1987, dissatisfaction with the federally imposed (and enforced) national maximum speed limit led Congress to modify the law to permit states to set speed limits of 65 mph on rural interstate highways only. It seems likely that this dissatisfaction reflected the politically important driver's inability to optimally balance travel times and fatality rates.

Even with the end of the concern for fuel conservation, the national maximum speed limit was retained in some form until repeal in 1995. Despite opposition, especially from western states, much of the support for national speed limits may have resulted from the unintended impact that this law appeared to have on motor vehicle fatalities. Figure 1 shows the history of fatalities per 100 million vehicle miles of travel (VMT) for 1966–93. It is apparent that fatalities per mile traveled have been declining during this entire period, but the decline of 15 percent (nearly 10,000 fatalities) immediately following passage of the 1974 Emergency Highway Energy Conservation Act is the largest ever recorded in a single year and was widely remarked on at the time.

Before we proceed, some clarification of the legislative history may be necessary. In 1995, Congress eliminated entirely the federal regulation of speed limits. By the end of 1997, only three states maintained a 55-mph speed limit on rural interstates: 20 states had rural interstate speed limits of 65 mph, 16 were at 70 mph, 10 were at 75 mph, and Montana had no daytime speed limit, returning to its policy in 1973. Uniform and reliable data on this later period are unavailable at this time, and hence we do not examine these further changes in speed limits.

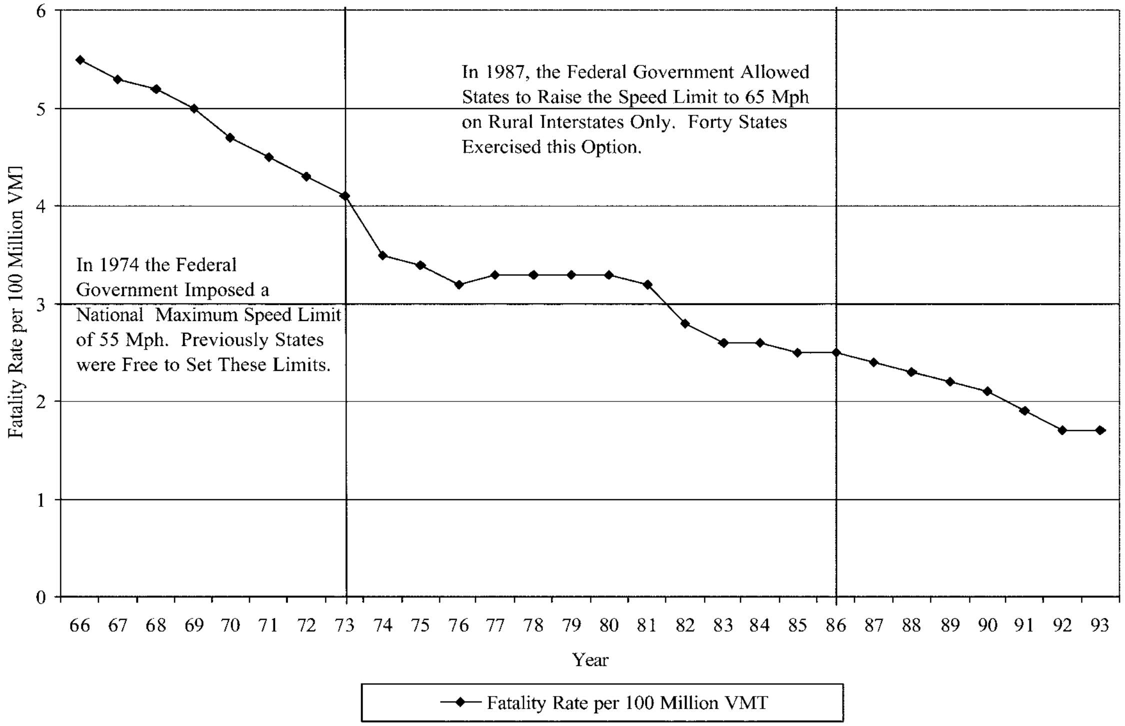

FIG. 1.—Trend in driving fatality rate on all road types, 1966–93

B. Research Design

By the end of 1987, 37 states had raised the maximum speed limit on their rural interstates and three more joined in 1988. Three states (including Washington, D.C.) had no rural interstate highways on which to adjust speed limits, and a final seven states maintained a 55-mph speed limit on all road systems into the 1990s.[5] Figure 2 graphically displays the location of the states that retained the 55-mph speed limit on rural interstates in the period following the 1987 legislation. It is apparent that these states are clustered closely together in the more densely populated and wealthy northeast section of the United States.

We use this institutional change to study the trade-off between the value of time saved and risk. We compare changes in fatality rates and speeds on rural interstates across states that did and did not adopt the 65-mph limit. In view of the geographic clustering documented in figure 2, it is possible that such comparisons would capture effects that were a result of geography only. As a possible solution to this problem, we exploit the fact that speed limits were permitted to increase only on rural interstates and make comparisons within states between the changes on rural interstates and those on other highways.

C. Do the Adoption Decisions Reflect the Median Driver's/Voter's Preferences?

This paper's goal is to use empirical estimates of how a change in speed limits affected fatality rates and speeds to infer the VSL. In order to apply this interpretation, it is crucial to understand whose values are reflected in the adoption decision. Our operating hypothesis is that the decisions reflect the preferences of the median driver/voter. The validity of this hypothesis rests on two assumptions: (*a*) lawmakers were aware of the trade-offs associated with a higher speed limit, and (*b*) the legislators' adoption decisions were socially optimal given the median driver's/voter's preferences. These assumptions cannot be subject to a rigorous test, but this subsection informally examines their validity.

There is ample anecdotal evidence that legislators were informed about the likely consequences of a change in the speed limit on fatality rates and speeds. First, it was well understood that speed can increase the incidence of accidents by reducing reaction times and the severity

[5] The seven states that maintained the 55-mph speed limit on rural interstates were Connecticut, Maryland, Massachusetts, New Jersey, New York, Pennsylvania, and Rhode Island. Three states (Delaware, the District of Columbia, and Hawaii) did not have roads classified as rural interstates throughout this period. The remaining 40 states adopted the 65-mph limit. Alaska also adopted the 65-mph speed limit in 1988, but since its rural interstates are not comparable to those in other states, they are excluded from the figures above and the subsequent analysis.

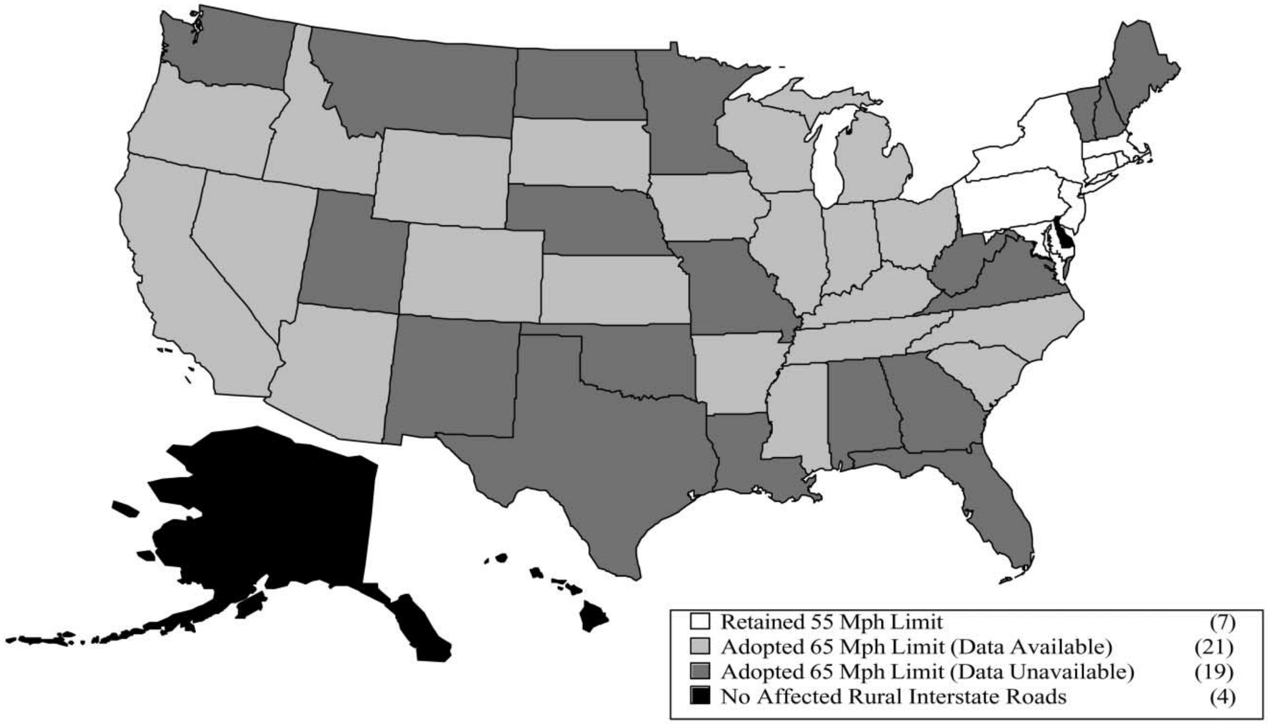

Fig. 2.—Adoption status and speed data availability, by state

of crashes because of the physical relationship between mass and speed to energy. Second, a 1984 National Research Council (NRC) report provided a review of the effects of the national maximum speed limit. The report concludes that the 55-mph speed limit was responsible for 3,000–5,000 fewer traffic fatalities annually (U.S. National Research Council 1984). Third, state governments also had access to more than a dozen studies of individual states' experiences with speed limits and roughly 10 studies from foreign countries. All of these studies were completed by the mid-1980s, and their estimates of the impacts of speed limits were similar to those produced by the NRC.

It also seems sensible to assume that state legislators understood that raising the speed limit would increase mean speeds on rural interstates. The NRC report documented a sharp decrease in mean speeds on these same roads after the 1974 legislation. Further, it is likely states believed that they could manage the magnitude of this increase through state troopers' policing policies. Consequently, it seems reasonable to assume that legislators were aware of the trade-offs that they were choosing for their constituents.

The more difficult question is whose preferences determined the decision to adopt the 65-mph speed limit on rural interstates. The conclusion of an Indiana Department of Transportation report on speed limits sheds some light on this issue:

> Speed limits represent *trade-offs between risk and travel time* for a road class or specific highway section reflecting an appropriate balance between the *societal goals of safety and mobility*. The process of setting speed limits is not merely a technical exercise. *It involves value judgments and trade-offs that are in the arena of the political process.* [Khan, Sinha, and McCarthy 2000, p. 144; italics added]

This quote highlights that states were aware of the trade-off associated with the choice of a speed limit and that they at least claim to maximize societal welfare.

Black (1948) shows that in the absence of nonpolitical frictions, the driver/voter whose interests are reflected in the social decision is likely to be positioned in the center of the distribution of preferences for safety, since no other decision will be more politically acceptable.[6] Although it is surely unlikely that decisions about speed limits reflect precisely the center of the distribution of driver/voter preferences,[7] it

[6] Black (1948) requires the assumptions that choices are one-dimensional and preferences are single-peaked.

[7] Levitt (1996) finds that less than one-quarter of the weight in U.S. senators' decision function is devoted to voter preferences.

seems less likely that these decisions reflect the preferences of extreme members of the population. Since drivers are the recipients of both the benefits (in reduced driving times) and the costs (an increased fatality risk), we think that our results are far more likely to reflect the center of driver/voter preferences than is the case for many other public decisions.

It is possible, of course, that this is not the case. For example, the adoption decision could be determined by a particular industry (e.g., trucking or insurance). However if states' initial adoption decisions were in conflict with driver/voter preferences, then one might expect the decisions to be reversed over time. Interestingly, none of the states exercised this option. Further, the results from the estimation of equation (9) for the probability of adoption (reported below) support the hypothesis that the decision to adopt is more likely when it is in the interests of the median driver/voter. Consequently, we proceed with the unverifiable assumption that the empirical results should be interpreted as an analysis of the preferences of the median, or politically representative, driver/voter.

III. Data Sources and Description

A. *Data Collection*

Our data on vehicle miles traveled, fatal accidents, and vehicle speeds come from several sources and reflect considerable effort. Vehicle miles traveled are readily available by state and road type from the Federal Highway Administration's *Highway Statistics* (various issues, table VM-2). These data are collected by taking annual average traffic counts on segments of highways between two entry or exit points and multiplying the traffic counts by the length of the highway segments.

Fatalities are available from the Fatal Accident Reporting System, which provides a census of all fatal vehicle crashes in the United States. This reporting system is maintained by the National Highway Traffic Safety Administration and is based on information obtained from state agencies on all accidents involving motor vehicles traveling on public highways that result in the death of one or more persons (U.S. Department of Transportation 1996).

Prior to 1982, speeds were monitored by radar. Since that time, speed has been monitored primarily with wire loops embedded in highway pavement. Loop monitors are difficult to detect, are not used for enforcement, provide better nighttime speed monitoring, and are generally more consistent across locations than radar monitors. We therefore confine our analysis of speeds (and fatalities) to the period since 1982.

Collecting data on travel speeds is considerably more difficult. From 1976 through 1994, the Federal Highway Administration required states to monitor speeds on highways that were posted at 55 mph. However, because the provisions requiring the reporting of speeds pertained only to highways posted at 55 mph, many states that increased speed limits on rural interstates discontinued collection of speed data on these roads in 1987.

Some of the states that adopted the 65-mph speed limit did continue to collect speed data after 1987 but did not report them to the federal government. We contacted all state departments of transportation and asked for whatever data existed on rural interstate speeds. Twenty-one of the 40 states that increased rural interstate speed limits provided the necessary data, and these data are the basis for our analysis.[8] The seven states with rural interstate roads that retained the 55-mph speed limit continued to collect speed data in accordance with federal regulations.

Figure 2 displays the geographic location of the states that increased their rural interstate speed limits. The 21 states for which we have been able to obtain data on the speeds actually traveled before and after 1987 are colored light gray, and the other adopters are dark gray. It is apparent that the adopting states for which we do have data are widely dispersed across the United States. Nevertheless, we suspect that the states for which data are not available may have been those that were most anxious to eliminate speed enforcement and reporting requirements. This suggests that the observed increases of speed in the states that we can monitor may be smaller than what occurred in the states that we could not monitor. Unless the relationship between fatalities and speeds is different in states that we can and cannot monitor, however, this should not affect our empirical analysis.

B. *Summary Statistics*

Table 1 provides some of the basic descriptive statistics for our data from the years 1982–86 before states had the option to increase the speed limit to 65 mph on rural interstates. Column 1 reports summary information for the states that adopted the 65-mph speed limit on rural interstates and provided postadoption data on speeds, and column 2 summarizes the data from the states that retained the 55-mph speed. Column 3 provides information for the states that adopted the higher limit but did not furnish post-1987 speed data. Of special interest are

[8] The 21 states that adopted the 65-mph speed limit and provided post-1987 speed data are Arizona, Arkansas, California, Colorado, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Michigan, Mississippi, Nevada, North Carolina, Ohio, Oregon, South Carolina, South Dakota, Tennessee, Wisconsin, and Wyoming.

TABLE 1

Sample Statistics for 1982–86 for States That Were Eligible to Raise the Speed Limit on Rural Interstates in 1987

	States Included in the Analysis		Excluded States
	Adopted 65 mph (1)	Retained 55 mph (2)	Adopted 65 mph (3)
	A. All States		
Number of states	21	7	19
1986 hourly wage (1997$)	$12.33	$13.97	$12.33
1986 rural interstate traffic density	.0480	.0945	.0450
	B. Rural Interstates		
Fatality rate	1.423	.957	1.592
Speed (mph)	59.6	59.3	60.2
Percentage of statewide VMT	9.3	5.8	9.8
Percentage of statewide traffic fatalities	5.0	2.5	5.7
	C. Urban Interstates		
Fatality rate	.887	.843	1.200
Speed (mph)	56.9	57.9	56.6
Percentage of statewide VMT	12.4	13.5	11.1
Percentage of statewide traffic fatalities	4.2	5.2	4.9
	D. Rural Arterials		
Fatality rate	3.785	3.195	4.000
Speed (mph)	55.4	53.7	54.9
Percentage of statewide VMT	16.2	14.2	18.5
Percentage of statewide traffic fatalities	23.4	20.8	27.2
	E. Statewide Totals		
VMT	374.2	446.4	315.1
Traffic fatalities	979.3	972.6	859.6
Fatality rate	2.617	2.179	2.729

Note.—Unless otherwise noted, the entries are the means across the five years preceding the passage of the 1987 legislation (i.e., 1982–86) that allowed states to raise the speed limit to 65 mph on rural interstates. Traffic density is measured as miles of paved road lanes per 100 million vehicle miles of travel (VMT) and is available only for rural interstates. The mean hourly wage in 1986 is calculated from the 1986 Current Population Survey Outgoing Rotation Group. It is calculated from all workers who report an hourly wage greater than $2.50. The survey top-codes the hourly wages of workers who are paid on an hourly basis at $99.99. We constructed an hourly wage for workers who are not paid hourly and also top-coded it at $99.99. The fatality rate is calculated as the number of fatalities per 100 million VMT, and speed is the mean traveling speed of vehicles. Both the fatality rate and speed entries are calculated as the weighted mean, where the weight is the VMT on the relevant road type. The hourly wage, rural interstate traffic density, VMT, and traffic fatality entries are the means across states within each category. See the text and fig. 2 for the identity of the states in each category.

comparisons among the states that adopted the 65-mph speed limit and the states that retained the 55-mph speed limit.

Panel A reports the number of states in each category, the 1986 mean hourly wage rate, and the 1986 mean traffic density on rural interstates. It is evident that, on average, adopting states had lower wage rates and lower traffic densities in 1986. Interestingly, the wage rates and traffic densities are virtually identical in the two sets of adopting states in columns 1 and 3. These entries suggest that traffic densities and wage rates may be related to the probability that a state will adopt the 65-mph speed limit, and we explore this possibility in more detail below.

Panels B–D report information on fatality rates, speeds, and shares of VMT and fatalities for three categories of roads from the years 1982–86. The mean fatality rate and speed entries are calculated as the weighted mean, where the weight is VMT. The three road categories are rural interstates, rural arterials, and urban interstates. These latter two road types are chosen because they generally have speed limits of 55 mph and design features that closely resemble those of rural interstates.[9] Importantly, states were *not* allowed to raise the speed limit on these roads in 1987. It is appealing to have data on these roads from adopting states because they provide a means to control for time-varying state-specific factors that might otherwise be confounded with the introduction of the 65-mph speed limit.

In the rural interstate panel (panel B), it is evident that states that adopted the 65-mph speed limit had substantially higher fatality rates, on average. Moreover, mean speeds were modestly higher in these states as well. Notably, these findings are true for both sets of 65-mph states. To the extent that level differences predict changes, these differences demonstrate the importance of the availability of intrastate comparisons. The entries also reveal that rural interstates are safer than the average road because they account for approximately 6–10 percent of VMT but only 2.5–6 percent of all traffic fatalities.

A few other regularities emerge from the table. First, the fatality rate on urban interstates (panel C) and rural arterials (panel D) is greater in adopting states than in nonadopting ones. Second, average speeds are highest on rural interstates, although they are similar on all three road types. Third, rural and urban interstates are substantially safer than rural arterials, and the fatality rates on these roads are much lower than the statewide fatality rate (see panel D). Fourth, fatalities on rural interstates constitute a small fraction of total fatalities, and any change in

[9] Both rural and urban interstate roads have multiple lanes, with traffic separated by direction and controlled access. Rural arterials generally have one lane in each direction, but they have wide lanes and shoulder lanes. Access to them is less controlled than access to interstates, but more than to any other type of road (see U.S. National Research Council 1984).

fatality rates on these roads will have only a modest impact on total traffic fatalities.

C. Unadjusted Estimates of the Effects of the 65-mph Speed Limit

Figure 3 presents annual mean fatality rates on rural interstates for 1982–93 for states that adopted the 65-mph speed limit with available speed data (the solid line) and states that retained the 55-mph limit (the dashed line).[10] The figure confirms the finding in table 1 that, prior to 1987, fatality rates were higher on rural interstates in adopting states. It also demonstrates that they were declining in both sets of states during these years.

Importantly, the figure shows that after the higher speed limit was introduced, fatality rates in the 55-mph states continued their downward trend. In contrast, fatality rates remained roughly constant in adopting states. Thus the figure suggests that the 65-mph speed limit is associated with a substantial relative increase in fatality rates. It also reveals substantial year-to-year variability, or noisiness, in fatality rates, which highlights that a long panel data file, such as the one used in this paper, is necessary for precise inferences.

Figure 4 provides an analogous depiction of trends in annual mean speed in the two sets of states. From 1982 to 1986, average speeds were approximately equal in the adopting and nonadopting states, although they were trending up at a modestly greater rate among adopters. The most striking feature of the figure is the upward mean shift in average speeds that is immediately visible in 1987.[11] This mean shift cannot be explained by a continuation of the pre-existing trends. Since average speeds were higher than the permitted 55 mph when the new limit was adopted, it is not surprising that the increase in vehicular speeds was considerably less than the 10 mph that some might have expected. Moreover, traffic density may also have limited the increase in speeds.

Simple analyses of the impact of adopting the 65-mph speed limit on rural interstates are reported in panel A of table 2. Column 1 reports the raw unadjusted difference in differences estimator of the effect of the 65-mph speed limit on fatality rates and speeds. This is calculated as the difference between the mean fatality rates and speeds between adopters and nonadopters for 1988–93 (i.e., the "postperiod") minus

[10] The annual mean fatality rates are calculated as the weighted mean in adopting and nonadopting states, respectively, where the weight is VMT. An unweighted version of this figure leads to the same qualitative conclusions.

[11] In fig. 3, the relative increase in fatality rates is not evident until 1988, but in fig. 4 the increase in speeds is observable in 1987. This difference arises because most states initially applied the 65-mph limit during the summer of 1987 and speed data are collected on the calendar year, whereas fatality data are based on the federal fiscal year that ends on September 30.

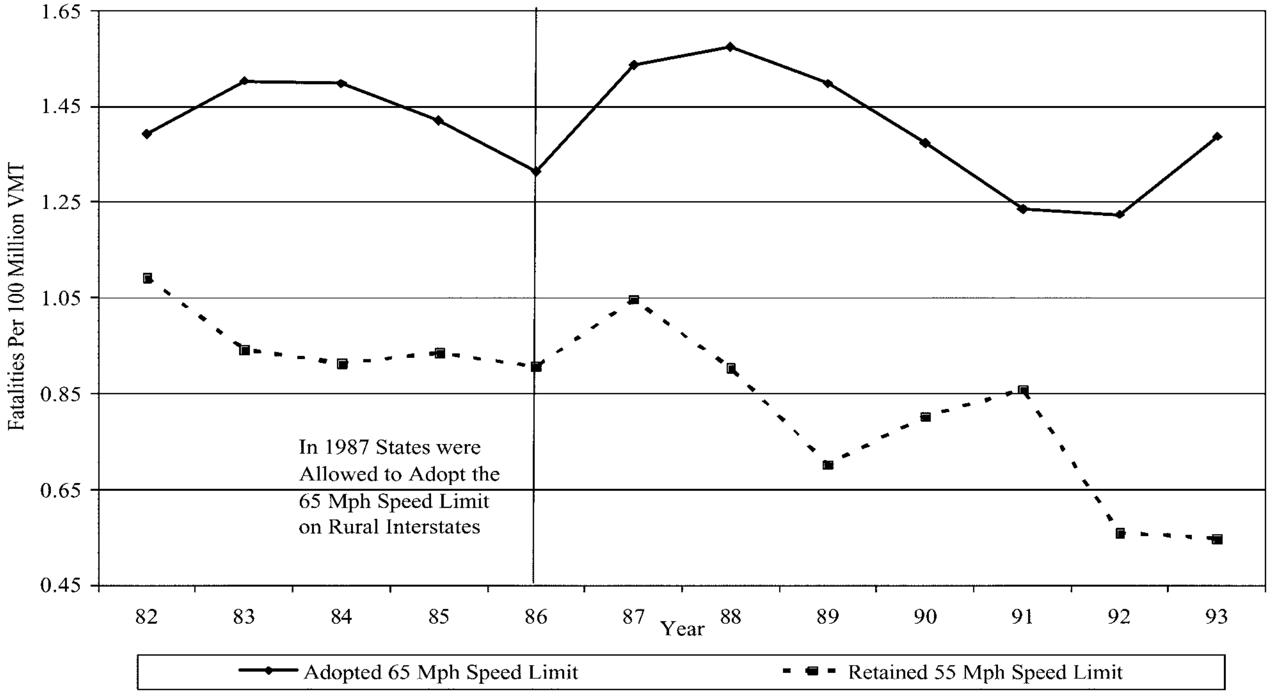

FIG. 3.—Trends in fatality rates on rural interstate roads, by adoption of 65-mph speed limit, 1982–93. The fatality rate is calculated as the weighted mean of the number of fatalities per 100 million VMT, where the weight is VMT.

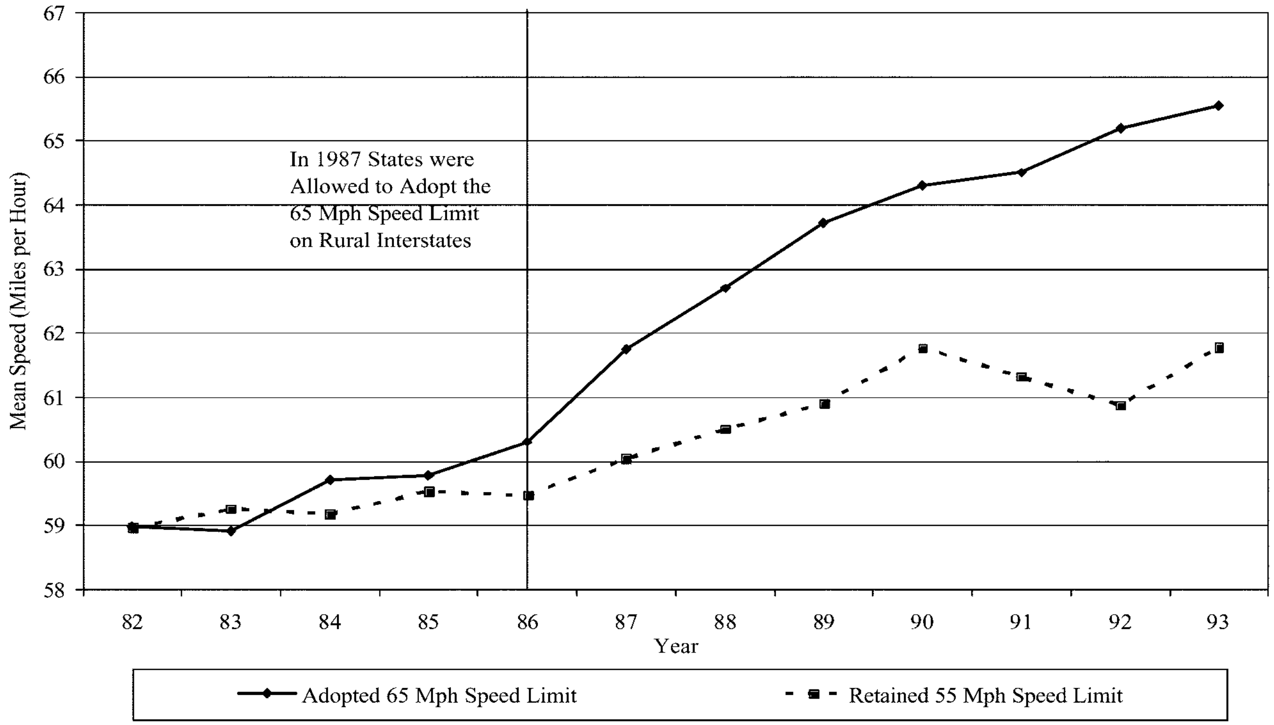

FIG. 4.—Trends in mean speeds on rural interstate roads, by adoption of 65-mph speed limit, 1982–93. Mean speed is calculated as the weighted mean, where the weight is VMT.

TABLE 2
DIFFERENCE IN DIFFERENCES (DD) ESTIMATES OF 65-MPH SPEED LIMIT ON FATALITY RATES AND SPEEDS

	DD of Levels (1)	DD of Levels Normalized by Preperiod Level in Adopting States (%) (2)	DD of Natural Logarithms (3)
	A. Rural Interstates (Affected Road Type)		
Fatality rate	.185	.130	.311
Speed	2.8	.047	.045
	B. Urban Interstates (Unaffected Road Type)		
Fatality rate	−.052	−.059	−.063
Speed	−.5	−.009	−.009
	C. Rural Arterials (Unaffected Road Type)		
Fatality rate	−.123	−.032	.005
Speed	.5	.009	.008

NOTE.—See the note to table 1. The entries represent three difference in differences estimates of the effects of the 65-mph speed limit on fatality rates and speeds. The col. 1 entries are the raw difference in differences estimates. In col. 2, the col. 1 entries are normalized by the preperiod level in adopting states. The col. 3 entries are calculated with the mean of ln(fatality rate) and ln(speed) for adopters and nonadopters in the pre- and postperiods. The entries are equal to the post − pre difference of weighted means among adopters minus the post − pre difference of weighted means among nonadopters, where the weight is VMT. The preperiod is defined as 1982–86 and the postperiod as 1988–93.

the same difference for 1982–86 (i.e., the "preperiod"). This estimator suggests that the adoption of the 65-mph speed limit increased fatality rates by 0.185 and speeds by 2.8 mph on rural interstates.[12]

The same analysis applied to the data on urban interstate and rural arterial roads is contained in panels B and C of table 2. These entries indicate that fatality rates decreased more in adopting states than in nonadopting states on both categories of roads.[13] When these declines are viewed in the context of the 1982–86 levels, they appear modest. If these relative declines in adopting states are due to an unobserved factor common to all roads in these states (e.g., the passage of a traffic safety law), then these roads should be used as intrastate comparisons. It is evident that controlling for this decline will increase the magnitude of

[12] Lave and Elias (1997) argue that the 65-mph speed limit induced an increase in fatalities on rural interstates *and* a decrease on other roads so that statewide the 65-mph speed limit did not increase fatalities. Greenstone (2002) reexamines this hypothesis and is unable to find evidence that the 65-mph speed limit caused a decrease in fatality rates on other roads.

[13] The difference in differences estimators of the effects of the 65-mph speed limit are generally insensitive to weighting. The principal exception is the effect on urban interstate fatality rates. In the unweighted case, the fatality rate declines by 0.203 more in adopting states. Greenstone (2002) found a similar relative decline on *noninterstate* roads in urban areas, which suggests that there is an unobserved factor (e.g., an improvement in the safety of the fleet of cars) that caused the larger declines on all urban roads in adopting states.

the estimated effect of the 65-mph limit on fatalities. The subsequent analysis presents estimates that do and do not use these roads as controls.

Average speeds increased at about the same rate in adopting and nonadopting states on these road types. Interestingly, the speed data contradict the popular "spillover" hypothesis that higher speed limits on one road cause drivers to increase their driving speed on all roads.

Columns 2 and 3 present two different unadjusted difference in differences estimators that aim to put the magnitude of the changes in some context. The column 2 entries are the raw difference in differences estimates from column 1, normalized by the preperiod level in adopting states. In the case of rural interstates, they indicate that the adoption of the higher limit is associated with increases of 13.0 percent in the fatality rate and 4.7 percent in speed. The changes in fatality rates and speed are −5.9 percent and −0.9 percent on urban interstates and −3.2 percent and 0.9 percent on rural arterials, respectively.

Although calculations such as those in column 2 are common, their shortcoming is that they are sensitive to the choice of denominator when there are preperiod differences in the levels. For example, the increase in the fatality rate on rural interstates is approximately 50 percent larger when the raw difference in differences estimates are normalized by the preperiod level in nonadopting states. The reason is that the preperiod fatality rate is roughly 50 percent higher in adopting states (i.e., 1.423 vs. 0.957). Since the preperiod speeds on rural interstates are approximately equal, this issue is not relevant for the difference in differences speed estimates presented in column 2. Overall, the sensitivity of the conclusions to the choice of denominator is a substantial limitation of this method.

Column 3 reports the results of the application of the difference in differences estimator applied to the ln transformation of the raw state by road type by year data. The ln difference approach is the only measure of relative change that is symmetric, additive, and normed (Tornqvist, Vartia, and Vartia 1985). In contrast to the column 2 estimates, these estimates are independent of the units of measurement (and differences in the preperiod levels), so they are our preferred ones. These estimates of the effects of the 65-mph limit on rural interstates are increases of 0.311 and 0.045 ln points for the fatality rate and speed, respectively. The estimated changes on urban interstates are −0.063 and −0.009 ln points and 0.005 and 0.008 on rural arterials.

In general, the entries in columns 2 and 3 are approximately equal. The most glaring exception is the case of the fatality rate on rural interstates, where the difference in the estimators is due to the differences in the preperiod levels.[14] This is troubling because our estimate

[14] Differences in the preperiod means are also relevant for the calculation of the fatality

of the trade-off between the monetary value of time and fatalities is proportional to changes in the fatality effect. Consequently, functional form assumptions are more important than we would like. The preferred results use the ln transformation because this method is independent of the preperiod differences. For the interested reader, the subsequent analysis also reports results when the fatality rates and speed are untransformed.

IV. Econometric Framework

This section discusses the econometric models used to estimate how individuals trade off time spent traveling against the probability of a fatality. The equation of interest is

$$\ln(\text{hours of travel})_{srt} = \beta \ln(\text{VMT})_{srt} + \theta \ln(\text{fatalities})_{srt} + v_{srt}, \quad (11)$$

where $v_{srt} = \alpha_{sr} + \eta_{rt} + \mu_{st} + \nu_{srt}$. Here, s references state, r indicates road type, and t indexes year. The dependent variable is the natural logarithm of hours of travel (i.e., the reciprocal of the average speed multiplied by VMT), and $\ln(\text{VMT})_{srt}$, the natural logarithm of vehicle miles of travel, is a control. Thus the parameter of interest, θ, measures the elasticity of hours of travel with respect to fatalities, with VMT held constant.

As the specification of the error term indicates, there are a number of potential sources of bias. When the sample is limited to a single road type (e.g., rural interstates), it is possible to include unrestricted state–road type (α_{sr}) and road type–year (η_{rt}) effects. We would like to be able to include covariates (e.g., characteristics of the drivers and their cars) at the state–road type–year level, but to the best of our knowledge such data are not collected. As an alternative, we can nonparametrically control for all state-year (μ_{st}) factors when the sample includes multiple road types. Thus in the multiple–road type samples, the estimated elasticity is robust to permanent factors specific to these state-road combinations (e.g., the quality of the road or the average number of days in a year with unsafe driving conditions due to bad weather), transitory factors common to a road type (e.g., federal expenditures to improve rural interstates), and transitory factors common across road types within a state (e.g., state-level economic conditions, changes in state traffic safety laws, or changes in local preferences for travel times).[15]

effect on rural arterials, where the mean fatality rate was 18 percent greater in adopting states from 1982 to 1986.

[15] For example, Keeler (1994), Ruhm (1996), and Dee (1999) show that state-level alcohol taxes and minimum-age drinking laws may influence alcohol consumption and consequently traffic fatalities.

However, it is not robust to time-varying state–road type determinants of fatalities.

As a starting point, we fit equation (11) by ordinary least squares separately on data from rural interstates, urban interstates, and rural arterials. The specification includes state–road type and road type–year fixed effects. The estimated elasticities (and standard errors) from these regressions are -0.003 (0.007), 0.006 (0.005), and 0.005 (0.007), respectively. The estimated elasticity is nearly identical when the sample is expanded to include all three road types and the specification includes state by year effects.

These results indicate that in an ordinary cross-section regression, speeds and fatalities are virtually uncorrelated. Although this may seem puzzling at first, it is not very surprising. After all, people will choose to travel more slowly in order to reduce the likelihood of a fatality when a road is unsafe (e.g., because of poor weather).[16] This illustrates the difficulty of making causal inferences when there is no exogenous variability in the data. Overall, these results are consistent with the possibility that the estimated θ from an ordinary least squares regression is biased upward because of individuals' compensatory behavior.

One solution to this identification problem is to find a variable that causes changes in speed but does not affect fatalities, except through speed. A plausible instrument is whether the 65-mph speed limit was in force. In this case, the instrumental variable estimate of the elasticity of time with respect to fatalities is a simple function of two reduced-form relations, the effects of the 65-mph speed limit on fatalities and hours of travel:

$$\ln(\text{fatalities})_{srt} = \lambda_F \ln(\text{VMT})_{srt} + \Pi_F 1(\text{65-mph limit in force})_{srt} + \epsilon_{srt} \qquad (12a)$$

and

$$\ln(\text{hours of travel})_{srt} = \lambda_H \ln(\text{VMT})_{srt} + \Pi_H 1(\text{65-mph limit in force})_{srt} + \epsilon'_{srt}, \qquad (12b)$$

where $\theta_{\text{IV}} = \Pi_H/\Pi_F$.[17] The indicator variable $1(\text{65-mph limit in force})_{srt}$

[16] The absence of variation that is unrelated to unobserved factors may explain why previous research has been unable to establish a systematic relationship between speed and fatalities. See Lave (1985, 1989), Fowles and Loeb (1989), Levy and Asch (1989), and Snyder (1989).

[17] We used the Box-Cox method to find the transformation of y (i.e., the dependent variables) in the reduced-form equations so that y^{λ} is approximately normally distributed. In the case of eq. (12a), the estimate of λ is .5346 with a 95 percent confidence interval of {.3980, .6810}, so both the linear and ln transformations are rejected. In contrast, the null hypothesis that the ln transformation is correct cannot be rejected for eq. (12b).

is equal to one if the observation is taken from a road on which 65 mph is the posted speed limit.[18] The error terms are specified identically to v_{srt} from equation (11), so the estimation of these equations can include the same set of fixed effects.

Two sufficient conditions for the instrumental variables estimator (θ_{IV}) to provide a consistent estimate of the elasticity of time with respect to fatalities are $\Pi_F \neq 0$ and $E[1(\text{65-mph limit in force})_{srt}\ v_{srt}] = 0$. From table 2 and figure 3, it is evident that the first condition holds. The second condition requires that a state's decision to raise speed limits in 1987 be orthogonal to unobserved determinants of speed, conditional on the road type–year, state–road type, and state-year fixed effects. This latter condition cannot be tested.

Before we proceed, it is important to clarify the econometric goal of the estimation of equations (12a) and (12b). In particular, we aim to obtain the causal effects of the adoption of the 65-mph speed limit on fatalities and travel times. But these estimates are unlikely to be the structural relationships between speed limits and these two outcomes. We suspect that the reason is that the adoption of the higher speed limit is accompanied by other changes that are unobservable to us but are intended to affect these outcomes. For example, states may accompany the introduction of higher speed limits with alterations in state trooper behavior that limit the increase in speeds or fatalities or both. Further, the higher speed limit may induce changes in the variance of speed that could have an independent effect on fatality rates (Lave 1985).

The available data do not permit an investigation of whether such relationships underlie the estimates from these equations, but such an investigation is unnecessary for our purposes. The important issue is that the adoption of the higher speed limit provided the median driver/voter a trade-off between increased fatalities and reduced travel times, whatever the precise mechanism.

V. Estimates of *V*, the Monetary Value of Time Saved per Marginal Fatality

This section uses the econometric framework outlined above to estimate three causal relationships: the effect of the adoption of the 65-mph speed limit on fatalities, with VMT held constant; the effect of the adoption of the 65-mph speed limit on hours of travel, with VMT held constant; and the time saved per marginal fatality. The first two "reduced-form" relationships are of interest in their own right as part of an

[18] This variable is set to one in all years after 1987. For observations from 1987, it is equal to the fraction of the calendar year in which the 65-mph limit was in force.

TABLE 3
Proportionate (Log) Effect of the Adoption of the 65-mph Speed Limit on Fatalities, Controlling for the Observed Mileage, by Road Type

	Affected Road Type	Unaffected Road Types	
Sample	Rural Interstates (1)	Urban Interstates (2)	Rural Arterials (3)
	A. Annual Effects		
1982–86, 1987	−.098	−.203	−.062
	(.195)	(.174)	(.119)
	[165]	[162]	[162]
1982–86, 1988	.351*	−.223*	−.073
	(.165)	(.111)	(.050)
	[167]	[163]	[162]
1982–86, 1989	.473	−.062	.021
	(.259)	(.142)	(.071)
	[167]	[162]	[162]
1982–86, 1990	.268	.073	.181*
	(.163)	(.161)	(.090)
	[166]	[163]	[162]
1982–86, 1991	.202	−.097	.238**
	(.123)	(.135)	(.073)
	[166]	[163]	[162]
1982–86, 1992	.399**	−.012	.140
	(.162)	(.190)	(.087)
	[164]	[162]	[162]
1982–86, 1993	.493**	−.059	.113
	(.179)	(.154)	(.077)
	[165]	[162]	[162]
	B. Average Effect		
1982–93	.360**	−.056	.082*
	(.091)	(.073)	(.040)
	[326]	[327]	[324]

Note.—The entries are estimated regression coefficients (heteroskedastic-consistent standard errors) from an indicator for whether the 65-mph speed limit was in force in the state by year in models in which the dependent variable is the natural logarithm of the number of fatalities. These models include the ln of VMT and state–road type and year–road type fixed effects as covariates. The potential sample includes the 28 states (21 adopted) with rural interstates and for which speed data are available. The observation entries (in brackets) report the actual sample. There are slight differences in numbers across subsamples because of missing values.
* Significant at the 5 percent level.
** Significant at the 1 percent level.

evaluation of the effect of the adoption decision. In the context of our theoretical model, however, the third is the key structural relation and provides an upper-bound estimate of the VSL.

A. *Estimated Effects of the 65-mph Speed Limit on Fatalities and Speeds*

Table 3 provides our basic empirical estimates of the proportionate effect of the adoption of the 65-mph speed limit on fatalities from the fitting of equation (12a). For 24 separate regressions, the table reports the parameter estimate on the 65-mph speed limit indicator (i.e., Π_F), its heteroskedastic-consistent standard error (in parentheses),[19] and the number of observations (in brackets). In column 1, the sample is restricted to data from rural interstates. In columns 2 and 3, the sample comprises data from urban interstates and rural arterial roads, respectively. Since the speed limit was unchanged on these roads, these results are intended as a test of whether there are transitory, unobserved state-level factors that bias the estimates in column 1.

The groups of rows in panel A present estimates from separate regressions in which the sample includes data from 1982–86 and one of the years in which the higher limit was in force. The panel B estimates are obtained from the full sample and are an average effect across all years. The aim of panel A is to present year-by-year estimates of the effect of the adoption of the 65-mph limit. These may be interesting in their own right. Further, the credibility of the overall effect will be undermined if it is due to a minority of the years.

The results in column 1 of table 3 indicate that the adoption of the 65-mph speed limit is associated with a large increase in the fatality rate on rural interstates in all years for which it was in force for the entire year.[20] In three of the six cases the individual estimated effects would be judged statistically significant by conventional test criteria. Together, the annual effects appear to reveal an immediate and permanent mean shift in fatality rates. Panel B provides an estimate that, because it pools the data for all the years, is considerably more precisely determined than the separate effects estimated by year. This summary result indicates that the adoption of the 65-mph speed limit increased the fatality rate by about 36 percent (measured in ln points) on rural interstates.

In contrast, the results from urban interstates and rural arterials provide little evidence of a systematic change in fatality rates in adopting states after 1986. In individual years, some of the estimates are as large as the smallest estimates from the rural interstate sample (and statistically significant), but in general they are of a smaller magnitude (and are less likely to be judged statistically significant at conventional levels). Further, there is not a consistent pattern to the signs of the estimates either within a road type over time or across the two road types within a year. The overall effects indicate that in adopting states there was a relative change of -0.056 ln points in the fatality rate on urban interstates and a relative increase on rural arterials of 0.082 ln points. In-

[19] Henceforth, all the standard errors of the reported regression parameters are corrected for unspecified heteroskedasticity (White 1980).

[20] Our estimate of the 1990 fatality effect is 0.268 ln points, which translates to a 30.7 percent increase in fatalities. It is noteworthy that a U.S. Department of Transportation report that evaluated the impact of the 65-mph speed limit on fatalities in 1990 concluded that the fatality toll on rural interstates in adopting states was roughly 30 percent greater than it would have been in the absence of the increased speed limit (U.S. Department of Transportation 1992).

TABLE 4
Proportionate (Log) Effect of the Adoption of the 65-mph Speed Limit on Hours Required to Travel the Observed Mileage, by Road Type

	Affected Road Type	Unaffected Road Types	
Sample	Rural Interstates (1)	Urban Interstates (2)	Rural Arterials (3)
	A. Annual Effects		
1982–86, 1987	−.039*	−.014	−.047
	(.018)	(.018)	(.025)
1982–86, 1988	−.041**	−.002	−.006
	(.009)	(.011)	(.007)
1982–86, 1989	−.038*	.004	.007
	(.018)	(.014)	(.013)
1982–86, 1990	−.025	−.011	.002
	(.017)	(.016)	(.013)
1982–86, 1991	−.043**	−.012	.005
	(.017)	(.015)	(.010)
1982–86, 1992	−.057**	−.021	.003
	(.017)	(.019)	(.014)
1982–86, 1993	−.054**	−.024	.002
	(.015)	(.014)	(.016)
	B. Average Effect		
1982–93	−.041**	−.009	−.000
	(.007)	(.007)	(.007)

Note.—See the note to table 3. The entries are estimated regression coefficients (heteroskedastic-consistent standard errors) from an indicator for whether the 65-mph speed limit was in force in the state by year in models in which the dependent variable is the natural logarithm of the number of hours required to drive the observed mileage. These models include the ln of VMT and state–road type and year–road type fixed effects as covariates. The potential sample includes the 28 states (21 adopted) with rural interstates and for which speed data are available. The actual sample sizes are identical to the parallel regressions in table 3 and are reported there.
* Significant at the 5 percent level.
** Significant at the 1 percent level.

terestingly, the increase on rural arterials would be judged statistically significant at the 5 percent level. It is evident, however, that the use of either of these road types as a control will have only a modest impact on the estimated effect of the speed limit on rural interstate fatality rates.

Table 4 provides a precisely parallel analysis of the proportionate effect of the adoption of the 65-mph speed limit on the hours required to travel a mile (i.e., the reciprocal of the average speed multiplied by VMT). These are the results of fitting equation (12b). It is striking that in the rural interstate sample, all but one of the annual effects are statistically significant at the 5 percent level or better. When the data are pooled to include all years, the estimate indicates that the hours required to travel a mile decreased by about 4 percent (so speeds increased by about 2.5 mph) on rural interstates as a result of the adoption of the higher speed limit. Speeds on urban interstates and rural arterial roads were unaffected by the adoption decision. All these estimated effects are quite precisely determined in a statistical sense.

TABLE 5
Testing the Robustness of the Effect of the 65-mph Speed Limit on Fatalities and Travel Times

Sample	(1)	(2)	(3)
	A. Dependent Variable: Ln(Fatalities)		
Rural interstates only	…	.360**	…
		(.091)	
Rural interstates and urban interstates	.312**	.417**	.414**
	(.097)	(.117)	(.130)
Rural interstates and rural arterials	.244**	.278**	.269**
	(.070)	(.099)	(.098)
All three road types	.280**	.349**	.337**
	(.073)	(.101)	(.096)
	B. Dependent Variable: Ln(Hours of Travel)		
Rural interstates only	…	−.041**	…
		(.007)	
Rural interstates and urban interstates	−.030**	−.032**	−.031**
	(.007)	(.010)	(.007)
Rural interstates and rural arterials	−.041**	−.040**	−.033**
	(.006)	(.010)	(.008)
All three road types	−.036**	−.036**	−.033**
	(.006)	(.009)	(.006)
ln(VMT) × road type	yes	yes	yes
State–road type indicators	yes	yes	yes
Year indicators	yes	no	no
Year–road type indicators	no	yes	yes
State-year indicators	no	no	yes

Note.—The two panels present results from regressions in which the dependent variables are the ln of fatalities and ln of hours driving, respectively. The entries are the estimated regression coefficients (heteroskedastic-consistent standard errors) for an indicator that is equal to one for observations from rural interstates when the 65-mph speed limit is in force. Road types are pooled for the analysis in different ways, as shown by the row labels. The bottom of the table lists the controls in each of the specifications. Number of observations is 326 for the rural interstates only sample, 653 for the rural interstates and urban interstates sample, 650 for the rural interstates and rural arterials sample, and 977 for the all-three sample.
* Significant at the 5 percent level.
** Significant at the 1 percent level.

Table 5 examines the sensitivity of the estimated effects of the adoption of the 65-mph speed limit on fatalities and speeds, using various alternative specifications. Road types are pooled for the analysis in different ways, as shown by the row labels. In the multiple–road type samples, the specifications include an indicator for observations from *any* road in a state-year when the 65-mph limit was in force on that state's rural interstates. However, the reported coefficient comes again from an indicator that is equal to one for observations from rural interstates in a state-year when 65 mph was the posted limit.

Three different specifications are fit. The specification in column 1 constrains the year indicators to be equal across road types. In column 2, these year indicators are replaced with year–road type dummies. The specification in column 3 may be of particular interest because it includes state-year indicators.

These estimates provide some sense of the extent to which the quantitative magnitudes of the estimated effects are dependent on the precise specification of the econometric model. In principle, if the quantitative magnitude of the estimates is not affected by the precise specification of the model, then the pooling of the data may result in a more precise estimator. In fact, as the table indicates, the estimated effect of the adoption of the increased speed limit on fatalities is between 24 percent and 42 percent (measured in ln points) and is not very sensitive to the precise specification.[21] Likewise, the estimated effect of the adoption of the increased speed limit on hours required to travel a mile is between 3.0 percent and 4.1 percent, and it too is not very sensitive to the precise specification.[22]

B. *Estimates of the Trade-off between the Value of Hours Saved and Fatalities*

Table 6 presents the results from the estimation of two versions of equation (11). In both cases, an indicator for whether the 65-mph speed limit was in force is used as an instrumental variable for the fatalities variable. The results are obtained from the same 10 combinations of specifications and samples as in table 5. For each regression, the table reports the instrumental variable parameter estimate on the fatality variable, the heteroskedastic-consistent standard error in parentheses, and the implied estimate of the monetary value of the time saved per marginal fatality, *V*, in brackets.

In panel A of table 6, the dependent variable is ln(hours of travel) and the explanatory variable of interest is ln(fatalities). The specification includes ln(VMT) as a control, and its effect is allowed to vary by road type. Consequently, the reported parameters are the estimated elasticity of hours required to travel a mile with respect to fatalities. Since the estimated effect is exactly identified, the estimates from a particular

[21] Although speed data are missing for 19 of the 40 adopting states, these states' fatality data are available. Across a variety of specifications, the increase in fatalities on rural interstates in these adopting states was approximately 0.08 ln points higher than in other adopting states. This difference was generally significant at the 10 percent level, but not when stricter criteria are applied.

[22] We experimented with adding state-specific and state by road type–specific linear time trends to the specifications presented in table 5. For both fatalities and hours required to travel a mile, the exclusion of a state-specific time trend in cols. 1 and 2 is rejected by *F*-tests. In these specifications, the effect of the 65-mph speed limit is essentially unchanged, and its standard error is modestly smaller. Although an *F*-test rejects the exclusion of state by road type linear time trends in cols. 1, 2, and 3, this model appears to be "overparameterized." In particular, the standard errors on the indicator for the 65-mph speed limit increase by 50–100 percent. Nevertheless, the fatality effect remains approximately constant and statistically significant at the 5 percent level. The hours point estimate declines by roughly one-half and would not be judged significant at conventional levels.

TABLE 6
ESTIMATES OF THE MONETARY VALUE OF THE TIME SAVED PER MARGINAL FATALITY

Sample	(1)	(2)	(3)
	A. Functional Form I: Ln Transformation		
Rural interstates only	…	−.113** (.037) [$1.64 million]	…
Rural interstates and urban interstates	−.095* (.040) [$1.38 million]	−.076* (.034) [$1.11 million]	−.076** (.031) [$1.11 million]
Rural interstates and rural arterials	−.166** (.057) [$2.42 million]	−.146* (.066) [$2.12 million]	−.122* (.051) [$1.78 million]
All three	−.128** (.042) [$1.86 million]	−.103** (.041) [$1.50 million]	−.099** (.034) [$1.44 million]
	B. Functional Form II: Untransformed		
Rural interstates only	…	17.03* (7.67) [$5.92 million]	…
Rural interstates and urban interstates	25.64** (9.42) [$8.91 million]	16.39* (7.46) [$5.69 million]	8.65* (3.84) [$3.00 million]
Rural interstates and rural arterials	4.01** (.51) [$1.39 million]	8.25 (4.32) [$2.87 million]	7.88* (3.79) [$2.74 million]
All three	6.97** (1.16) [$2.42 million]	11.98* (5.06) [$4.16 million]	8.80** (3.57) [$3.06 million]
Year indicators	yes	no	no
Year–road type indicators	no	yes	yes
State–road type indicators	yes	yes	yes
State-year indicators	no	no	yes

NOTE.—See the note to table 5. For panel A, the entries report the results from regressions of ln(hours of travel) on ln(fatalities), where an indicator for whether the 65-mph speed limit was in force is an instrumental variable. ln(VMT) is a control, and its effect is allowed to vary by road type. For panel B, the entries report the results from regressions of speed on fatality rates, where an indicator for whether the 65-mph speed limit was in force is an instrumental variable. The equation is weighted by the square root of VMT. All specifications in panel A include ln(VMT) as a control, and its effect is allowed to vary by road type. The bottom of the table lists the other controls for each of the specifications. The entries are the parameter estimates and heteroskedastic-consistent standard errors (in parentheses) on ln(fatalities) and the implied monetary value of the time saved per marginal fatality, V (in brackets). The sample size for each of the regressions is reported in the note to table 5.
* Significant at the 5 percent level.
** Significant at the 1 percent level.

specification and sample are the ratio of the estimate in panel B of table 5 to the estimate in panel A from the same sample and specification. The V's are obtained by multiplying the relevant elasticity by the ratio of hours traveled to total fatalities on rural interstates in adopting states for 1982–86 (6.122 billion/5,187) and by the 1986 average wage rate in adopting states ($12.33 in 1997 dollars).

Across all the samples and specifications, the fitting of this equation yields a relatively narrow range of estimates of the elasticity and, in turn, V. The elasticities are between -0.076 and -0.166, implying that a 10 percent increase in fatalities is associated with a 0.76–1.66 percent reduction in travel times. These estimates would generally be judged significantly different from zero at conventional test levels.

The estimated V's range between \$1.11 million and \$2.42 million. Our preferred estimate of V is the weighted average of the estimates from the rural interstate only sample and the column 3 specification with all three road types in the sample, where the weight is the inverse of the standard errors on the elasticities. This is our preferred estimate because it is based on estimates that do and do not rely on the other road types to control for all unobserved time-varying state-specific factors, and a priori, it is not clear whether such controls are necessary. This summary measure of V is \$1.54 million. Other estimates will be higher or lower, depending on the precise parameters used, and some readers may prefer to make different calculations.

In panel B of table 6, mean speed is the dependent variable, and the explanatory variable of interest is the fatality rate. This equation is weighted by VMT. This approach is *not* our preferred one because the results are not independent of differences in preperiod levels (recall the discussion of table 2). The sensitivity of the estimates to differences in preperiod fatality rates is evidenced by the greater variability in the estimates of V as the sample is changed within each of the columns. In this untransformed case, the "first-stage" fatality effect is generally smaller (recall table 2), so the estimates of V are larger on average.[23] Overall, these estimates of V range from \$1.39 million to \$8.91 million.[24]

VI. Estimates of V^*, the Value of a Statistical Life

As noted in the conceptual framework, the estimate of V, the trade-off between the value of hours saved and a marginal fatality, provides only an upper bound to the VSL. In this section, we report on our efforts to empirically implement the theoretical model and obtain an estimate of the VSL, V^*.

We begin by obtaining state-by-state estimates of V for the 21 adopting states that provided speed data. Table 7 presents state-by-state estimates and standard errors of the effects of the 65-mph speed limit on fatalities and the time required to travel a mile (cols. 2 and 3) and the instrumental variable elasticity between time saved and fatalities (col. 4). These

[23] The "reduced-form" results are available from the authors on request.

[24] In order to obtain these estimates of V, the parameter estimates are converted into elasticities. This is done by multiplying the parameter estimates by the ratio of the mean fatality rate to the mean speed in adopting states during the 1982–86 period (1.423/59.6). The estimate of V is then obtained as described in the discussion of panel A of table 6.

TABLE 7

State by State Estimates of Monetary Value of Time Saved per Marginal Fatality

	Observations for All Three Regressions (1)	Fatality Effect (2)		Speed Effect (3)		IV Elasticity (4)		Estimated Value of Time Saved per Fatality[a] (5)
Arizona	274	.229	(.178)	−.055**	(.017)	−.239	(.215)	$1.92
Arkansas	274	.348	(.250)	.033**	(.010)	.094	(.072)	−$1.12
California	274	.139	(.123)	−.060**	(.012)	−.431	(.403)	$4.75
Colorado	274	.396*	(.179)	−.084**	(.013)	−.212*	(.089)	$2.31
Idaho	274	.335	(.279)	−.081**	(.011)	−.241	(.201)	$2.05
Illinois	274	.310*	(.155)	−.040**	(.008)	−.128	(.076)	$3.19
Indiana	205	−.039	(.271)	.002	(.012)	−.045	(.497)	$.70
Iowa	274	.505**	(.184)	−.053**	(.014)	−.106*	(.053)	$2.97
Kansas	274	.377	(.234)	−.042**	(.014)	−.113	(.077)	$1.96
Kentucky	251	.461*	(.214)	−.033	(.019)	−.071	(.049)	$1.24
Michigan	274	.591**	(.221)	−.019	(.015)	−.033	(.029)	$.99
Mississippi	251	.193	(.205)	−.015	(.017)	−.079	(.120)	$.76
Nevada	274	.261	(.253)	−.022	(.017)	−.082	(.098)	$.49
North Carolina	274	.612*	(.290)	−.037**	(.012)	−.061	(.035)	$1.09
Ohio	228	.553*	(.232)	.007	(.015)	.013	(.027)	−$.47
Oregon	274	.141	(.220)	−.048**	(.013)	−.340	(.536)	$5.41
South Carolina	274	.405	(.247)	−.040**	(.014)	−.099	(.077)	$1.68
South Dakota	267	.656*	(.274)	−.072**	(.013)	−.110*	(.049)	$1.92
Tennessee	274	.316*	(.147)	−.006	(.013)	−.020	(.043)	$.29
Wisconsin	274	.118	(.227)	−.039**	(.014)	−.330	(.609)	$9.71
Wyoming	250	.301	(.315)	−.017	(.016)	−.055	(.088)	$.50

Note.—See the notes to tables 5 and 6. The entries in cols. 2, 3, and 4 are estimates of Π_F, Π_H, and θ_{IV} and the associated heteroskedastic-consistent standard errors from the fitting of eqq. (12a), (12b), and (11), respectively. The sample for each of these regressions includes observations on rural interstates, urban interstates, and rural arterials from the seven states that retained the 55-mph limit and the state for which the estimate applies. The controls include the logarithm of VMT interacted with road type and state–road type, road type–year, and state-year fixed effects. Five states (Indiana, Kentucky, Mississippi, Ohio, and Wyoming) had missing speed data for one to three years during the period in which the 65-mph speed limit was in force. In the estimation of the state-specific parameters for these states, the observations from the states that retained the 55-mph limit were dropped in those years. Col. 1 reports the sample size after this restriction. The state-specific instrumental variable estimated values of time saved per marginal fatality are calculated by multiplying the instrumental variable elasticity (col. 3) by the state-specific ratio of hours traveled to total fatalities and the state-specific mean hourly wage.

[a] In millions of 1997 dollars.

* Significant at the 5 percent level.

** Significant at the 1 percent level.

estimates are obtained from fitting versions of equations (12a), (12b), and (11), respectively, that include the logarithm of VMT interacted with road type and state–road type, road type–year, and state-year fixed effects as controls. The sample for each of these regressions includes observations on rural interstates, urban interstates, and rural arterials from the seven states that retained the 55-mph limit and the state for which the estimate applies. The sample sizes are reported in column 1.

Finally, column 5 reports the state-specific estimates of V. They are calculated by multiplying the elasticities by the state-specific ratio of hours traveled to total fatalities on rural interstates and the state-specific mean hourly wage.

Although the parameter estimates from the state-specific regressions are relatively imprecise, they almost all have the expected sign. The estimated effects of the higher limit on fatalities range from -0.039 (Indiana) to 0.656 (South Dakota) ln points, whereas the speed effects range from 0.033 (Arkansas) to -0.084 (Colorado). Nineteen of the 21 instrumental variable elasticities have a negative sign, but only three of them would be judged statistically significant at conventional levels. The estimated values of time saved per marginal fatality (i.e., the V_i's) range from $-$\$1.12 million (Arkansas) to \$9.71 million (Wisconsin) (1997 dollars).

Table 8 reports on our efforts to use these estimated V_i's to obtain a structural estimate of V^*, the VSL. Panel A contains estimates from the probit model (recall eq. [9]) of the states' likelihood of adopting the higher limit. We assume that the hours saved per fatality will be lower the greater the traffic density in a state is, which implies that in the probit function, traffic density will also be negatively related to the probability that a state will adopt the 65-mph speed limit. We also suppose, in accord with some suggestive evidence surveyed by Viscusi (1993), that the VSL, V^* in equation (7), may be positively related to the average wage rate in a state. This implies that the average wage rate will have a negative effect on the probability that a state will adopt the 65-mph speed limit.

The probit function is fit to data on the probability of adoption of the 65-mph speed limit for the full 47-state sample. The results provide strong support for the hypothesis that adoption of the 65-mph speed limit is negatively related to the available time savings. This is suggested by the estimated coefficient on traffic density, which is roughly four times its estimated standard error. The estimated effect of the average wage has the sign anticipated and would be judged statistically significant at the 10 percent level, but not by stricter criteria. These results are consistent with the assumption of our model that states' decisions whether to adopt the higher limit reflect the preferences about benefits and costs of their median driver/voter.

TABLE 8
Recovering the Value of a Statistical Life, V^*

	(1)	(2)	(3)	(4)
	A. Probit Estimates for Probability of Adoption			
Constant	8.24**	8.24**	8.24**	8.24**
	(2.87)	(2.87)	(2.87)	(2.87)
Traffic density in 1986	−32.21**	−32.21**	−32.21**	−32.21**
	(8.00)	(8.00)	(8.00)	(8.00)
Mean wage in 1986	−.56	−.56	−.56	−.56
	(.32)	(.32)	(.32)	(.32)
	B. Estimates from Value of Time Saved per Fatality (i.e., V) Equation			
Constant	2.55**	2.46**	2.77**	2.37*
	(.78)	(.63)	(.92)	(.99)
Traffic density in 1986	−21.89	−23.13	−35.74	−32.21
	(20.97)	(13.42)	(26.92)	(23.52)
Inverse of Mills ratio	3.32	−.17	5.42	4.53
	(2.09)	(2.09)	(5.88)	(2.22)
Huber-White standard errors	yes	yes	no	no
Bootstrapped standard errors	no	no	yes	no
Weight by inverse of elasticity's standard error	no	yes	no	no
Median regression	no	no	yes	no
Robust regression	no	no	no	yes
	C. Value of a Statistical Life			
V^* (millions of 1997$)	$1.64	$1.50	$1.29	$1.03
	[$2.02]	[$1.19]	[$1.68]	[$1.50]

Note.—Panel A presents estimates and Huber-White standard errors (in parentheses) from the probit equation for the probability that a state adopted the 65-mph speed limit, which is eq. (9) in the text. This equation is estimated on the 47 states with rural interstates. Panel B presents estimates from the fitting of the V equation, which is eq. (10). Here, the sample comprises the 21 states that adopted the 65-mph speed limit and that had nonmissing mean speed data. The inverse of the Mills ratio is calculated with the results from the probit equation. These equations are estimated in two steps because estimates of V are unavailable for the 19 adopting states with missing speed data. The row labels at the bottom of panel B provide estimation details on the second step. Panel C presents the estimated value of a statistical life, V^*. It is calculated with the parameters from panels A and B as described in Sec. I*D*. The parameter estimate from the constant in a regression of the V_i's only on a constant is reported in brackets below the estimated V^*. The estimation details in the row labels in panel B apply to this simple regression as well.
* Significant at the 5 percent level.
** Significant at the 1 percent level.

Panel B presents estimates from modeling the monetary value of the time saved per marginal fatality, V. As specified in equation (10), the control variables are traffic density and the inverse of the Mills ratio, calculated using the probit results. Equations (9) and (10) are estimated in a two-step estimation procedure (rather than jointly) because we do not have estimates of V for 19 of the 40 adopting states, but we do want to use data from those states to estimate the probit (Heckman 1979).

In equation (10), the estimated V_i's are the dependent variable. The precision of these estimates depends on the precision of the estimates of the state-specific instrumental variable elasticities, mean wages, mean speeds, and VMT. An examination of the 21 estimated standard errors of the instrumental variable elasticities highlights that by itself the estimation of the elasticities introduces a great deal of imprecision. For example, although the mean of the standard errors of the 21 elasticities is 0.164, the standard errors associated with the Oregon and Wisconsin elasticities are 0.536 and 0.609, respectively. Interestingly, these states' estimated V_i's are the two largest at \$5.41 and \$9.71 million, and these two poorly estimated V_i's are likely to have an important influence on the estimation of equation (10). More generally, it is evident that the standard homoskedasticity assumption is unappealing.

We take a number of approaches to remedy this problem of heteroskedasticity. Column 1 is the result of unweighted least squares estimation, although the standard errors are corrected for unspecified heteroskedasticity (White 1980). Column 2 weights equation (10) by the square root of the reciprocal of the standard errors of the instrumental variable elasticities and corrects the standard errors for heteroskedasticity. Column 3 reports median regression results, and column 4 presents the results from a robust regression routine. This routine begins by excluding outliers, defined as observations with values of Cook's $D > 1$ and then weights observations on the basis of absolute residuals so that large residuals are down-weighted.[25] These estimation details are summarized in the rows at the bottom of the panel.

The results from modeling the V_i's from table 7 are disappointing. The sign of the parameter on the estimated inverse Mills ratio is positive in three of the four specifications, indicating a positive correlation between the unobserved determinants of the probability of adoption and V. However, it is apparent that our estimates of this equation are poorly determined. No doubt this is, in part, a result of the very imprecise estimates of V for individual states that make up the observations.

Nevertheless, panel C of table 8 concludes our exercise and lists the estimates of the VSL associated with each of the four specifications. We take the parameters from the fitting of equations (9) and (10) and follow the procedure outlined in Section I*D* to determine V^*. In brackets below the estimated V^*, we list the parameter estimate from a constant

[25] After the outlier observations are excluded, the routine obtains optimal weights for the remaining observations in an iterative process. This process begins with the estimation of the linear regression on the restricted sample and the calculation of the estimated residuals from this regression. These residuals are used to obtain weights so that observations with large absolute residuals are down-weighted. The regression is then fitted again using these weights, and the residuals from this new regression are used to derive a new set of weights. This iterative procedure continues until the change in weights is below some threshold. Huber weights (Huber 1964) are used until convergence is achieved, and then biweights (Beaton and Tukey 1974) are used until convergence is achieved with them. Street, Carroll, and Ruppert (1988) provide a method to calculate the standard errors. Also see Berk (1990) on robust regression.

TABLE 9
ADJUSTMENTS TO THE ESTIMATES OF V AND V^*

Adjust Hours Saved for the Average Number of Car Occupants in 1986	Adjust the Value of an Hour for nonwage Compensation in 1986: No	Adjust the Value of an Hour for nonwage Compensation in 1986: Yes
No	1.00	1.22
Yes	1.70	2.07

NOTE.—The estimated number of occupants per vehicle in 1986 is obtained by taking the mean of this variable from the 1983 and 1990 Nationwide Personal Transportation Surveys. These surveys are not representative at the state level, so we cannot calculate state-specific estimates of hours saved that account for the number of occupants per vehicle. The best estimate of nonwage compensation comes from the National Income and Product Accounts (NIPA) of the United States. Total compensation to employees was \$2.571 (in billions) in 1986. Wage and salary accruals accounted for \$2.114, and supplements to wages and salaries constituted the remaining \$0.456. The Current Population Survey Outgoing Rotation Group measure of wages is similar to the NIPA wage and salary accruals category, so we estimate that nonwage compensation was equal to approximately 22 percent of wage and salary income in 1986.

in a regression of the V_i's on only the constant. They are presented in order to allow for comparisons of our estimates of V^* to the mean and median of the V_i's from the different procedures. The estimation details in the bottom rows apply to this simple regression as well.

The estimates of the VSL range from \$1.03 million to \$1.64 million (1997 dollars). The relatively tight range of these estimates is reassuring. In three of the four cases, these estimates of V^* are less than the relevant estimates of the mean and median of the V_i's as our structural model predicts. However, we have not calculated a sampling error for these estimates of the VSL, but it would no doubt be very large.

VII. Interpretation

The estimates above of the VSL are based on our analyses of fatalities and driving time saved as a result of the introduction of the 65-mph speed limit. They have several conceptual limitations that deserve note. In general, any underestimation of driving time saved, or overestimation of fatalities incurred, will lead to a downward-biased estimate of the VSL. Likewise, any underestimation or overestimation of the economic value of driving time saved will lead to similar biases.

There are plausible reasons for believing that some further adjustments in the estimates may be useful. First, in our analysis we have assumed that each vehicle contains only one passenger, so that vehicle miles traveled are equivalent to passenger miles traveled. In fact, as indicated in table 9, the typical U.S. vehicle contains 1.7 passengers. If it could be assumed that the value of the time of each of these passengers was the same as the mean wage rate per state and if it could be assumed that the number of passengers per vehicle was the same in each state, it would be appropriate to simply adjust all our estimates of the VSL upward by this amount.

There are also reasons to believe that the wage rate may not be wholly appropriate as a measure of the value of the passenger time saved. In particular, the wage rate is only one part of total compensation. As table 9 indicates, total compensation is typically 22 percent greater than the wage rate. If it could be assumed that this additional compensation was variable by hour, then it would be appropriate to adjust our earlier estimates of the VSL upward accordingly.

On the other hand, there is a long tradition in the literature on the economic valuation of travel time that suggests the wage rate may be an overestimate of the value of travel time saved (Beesley 1973). Some of the most convincing evidence on this issue, however, is Deacon and Sonstelie's (1985) analysis of drivers' tolerance of waiting times (to obtain less expensive gasoline), which supports the use of the wage rate as a measure of the value of drivers' time.

At the present time, because of data limitations and the inherent subjectivity that would be added to our estimates, we do not think it is appropriate to simply modify the VSL estimates given above using the adjustment factors in table 9. With better data, however, this may be appropriate at some future time.[26]

Another issue of interpretation is that the increased speeds and fatality rates may be concentrated in particular subgroups of drivers. For example, the reduced travel times may disproportionately accrue to drivers of sports cars, and the increased fatalities may be concentrated among drunk drivers. If this is the case, the trade-off faced by the median driver/voter may differ from the one estimated here.

Finally, there now exist many estimates of the VSL both in the United States and for other countries; in fact, there are several surveys of these estimates, including those by Viscusi (1993, 2000), de Blaeij et al. (2000), and Blomquist (2001). As a general rule, these studies suggest that average valuations across studies typically fall in the range of $1.0–$5.0 million, although individual studies often provide estimates far outside this range. Our estimates virtually all fall within this range, and we think that they provide fairly strong evidence that the more extreme valuations sometimes reported are likely to be a result of one or more conceptual or econometric problems.

[26] Our estimates should also be adjusted for any change in the total costs of nonfatal injuries. Unfortunately, comprehensive data on nonfatal injuries and their costs are unavailable in this period.

VIII. Conclusion

Although subject to a number of limitations, our estimates of the value of a statistical life fall in the range between $1 million and $10 million, and our preferred estimates are much closer to the former than to the latter. We think that these estimates are a particularly credible indication of public attitudes toward the trade-off between wealth and fatality risk. First, these estimates are based on the exogenously offered opportunity for states to vote (and then retain) an increase in their rural speed limit and thereby save vehicle travel time at the risk of increased fatalities. Second, the nature of the risk/wealth trade-off poses a relatively simple public choice decision in which drivers/voters both receive the benefits and suffer the costs and in which agency problems and the resulting distortions are not likely to be strong. Finally, there is evidence that the state legislators were aware of the trade-offs involved and that the states that implicitly were offered better terms (greater hours saved per fatality) were more likely to accept the offer.

On the basis of these results, it appears that measuring the choices that result from exogenous changes in the trade-off between safety and wealth are the key to providing real progress in estimating the VSL. As would be expected, where drivers select their speeds and thus their fatality risks in response to road conditions, the simple cross-sectional correlation between speeds and fatality risks is negligible. On the other hand, both speeds and fatality risks increase in response to an exogenous change in speed limits, providing just the information critical to the credible measurement of the VSL.

Our results also indicate that there is room for much additional research in the valuation of safety risks. Recent years have seen the demise of all federally mandated speed limits and the evolution of considerable variability within states and across states in traffic safety legislation. In principle, this variability could be used to measure the terms of wealth/risk trade-offs at different levels of risk and provide better-informed discussions of public policies toward safety.

References

Atkinson, Scott E., and Robert Halvorsen. 1990. "The Valuation of Risks to Life: Evidence from the Markets for Automobiles." *Rev. Econ. and Statis.* 72 (February): 133–37.

Beaton, Albert E., and John W. Tukey. 1974. "The Fitting of Power Series, Meaning Polynomials, Illustrated on Band-Spectroscopic Data." *Technometrics* 16 (May): 147–85.

Beesley, Michael E. 1973. *Urban Transport: Studies in Economic Policy*. London: Butterworth.

Berk, Richard A. 1990. "A Primer on Robust Regression." In *Modern Methods of*

Data Analysis, edited by John Fox and J. Scott Long. Newbury Park, Calif.: Sage Pubs.

Black, Dan A., and Thomas J. Kniesner. 2003. "On the Measurement of Job Risk in Hedonic Wage Models." Manuscript. Syracuse, N.Y.: Univ. Syracuse.

Black, Duncan. 1948. "On the Rationale of Group Decision-Making." *J.P.E.* 56 (February): 23–34.

Blomquist, Glenn C. 2001. "Self Protection and Averting Behavior, Values of Statistical Lives, and Benefit Cost Analysis of Environmental Policy." Manuscript. Lexington: Univ. Kentucky.

Deacon, Robert T., and Jon Sonstelie. 1985. "Rationing by Waiting and the Value of Time: Results from a Natural Experiment." *J.P.E.* 93 (August): 627–47.

de Blaeij, Arianne, Raymond J. G. M. Florax, Piet Rietveld, and Erik Verhoef. 2000. "The Value of a Statistical Life in Road Safety: A Meta-Analysis." Discussion Paper no. 2000–089/3. Amsterdam: Tinbergen Inst.

Dee, Thomas S. 1999. "State Alcohol Policies, Teen Drinking and Traffic Fatalities." *J. Public Econ.* 72 (May): 289–315.

Domencich, Thomas A., and Daniel McFadden. 1975. *Urban Travel Demand: A Behavioral Analysis.* Amsterdam: North-Holland.

Dreyfus, Mark K., and W. Kip Viscusi. 1995. "Rates of Time Preference and Consumer Valuations of Automobile Safety and Fuel Efficiency." *J. Law and Econ.* 38 (April): 79–105.

Fowles, Richard, and Peter D. Loeb. 1989. "Speeding, Coordination, and the 55-MPH Limit: Comment." *A.E.R.* 79 (September): 916–21.

Ghosh, Debapriya, Dennis Lees, and William Seal. 1975. "Optimal Motorway Speed and Some Valuations of Time and Life." *Manchester School Econ. and Soc. Studies* 43 (June): 134–43.

Greenstone, Michael. 2002. "A Reexamination of Resource Allocation Responses to the 65-MPH Speed Limit." *Econ. Inquiry* 40 (April): 271–78.

Heckman, James J. 1979. "Sample Selection Bias as a Specification Error." *Econometrica* 47 (January): 153–61.

Hersch, Joni. 1998. "Compensating Differentials for Gender-Specific Job Injury Risks." *A.E.R.* 88 (June): 598–627.

Huber, Peter J. 1964. "Robust Estimation of a Location Parameter." *Ann. Math. Statis.* 35 (March): 73–101.

Keeler, Theodore E. 1994. "Highway Safety, Economic Behavior, and Driving Environment." *A.E.R.* 84 (June): 684–93.

Khan, Nisar, Kumares C. Sinha, and Patrick S. McCarthy. 2000. "An Analysis of Speed Limit Policies for Indiana." Joint Transportation Research Project, FHWA/IN/JTRP-99/14. West Lafayette, Ind.: Purdue Univ.

Lave, Charles. 1985. "Speeding, Coordination, and the 55-MPH Limit." *A.E.R.* 75 (December): 1159–64.

———. 1989. "Speeding, Coordination, and the 55-MPH Limit: Reply." *A.E.R.* 79 (September): 926–31.

Lave, Charles, and Patrick Elias. 1997. "Resource Allocation in Public Policy: The Effects of the 65-MPH Speed Limit." *Econ. Inquiry* 35 (July): 614–20.

Lee, N., and M. Q. Dalvi. 1969. "Variations in the Value of Travel Time." *Manchester School Econ. and Soc. Studies* 37 (September): 213–36.

Levitt, Steven D. 1996. "How Do Senators Vote? Disentangling the Role of Voter Preferences, Party Affiliation, and Senator Ideology." *A.E.R.* 86 (June): 425–41.

Levy, David T., and Peter Asch. 1989. "Speeding, Coordination, and the 55-MPH Limit: Comment." *A.E.R.* 79 (September): 913–15.

Ruhm, Christopher J. 1996. "Alcohol Policies and Highway Vehicle Fatalities." *J. Health Econ.* 15 (August): 435–54.

Snyder, Donald. 1989. "Speeding, Coordination, and the 55-MPH Limit: Comment." *A.E.R.* 79 (September): 922–25.

Street, James O., Raymond J. Carroll, and David Ruppert. 1988. "A Note on Computing Robust Regression Estimates via Iteratively Weighted Least Squares." *American Statistician* 42 (May): 152–54.

Thaler, Richard, and Sherwin Rosen. 1976. "The Value of Saving a Life: Evidence from the Labor Market." In *Household Production and Consumption*, edited by Nestor E. Terleckyj. Cambridge, Mass.: Columbia Univ. Press (for NBER).

Tornqvist, Leo, Pentti Vartia, and Yrjo O. Vartia. 1985. "How Should Relative Changes Be Measured?" *American Statistician* 39 (February): 43–46.

U.S. Department of Transportation. 1992. "Effects of the 65 MPH Speed Limit through 1990: A Report to Congress." Washington, D.C.: Nat. Highway Traffic Safety Admin.

———. Bureau of Transportation Statistics. 1996. *Fatal Accident Reporting System, 1975–94.* Traffic Safety CD-ROM. Washington, D.C.: U.S. Dept. Transportation.

U.S. National Research Council. Transportation Research Board. 1984. *Fifty-five, a Decade of Experience.* Washington, D.C.: Nat. Acad. Press.

Viscusi, W. Kip. 1993. "The Value of Risks to Life and Health." *J. Econ. Literature* 31 (December): 1912–46.

———. 2000. "The Value of Life in Legal Contexts: Survey and Critique." *American Law and Econ. Rev.* 2 (Spring): 195–222.

Waters, William G., II. 1996. "Values of Travel Time Savings in Road Transport Project Evaluation." In *World Transport Research: Proceedings of 7th World Conference on Transport Research*, vol. 3, *Transport Policy*, edited by David A. Hensher, Jenny King, and Tae Hoon Oum. Oxford: Elsevier Sci.

White, Halbert. 1980. "A Heteroskedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity." *Econometrica* 48 (May): 817–38.