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Highway Safety, Economic Behavior, and Driving Environment

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HIGHWAY SAFETY, ECONOMIC BEHAVIOR, AND DRIVING ENVIRONMENT

Theodore E. Keeler*

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Economic analysis has enhanced our understanding of the efficacy of highway safety regulations. Specifically, a consumer-theoretic literature has developed on drivers' responses to regulations, based on ideas first set forth by Lester Lave and W. E. Weber (1970) and more fully thought out by Sam Peltzman (1975). Meanwhile, an empirical literature has also developed, testing hypotheses relating to the effects on safety of speed limits, safety-device regulations, and alcohol policies, among other things.[1] Yet, despite extensive research, controversies remain as to the effects of regulations on highway safety.

This paper contributes to the literature on economic aspects of highway safety in an four important ways: First, it is based on a county-level data set for the U. S. for 1970 and 1980, affording over 2,600 observations each year in a consistent panel, many more observations than previous studies have used. Second, this study uses different and more appropriate estimation procedures than most previous studies, allowing for the count nature of highway fatalities and correcting for omitted-variable bias otherwise possible in cross-section analysis. Third, the model used here controls for more variables in a single model than previous studies. Finally, this study allows for important differences in the estimated coefficients between urban and rural driving environments.

The results shed new light on the effectiveness of regulatory policies on highway safety. Furthermore, the study contributes to the literature on health and safety, showing more clearly the effects of income and education the demand for safety: in particular, correction for "omitted variable" bias done in estimation here sheds new light on the controversy as to the relationship between education and health-enhancing behavior.

The first section is concerned with appropriate specification of the equations to be estimated and the data set used. The second section considers issues of estimation. The third section presents the results, and the fourth sets forth conclusions.

I. SPECIFICATION AND DATA

Form of equation. The probability that a member of a population will be killed in an auto accident during a given period is low; there is a finite count of fatalities in a given population. In such situations of regression with a small-count variable as dependent variable, a Poisson process best describes the dependent variable, suggesting a specification of this form:

$$Y_{it} = EXP(a_0 + \Sigma_j \, a_{jt} \, X_{ijt} + e_{it}) \quad (1)$$

In this equation, the Y_i are observations on the dependent variable (fatalities, described below), X_{ijt} is the i'th observation on the j'th explanatory variable for year t (t=1 for 1970 and t=2 for 1980) , and the e_i represent an econometric error term. Appropriate variables are discussed below and estimation of this equation is discussed in the next section.

The dependent variable. Motor vehicle fatalities would itself seem an appropriate variable. Standardization for county size (larger counties have more accidents) is achieved by dividing the dependent variable by population (in thousands).[2]

Safety device regulation and offsetting behavior. The advent of safety device regulation in the 1960 might be expected to reduce fatalities. This idea was challenged, however, first (among economists) by Lave and Weber (1970), and with a more complete argument by Peltzman (1975). In making a trip, a motorist is likely concerned with both the time it takes and his/her safety. If a regulatory policy (such as the requirement of a more crash-resistant car) is imposed making car travel safer, the motorist, by this theory, will likely offset the higher level of safety with faster driving, so that some of the enhanced safety of the car is used to provide a faster trip. Indeed, Peltzman shows it is possible that overall fatalities could actually rise as a result of the enhanced safety regulations. Our panel analyses of highway safety for 1970 and 1980 will allow us to test the extent to which regulation-mandated safety differences across counties and states lead to different safety levels[3]--a test of the offset hypothesis.

Offsetting behavior and the effects of other regulations. Although Peltzman and others have applied this analysis most especially to safety-device regulation (such as seat belts and airbags), it should be evident that it is applicable in other areas of auto safety regulation. For example, some offsetting behavior might be possible with speed limits (faster acceleration, braking, and cornering, as well as evasion, which is similar to but goes further than offsetting behavior). Similarly, vehicle inspection programs could induce offsetting behavior, as could other forms of regulation. We consider these regulatory variables next.

As a measure of speed, most previous studies have used the observed speed on rural roads as an exogenous variable, and possibly the variance in speed, as well. But observed speed is not an exogenous variable from the viewpoint of public policy. As a result, the present study analyzes the effects not of speed itself, but of publicly-imposed speed limits. Our focus will be on maximum speed possible on expressways and rural roads, since that is the object of the most controversy.[4] Use of a population density variable will control for situations in which urbanized counties have little opportunity to use the maximum speed, as will different slope coefficients for the speed limit between high- and low-density counties.[5]

There are other important regulatory variables affecting motor-vehicle safety. The existence of a state vehicle inspection program[6] has been found by some previous studies to have an effect, as has more frequent license renewal testing.

Yet another set of regulatory variables relates to alcohol. States control availability and price of alcohol, through taxes, licensing restrictions, price regulations, hours of sale, minimum drinking ages, and, in some cases, prohibition on the sale and consumption of alcoholic beverages. Minimum drinking age is controlled at the state level,[7] and data on it is readily available, so it is included in the equations as a variable, county by county (0 if the drinking age was 21 and 1 if it was lower).

A variable directly reflecting alcohol consumption is also appropriate. Initial work with the data indicated that total alcohol consumption is a better explanatory variable than is consumption of beer alone, and that is the variable used.[8]

Personal and economic variables. Previous analysts in the economics of health and safety have noted that income can have a positive or negative effect on safety (Victor Fuchs, 1974, Peltzman, 1975). Higher income implies, on the one hand, that the consumer can afford to invest in things which improve safety (such as safer cars, and may also have access to superior health care in the event of injury from an accident). On the other hand, higher income can also mean riskier behavior: faster cars, and possibly (as pointed out by Peltzman, 1975) taking more chances in driving. In any event, income would appear to be an important variable for inclusion.[9]

Another important demand variable is a measure of the overall amount of driving done. Typically, the variable used is vehicle-miles traveled. This variable is not available at the county level, but another, closely-related to vehicle use, is available: retail sales of highway vehicle fuel, and we use it as a proxy for vehicle-miles traveled.[10]

Another demographic variable which economists have found to be important in explaining behavior with respect to health and safety is education. The work of Michael Grossman (1972, 1975) has shown theoretically that education is likely to have a positive effects on health-promoting behavior, and there is evidence in many areas that this is in fact true. (See, for example, Philip Farrell and Victor Fuchs, 1982). Indeed, Fuchs (1982) has found that people with higher levels of education are more likely to use seat belts, and Victor Fuchs and Irving Leveson (1967) have found some direct evidence of a relationship here, also.

It is thus clear that education is an important potential variable for explaining motor-vehicle accidents. The present study includes two education variables: the per cent of the population over 25 with high school and college educations, respectively.[11]

Another demographic variable relevant to motor vehicle accidents is the per cent of the population made up of young people, who have a higher accident rate than other age groups in the population. Specifically, young men have accident rates higher than other parts of the population,[12] which suggests a variable indicating the percent of the population made up of males aged 15-24. Finally, the per cent of the population which is elderly can have an effect on auto fatalities, as well: we include as a variable for the per cent of the population over the age of 65.[13]

Technological and other variables. Quick availability of emergency medical care may facilitate saving lives in the event of auto accidents, and as a result, the distance of the nearest hospital is likely to be important; hence we include a variable for hospitals in the equation. A priori, it would seem that the proximity of one hospital in a given area would have a strong effect, but that the incremental effect of many hospitals in an area would be weaker, and evidence confirmed this to be the case. As a result, we have created a variable that reflects both availability a hospital and the space which that hospital serves: it is the product of a 0-1 discrete variable, taking on the value of one if there is no hospital in the county, multiplied by the area of the county in square miles. The expected sign of the coefficient to this variable should be positive.[14]

Population density is likely to affect fatality rates, because high densities imply a type of driving (frequent stops) which should, all other things equal, reduce the likelihood of fatal accidents. Therefore, as previously mentioned, population density is included as a variable.

II. Estimation

There are two issues in estimation of (1). The first relates to the appropriate estimation procedure, and the second relates to the problem of "omitted variable" or "heterogeneity" bias that often occurs in cross-section data, such as the panel of counties analyzed in this research. We consider first the issue of estimator.

One logical method to estimate (1) is to take the natural logarithm of both sides. Then, all the terms are linear, including the error term, making the equation well-suited to linear estimation techniques (Ariel Pakes and Zvi Griliches, 1980; Phillip Cook and Glen Tauchen, 1984). This generally produces consistent estimates of the parameters and allows for all the desirable properties of linear estimation.[15] More elaborate solutions to this problem entail nonlinear estimation, such as a Poisson estimator (Nancy Rose, 1990), or a quasi-generalized pseudo-maximum-likelihood estimator (C. Gourieroux, A. Montfort, and A. Trognon, 1984). This latter approach was also tried for this research; however, the results are not reported here, because these techniques do not lend themselves to analysis of "omitted variable" bias, discussed below. In pure cross-sectional analysis, however, our qualitative results regarding regulatory variables are remarkably similar to the ones reported here.

Checking and correcting for "omitted variable" bias. All cross-section analysis using diverse data faces a potential problem of bias from omitted variables:[16] if one were to try to predict the changes of, say, the speed limit on fatalities in a given county, estimated cross-section coefficients could give biased estimates of these effects because they fail to control for all the possible variables that make one county different from another. A panel of two or more cross sections makes it possible to test and control for this bias, as elaborated by Gary Chamberlain (1984), and empirically applied (for example) by Orley Ashenfelter and Alan Krueger (1992).

To present this model (changing notation slightly from (1)), let us assume first a random-effects model, based on two cross sections, one for 1970 and one for 1980. Let the vector y_1 represent the vector of observations on log of fatalities per capita in 1970, and y_2 the vector of those observations for 1980. Similarly, let x_1 be the vector of independent variables for 1970 and x_2 the vector of those variables for 1980. Let α be the vector of coefficients for 1970 and β the vector of coefficients for 1980. Then the random-effects model can be written

(2) $$y_{i1} = \Sigma_j\ \alpha_j x_{ij1} + \epsilon_{i1} + u_i$$

(3) $$y_{i2} = \Sigma_j\ \beta_j x_{ij2} + \epsilon_{i2} + u_i$$

We then assume a "missing" variable (or selection) equation, where the u_i term is taken to be correlated with the x's:

(4) $$u_i = \Sigma_j\ \lambda_{j1} x_{ij1} + \Sigma_j\ \lambda_{j2} x_{ij2} + v_{i,}$$

but where v_i is uncorrelated with x_{1i} and x_{2i}. Substituting (4) into (2) and (3), we have

(5) $$y_{i1} = \Sigma_j\ (\alpha_j+\lambda_{j1}) x_{ij1} + \Sigma_j \lambda_{j2}\ x_{ij2} + \epsilon_{i1} + v_i$$

(6) $$y_{i2} = \Sigma_j\ \lambda_{j1} x_{ij1} + \Sigma_j\ (\beta_j+\lambda_{j2}) x_{ij2} + \epsilon_{i2} + v_i$$

Thus, if there is omitted-variable bias (consistent with the assumptions of this model, at least), it will manifest itself in significant estimates of λ_{j1} and λ_{j2}. In short, if variables dated 1980 (1970) are significant explanatory variables for fatailities in 1970 (1980), then a pure cross-section analysis will suffer from omitted-variable bias. To achieve efficient estimators of the coefficients in (5) and (6), we have used Zellner's method of seemingly-unrelated coefficients to jointly estimate two equations.[17] To test whether heterogeneity bias exists in "pure" cross-section equations, we perform a log-likelihood ratio test in specifications estimated in this way with and without λ_{j1} and λ_{j2} included.

The unrestricted fixed-effects estimation is obtained by differencing (5) and (6), to get:

(7) $$y_{i2} - y_{i1} = \Sigma_j\ \beta_j x_{ij2} - \Sigma_j\ \alpha_j x_{ij1} + \Delta\epsilon_i$$

If the coefficients of (7) were the same in 1970 and 1980, it would be appropriate to constrain each α_j to equal its equivalent β_j. When this was done, however, it was found in that overall, the coefficients differed between the two years at the 5 per cent level.[18] As a result, coefficients constrained to be equal between the two years are not shown here, though they are qualitatively quite consistent with the results shown.

Furthermore, it was found that the results from estimation of (7) were computationally identical to those of the random-effects model with λ_{j1} and λ_{j2} included; thus we report only the random effects model here. As a test of whether (5) and (6) are preferable to random effects without the λ's included, we performed joint (log-likelihood ratio) tests of their significance and also Hausman tests as to whether the estimators differ. The results of the Hausman tests were consistent with the log-likelihood test, which will be discussed below.

<u>The data sample</u>. The sample used here is for all counties in the U. S. A., excluding Alaska (and also Oklahoma, because it does not report the automobile registration data needed for the regulation variable). In order to take advantage of the panel nature of our data, we have restricted our sample to counties for which all data are available for both 1970 and 1980; this allows for a total of 2,627 observations in each year.

<u>High-density versus low-density coefficients</u>. As previously indicated, there is reason to believe that, especially for the regulatory variables in these equations, high-density and low-density coefficients could differ. As a result, we have

specificied our equations to allow the slope coefficients to vary by density for both safety-device regulation and for the speed limit. This difference in slope coefficients is between above-median and below-median population densities, the median being calculated on the basis of average densities between 1970 and 1980. For each variable, the base value of the coefficient is for low-density values of the independent variables, and the high-density coefficient represents the amount by which the slope changes for high-density environments.

IV. Results

Before examining the results (shown in Tables 1 and 2) in detail, it is important to know the extent of heterogeneity bias. To do that, we also estimated (5) and (6) assuming all the λ's were zero, and we did a log-likelihood ratio test on the SUR results of the two sets of equations. The results were unambiguous: The difference between the log-likelihood ratios generates a test statistic of $\chi^2(36) = 118$, which indicates that one can reject the hypothesis that the λ's are 0 at the .005 level. This offers strong support for the hypothesis that use of a "pure" cross section would generate heterogeneity bias.

The results themselves (presented only with the λ's) are revealing in several ways. First, they provide little support for the hypotheses that safety device regulation has been effective. None of the estimated parameters for regulation are significant. This is at best weak support for regulation, and the insignificance would seem to support the offset hypothesis.

Second, as relates to the speed limit, the results for 1970 (the only year with variations in speed limits on rural roads) indicate that only in high-density environments does a lower speed limit have a salutary effect on safety. In rural environments, the sign of the speed limit variable is opposite that expected and insignificant. This result is possibly due to any one (or a combination) of three effects: first, as we have previously pointed out, offsetting behavior can reduce the effect of a speed limit, and that is more feasible in rural than in urban environments. Second, evasion of speed limits may be easier in rural environments. Third, it is possible that it is the variance of speeds that causes fatalities, as hypothesized by Lave (1985), and one would expect the variance effect to be weaker in uncongested areas.

The coefficients for income are consistent with Peltzman's hypothesis, in that Peltzman argued that higher levels of income could as easily increase fatalities as reduce them. Our results indicate a relation negative in 1970, but positive in 1980.

The effects of education are as expected, at least for college education: more education appears to consistently increase safety, and to do so by a substantial and significant amount. The education component is strongly consistent with Grossman's and others' hypothesis about the relationship between education and safety, and indeed, the results shed light on an issue of controversy on the relationship between education and health: it has been debated extensively (Farrell and Fuchs, 1982) whether higher levels of education are directly related to higher levels of health, or whether the relationship is a spurious one, caused by omitted variables. This paper has applied a method used in labor economics of controlling for this "omitted variable" problem in the relationship between education and earnings, and it has found that, especially for college education and safety, the results are quite robust to this procedure. This provides some evidence that, at least for auto safety, the relationship between education and health is not a spurious one caused by omitted variables.

Among the technological effects, vehicle-miles traveled (proxied by fuel consumption) have a significant effect as expected, as does population density (which results in lower fatalities, all other things equal).

Alcohol consumption always has the expected effect of increasing fatalities, though its effect is often not significant.

Longer license renewal periods do indeed reduce safety--license testing, like education, seems to have a strong effect, with no offsetting. Vehicle inspection programs, on the other hand, were successful in 1970 but not in 1980. Light trucks are often thought to be less safe than cars, because they do not have the same safety device regulations. Regarding the young and the elderly, the estimates are plausible: both are more prone than the rest of the population to highway fatalities, though the difference is not significant.

IV. Conclusions

In sum, our results provide some support for the offset hypothesis. Furthermore, the paper gives strong evidence that, at least as of 1970, when speed limits varied widely, lower rural speed limits had no effect in reducing fatalities, though urban limits did. This is possibly due to the offsetting effect, or to its close relative, evasion, or to the possibility that it is variance in speed, rather than speed itself, which influences fatalities. This has an important implication for public policy, namely that recent reforms allowing a 65-mile-per-hour speed limit on rural expressways should impose little cost to safety. Of other regulations analyzed, the only one with a consistent effect is frequent license renewal testing. A further conclusion of our study is that effect of education on safety is positive, most significantly so for college education, and results here suggest that the returns to education may go beyond additional earning power. They also further confirm the relation between education and health, first found by Grossman and by Fuchs; indeed, they provide some evidence that this relationship is a direct one, rather a spurious correlation caused by omitted variables.

Table 1. Results from Seemingly-Unrelated Coefficients, 1970 (jointly estimated with equation in Table 2; estimated standard errors are in parentheses below estimates)

Variable	Alpha Estimate	Lambda Estimate	Alpha, without lambda's
Constant	1.978790		-1.063830
	(4.817250)		(3.254400)
Alcohol	0.026379	-0.093432	0.070178
	(0.171444)	(0.121142)	(0.037674)
College	-0.031868	0.007862	-0.020057
	(0.009740)	(0.006882)	(0.003864)
Density	-0.000071	0.000009	-0.000053
	(0.000013)	(0.000009)	(0.000007)
Drinking Age	0.017721	-0.015538	0.005533
	(0.014008)	(0.009898)	(0.009966)
Elder	0.007993	-0.007606	-0.005687
	(0.009653)	(0.006821)	(0.003192)
Gas/Capita	0.382305	-0.027488	0.358660
	(0.034958)	(0.024702)	(0.026190)
Heavy Trucks	0.002577	0.013314	-0.001869
	(0.009632)	(0.006806)	(0.005616)
High School	0.000172	-0.000059	0.000097
	(0.000226)	(0.000160)	(0.000173)
Hospital	0.068176	-0.071872	-0.010407
	(0.053072)	(0.037501)	(0.028819)

Table 1, Continued

Variable	Alpha Estimate	Lambda Estimate	Alpha, without lambda's
Income	-0.000086	-0.000009	-0.000103
	(0.000050)	(0.000035)	(0.000029)
Inspection	-0.081609	-0.020718	-0.075578
	(0.039788)	(0.028114)	(0.029199)
Light Truck	-0.002697	0.014745	0.008023
	(0.005906)	(0.004173)	(0.002445)
Reg. High	-4.047670	8.926680	-1.148120
	(27.060300)	(19.120800)	(0.427535)
Regulation	18.635500	-54.688200	-7.045620
	(25.865400)	(18.276500)	(3.760760)
Renewal	0.043376	0.001772	0.026598
	(0.020828)	(0.014717)	(0.011043)
Spd. High-Den.	0.025846	-0.016497	0.012661
	(0.007466)	(0.005276)	(0.005506)
Spd. Limit	-0.018573	0.016147	-0.003965
	(0.006815)	(0.004816)	(0.004998)
Young Male	0.000074	-0.010744	-0.009876
	(0.008197)	(0.005792)	(0.004487)
Zero Fatal.	7.631020	0.119889	7.777150
	(0.094774)	(0.066967)	(0.071300)
R Squared	.8396		.8369
Sample Size	5,254		5,254

Table 2. Seemingly Unrelated Regression Results, 1980 (jointly estimated with equation in Table 1; estimated standard errors are in parentheses below estimates).

Variable	Beta Estimate	Lambda 2 Estimate	Beta, without Lambda's
Constant	3.348400		-6.535880
	(4.447190)		(2.678710)
Alcohol	0.094014	0.063077	0.082807
	(0.105889)	(0.081047)	(0.022927)
College	-0.020392	0.002114	-0.011753
	(0.007343)	(0.005620)	(0.002899)
Density	-0.000146	0.000031	-0.000110
	(0.000026)	(0.000020)	(0.000014)
Drinking Age	0.010403	0.001712	-0.001207
	(0.016406)	(0.012557)	(0.009153)
Elderly	0.007234	-0.004791	-0.002987
	(0.009221)	(0.007057)	(0.002964)
Gas/capita	0.065583	0.038304	0.132560
	(0.042981)	(0.032898)	(0.029630)
Heavy Trucks	0.008266	-0.033650	-0.012486
	(0.010844)	(0.008300)	(0.005815)
High Sch.	-0.001592	-0.001022	-0.003405
	(0.002533)	(0.001939)	(0.001388)
Hospital	-0.001283	0.004838	-0.042154
	(0.060993)	(0.046684)	(0.030897)

Table 2, Continued

Variable	Beta Estimate	Lambda 2 Estimate	Beta, without Lambda's
Income	0.000004	0.000001	0.000006
	(0.000006)	(0.000005)	(0.000004)
Inspection	0.049771	-0.042054	0.033000
	(0.044980)	(0.034427)	(0.024314)
Light	0.003413	-0.002980	0.013508
	(0.005825)	(0.004458)	(0.002188)
Reg. High	-2.046680	-5.738180	-0.214730
	(26.970500)	(20.643000)	(0.026544)
Regulation	15.001500	25.489900	-1.902250
	(21.967700)	(16.814000)	(3.022010)
Renewal	0.035416	-0.002292	0.037716
	(0.023603)	(0.018066)	(0.013357)
Young Male	0.000831	0.002753	-0.005027
	(0.010864)	(0.008316)	(0.005156)
Zero Fatalities	7.838720	0.120988	7.989530
	(0.083084)	(0.063592)	(0.058551)
R Squared	.8925		.8883
Sample Size	5,254		5,254

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ENDNOTES

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1. Peltzman also contributed to this empirical literature, which is quite extensive. Recent examples include a series of articles in the AER, starting with Charles Lave (1985) and including Fowles and Peter Loeb (1989), Levy and Asch (1989) and Donald Snyder (1989).
2. Fatality data are from U. S. Department of Health and Human Services (1970) and (1980).
3. The variable we use to measure safety regulation is a variable between zero and one, intended to reflect relative safety levels of fleets. A value of 1 reflects safety levels of pre-1966 vehicles; a value of .6 reflects NHTSA's estimate of the relative safety of a post-1975 vehicle. Values for a county reflect statewide fleet averages, based on relative estimated

vehicle-miles for each vintage car. This variable, for both 1970 and 1980 is originally based on calculations of John Graham (1983): a safety regulation variable based on the relative (technologically-expected) safety levels of each vintage of auto, as estimated by the U. S. Department of Transportation Fatal Accident Reporting System database. Thus, a pre-1965 car has a value of 1, but an auto from the late 1970's had a value of .6. Values of this variable were calculated for each state, and each state's value was applied to counties within the state. The value for each state was calculated by finding the number of vehicles for each vintage of the previous 10 years, and estimating the vehicle-miles each vintage contributed to the total, based on U. S. Department of Transportation estimates of utilization for each vintage over a car's ten-year life. The formula for calculation of the safety variable is therefore (for each state i, with t = 1,...,10 vintages)

$$\text{REGULATION} = \Sigma_t \; s_{it} \; R_t$$

where s_i is the share of each vintage t in vehicle-miles traveled, and R is the regulation-related safety level (less than or equal to one) of vehicles in that vintage. The source for calculation of vintages for each year is U. S. Federal Highway Administration, (1960 through 1980).

4. For 1980, this value was 55 miles per hour everywhere. In 1970, it varied between 55 and "reasonable and prudent," taken here as 80 miles per hour. Source: Rand McNally Road Atlas, 1970.

5. Sources for both populations and areas of counties are U. S. Bureau of the Census (1970) and (1980).

6. Our variable for inspection is discrete and valued at zero if the state does not have an inspection program, and one otherwise. The basic source is Callahan (1970) for 1970, and American Automobile Association (1981).

7. The source for our minimum drinking age variable is Cook and Tauchen (1984), pp. 187-188.

8. This variable is measured in gallons per capita. Source: U. S. Brewers' Association, 1973 and 1981.

9. The source of these data is U. S. Bureau of the Census (1970) and (1980).

10. Figures for 1970 are from 1972 and are from U. S. Bureau of the Census (1972); 1980 figures are for 1982, and are from the U. S. Bureau of the Census (1982).

11. For the source of education data, see U. S. Bureau of the Census (1970) and (1980).

12. See, for example, Cook and Tauchen, 1984.

13. For the source of data on elderly and youth see U. S. Bureau of the Census, (1970) and (1980).

14. For 1970, the source is "Hospitals" (1970) For 1980, it is American Hospital Association (1980.

15. A problem with this approach occurs if the dependent variable takes on a zero value, in which case the log cannot be taken. To solve this for linear estimation, we use the procedure of Ariel Pakes and Zvi Griliches (1980), which is to define a

dummy variable equal to 1 if the dependent variable is zero and zero otherwise, and to allow the dependent variable then to be transformed to a value of 1 (hence log = 0) when the dependent variable is zero.

16. In the context of panels of data based on individuals, this bias is often called "selection" bias. That term is avoided here, because the sample we start with is very near an exhaustive one).

17. This method makes sense in this case: if a county is a positive outlier in one year, there is reason to believe that it would be a positive outlier in another, as well. That is the principle behind seemingly-unrelated coefficients, and the reason it is used by Orley Ashenfelter and Alan Krueger (1992).

18. Estimation of the unconstrained equation (7) allows identification of 15 additional parameters beyond the constrained equation; comparison of the sums of squared residuals of the constrained and unconstrained equation generated $F(15,2593) = 1.93$, while the critical value for 5 per cent significance is 1.75.