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The Case of Youth Traffic Fatalities

by

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Key Words: Income and Health; Traffic Legislation, Youth Mortality

JEL Codes: I14, H73

I. Introduction

One of the better and longer-established relationships in public health is that between income and mortality in cross-national studies. As Pritchett and Summers (1996) put it in the title of their paper: "Wealthier is Healthier". Nations with higher per-capita incomes have lower rates of infant mortality and higher life expectancies. Though the negative gradient between income and health has been documented for more than 200 years (Deaton, 2001), the modern representation is the log-linear "Preston Curve" between income and life expectancy in Preston (1975). A stable Preston Curve implies, as argued by Pritchett and Summers, that the absolute best measure for improving public health is a focus on economic growth.

The mechanism behind the income-health gradient has been the subject of some controversy and much literature. The issue of causality is contentious, as it is reasonable to believe that healthier individuals are more productive, and therefore that better health leads to higher incomes. Education may be an intervening factor, as healthier children learn better, accumulate more human capital, and tend to avoid riskier behaviors associated with lesser-educated, poorer populations. Careful analysis suggests however, that even taking reverse causality and education into account, there is a strong causal path from income to health (Deaton, 2001; Pritchett and Summers, 1996).

This paper uses the income-health gradient to help explain the differential success in reducing traffic fatalities in young people across the U.S. states. Traffic accidents are the leading cause of death among teens ages 15 to 19, accounting for more than one in every three deaths in this age group (Centers for Disease Control and Prevention [CDC], 2010). Teen drivers are especially prone to accidents, given their relative inexperience behind the wheel, their tendency to engage in reckless behavior such as speeding and alcohol use (especially by male drivers), and their lower rate of seat belt use compared to older drivers.

Fortunately, both the number and the rate (per capita) of teen traffic deaths have fallen by about two-thirds over the past three decades. In 1979, over 9,000 teens died in traffic accidents, or about 43 per 100,000 teens; the comparable numbers in 2009 were around 3,000 and 14 (CDC, 2010).[1] Improvements in automobile safety and road quality have contributed to the decline in youth fatalities, as they have to the general driving population, but other factors

[1] Some of the decline in fatalities is no doubt due to fewer driving teens: in 2008 only about 55% of teens ages 16-19 had driver's licenses, versus 70% of teens in 1983 (Sivak and Schoettle [2011]). Using the fatality rate controls for the decline in driving, but the removal of a large number of risky drivers undoubtedly has an effect on mortality rates of all ages.

specific to young drivers have been adduced as well, including restrictions on teen driving hours and circumstances, better instruction, and tougher laws on underage drinking.

The welcome decline in fatalities has not been uniform across U.S. states. Massachusetts saw the steepest decline, with the five-year average rate falling by 63 percent from 1984 to 2009; over the same period, Mississippi's rate fell by only four percent.[2] The mean decline for states was 42 percent (median decline 44 percent) with a standard deviation of 15 percent. Mississippi is definitely an outlier, but there were another 10 states with declines less than 30 percent.

Persistence of Traffic Fatalities, 15-19 Years

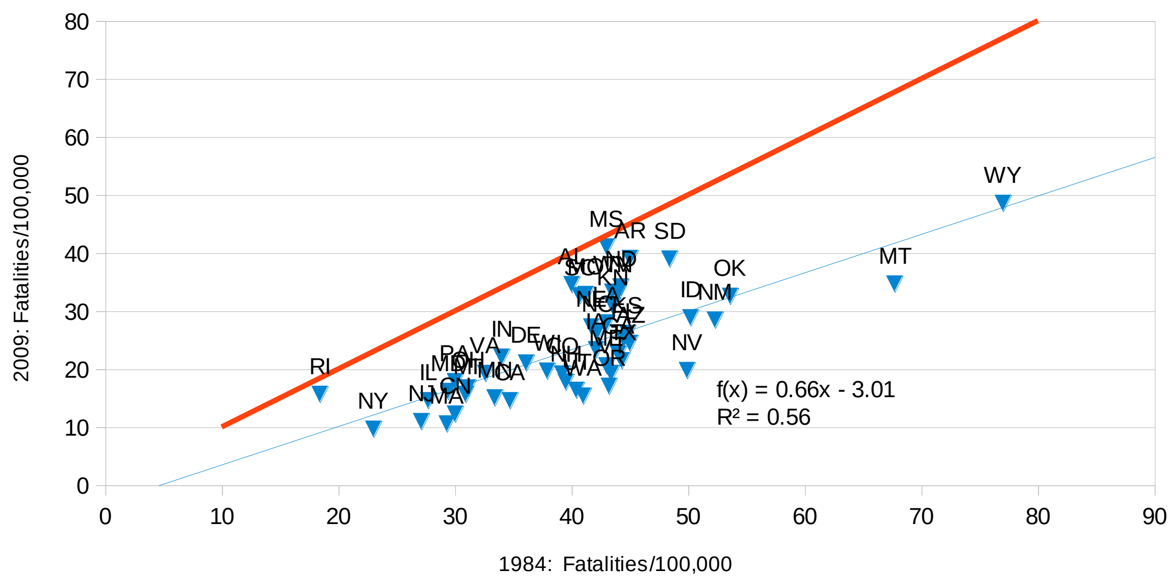

Figure 1 illustrates both the progress in reducing youth fatalities and the persistence in the variation across states. The upper, heavier line in the figure is 45 degrees; any state's vertical distance from the line is its improvement over the period. The lighter line through the data is the

[2] As explained below, five-year averages are used to mitigate problems of serial correlation and because data are not reported for some of the smaller states every year.

regression line; over half the variation in 2009 state fatality rates is explained by the variation in 1984. There is a relatively large cluster of states with fatality rates of 40-45 per 100,000 in 1984 that have a much wider variation in 2009 rates.

There is a large extant literature on the effects on fatalities of traffic legislation by the various states, and many of these papers use income as a controlling variable. Because most of this research uses annual data, however, income variability is partly the result of business cycle fluctuations. The objective of this paper is rather to test the hypothesis that *initial* income differences across states explain a significant share of the variation in the *subsequent* improvement in youth traffic fatalities. In this context, income is resource availability: higher incomes allow a newer and therefore safer vehicle fleet, better infrastructure, and more resources available for education and training. In essence, the position is taken that safety and traffic enforcement are normal goods, and that states with higher incomes purchase more of both.

There is no expectation that fatality rates will absolutely converge across states even with identical incomes. States have permanent or at most slow resolving differences in topography, density, urbanization, size and other factors that all but assure permanent differences in fatality rates. Nevertheless, given the increasing use of federal mandates, usually through the threat of highway funding cuts, to enact various pieces of traffic control legislation like Minimum Legal Drinking Age (MLDA) and Blood Alcohol Limits (BAC laws) common to all states, it would be reasonable to expect some degree of convergence as state laws become more uniform.

. Higher incomes should be associated with lower incidence of any form of death, as states with higher incomes will likely have more medical personnel per capita, better medical facilities, broader access to technology, and other means to treat illness as well as injury. We may expect, however, that income levels will act differentially on “preventable” versus “non-preventable” mortality. As used in this paper, “preventable” mortality includes traffic fatalities, violent crime, suicide, and other causes of death where intervention or prevention may have an ameliorating effect. “Non-preventable” mortality includes infectious disease, congenital conditions, or other “natural” causes. Traffic mortality is a natural test case, as most traffic deaths are preventable, at least in principle, and the measures that are taken to prevent traffic deaths – speed limits, alcohol consumption controls, graduated licenses for young drivers, and so on – are expensive to enforce, a fact usually not addressed in studies of the effectiveness of these measures.

This paper includes controls for the various traffic safety measures enacted since 1980; these laws were passed at different times for the various states, allowing the researcher to exploit the timing variation across states to measure their effects. The 15-19 year age group is a particularly interesting case, as there are several laws unique to pre-21 drivers, including graduated driver’s license (GDL), zero tolerance for alcohol (ZT), and minimum legal drinking age (MLDA). Seat belt requirements, administrative license revocation (ALR), and blood alcohol content limits (BAC) will also be included, as the laws applying to the general population may have differential effects on young people. This paper departs from much of the previous research by using different levels of GDL and seat belt requirement variables depending on the ratings established by the Insurance Institute for Highway Safety (IIHS) to more precisely estimate the effects of these initiatives on youth fatalities.

The major findings of the research are that initial state median income is the most significant single predictor of the change in fatality rates, whether measured in long-run cross-section or in panels of non-overlapping five-year averages. With the exception of GDL, traffic safety laws aimed specifically at young people have insignificant or perverse effects on mortality, though seat belt laws are effective for this age group. The findings also indicate that the effect of initial income on traffic mortality is two to three times as large as it is for non-injury mortality. An important implication of this finding is that simply passing legislation without the resources to enforce it will not reduce mortality.

The paper is organized as follows: Section II presents a brief review of previous research, Section III describes the data and methodology, Section IV presents the empirical findings, Section V describes tests of prediction for the models, and Section VI concludes.

II. Previous Research

This paper combines two strands of literature: that on the relationship between income and health, and that on the effects of traffic control legislation on youth fatalities. Both are voluminous, so this brief review will focus on the most relevant research.

The positive relationship between income and health is well-established at the cross-national level. Preston (1975), Newhouse (1977), and Pritchett and Summers (1996) consistently find a strong relationship between average national income and health indicators such as life expectancy and infant mortality. The so-called "Preston Curve" levels out at national incomes of about $20,000 (measured at 2005 PPP), however, so it appears that there are limits to the protective power of higher average incomes. Because of this, the relationship between income and health *within* countries is less well established. Deaton (2001), for example, citing earlier work by Kaplan et al (1996) and Kennedy et al (1996), states that "there is no relationship between average income and mortality across the states [of the U.S.]" (p.3).

Singh and Siahpush (2006), however, show that counties in the U.S. with "higher deprivation" (as measured by an index[3] including income, education, and other quality of life indicators) have significantly lower life expectancy than the least deprived counties, and that the discrepancy between the top and bottom deciles of counties have been widening over time, both with respect to deprivation and with respect to life expectancy.

Certainly there is little doubt that income is predictive of health at the individual level, at least in the U.S. Waldron (2007) uses Social Security earnings to show that the top half of the earnings distribution has shown faster improvements in mortality than the bottom half over a thirty year period. The gap in longevity increased from a 1.2 year advantage to the top half of the earnings distribution of workers born in 1912 to a 5.8 year advantage to higher earning workers born in 1941.

A more controversial claim is that income inequality within an economic unit has negative effects on health. As advanced prominently by Wilkinson (1996), the "relative income hypothesis" holds that societies with more unequal distributions of income are less healthy. This has been used to explain why, for example, the United States has a much higher per capita income than say, Greece or Costa Rica, but ranks lower in many measures of social health. The volume edited by Kawachi, Kennedy, and Wilkinson (1999) summarizes much of this research, with Kaplan, et al. (1996) producing evidence for the U.S. that more unequal states have higher mortality and rates of social dysfunction, including violence and poverty.

As shown below, the present paper confirms that differences in median income can be associated with differences in mortality, and that the type of mortality matters. It does not appear

[3] Income had the highest factor loading in the index, at 0.90, so it is fair to assume that the results with income alone would be similar.

that state-wide measures of inequality matter, however, in explaining mortality differences. While not a direct test of the relative income hypothesis, this evidence tends to support the position that it is absolute rather than relative deprivation that makes a difference.

The literature on income and traffic mortality is focused mainly on cross-country analysis, and is mainly a story of development. Kopits and Cropper (2003) show that traffic mortality has an inverted U-shape with respect to income: at low incomes, there are few motor vehicles but many pedestrians and cyclists, so that the number of fatalities per vehicle is high. As incomes increase, the number of fatalities increases, but at a decreasing rate due to the increased incidence of vehicle-vehicle crashes, in which mortality is lower. At high incomes, crashes are more common as more vehicles take to the road, but fatalities per vehicle continue to fall as roads are increasingly dedicated to vehicular traffic and pedestrian and cyclist deaths become less common. Anbarci, Escaleras, and Register (2009) expand on this idea to show that within-country inequality can be associated with higher traffic mortality: unequal incomes lead to more crashes of vehicles with unequal sizes, which have higher probability of death than crashes involving vehicles of equal sizes.

The economics literature on traffic mortality in young people has focused mainly on the various traffic control measures that have been established by the states with the specific intent to reduce fatalities in teenagers. One group of laws imposes sanctions on underage drinking and driving through Zero Tolerance [ZT] laws that prohibit young drivers from consuming any amount of alcohol and through statutes establishing the minimum legal drinking age [MLDA] at 21. Other types of legislation require young people to undergo a series of probationary steps before attaining full driving privileges via graduated driver's license [GDL] statutes.

Because the states have imposed these measures at different times, researchers have often employed panel data techniques such as "difference-in-differences" estimators to exploit the spatial and temporal variation in the data. The timing differences mimic a sort of "natural experiment" among "treatment" states (those that have passed the law) and "control" states (those that have not). Because these techniques can control for unobserved heterogeneity across states and over time, they are seen as preferred to state-by-state case studies or national time series.

Refinement in technique notwithstanding, there is no general consensus that any of these measures has been an unmitigated success. GDL laws have been shown to reduce crash involvement among young people by 20-40% (Williams and Shults, 2010), but whether this has been accomplished by making younger people better drivers or simply reducing their driving incidence is still an open question (Karaca-Mandic and Ridgeway, 2010). Other studies have found evidence that postponing driving actually raises crash incidence at 18 (the "practice" effect; Males, 2007), but other studies (McCartt, et al., 2010) have found continued beneficial effects in older drivers.

Traffic safety groups and organizations like Mothers Against Drunk Driving (MADD) have been enthusiastic proponents of MLDA laws, and much of the literature tends to support the effectiveness of MLDA in reducing fatalities among young people. Carpenter and Dobkin (2011) provide recent evidence in support of MLDA, estimating that lowering the drinking age will kill an estimated 0.77 people annually for every 100,000 18-20 year-olds allowed to drink (p. 153). Carpenter and Dobkin argue that a reduced drinking age will also result in greater incidence of assault and robbery, among other crimes.

Miron and Tetelbaum (2009) argue however that MLDA appears to have had only a minor effect on teen driving fatalities or on teen drinking. Miron and Tetelbaum find that 1) initial findings of large effects of MLDA were driven by early adopting states; 2) the impact of MLDA did not persist much past the initial year of adoption; and 3) MLDA had only minor effects on teen drinking. Grant (2011) shows that early empirical studies of traffic control laws, including MLDA, tend to have much larger estimates of effectiveness than later studies, a finding consistent with Miron and Tetelbaum. In addition, there were a great many initiatives emplaced against drinking and driving contemporaneous with the increase in the MLDA (federal legislation mandating an MLDA of 21 under pain of losing highway funds was enacted in 1984), and many safety innovations such as anti-lock brakes and airbags became standard equipment about this time. Disentangling the causes of the improvement in fatality rates during this critical period is thus particularly challenging.

The evidence on Zero Tolerance laws is decidedly mixed, with the most careful study to date, Grant (2010a), finding little or no difference in the effect of ZT laws on daytime and nighttime youth traffic fatalities, even though the vast majority of drinking-related fatalities occur at night. Earlier studies by Dee (2001) and Eisenberg (2003) had found small effects of ZT laws on youth fatalities (on the order of 5% reductions), but even these findings seem unrealistic given the relatively small number of fatalities in accidents involving very small amounts of alcohol consumed. For this reason, ZT laws may even have a perverse effect: once the “zero” threshold has been crossed, there is no further incentive to moderate one’s drinking.

There are of course many other alcohol control laws intended to reduce the incidence of drunk driving such as Blood Alcohol Content (BAC) limits and Administrative License

Revocation (ALR) statutes.[4] There are also laws intended to reduce accidents and their effects, such as speed limits and seat belt requirements. As these are aimed at the general population, they will receive less emphasis here. We will, however, use some of them as controls in the empirical estimation that follows.

III. Data and Methodology

The basis for the dependent variable in the regressions below is the mortality rate (i.e., deaths per 100,000 population in the age group) from motor vehicle accidents for 15-19 year olds, taken at the state level annually from 1979-2009.[5] For comparison purposes, the mortality rate for the same age group for deaths due to non-injury causes will be used in separate regressions. Obviously, resources are also important in the treatment and prevention of, say, infectious diseases, and we shall see that income differentials explain a significant share of the variation in mortality from disease. In this paper, however, we wish to establish not only that income differentials have material effects on mortality outcomes for young people, but also that these effects are much larger for traffic fatalities, where larger incomes make affordable additional resources such as enforcement, better infrastructure, trauma care, and newer, safer vehicles.

Table 1 provides descriptive statistics for median household income and mortality rates for 15-19 year olds at five-year intervals for the U.S. States beginning in 1984. The mean and variance of the logarithms of median income and traffic mortality rates are presented for the 48

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[4] ALR provides for the immediate suspension of driving privileges upon either refusal to submit to an alcohol test or upon test results exceeding a BAC of 0.08

[5] See the Data Appendix for the source of all data used in this paper.

contiguous states, as is common in the literature for alcohol and traffic control laws (Dee, 2001).[6] Non-injury mortality rates are computed for 41 states.[7] The CDC does not report state years when deaths are fewer than 10 for any cause. A state was excluded if no mortality rates were reported for three or more years for any five-year period. This introduces the possibility of bias from omitting small states, but as shown below, the exclusion does not appear to affect the principal findings.

[Table 1 about here]

Average median income remained in the mid-$40,000 range until the long expansion of the 1990s, when it moved up to around $50,000 and remained when median income growth stagnated during the 2000s. The variance of log income, a measure of convergence when taken through time (Sala-i-Martin, 1996), has remained roughly constant since 1994. Both traffic and non-injury mortality for 15-19 year olds have fallen through time, with some flattening of both in the late 1990s and early 2000s before accelerating in 2009 (2007 for non-injury mortality). Of interest is the increase in the dispersion of both mortality rates over time, with the latest variance larger by a factor of 2-3 over the minimum measured for each rate over the time periods. As shown in detail below, this increase in mortality dispersion is consistent with the relatively stable dispersion of income and the protective effects of higher income on mortality. Wealthier states have not only stayed wealthy, they have managed to live longer, too.

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[6] Variance of the natural logarithm is used to provide better comparability across variables of different dimensions and magnitudes.

[7] Omitted states are Delaware, Montana, North and South Dakota, Vermont, and Wyoming.

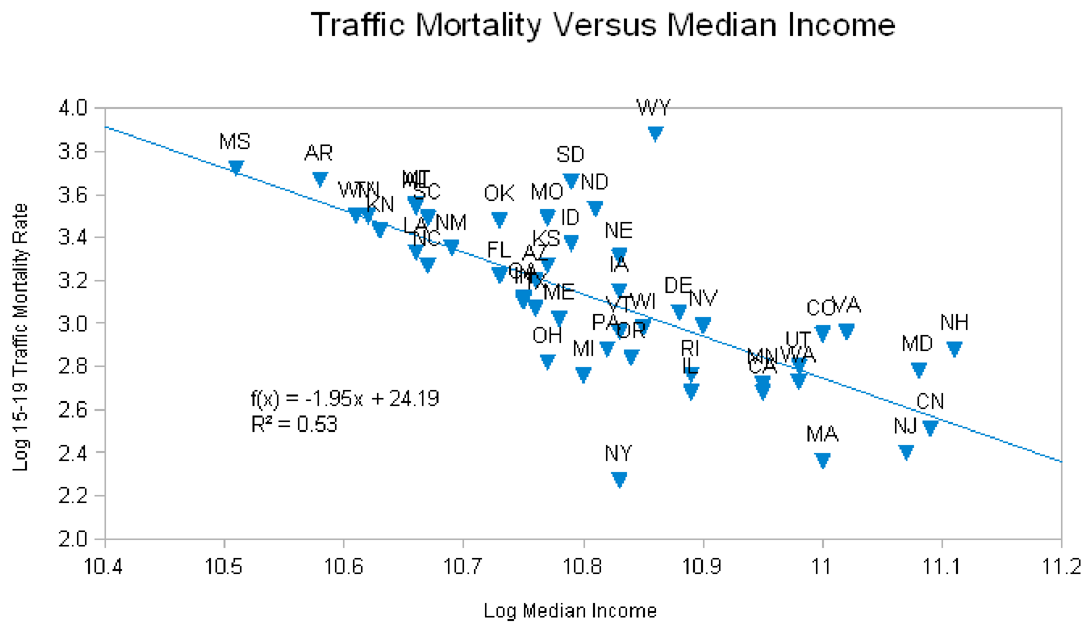

Figure 2: Age 15-19 Traffic Mortality and Median Income, 2009

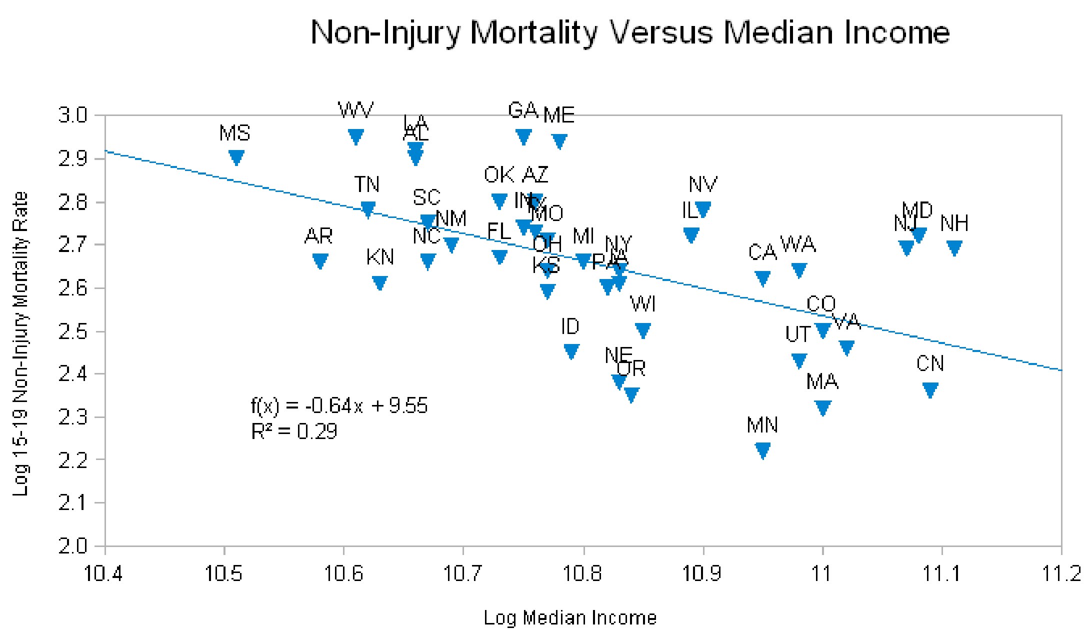

Figure 3: Age 15-19 Non-injury Mortality and Median Income, 2007

Figures 2 & 3 provide snapshots of the relationships being developed in this paper. Both plot the natural logarithm of 2009 state mortality rates for the 15-19 year-old population against the logarithm of state median income; Figure 2 with traffic fatalities on the vertical axis and Figure 3 with non-injury mortality.[8] Over half the variance in traffic mortality is explained by state median income; a one percent increase in median income is associated with an almost two percent reduction in the fatality rate (t = 4.08). The national average traffic mortality rate for 15-19 year-olds is approximately 24 per 100,000 population, so a two percent reduction is about one-half fatality. In a state with 10 million people, about seven percent or 700,000 would be aged 15-19, so each one percent increase in income is associated with about three and one-half fewer fatalities in this age group every year.

Non-injury mortality among 15-19 year-olds, the comparison group, is also negatively associated with state median income, with a one percent increase in income associated with six-tenths of one percent decrease in mortality (t = 2.05). Non-injury mortality for this age group averages about 14.5 per 100,000 annually, so calculations similar to the above would result in a reduction of one-half non-injury fatality annually for each one percent increase in median income in a state with 10 million people.

Thus we can establish for the latest five year periods that: a) median income is associated with lower youth mortality; and b) the estimated effect of a difference of one percent in state median income is three times as large with respect to traffic mortality than non-injury mortality.[9] These results are only suggestive, however, as they cover a short time period, omit other factors contributing to mortality, and ignore the possibility of two-way causality. On this last point, it is

[8] Again, all data are in five-year averages.
[9] Obviously this is the *total* income effect, not accounting for other factors contributing to mortality that may be correlated with income.

plausible that populations with higher mortality due to risk-seeking behavior, poor self-care, etc., make be less productive and therefore earn lower incomes. It may also be the case that some of the same factors that contribute to higher mortality—topography, rural/urban mix, north/south location—also influence state incomes. This issue cannot be completely resolved, but in what follows the analysis will show that *initial* levels of income are predictive of *future* mortality outcomes.

Our approach is two-fold. The first set of estimates examines mortality over the long run. Borrowing from the income convergence literature (e.g., Sala-i-Martin, 1996), we have

$$\Delta lmort_{i,T} = \alpha + \beta\ lmort_{i,t} + Y_{i,t}\gamma + \sum_{25} \boldsymbol{Z_i} \boldsymbol{\delta} + \epsilon_i. \qquad (\mathbf{1})$$

where $\Delta lmort_{i,T}$ denotes the average growth rate in mortality over the period $T - t$ for state *i*, $lmort_{i,t}$ is the mortality rate and $Y_{i,t}$ the logarithm of median income (in 2009 dollars) in the initial year, and $\boldsymbol{Z_i}$ is a vector of indicator variables for various alcohol and traffic control laws. These indicator variables are averaged over the period and thus represent the proportion of the time each law has been in effect for the state *i*.

Two modifications to the conventional binary treatment of traffic legislation occur with the GDL and the Safety Belt (SB) variables. Graduated Driver's License programs have several provisions. The Insurance Institute for Highway Safety (IIHS) designates three stages of GDL, learner, intermediate, and full privilege, and recommends minimum periods, driving curfews, and passenger restrictions for the first two stages. Adoption of GDL requirements began in the mid-1990s; by 2009, all states had some measure of GDL, but the number of restrictions differ. In what follows below, we use the ranking system of Trempel (2009), where points are assigned from 0 to 10 depending on the number of recommendations adopted by a state. Accordingly, the

GDL variable used in the analysis assumes values in each state –year from 0 to 1.0 by tenths, with 1.0 representing full compliance with IIHS recommendations. Appendix Table A2 reproduces the point system from Trempel (2009).

The IIHS also has a list of recommended safety belt and child restraint laws, and provides a rating system for the states depending on the number of measures adopted.[10] A state is rated poor if no mandatory safety belts are required; marginal if enforcement is secondary, that is, if a citation cannot be issued except when another offense has occurred; fair if the law allows a citation to be issued solely for safety belt violations (i.e., primary enforcement) but doesn't require belt use in rear seats; good if the law requires primary enforcement and requires belt use in rear seats. Accordingly, the SB variable is assigned a value of 0,1,2, or 3 if a state's rating is poor, marginal, fair, or good, respectively in that year.

The specification in (1) allows us to test the proposition that initial incomes matter in subsequent mortality outcomes, offers a mechanism for additional evidence on the efficacy of the various alcohol and traffic safety laws that have been passed over the past thirty years, and provides a means of comparison for different types of mortality. In addition, the coefficient β can be interpreted as a test of mortality convergence across the states.[11] The states have become more uniform in the body of legislation pertaining to alcohol and underage driving, occasionally under threat of loss of federal highway funds. This increased uniformity should, if laws are effective, lead to convergence in traffic death rates. This convergence will not be absolute, however, given differences in topography, weather, urban/rural mix, and other heterogeneous factors across states.

[10] Or at least it did, until June, 2011. The rating system and the states' ratings were accessed via http://www.iihs.org/laws/safetybeltuse.aspx on June 30, 2011. The current page (same address) contains provisions of state laws, but no rating system. (Accessed December 27, 2011).

[11] The *speed* of convergence, *b*, can be calculated as $b = -\ln(1-\tau\beta)\;\; \tau$ where $\tau = T - t$; see Sala-i-Martin (1996).

To further address the latter point, and to control for changes across states over time, such as improved safety features in automobiles, better medical treatments, and changed attitudes toward drinking and driving, we also estimate a pooled cross section model of the form

$$\Delta lmort_{i,t} = \alpha_t + \beta\ lmort_{i,t-5} + Y_{i,t-5}\gamma + \textstyle\sum_5 \boldsymbol{Z_{i,t}\delta} + \epsilon_{it}. \qquad (\mathbf{2})$$

In this specification, five cross-sections of non-overlapping five year averages beginning in 1989 and ending in 2009 are estimated.[12] As before, average growth in mortality rates is regressed against mortality rates and income levels at the beginning of the five year interval, and against the average number of state-years that various traffic laws have been in effect. The advantages of this form are additional degrees of freedom, for more precise estimation; the inclusion of time fixed effects (α_t) to control for national-level changes; and closer temporal correspondence of changes in laws with changes in mortality.

The following section presents the results of estimating models (1) and (2) for traffic and for non-injury mortality.

IV. Empirical Results

Table 2 presents the results of estimating Model (1). In the first column, average growth in fatalities is regressed only on initial fatalities for a test of unconditional convergence. As coefficient is positive and insignificant, there is no evidence of absolute convergence.

The addition of initial income in column 2 changes the picture completely. The coefficient of income is negative and significant, and conditional on income, traffic fatality rate converge

[12] Five year averages are typical in the convergence literature; see Abreu, de Groot, and Florax (2005).

across states over the sample period. Convergence is quite slow, with a half-life to convergence of 60 years.[13] The income effect is materially significant, with a one standard deviation change in income (about $6,400) accounting for about a 0.8 percent annual decrease in traffic fatalities, or about 30 percent of the average decline across the sample. Holding incomes constant, there is a tendency for traffic fatality rate to converge, but the persistent disparity in state median incomes is a centrifugal force offsetting the centripetal force of more uniformity in driving standards.

Column 3 adds the Gini coefficient in 1984 for each state to address a strand of literature that emphasizes the effect of relative deprivation on mortality. This view is associated with Wilkinson (1992) and many others (see the volume edited by Kawachi, Kennedy, and Wilkinson, 1999), but has been criticized as an ecological fallacy, as well as on other grounds (Deaton, 2001; Judge, Mulligan, and Benzeval, 1997). The Gini coefficient has the wrong sign from that predicted by the relative income hypothesis, and is insignificant in any case. Income inequality across states has an effect on mortality, but income inequality within states apparently does not.[14]

Column 4 adds traffic laws to the estimation. The first three laws (MLDA, ZT, and GDL) are particular to young drivers; the rest apply to all. All of the laws (except ALR, which is zero to three decimal places) have the expected sign, but only one, GDL, is significantly different from zero. Notably, the coefficient of income is little changed, but the speed of convergence is almost doubled, with an implied half-life of about 27 years in this specification. This makes

[13] Calculated as $half = -ln(0.5)/b$, where b is the speed of convergence as defined in fn. 11 above.
[14] Though see Singh and Siahpush (2006) for an analysis of the effect of income differences on life expectancy at the county level.

sense, as the narrowing of the differences in the legal restrictions on driving should lead to more similarity in mortality outcomes.[15]

Columns 5 and 6 provide the comparison between "preventable" and "non-preventable" mortality. The dependent variable in column 5 is the non-injury mortality rate for 15-19 year-olds. As noted earlier, data for some smaller states are not available, reducing the number of states for analysis to 41. In addition, non-injury fatality rates are available only through 2007. Accordingly, the analysis in column 6 imposes the same restrictions on the traffic mortality data. The effect of initial income on non-injury mortality in column 5 is less than half the effect on traffic mortality in column 6, which is basically unchanged from the full sample in column 2.

Estimated convergence conditional on income for non-injury mortality is more rapid than for traffic mortality. This is consistent with the idea that the obstacles to equalizing across states the adoption of medical technology and provision of access to medical care are fewer than equalizing driving conditions.

Column 8 is a robustness check using fatality rates for 20-24 year-olds. Income continues to be protective against mortality, and none of the laws are significant. Some recent research suggests that GDL mainly works by limiting driving experience in teenagers, and may actually contribute to higher fatalities in the over-18 year old cohort due to less "practice" and lower driving skill (Karaca-Mandic and Ridgeway, 2010). We find no evidence that GDL raises mortality rates in the older group, counter to the "less practice means more accidents later on" hypothesis, but the lack of significance indicates no support either for long-term salubrious effects of more training in younger drivers.

[15] By 2009, all states had MLDA of 21, ZT laws, some form of GDL, some form of SB, and BAC limits of 0.08. All but eight states had ALR laws.

More troubling is the fact that no laws appear to be effective in reducing mortality in this age group. As the regressions in Table 2 cover a very long time period and are not well-suited for measuring short-term effects, these findings may be confirming Grant (2010b), who shows that early-adopting states typically show much stronger response to traffic initiatives, and that the longer the time from initial passage, the weaker the effect on drinking drivers.

[Table 3 about here]

Table 3 presents results from the panel-type analysis of the effects of income on mortality. The approach is similar to that of the cross-section analysis in Table 2, except that now the dependent variable is expressed as a five-year log average and the initial values of income and mortality are in the initial year of each five-year period. Traffic laws are expressed as the proportion of the number of years effective during the five year period. The panel-type analysis allows for the introduction of time effects to control for unobserved factors such as technology, better safety features in vehicles, attitudes toward drinking and driving, the increased popularity of light trucks and SUVs, and so forth.

The first two columns reinforce the findings from the cross-section in Table 2: traffic mortality does not converge unconditionally, but does when conditioned on initial income. Convergence is faster than in the cross-section, a common finding when estimating growth-type regression using panel data (Abreu, de Groot, and Florax, 2005); half-life to convergence is about 18 years. Adding traffic legislation in column 3 has little effect on the coefficients of initial income and mortality. The coefficient on MLDA is positive, contrary to expectation.

GDL is negative and significant, as before, and seat belt laws have a small but significant negative effect on mortality. ZT has a negative sign, but is insignificant at conventional levels.[16]

Columns 4 estimates convergence of non-injury mortality. Non-injury mortality converges relatively rapidly, conditional on income, with a half-life of about 15 years. Initial income has a significant but much smaller on non-injury versus traffic mortality across states. Because some states had to be omitted in the non-injury estimations, column 6 restricts traffic mortality for comparison purposes to the same states available for non-injury. As before, the speed of convergence is slower with traffic mortality, but the sensitivity to initial income more than twice as great.

As before, the last column is a robustness check using 20-24 year-old traffic mortality rates. Speed of convergence and sensitivity to initial income are similar to the younger group, and none of the traffic laws are significant. Without further analysis, it is impossible to know whether previously reported results showing the efficacy of these laws reflect only the response of older groups, or whether the effects of these statutes are simply not measurable using the five-year increments in the present analysis. At least for this age group, highly touted measures for traffic safety appear to have little effect.

V. Prediction of Mortality

Although out-of-sample testing is common in time series models, until recently there have been few applications to panel data (Baltagi, 2007). In particular, none of the referenced papers on the efficacy of traffic laws on mortality tests whether these models are superior to alternatives

[16] Interactions of income and traffic laws were included in some specifications, but were never significant.

in predicting future mortality. In this subsection we re-estimate the panel type models through 2004, reserving the last five-year period as a test of forecast accuracy. Because in the case of traffic legislation we use known values of the regressors in computing the predicted values of the dependent variable, these estimates are not true forecasts, but they can still serve as guides to "what-if" type analyses. It is often the case, moreover, that the likelihood of future legislation can be ascertained from political action, legislative initiatives, and so on.

For traffic mortality, we use three cases for comparison: 1) a no-change model that assumes growth in state traffic mortality for 15-19 year-olds during the 2005-2009 period will be the same as growth during the 2000-2004 period; 2) an income plus convergence only model (column 2 from Table 3 above); and 3) an income plus convergence plus traffic laws model (column 3 from Table 3). The criterion for comparison will be Mean-Squared-Error (MSE) of the one-step-ahead predictions of each of these models. For non-injury mortality, only cases 1) and 2) will be compared.

[Table 4 about here]

Table 4 presents the results of the forecast tests. Forecast accuracy is better for non-injury mortality in both the comparable cases, but referring back to Table 1, there is much less variability for this type of mortality. Using income and convergence alone improves the MSE for both types of mortality, much more so in the case of non-injury mortality; MSE in the latter is reduced by more than half from the no-change case.

Adding the actual values of legislation during the forecast period improves considerably the MSE for traffic mortality. This improvement is not completely realistic, of course, as it uses information not available at the time of forecast, but it does indicate that knowledge of upcoming legislation is valuable. Of particular importance in this case is GDL; there is no change in MLDA and only one change in ZT over the forecast horizon.

VI. Conclusion

This research set out to test three hypotheses: 1) that income levels protect against early mortality; 2) that the income gradient should be steeper for preventable types of mortality; and 3) that various measures of traffic legislation have been effective in reducing mortality. All three hypotheses receive support from the empirical analysis, with some important qualifications on the effectiveness of traffic legislation.

Income levels are shown to be negatively related to both traffic and non-injury mortality in young people, whether measured contemporaneously across states or, in a more demanding test, as a predictor of future changes in mortality. This evidence is consistent with previous findings on the income-mortality gradient, but up to now there have been few studies at the U.S. state level.

More novel is the finding that income has a differential effect on types of mortality. All types of mortality should be susceptible to the ameliorating effects of income; higher income buys better treatment, more medical facilities, better infrastructure, and all the other components that make for a healthier society. Still, in the particular case of traffic mortality in young people, income has an additional role to play in paying for more enforcement, better training, and not least, in better, newer, and safer vehicle stock. For example, Anbarci, Escaleras, and Register (2009, Table 1, p. 253) show that injury incidence and vehicle price are negatively related, with luxury SUVs having less than half the injury incidence of 2-door cars, but at three times the price.[17] In the area of enforcement, the correlation across all states between sworn law enforcement personnel per capita and income per capita in 2008 was 0.66 (U.S. Bureau of Justice, 2011). These data are only suggestive of course, as a full analysis of the linkages between income and the mechanisms for improved mortality are beyond the scope of this paper.

Some traffic legislation is shown to be associated with reductions in traffic mortality among young people. In particular, Graduated Driver's License laws are consistently effective across specifications. There is no evidence from this analysis that Zero Tolerance or Minimum Legal Drinking Age laws work as designed. Among laws aimed at the general population, seat belt requirements appear to save lives among 15-19 year-olds, but do not show effectiveness in the 20-24 year age group.

The findings of the full sample estimation are confirmed in tests of one-period-ahead forecasts. Models including initial income have lower MSEs than no change models, and in the case of traffic mortality, including legislation is a material improvement over income alone.

The association of higher levels of income with lower mortality does not lend itself to easy policy solutions. Higher income is a goal worth achieving for a host of reasons, improvements in life expectancy and longevity among them. Still, even when resources are constrained, measures that save young lives have a large payoff in measured against the expected years of life lost to early death from traffic mortality. Policy measures that appear to work include the full IIHS recommended Graduated Driver's License and Safety Belt programs, and these measures should be high on the legislative agenda of any state that lacks them.

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[17] Much of the price differential is due to the extra size and weight of more expensive vehicles, but the relationship is not one-for-one. Luxury cars, for example, weigh less than non-luxury SUVs, but cost more and have lower injury incidence.

Table 1. Descriptive Statistics, Median Household Income and Mortality Rates for 15-19 Year-old Population, U.S. States, 1984-2009.

	Median Income		Traffic Mortality		Non-injury Mortality	
5-year period ending	Mean	Variance[b]	Mean	Variance	Mean	Variance
1984	$43,961	2.39	40.6	6.27	15.8	1.25
1989	$46,354	2.79	38.4	5.34	14.5	1.64
1994	$45,321	2.09	32.9	8.36	14.3	1.18
1999	$50,107	2.04	29.9	7.20	13.7	2.00
2004	$50,146	2.10	28.8	10.24	13.2	2.98
2009[a]	$50,325	2.03	23.9	14.68	12.3	3.26

[a] 2007 for Non-injury Mortality
[b] Variance of the natural logarithm of each variable × 100.

Table 2 Regression of long-term growth in 15-19 year-old mortality rates on initial mortality and income, driving laws. State-level, 1984-2009.

Variable	(1) Traffic	(2) Traffic	(3) Traffic	(4) Traffic	(5) Non-injury	(6) Traffic, 41 states	(7) Traffic (20-24 years)
Fatality rate in 1984	0.007 (1.10)	-0.010* (2.19)	-0.011* (2.24)	-0.019* (3.33)	-0.017* (4.66)	-0.008* (2.05)	-0.001 (0.10)
Median Income, 1984		-0.055* (7.46)	-0.058* (6.33)	-0.052* (7.12)	-0.020* (3.43)	-0.053* (7.07)	-0.043* (-5.36)
Gini Coefficient, 1984			-0.039 (0.58)				
Minimum Legal Drinking Age				-0.002 (0.99)			0.001 (0.48)
Zero Tolerance Law				-0.001 (0.84)			-0.001 (0.56)
Graduated Driver's License				-0.008* (2.53)			-0.004 (1.00)
Seat Belts				-0.003 (0.89)			-0.001 (0.10)
Administrative License Revoc.				0.000 (0.32)			-0.001 (0.07)
BAC Limit				-0.002 (1.44)			0.002 (0.60)
Adjusted R^2	0.02	0.54	0.54	0.59	0.24	0.60	0.39
N	48	48	48	48	41	41	48

Notes to Table: The dependent variable is the average growth rate in the mortality rate (deaths/100,000 population) over the 25-year period (23 years for non-injury mortality). Initial (1984) values measured in logarithms, except for traffic control laws, measured in sum of effective state-years. Absolute values of *t*-statistics computed from robust standard errors in parentheses. * denotes significance at 0.10 or less.

Table 3. Regression of the growth in 15-19 year-old traffic fatality rates on initial income, driving laws, and economic variables. State-level, 1984-2009 in five-year non-overlapping averages.

Variable	(1) Traffic	(2) Traffic	(3) Traffic	(4) Non-injury	(5) Traffic, 41 states	(6) Traffic (20-24 years)
Fatality level year *t*-5	-0.004 (0.65)	-0.038* (4.58)	-0.041* (4.88)	-0.045* (4.41)	-0.027* (3.24)	-0.037* (4.28)
Median Income year *t*-5		-0.093* (6.23)	-0.090* (6.20)	-0.038* (4.30)	-0.082* (5.93)	-0.087* (-6.11)
Minimum Legal Drinking Age			0.019* (2.56)			0.003 (0.53)
Zero Tolerance			-0.010 (1.39)			-0.002 (0.32)
Graduated Driver's License			-0.039* (2.43)			-0.001 (0.06)
Seat Belts			-0.005* (2.32)			-0.001 (0.55)
Administrative License Revoc.			0.000 (0.08)			0.002 (0.40)
BAC Limit			-0.004 (0.54)			0.005 (0.58)
Adjusted R^2	0.003	0.273	0.322	0.281	0.397	0.240
N	240	240	240	205	205	240

Notes to Table: The dependent variable is the average growth rate in the mortality rate (deaths/100,000 population) over each five year period beginning in 1979 (3 year final period for non-injury mortality ending in 2007). Initial values measured in logarithms, except for traffic control laws, measured in sum of effective state-years. All regressions contain time effects. Absolute values of *t*-statistics computed from robust standard errors in parentheses. * denotes significance at 0.10 or less.

Table 4. Forecast Accuracy of Traffic and Non-Injury Mortality, 15-19 Year-Olds: Mean-Squared –Error of One-Step-Ahead Forecasts of 2005-2009 Growth

Model	2000-2004 Growth (No Change)	Income Only	Income + Laws
Traffic Mortality	0.00312	0.00231	0.00144
Improvement from No Change		-28.2%	-55.3%
Non-Injury Mortality	0.0011	0.0004	NA
Improvement from No Change		-63.5%	

Notes to Table 4: Forecasts of growth of 2005-2009 mortality (2005-2007 for non-injury) using data through 2004. "No change" is 2000-2004 growth.

Data Appendix

Table A1. Data Sources

Variable	Source
Traffic and non-injury Mortality, 1979-2007	Centers for Disease Control, CDC WONDER Compressed Mortality File at http://wonder.cdc.gov/mortSQL.html. Last accessed December 27, 2011
Traffic Fatalities, 2008-2009	National Highway Traffic Safety Administration (NHTSA) Fatality Analysis Reporting System (FARS) at http://www-fars.nhtsa.dot.gov/Main/index.aspx. Last accessed December 27, 2011
State Median Income	U.S. Department of Commerce, Bureau of the Census, Historical Income Data at http://www.census.gov/hhes/www/income/data/historical/household/. Last accessed December 27, 2011.
Graduated Driver's License and Safety Belts Laws and Ratings	Institute for Highway Safety (IIHS) Laws and Regulations at http://www.iihs.org/laws/default.aspx. Last accessed December 27, 2011. Accessed for data June 30, 2011.
MLDA, Zero Tolerance, BAC and ALR Laws	Various sources, originally from Zador, et al. (1989) and updated in Dee (2001) and Freeman (2007); most recent data from IIHS at http://www.iihs.org/laws/dui.aspx.

Table A2. Ratings of State Graduated Licensing Systems

Graduated Licensing Law	Requirement	Points
Permit age	16 or older	1 point
	Less than 16	0 points
Permit holding period	6 or more months	2 points
	3-5 months	1 point
	Less than 3 months	0 points
Required practice hours	30 or more hours	1 point
	Less than 30 hours	0 points
Restriction on night driving	10 pm or earlier	2 points
	After 10 pm	1 point
	No restriction	0 points
Restriction on number of passengers	1 or fewer	2 points
	2	1 point
	3 or more	0 points
Duration of night restriction	12 months or more from minimum licensing age	1 point
	Less than 12 months	0 points
Duration of passenger restriction	12 months or more from minimum licensing age	1 point
	Less than 12 months	0 points

Notes: Table reproduced (except for variable values) from Rebecca E. Trempel, "Graduated Driver Licensing Laws and Insurance Collision Claim Frequencies of Teenage Drivers" Highway Loss Data Institute, November 2009. Available at: http://www.iihs.org/research/topics/pdf/h0101.pdf.

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