THE DYNAMICS OF DRINKING AND DRIVING IN THE U.S.:  
THE ROLE OF SOCIAL FORCES AND THE ROLE OF LAW

Darren Grant  
Department of Economics and International Business  
Sam Houston State University  
Huntsville, TX 77341-2118  
dgrant@shsu.edu

Abstract: The dynamics of drinking and driving can be adequately described using the fraction of accidents involving drivers who had been drinking. Evaluating drunk driving legislation using this measure implicitly controls for unobservable “general risk” influences on traffic safety, reducing bias and variability in estimates of laws’ effects, especially in the early studies that influence lawmakers most strongly. Using this approach, we find that the widespread enactment of seven key drunk driving laws explains one-fifth of the reduction in drinking and driving since 1982, comparable to the effects of demographics and alcohol consumption, and less than the effect of “social forces.”

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*** This paper has several pages of figures that are best viewed in color. ***

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Studies of drunk driving legislation are staples of the health economics and health policy literatures. As a result, many key laws, such as .08 laws or zero tolerance laws, have been studied intensively. Yet many of the larger questions remain unaddressed. How have alcohol-related traffic fatalities declined: through reductions in the incidence of drinking and driving, the amount of alcohol such drivers consume, or the number of fatalities such drivers cause? What is the relative importance of state, regional, and national innovations? And, most importantly, what is the aggregate role of major legislation in reducing accidents involving alcohol, and how does it compare to that of other factors that have contributed toward the same goal? We address these questions and others, via the first comprehensive analysis of the dynamics of these accidents over the past two generations.

Doing this requires accounting both for tangible factors that can be measured and for intangibles that can’t. Our list of tangibles includes everything supported by the literature: several drunk driving laws, along with alcohol consumption, economic factors, and demographics. Beyond these are two intangibles of particular interest. The first, termed “general risk,” affects all drivers, not just those who have been drinking. It reflects a panoply of unmeasurables that are unquestionably important: the safety features of vehicles, quality of roads, effectiveness of emergency medical care, etc. The second, termed “social forces,” reflects public attitudes towards traffic safety in general and drunk driving in particular, and is equally difficult to measure. Because of the magnitude of both intangibles and their interplay with drinking and driving, accounting for their influence is essential.

The vehicle that we employ for this end, a simple latent variable model, is rooted in elementary theory, basic facts about the dynamics of drinking and driving, and previous studies of drunk driving legislation. This model helps reveal the roles of general risk and social forces on traffic safety, at the national and sub-national level, and supports an estimation approach that can reduce the bias they engender in estimating the effects of tangible factors.
This latent variable model, along with panel regression analyses and detailed descriptive statistics, describe how traffic safety in the United States has evolved over the past forty years, and reveal the footprint of the social processes shaping this evolution. The narrative supported by our empirical findings begins with social forces, which not only influence drinking and driving directly, but also presage future changes in the law. Initial law changes coincide with these social forces, leading their effects to be overestimated. This, and the occasional fiat of the federal government, then encourages the diffusion of these laws throughout the country, where their effects are smaller than anticipated.

This narrative clearly has policy relevance, and not only for what it says about the evaluation of drunk driving legislation. It reprises a debate that raged forty years ago, during the earliest years of our data, over the relative efficacy of legislation and social suasion. This debate was largely won by the former, a triumph of deterrence theory. But this triumph has been unmatched by persistent declines in drinking and driving, which is nearing two full decades of stasis. This stasis coincides not with the end of legislation, which is ever-increasing, but with static social attitudes. Even during the heyday of deterrence, during the 1980s, social forces appear to explain at least as much of the decline in drinking and driving as laws do.

Section I kicks things off by introducing the latent variable model and using it to conduct a comprehensive descriptive analysis of drunk driving dynamics. This model is then used to investigate the effects of laws, in Section II. The paper culminates in Section III, which decomposes the nationwide decline in drinking involvement in fatal accidents into components associated with laws, demographics, alcohol consumption, and social forces and other residual factors. Section IV concludes.
I. The Basic Dynamics of Drinking and Driving.

Some of the questions posed in the introduction can be answered with a simple descriptive analysis of the aggregate (national or state level) dynamics of drinking and driving.

The Data. By necessity, our analysis relies on the only extant long, nationwide panel of U.S. traffic outcomes: the Fatality Analysis Reporting System (FARS) of the National Highway Traffic Safety Administration (NHTSA). This records accident, vehicle, and driver characteristics for all fatal traffic accidents on public highways since 1975. Driver blood alcohol concentration (BAC) is reported in more than half of these, and, since 1982, is imputed for the others, mostly nondrinkers. Our primary analysis uses data through 2004, for reasons given later, for a total span of 30 years, 23 of which have reported or imputed BAC for each involved driver.\(^1\) Descriptive statistics are reported for all fifty states plus the District of Columbia; following Dee (1999), Freeman (2007), and others, regressions are conducted on the 48 continental states. While the FARS data are not a sample, for convenience we use the term “sample period” and call the random variation inherent in any probabilistic process, such as traffic fatalities, “sampling error.” The underlying fatality risk in any interval of time and space is imperfectly revealed by the observed fatality rate, because, fortunately, fatal accidents are

\(^1\) NHTSA’s imputation system provides ten imputed values for every non-reported BAC. The first value is utilized in the results reported in this paper; the others yield similar findings. Also, some initially low-reporting states dramatically increased BAC reporting in a discrete jump in the early 1980s; this is associated with discrete jumps in measured drinking involvement. This sharply biases the estimated effect of the raised MLDA, passed contemporaneously, toward zero. Thus, the regressions below omit from the sample those few years prior to the jump in reporting in those states. The affected states and the last year of omitted data are as follows: AL, 1982; AR, 1989; FL, 1985; ID, 1984; IN, 1985; IA, 1982; KS, 1987; MS, 1991; MD, 1985; NC, 1982; ND, 1984; TX, 1985.
infrequent, following a Poisson process around their expected value.

The data has two major limitations. First, it contains only fatal accidents. These do, however, generate half of all the economic costs of accidents involving alcohol (Blincoe et al., 2002). Second, the imputation of some BACs could affect estimates of state laws’ effects, as imputations are not conditioned by state. This should not be a major problem, because few drinkers’ BACs are imputed and because the strongest predictors of driver BAC are accident-specific factors such as driver age, passenger BAC, and police reported drinking involvement. Estimates presented below, and others available from the author, indicate the basic findings are not corrupted by imputation. Neither limitation has prevented numerous researchers from using this data to analyze the effects of drunk driving laws.

With this data we now identify the statistic on which to focus our investigation, determine its contribution to fatalities, and delineate the temporal and spatial scales over which it evolves.

The Dominance of the Extensive Margin. The foremost fact about the aggregate dynamics of drinking and driving is simple: they take place almost wholly on the extensive margin—whether to drink and drive. The intensive margin, BAC conditional on drinking (BAC > 0), is essentially static.

To show this at the national level, Figure 1 documents the 25th, 50th, and 75th percentiles of BAC for all drinking drivers involved in fatal accidents in the U.S. between 1975 and 2004. In all years the BAC distribution is essentially normal with a mean of about 0.16, a standard deviation of about 0.08, and an interquartile range of about 0.05, whether or not the imputed BACs are included.

To show this at the sub-national level, we calculated the 50th percentile of BAC (conditional on drinking) within each state*year cell, and regressed these values on a full set of state and year
dummy variables. The standard deviation of the state dummies was .008, indicating geographic stability, while the standard error of the estimate—some of which derives from sampling error—was .011, indicating that temporal stability extends to the state level. The 25th and 75th percentiles yielded very similar results.

Thus, changes in drinking and driving can be tracked using simple measures of alcohol involvement, such as the fraction of fatal accidents involving BAC-positive drivers, or the fraction of fatalities occurring in such accidents. Our primary estimations use the former measure, which we call HBD (Had Been Drinking), as the accident is perhaps the most natural unit of analysis. This choice is also facilitated by the irrelevance of another intensive margin, between BAC and the number of fatalities per accident. Relating this to state dummies, year dummies, and the highest BAC among the drivers involved, we find that each .01 increase in BAC generates a minuscule additional 0.0008 fatalities per accident. Drinking materially affects only the chance an accident will occur in the first place (except for extremely rare, extremely high BACs, in the neighborhood of 0.50).

National Dynamics. At the national level, it is easy to describe how drinking and driving has changed over time. According to NHTSA, HBD nationwide fell from 55% in 1982 to 36% in 1997, and has been flat since then. (The fraction of accidents involving drivers with a BAC of at least .08, the current per se illegal threshold, exhibits the same trend, or lack thereof.) This decrease, depicted in the top line of Figure 2 for a subset of accidents analyzed below, will be explained in the decomposition culminating this paper.

This decline was the result of an evolutionary process in which changed attitudes toward drunk driving, and their political consequences, coursed through society. This can be illustrated with
age profiles of the percentage of BAC-positive drivers that are involved in fatal accidents. The first two panels of Figure 3 shows how these profiles have changed over time, using five-year time intervals, both including and excluding the imputed data. (Including the imputations increases the magnitude of change but leaves the relative rates of change unaffected.) The last panel of the figure depicts the change in the fraction of BAC-positive drivers relative to the “base years” of 1975-1979. (This uses the unimputed data, the only data that extends back to 1975.)

The vanguard of decreased drinking and driving, apparent in the latter two panels, was an early-1980s decline in alcohol involvement among drivers over forty. This probably resulted from increased awareness of the dangers of drunk driving during this period, and reduced tolerance for it, both of which are well-documented (see below), and both of which are political precursors to the legislative activity on this issue that was initiated toward the end of this period (see Howland, 1988).

This was followed, in the late 1980s and early 1990s, by a substantial decline in drinking and driving among all ages. The greatest progress clearly occurs during this period. By the mid-1990s declines in alcohol involvement among drivers over forty had largely played out, with the remaining decreases concentrated among drivers aged 20-40. While drunk driving legislation could certainly have played a role in this evolution, it does not easily explain these age patterns, which are not closely connected to the primary legislation enacted during each of these periods. Thus, while the figure suggests the relevance of legal sanctions in reducing the rate of drinking and driving, it also indicates the limitations of these laws and, thus, the relevance of other factors.

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2 The same conclusion comes from considering birth cohorts. Their evolution can be traced by connecting the dots diagonally, as in the dashed line in Figure 3c, which tracks one of the last cohorts free to drink during their formative years (individuals aged twenty-one in 1984).
Alcohol Involvement and Traffic Safety. A simple latent variable model puts these declines in context and illustrates their effect on fatalities. While such a model is not needed to analyze national dynamics—back of the envelope calculations would do—it will be vital for analyzing sub-national dynamics and for the estimation approach adopted in the next section.

Define the following variables, using upper case for those that can be observed and lower case for those that cannot:

- $s$ = the miles driven by sober drivers,
- $d$ = the miles driven by drinking drivers,
- $r$ = the general risk environment, due to weather, road quality, automobile technology, general safety laws, general safety attitudes, etc., and
- $M = s + d$ = total miles driven.

The actual outcomes and the latent variables—the expected outcomes—are defined as follows:

- $F$ = the number of fatal accidents,
- $f = \text{the expected number of fatal accidents, so that } F \sim \text{Poisson}(f)$,
- $H = \text{HBD, the fraction of fatal accidents involving drinking drivers, and}$
- $h = \text{the expected value of } H \text{ given } d \text{ and } s, \text{ so that } F \cdot H \sim \text{Binomial}(F, h)$.

If $k$ is the average fatal crash risk of drinking drivers relative to sober drivers, then $h = \frac{kd}{s+kd}$.

While only a small fraction of drivers drink, the typical drinking driver is far more risky than a sober driver is. In an exhaustive study that extends decades of epidemiological research, Blomberg et al. (2005, 2009) carefully assess how BAC influences crash risk. Colloquially, this risk doubles with each standard drink beyond two. Given the BACs of accident-involved drinkers, the average crash risk of drinking drivers is sixteen times that of sober drivers, ceteris paribus; the fatal crash risk is higher (Blincoe et al., 2002). Thus, $k >> 1$ and almost all collisions between sober and drinking drivers are the drinking driver’s fault (Levitt and Porter, 2001).

Using this fact, a simple, intuitive decomposition of accident frequency can be generated. Fatal accidents equal the sum of those involving only sober drivers and those involving drinking
drivers: $F = F_{SOBER} + F_{DRINKING}$. In expectation, the latent variable equivalent is $f = f_{SOBER} + f_{DRINKING}$.

Without any loss of generality, let $f_{SOBER} = rs$ and $f_{DRINKING} = rkd$. Then:

$$f = (s+kd) \cdot r = (s+d) \cdot r \cdot \left(\frac{s+kd}{s+d}\right) = M \cdot r \cdot \left(\frac{s+kd}{s+d}\right) = M \cdot r \cdot \frac{1}{1-h} \left[1 + \frac{h}{k(1-h)}\right]$$  \hspace{1cm} (1)

When $k(1-h) \gg 1$ the bracketed term approaches one, yielding the following close approximation:

$$\log(f) - \log(M) = \log(r) - \log(1-h)$$  \hspace{1cm} (2)

Expected per mile fatal accidents are directly proportional to general risk and inversely proportional to the expected fraction of crash-involved drivers who had not been drinking, which we call “relative sobriety.”\(^3\) The same relation applies to fatalities, where $h$ is the expected fraction of fatalities occurring in accidents involving drinking drivers; it holds in difference form as well.

Equation (2) approximates an identity. Our finding that the distribution of BAC among drinking drivers is static recommends HBD for analysis but is not required for this derivation. Neither is the finding, discussed at length in Grant (2015a), that $k$ appears to have changed little in decades. All that is required is that $k(1-h) \gg 1$ at all points in time. This is easily satisfied.

At the national level the effect of sampling error is minimal, so $f$ and $h$ in equation (2) can be replaced with their empirical counterparts and $r$ solved for directly. Doing this for every year, treating 1982 as the base year, yields the decomposition in Figure 4, which depicts the effects of general risk and relative sobriety on total U.S. traffic fatalities from 1982-2012. The upper line denotes the projected growth in log fatalities, relative to the base year, that would be required to

\(^3\) Scaling fatalities by miles, a common and natural normalization in studies of traffic safety, is a serviceable approximation for purposes of this section, and is roughly consistent with estimates of the model introduced below, which does not mandate that fatalities be proportional to miles.
“keep up” with the increase in miles driven, so that fatalities per mile remained constant. The top shaded area indicates the “shortfall” in fatalities, below this projection, that is attributable to reductions in drinking and driving. The bottom shaded area indicates the shortfall attributable to reductions in general risk.

The improvements in traffic safety occurring over this period can be broken down into three distinct phases. In the first phase, comprising most of the 1980s, all reductions in fatalities are attributable to reductions in drinking and driving. While improvements in vehicle technology and road quality probably helped bring down general risk during this phase, these were more than offset by a large reduction in real gas prices and the changed driving behaviors that accompanied it (see Grabowski and Morrisey, 2004). After real gas prices stabilized in the late 1980s, steady declines in general risk accompanied continued declines in drinking and driving, for a reduction in overall fatalities despite an increase in miles driven. This continued until the end of the second phase, in the late 1990s. The relative sobriety component remained unchanged throughout the third phase, consistent with the post-1997 constancy of HBD, but reductions in general risk continued apace, accelerating toward the end of the period, consistent with the decreased economic activity and increased real gas prices of the Great Recession. The stark implication of this finding: drunk driving is essentially unchanged in nearly twenty years. The decline in alcohol-related fatalities is attributable only to reductions in general risk, which have improved safety for both sober and drinking drivers.

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4 The value of this underappreciated improvement in traffic safety amounted to 1% of Gross Domestic Product per year. Fatalities fell by 10,000 people annually; multiplying this by a Value of Statistical Life of $7 million (consistent with the guidelines of the Department of Transportation), and then by two to account for damages in non-fatal accidents (Blincoe et al., 2002) yields a total of $140 billion, which is roughly 1% of GDP.
Sub-national Dynamics. The exercise we have just conducted emphasizes the conceptual distinction between relative sobriety and general risk, and the important role each plays in traffic fatalities. These points emerge even more strongly in an exploration of sub-national dynamics.

This exploration can also be conducted using the latent variable model, by evaluating each term within state*year cells. At this level of analysis, however, sampling error is prevalent, so we cannot apply the model directly. Rather, as the Appendix shows, using $F$ and $H$ we can estimate the statistical properties of the underlying latent variables, $f$ and $h$, after subtracting state and year fixed effects. Then, using equation (2), we can break down the analogous variation in logged fatalities per mile into components associated with $\log(r)$, $\log(1-h)$, and their interaction, and use the method of moments to estimate the properties of each.

To reduce extraneous variation, we focus on the accident type and age group with the greatest drinking involvement: single vehicle accidents involving drivers aged 21-40. Selecting all of these over the 1982-2004 period, we calculated HBD by state by year (1173 state*year cells), and then regressed these values on state and year fixed effects. The state effects have a standard deviation of 6.9 percentage points, with heavy-drinking Wisconsin at the top and light-drinking Utah at the bottom, thirty percentage points below. These and the year effects explain about two-thirds of the variance in the dependent variable. Still, the residuals, which track within-state variation in HBD that has been purged of national trends, have a standard deviation of 5.7 percentage points.

Most of that variation is attributable to sampling error, however, as Table 1 shows. The first row presents the standard deviation, spatial correlation, and serial correlation of observed HBD,\(^5\)

\(^5\) Spatial correlations are calculated across matched pairs of neighboring states, like those used in “case-control” studies of drunk driving laws (e.g., Williams et al., 1983; Arnold, 1985). Vermont is matched with New Hampshire, for example, and New Mexico with Arizona; the full set of pairs is listed in the note to Table 1. The case-control design deems the paired states to be identical but
unadjusted for sampling error, while the adjusted values, for the latent variable, are placed in the third row. The second and fourth rows present the analogous values for log fatalities per mile. For both \( h \) and \( f \), the spatial correlations are 0.4, indicating only modest behavioral spillovers across states, but the serial correlations are 0.8 at a one year lag. For HBD, this correlation equals 0.7 at a three year lag, then drops off sharply, suggesting a moving average, rather than autoregressive, component.

The variation in log fatalities per mile that is not attributable to state fixed effects, year fixed effects, and sampling error is 8.1 percentage points. The contributions of general risk, \( \log(r) \), and relative sobriety, \( \log(1-h) \), to this variation are found in the table’s last two rows. Locally, the variance of the general risk component is double that of the relative sobriety component. The two components are only weakly correlated (probably from local economic activity, which decreases alcohol involvement while increasing general risk, through more aggressive driving). Altogether, drinking and driving evolves along the extensive margin, non-uniformly by age, and is characterized by a large national component punctuated by brief (three to five year), local (state-specific) innovations that are dwarfed by, and largely independent of, local innovations to general risk.

II. Estimation.

Incorporating laws into our understanding of local dynamics requires estimation. Unfortunately, the results in Table 1 bode ill for panel fatality analyses: even in the set of accidents with the highest drinking involvement, most of the local variation in the dependent variable has nothing to do with drinking. Even modest partial correlations between drunk driving laws and 

for the law in question; this is contradicted by the low values reported.
general risk could substantially bias estimates of laws’ effects. An estimation method based on our latent variable model confirms the presence of this bias and eliminates it, in the process revealing the effects of social forces at the sub-national level and their connection with drinking and general risk.

Assume the presence of three sets of variables: general risk adjusters, \( G \); factors affecting drinking and driving but not general risk, \( X \); and factors that affect both, \( Z \). Then:

\[
F \sim \text{Poisson}(f) \\
\log(f) = \mu \log(M) + \log(r(G, Z)) - \log(1 - h(X, Z))
\]  

(3)

where we have relaxed the elasticity between miles and fatalities to be a parameter, \( \mu \), that need not equal one. Then the effects of \( X \) variables, such as drunk driving laws, on fatalities are:

\[
\frac{\Delta \log(f)}{\Delta X} = \frac{1}{1 - h} \frac{\Delta h}{\Delta X}
\]

(4)

In this formulation, the effect of \( X \) on fatal accidents is mediated through \( h \): drunk driving laws reduce fatalities because they reduce drunk driving. This is equivalent to restricting the \( X \) variables to have no effect on general risk. As general risk is identified from accidents involving sober drivers only, this restriction appears to be unexceptional.

The difference between this formulation and a traditional fatality analysis that relates laws directly to fatalities is threefold: it eliminates bias through unmeasured elements of \( G \) that correlate

\[\text{Compensating variation, in which drivers become less careful because they expect fewer drunks to be on the roads, would imply } \Delta r/\Delta X \geq 0 \text{ for laws that reduce drunk driving. While compensating behavior exists for vehicle factors such as seatbelts and antilock brake systems, its presence here is unclear, because it would not entail preventing one’s own driving foibles, but rather anticipating those of others. There does not appear to be an argument (or evidence) in the literature that drunk driving laws are materially weakened by this kind of compensating behavior. Nonetheless, the constraint here binds, as the bias uncovered below goes in the other direction.}\]
with $X$ in the population, reduces bias through similar unmeasured elements of $Z$ (to the extent they influence $r$), and reduces variability in the estimates arising from “incidental” (sample) correlations of unmeasured risk influences with $X$ (which can easily arise in the 10-20 year panels common in the literature, as some elements of $G$, $X$, and $Z$ evolve slowly). Systematic differences in the two sets of estimates therefore suggest the presence of bias due to unmeasured general risk confounders.

To evaluate equation (4) with state panel data, first estimate $\Delta h/\Delta X = \beta$ as follows:

$$
F_{s,t}; H_{s,t} \sim \text{Binomial}(F_{s,t}, h_{s,t})
$$

$$
\hat{h}_{s,t} = \beta X_{s,t} + \gamma Z_{s,t} + \sigma_s + \tau_t + \epsilon_{s,t}
$$

where $s$ indexes states and $t$ time; $\gamma$ is a coefficient vector; and $\sigma$ and $\tau$ are state and year fixed effects.\(^7\) This specification is similar, but not equivalent, to a standard panel regression regressing $H_{s,t}$ on the independent variables: it specifies the sampling variation directly, in the top line, introducing a random effect, $\epsilon$, to capture unmeasured factors. Here, both specifications yield very similar results: equation (5) is used because it follows directly from the latent variable model and has a natural form, that of a generalized linear mixed model, or GLMM (McCulloch, 2006).\(^8\) After $\beta$ is estimated, the estimated population change in log(fatalities) is $\hat{\beta} / (1-H_{M})$, where $H_{M}$ is the grand mean of $H$.

\(^7\) The term that is dropped when deriving eq. (2), $h/[k(1-h)^2]$, is small only when $h$ is not close to one. This rules out using $\log(1-H)$ directly as a dependent variable when there are few fatal crashes within state*year cells (as for young drivers). Then $H$ deviates substantially from $h$, due to sampling error, and can be very close to one; the resulting bias is severe.

\(^8\) The GLMM can be considered a hybrid of OLS and “traditional” WLS using population weights. Large states are weighted more than smaller ones, but not so much as in traditional WLS. The issue of weighting is surprisingly thorny (Solon, Haider, and Wooldridge, 2013). By formally accounting for sampling variation as well as specification error (via a random effect), the weighting implicit in the GLMM should be more sound than either OLS or traditional WLS.
Implementation. Our estimations focus, though not exclusively, on three drunk driving laws: the minimum legal drinking age (MLDA), zero tolerance (ZT) laws lowering the per se illegal BAC for youth to .01 or .02, and laws lowering the per se illegal BAC for adults to .08. This is natural, both to compare estimation approaches and to explain reductions in drunk driving. Each of these laws has a mature, reasonably convergent literature, amounting to more than one hundred published studies, and is universal within the U.S., partly due to federal legislation withdrawing highway funds from “non-adopters.” Our sample period of 1982-2004 encompasses virtually all state adoptions of these laws, comfortably exceeding the post-1997 stasis in HBD without much exceeding the samples of the longer studies in the literature. All law variables range in value from zero (nonexistent) to one (full coverage in that state all year long).\(^9\)

To facilitate a comparison of estimation approaches, we adopt those controls that are reasonably standard in the literature. Based on the review of Grant (2011), these are as follows. General risk controls, G, include seat belt laws and speed limits. Factors affecting drinking and driving, X, include drunk driving laws and a measure of alcohol price or consumption. Factors affecting both drunk driving and general risk, Z, include economic factors and demographics (added in the individual-level regressions found in the next section).\(^10\) The X and Z vectors are included in fatality and HBD regressions, while G is included in the fatality regressions only (and is generally

\(^9\) In intermediate cases the value equals the fraction of the relevant population covered by the law in that state during that year. For MLDA laws, then, the fraction of 18-20 year olds covered by the law is multiplied by the fraction of the year the law was in effect. All laws are coded from the *Digest of State Alcohol-Highway Safety Related Legislation*, supplemented occasionally with Dang (2008) or Grant (2010).

\(^10\) Measures of religious affiliation, included in several studies, are omitted here, because they are measured in just two years, 1980 and 1990, and extrapolated to the rest (see, for example, footnote 21 of Benson, Rasmussen, and Mast, 1999).
insignificant in the HBD regressions, as expected). All variables are measured at the state*year level.

To reflect the variety of specifications in the literature, we increase the number of controls in stages. Our basic set includes, for G, dummies for primary and secondary seat belt laws and for the maximum speed permitted in that state that year; for Z, the unemployment rate; and for X, the laws above, along with dummies for .10 per se laws and administrative license revocation (ALR) laws that allow the state to suspend or revoke an individual’s license immediately upon testing positive for drunk driving or refusing to be tested. (The .10 law dummy equals one whenever the per se limit is at or below .10, so the .08 law coefficient estimates the effect of lowering the per se limit from .10 to .08.) An “extended” set of controls adds (to X) per-capita alcohol consumption and dummies for open container and dram shop laws, the only other drunk driving laws that receive consistent support in the comprehensive panel studies in the literature (Benson and Rasmussen, 1999; Eisenberg, 2003; Ruhm, 1996; Whetten-Goldstein et al, 2000).

Drivers under 18 were not directly affected by the raised MLDA, while drinking involvement among drivers over 60 is quite low (see Figure 3). Thus, we conduct estimation separately for two age ranges: adults aged 21-60, for whom ZT and MLDA laws are excluded, and youth aged 18-20, for whom they are included. (The youth regressions retain the ALR, .08, and .10 law controls, but these coefficients are not reported below.) HBD is defined as the number of fatal accidents or fatalities involving at least one driver in the specified age range who had been drinking, divided by the number of fatal accidents or fatalities involving drivers in the specified age range.

**Basic Results.** Our initial GLMM estimates use equation (5) and its counterpart for fatal accidents:
The tables below present only the coefficient estimates on the law variables, multiplied by one hundred. For this semi-log specification, these predict the percentage change in fatal accidents resulting from that law’s implementation. For the HBD estimates these predict the change in HBD, in percentage points, resulting from that law’s implementation.

Three different fatality measures are employed in equation (6): the number of fatal accidents involving drivers in the given age range, the total number of fatalities in those accidents, or the number of fatal single-vehicle accidents involving said drivers. The findings for these are presented in the first two columns of Table 2, and the findings for the analogous HBD measures in the last two columns of the table. This table is organized by law, not regression: thus, the upper-left cells of the .10 law, .08 law, and ALR panels in the table each come from the same regression, and so on. As expected, given the dominance of the extensive margin documented above, the estimates are not sensitive to the fatality/HBD measure used.

The fatality estimates, however, change substantially when counting only those accidents with a drinking driver, moving from the first column of Table 2 to the second. This sensitivity occurs along other dimensions, too, as shown in Table 3. Estimates respond to the weighting applied to the observations, across the first, second, and third rows of the table, and to the inclusion of additional controls, in the fourth row. Coupled to this are some unusual findings, implying that .10 and .08 laws raise fatalities and that the drinking age is ineffectual. In toto, these results are indicative of incidental or systematic correlations of these laws with sizeable, slow-moving general risk factors not captured
by the limited controls found in these (and other) fatality regressions.\textsuperscript{11}

If this is true, the HBD regressions in Tables 2 and 3 should cancel out the effect of these factors, generating improved estimates. They do, and they are—in three ways. The estimates are more credible: .08 laws now lower fatalities by 2%, while raised MLDA’s reduce them by 3-6%; ALR, ZT, and .10 laws are impotent. They are more robust: laws’ estimated effects on fatalities or fatal accidents are remarkably stable, rarely varying by more than one percentage point across specifications. And they are more precise: the standard errors of these effects, found in the second panel of Table 3, are far smaller than in the analogous fatality regressions. All this is consistent with the presence of unmeasured general risk factors that substantially impact fatality regressions, but not those using HBD.

Altogether, the fatality reductions implied by the HBD estimates not out of line with those in the body of extant panel fatality analyses, just somewhat milder than the norm. Early panel studies of .08 laws, particularly Dee (2001) and Eisenberg (2003), find that the .08 law’s net effect is about 3%, while more recent panel studies by Young and Beilinska-Kwapisz (2006) and Freeman (2007) find effects that are smaller or nil. Similarly, while the early MLDA literature finds very large effects, six later panel estimates (Dee, 1999; Eisenberg, 2003; Young and Likens, 2000; Young and Beilinska-Kwapisz, 2006; Polnicki et al., 2007; and Miron and Tetelbaum, 2009) average six or seven percent. A similar trend is also found for ZT laws; the most recent fatality analyses, by Dee, Grabowski, and Morrisey (2005), Grant (2010), and Anderson, Hansen, and Rees (2013) find no

\textsuperscript{11} Compared to the literature, these findings for the MLDA, .08 laws, and .10 laws are quite inauspicious. This reflects the relatively long sample period, as shown below, and, for .08 and .10 laws, the fact that the GLMM does not weight all states equally. Higher-profile studies of these laws do not use weights; as Table 3 shows, these yield most favorable estimates.
material effect. (Grant, 2011, extensively reviews all three literatures.) Our nil findings for .10 laws and ALR are also milder than is typical in the literature (Dee, 2001; McArthur and Kraus, 1999).

A few other studies in the literature also use HBD regressions, or their equivalent.\(^{12}\) Ironically, their very favorable findings differ from ours more than panel fatality analyses do. But this difference is more illusory than real. These studies almost universally omit state fixed effects, allowing cross sectional variation to influence coefficient estimates—which it does, strongly, in estimations conducted on our data. Thus, these studies’ findings stem from omitted variable bias.

**Stability of Estimates across Sample Periods.** Though general risk induces, at best, a modest degree of bias in the long panel fatality regressions that dominate the literature, its effects do not end there. They appear, more subtly, in “real time estimation” of the effects of new laws that have only been adopted by a few states. These early estimates greatly influence the extent of a law’s diffusion throughout the country (Grant 2011, 2015b). In Grant’s (2011) review of the MLDA, ZT, and .08 literatures, such studies are much more favorable than later studies of the same law.

This trend exists even within a given regression specification. To show this, we conduct WLS fatality regressions—the most common type in the literature—for a succession of increasingly lengthy sample periods. (These used the basic set of controls, and maintained the same age categories as before. Other estimators yield similar patterns.) We begin with the period 1982-1992, before the

\(^{12}\) This has been obscured by the fact that, instead of analyzing HBD directly, many such studies scale \(F_{\text{DRINKING}}\) by \(F_{\text{SOBER}}\), which is a simple transformation of HBD (Hingson, Heeren, and Winter, 1996; Robertson, 1989; Voas, Tippetts, and Fell, 2000, 2003; Fell et al., 2008). These obtain large reductions in fatalities: 11-40% for the MLDA, 8-16% for the net effects of .08 laws, and 14-24% for ZT laws. Of the remaining studies, Levitt and Porter (2001) do not examine the laws studied here, Dang (2008, Table 9) is discussed below, and Grant (2010)—the only fully longitudinal HBD analysis—supports the results obtained here.
widespread adoption of .08, ALR, and ZT laws, and repeatedly lengthen the sample by three years until we reach the full sample period of 1982-2004, in which virtually all these laws were adopted nationwide.

The results are found in the top panel of Table 4. Trends in the estimates of the effect of any given law can be gleaned by reading vertically within the panel. The early ZT estimates are imprecise and aberrant, as very few states adopted ZT laws before 1993. Excepting these, the coefficient estimates trend strongly as the sample period lengthens, generally in a less-favorable direction: the strongest effects occur in early studies, as suggested above. The typical difference between the most-favorable and least-favorable estimate of each law’s effect is a sizeable four percentage points.

This trend should be interpreted as bias accruing from general risk. The analogous HBD estimates, in the bottom panel of the table, are (except for the earliest ZT coefficient) remarkably stable across sample periods, exhibiting neither trend nor substantial variability. The typical difference between the most-favorable and least-favorable estimate of each law’s effect on fatalities is one percentage point. “Real time” HBD estimates, which factor out bias from general risk, are far more reliable indicators of new laws’ effects on traffic safety, and thus are particularly useful to policymakers considering whether to adopt a law based on the experience of other states that have recently done so.

What causes this bias? The most plausible candidate is the mechanism that brings these laws into being (see Besley and Case, 2000; Boettke, Coyne, and Leeson, 2008; and Grant, 2011, 2015b). For new laws, which have not been widely adopted nationwide, adoption is encouraged by a decreased public willingness to engage in or tolerate unsafe driving behavior (see Reinarman, 1988, for the MLDA, and Vereeck and Vrolix, 2007, for traffic safety generally). This general risk factor
biases early fatality estimates of the law’s effect, but not HBD estimates. Eventually, as support for
the law builds, reinforced by federal incentives, such social forces are no longer needed to enact it,
and later adoptions in laggard states are not accompanied by these general risk factors. Fatality
estimates eventually become less favorable and more closely resemble HBD estimates, as we found
in the previous subsection.

This explanation relies on social forces that affect drinking and sober drivers alike: the
intersection of social forces and general risk. But other social forces can be directed at drunk driving
specifically. The next section explores the magnitude of such social forces, which appear to operate
on a similar dynamic: they are most prevalent initially, reinforcing and giving rise to later legislation,
which slowly diffuses throughout the country.

III. Explaining the Decline in Alcohol Involvement in Fatal Accidents.

In this section, we conduct a decomposition of the decline in alcohol involvement in fatal
accidents since 1982. This is best done using an individual-level specification that can be applied to
microdata, which controls for driver and accident-specific factors that correlate with drinking.

Estimation on Microdata. Consider a logit model, in which the probability that an accident-involved
driver has been drinking depends on driver and accident-specific factors, contained in a vector \( D \); a
vector of laws, \( L \); and other state-level factors, such as unemployment or alcohol consumption,
contained in a vector \( S \). Define \( \eta \) as a dummy variable indicating whether a particular driver has a
positive BAC, and let \( \Lambda \) be the logistic function. Then:
An ideal comparison removes the age dummies, which affect the estimates little except for the coefficient on the MLDA. This changes substantially because, early in the sample period, many states set the MLDA at nineteen or twenty; thus the incidence of the MLDA is correlated with age, which itself correlates with drinking. Age dummies must be included to remove this bias.

\[ P(\eta_{i,t} = 1) = \Delta(\phi D_{i,t} + \gamma L_{i,t} + \phi S_{i,t} + \sigma_t + \tau_s + \epsilon_{i,t}) \] (7)

where \( i \) indexes individuals, \( \phi, \gamma, \) and \( \psi \) are coefficient vectors, and \( \epsilon \) is a normally distributed state*year random effect that is absent in the pure-logit version of this model and present in the GLMM version. For single-vehicle accidents, clearly, the average marginal effect of a one-unit change in any variable \( X \), calculated numerically, estimates \( \Delta h/\Delta X \). As we will show, estimates can be obtained for multiple-vehicle accidents as well.

This individual-level specification, possible only in an HBD analysis, has two unique features. It accounts for demographics more effectively than state-level analyses using population averages, such as the fraction of drivers in a given age range. Increases in miles driven by females and older drivers, who drink less and crash less even when sober, make demographics potentially important, despite their scarce attention in the literature (Dang, 2008). It also specifies the law variables precisely for each driver, using the accident date and driver age (see footnote 9).

The last panel of Table 3 presents individual-level logit coefficients estimated using this specification, using the basic set of controls identified above, supplemented only with a set of age dummies and a set of dummies for the number of vehicles involved in the accident.\(^{13}\) The estimates in the first row, for all accidents, closely resemble those for single-vehicle accidents only, in the second row, for which a logit specification is most conceptually appropriate. Similarly, coefficients are little affected by the inclusion of state*year random effects or the extended controls. But

\(^{13}\) An ideal comparison removes the age dummies, which affect the estimates little except for the coefficient on the MLDA. This changes substantially because, early in the sample period, many states set the MLDA at nineteen or twenty; thus the incidence of the MLDA is correlated with age, which itself correlates with drinking. Age dummies must be included to remove this bias.

21
demographics matter. The implied percentage change in fatalities for the last two specifications are found in the “transition panel” between the state-level and individual-level HBD results. In the extended-controls specification, laws’ effects on fatalities closely resemble those in the comparable state-level specification. The effects fall modestly when driver and accident controls are included.

This model can be used to offer a reasonably comprehensive explanation of the nationwide reduction in HBD, because—with one exception—its independent variables include all primary influences on drinking and driving: legal incentives, demographics, alcohol consumption, and economic variables, the last two grouped together in the vector of state factors. (An equivalent explanation for fatalities remains elusive: too many general risk factors, such as improvements in vehicles and roads, cannot be quantified.) The remainder then contains the nationwide effect of the one unmeasurable influence, social forces, along with that of any other residual factors.

Define \( t=0 \) as a base year, and consider the following four equations:

\[
H_t = \bar{H}_t = \bar{H}_0 = \frac{A(\phi D_{L,D} + \sigma_d + \gamma L_{L,D} + \psi S_{L,D} + \tau_i)}{L_t} = H_t
\]  

(8)

\[
E(H_t | L = L_0) = \frac{A(\phi D_{L,D} + \sigma_d + \gamma L_{L,D} + \psi S_{L,D} + \tau_i)}{L_t} = \hat{L}_t
\]  

(9)

\[
E(H_t | L = L_0, S = S_0) = \frac{A(\phi D_{L,D} + \sigma_d + \gamma L_{L,D} + \psi S_{L,D} + \tau_i)}{L_t} = \hat{S}_t
\]  

(10)

\[
E(H_t | L = L_0, S = S_0, t = 0) = \frac{A(\phi D_{L,D} + \sigma_d + \gamma L_{L,D} + \psi S_{L,D} + \tau_i)}{L_t} = \hat{S}_0
\]  

(11)

The difference between HBD nationwide in any given year, \( H_t \), and HBD in the base year, \( H_0 \), can be broken down into four components: laws, the difference between the first two equations; state factors, the difference between the next two equations; social forces and other residual factors, the difference between the two equations after that; and demographics, the difference between equation
(11) and $H_0$. (Note that the state fixed effects have been subsumed into the category of demographics.) An alternative decomposition, using base year demographics, yields similar results.

Though social forces cannot be precisely measured, their relevance is widely recognized in the drunk driving literature and by policymakers, and their presence during our study period is well documented. The media coverage devoted to drunk driving, and the number of organizations dedicated to combatting it, both increased rapidly during the 1980s (Howland, 1988). Surveys (Greenfield and Room, 1997), academic analysis (Reinarman, 1988; Linkenbach and Young, 2012), and numerous contemporaneous quotes by traffic safety officials (Grant, 2015b) testify to a concomitant change in social attitudes. None of this is new: DeCicca et al. (2008) document a similar effect of anti-smoking sentiment on smoking, which seems to favorably bias the estimated “deterrent” effect of cigarette taxes, while the ratification (Okrent, 2010) and repeal (Kyvig, 2000) of Prohibition were accompanied by similar phenomena.

Likewise, the technique we use to ascertain the effects of social forces is grounded in an academic precedent: the standard wage decomposition for estimating the effects of labor market discrimination. That literature contains hundreds of studies in which the effects of gender and race discrimination are inferred to be the group-wise difference in productivity-adjusted wages. Here, the effects of social forces are inferred to be the temporal difference in alcohol involvement, adjusted for the effects of demographics, state factors, and laws.

**Implementation.** The validity of this indirect approach depends on our ability to account for all other primary influences on the dependent variable. Accordingly, we err on the side of inclusiveness, employing in this model all variables used in the expanded specification above.
The vector $D$ includes driver age and gender, dummies for the hour, day, and month of the accident, and—when warranted—dummies for the number of vehicles involved in the accident.

The vector $S$ includes per capita alcohol consumption and the unemployment rate. The alcohol consumption measure included in $S$ could be influenced by economics, social forces, or laws, and below we will suggest the relative importance of each.

The vector $L$ includes indicator variables for all seven drunk driving laws analyzed above, including those in the expanded specification. The practical case for legislative drunk-driving countermeasures rests squarely on these seven laws: no others have received appreciable support in the academic literature, strong financial incentives from Congress, or emphasis from NHTSA. For example, these laws include five of the six “most important pieces of alcohol safety legislation in the last quarter century,” according to NHTSA’s *Alcohol and Highway Safety* (2006).14

As these law variables can now be directly assigned by driver age, we no longer need to analyze youth and adults separately, and can also extend the age range to 15-60, all ages with appreciable alcohol involvement. Using the logit estimates, the components described above are then calculated for each year of the sample, using 2004 as the base year. For simplicity, the sample includes only single-vehicle accidents, and random effects are omitted.

**Results.** The decomposition is presented in Figure 2. HBD falls nearly fifteen percentage points over the sample period; concomitantly, the contribution of each factor is largest in 1982. Laws explain three percentage points of the reduction in HBD, one-fifth of the total. This slightly exceeds the

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14 The remaining piece of legislation, increased sanctions for repeat DWI offenders, has received relatively little study and is often implemented at the local level, rather than the state level. It is thus omitted from $L$. See Lapham et al. (2006), Jones and Lacey (2000), and NHTSA (1996).
contribution of state factors (two percentage points), but falls short of the contributions of demographics (four percentage points) and social forces and other residual factors (five percentage points). Timing matters too. The social forces component is most prevalent in the early years of the sample, the period in which social forces are believed to be strongest. In contrast, the effect of laws builds gradually, reflecting ever-stricter legal sanctions against drunk driving.

The interpretation of the state factors component can be clarified by additional regressions whose results we now report. First, it mostly reflects changes in per-capita alcohol consumption; once this is controlled for, the coefficient on unemployment disappears. Second, the component’s downward trend is not explained by alcohol prices, which slightly trailed inflation throughout the period, nor by the demographic and law variables in our regressions, which explain very little of the variation in consumption. (In Table 3, the estimates on the law variables changed little when per capita consumption was controlled for.) Thus, part of the decline in per capita alcohol consumption may itself be attributable to social forces (see Greenfield, Midanik, and Rogers, 2000, and Linkenbach and Young, 2012).

These findings speak to a venerable debate over the relative efficacy of law and social forces in combatting drunk driving (see, for example, Whitehead, 1975, and Ross, 1992). This debate, begun when efforts to address this problem were in their infancy, had its origins in policy: laws and social suasion were viewed as alternative ways to expend political and social capital in order to reduce drunk driving. This tradeoff has since been obscured, by the subsequent dominance of deterrence in U.S. policy (Ross, 1992; Grant, 2015b) and by the academic literature, which has repeatedly quantified the effects of laws, but never those of social forces. This decomposition suggests that this tradeoff is very much alive, and is a potential source of gains in traffic safety at a time when the
deterrence approach is encountering diminishing returns. Despite ever-increasing numbers of drunk driving laws, alcohol involvement today is no lower than in 1997.

The only other study that attempts to quantify the aggregate effects of laws on HBD, Dang (2008), nicely illustrates the underplaying of social forces in the existing literature. Dang’s methods mimic those used here, except that she uses a pooled TSCS regression model, common in traffic safety studies outside of economics, that omits state and year fixed effects. This favorably biases estimates of drunk driving laws’ effects, as the incidence of these laws trends in the opposite direction from HBD over the sample period. The absence of these fixed effects explains the difference between our findings and hers, which indicate that laws explain nearly half of the decline in HBD.

IV. Conclusion.

Though our primary sample period spans twenty-three years, the narrative that emerges from our estimates spans four decades. It begins, in the late 1970s and early 1980s, with changed social attitudes towards traffic safety in general and drunk driving in particular. These had a direct effect on drinking and driving, first among older drivers, while also leading to the passage of raised drinking ages and other drunk driving laws in several early-adopting states. This, along with changed demographics and reduced alcohol consumption, contributed to a substantial decrease in drunk driving, which was the main reason that per-mile traffic fatalities fell throughout the 1980s.

Beginning in the late 1980s, however, decreases in drunk driving diminished: traffic fatalities continued to fall mostly because of decreases in general risk. This reduced rate of progress coincided with the playing out of a change in attitudes across a sequence of birth cohorts. Since 1997 the rate
of alcohol involvement in fatal accidents has remained static, despite ever-increasing legislation. The formidable power of social forces may need to be harnessed in order to push it down further.

By highlighting the interrelationship between laws, social forces, and general risk, our methods offer an alternative to traditional fatality analyses of drunk driving legislation, which are somewhat sterile in that they allow the effects of intangible factors to remain opaque and largely unaccounted for. Any evaluation of drunk driving countermeasures that do not take these intangibles into account is incomplete and potentially biased, particularly in the early studies that most strongly influence policymakers.
APPENDIX

In addition to the variables defined in the text, let $F^*$ and $H^*$ be the number of accidents and HBD predicted from state and year fixed effects. Also, let $C$ be the total number of state*year cells. Note that $F$, the number of fatal accidents, is also the number of observations within each state*year cell.

Then, summing across state*year cells:

$$\Sigma (H - H^*)^2 = \Sigma (H - \frac{h}{F})^2 + \Sigma (h - H^*)^2 = \frac{\Sigma h(1-h)}{F} + \Sigma (h - H^*)^2$$  \hspace{2cm} (12)

Numerical experiments confirm that the approximation of sampling error, achieved by replacing $h$ with $H^*$, is very close. The sample analog of $\text{var}(h - H^*)$ is then:

$$\frac{\Sigma (h - H^*)^2}{C} = \frac{\Sigma (H - H^*)^2}{C} - \frac{\Sigma H^*(1 - H^*)}{C \cdot F}$$  \hspace{2cm} (13)

The adjusted serial and spatial correlations of $h - H^*$ are calculated by scaling the unadjusted correlations by the estimate of $\text{var}(H - H^*)/\text{var}(h - H^*)$.

Similarly, the properties of $\log(f/M) - \log(F^*/M)$ can be inferred by extracting sampling error as follows:

$$\Sigma \left(\frac{F - F^*}{F^*}\right)^2 = \Sigma \left(\frac{f - \frac{f}{F^*}}{\frac{f}{F^*}}\right)^2 = \frac{\Sigma f}{F^*} + \Sigma (\frac{f}{F^*} - 1)^2$$

$$\frac{\Sigma 1}{F^*} + \Sigma \log^2\left(\frac{f}{F^*}\right) = \frac{\Sigma 1}{F^*} + \Sigma (\log(f) - \log(F^*))^2 = \frac{\Sigma 1}{F^*} + \Sigma (\log(f/M) - \log(F^*/M))^2$$

The adjusted correlation between HBD and log fatalities per mile, that is, between $h - H^*$ and $\log(f/M) - \log(F^*/M)$, can also be calculated by scaling their unadjusted correlation.

Finally, to identify the variance of the general risk factor, $r$, and its correlation with $h$, define $n = 1 - h$, and $N$ and $N^*$ accordingly. Also define $n' = n/N^*$, and $f'$ and $r'$ accordingly. Then:

$$\Sigma \left(\frac{N - N^*}{N^*}\right)^2 = \Sigma \left(\frac{N - n}{N^*}\right)^2 + \Sigma \left(\frac{n - N^*}{N^*}\right)^2 = \frac{\Sigma \log(1-n)}{F \cdot N^*} + \Sigma \left(\frac{n}{N^*} - 1\right)^2$$

$$= \frac{\Sigma 1 - N^*}{F \cdot N^*} + \Sigma (\log(n) - \log(N^*))^2$$
\[ \text{cov}(\log(F) - \log(F^*), \log(N) - \log(N^*)) = \text{cov}(\log(f'), \log(n')) = \text{cov}(\log(r') - \log(n'), \log(n')) = \text{cov}(\log(r'), \log(n')) \]

\[ \Sigma(\log(fM) - \log(F^* M))^2 = \Sigma(\log \frac{r/n}{r^*/N^*})^2 = \Sigma([\log(r) - \log(r^*]) - [\log(n) - \log(N^*)])^2 \]

\[ \cdot C'[\text{var}(\log(r')) + \text{var}(\log(n')) - 2\text{cov}(\log(r'), \log(n'))] \]

The first relationship identifies the variance of \( \log(n') \), the second the covariance of \( \log(r') \) and \( \log(n') \), and the third–along with equation (14)–the variance of \( \log(r') \).
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Table 1. Standard Deviations and Various Correlations of HBD, Log Fatalities, and More.

<table>
<thead>
<tr>
<th></th>
<th>Standard Deviation</th>
<th>Spatial Correlation</th>
<th>Serial Correlation</th>
<th>Cross Correlation</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Raw (Unadjusted)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>HBD (perc. points)</td>
<td>5.7</td>
<td>0.07</td>
<td>0.15</td>
<td>0.08</td>
</tr>
<tr>
<td>Log Fatalities (times one hundred)</td>
<td>13.2</td>
<td>0.18</td>
<td>0.32</td>
<td>0.08</td>
</tr>
<tr>
<td><strong>Adjusted for Sampling Error</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>HBD</td>
<td>2.3</td>
<td>0.41</td>
<td>0.81</td>
<td>0.32</td>
</tr>
<tr>
<td>Log Fatalities</td>
<td>8.1</td>
<td>0.46</td>
<td>0.84</td>
<td>0.32</td>
</tr>
<tr>
<td><strong>Latent Variables</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Implied General Risk Factor (percent)</td>
<td>7.5</td>
<td>----</td>
<td>----</td>
<td>0.19</td>
</tr>
<tr>
<td>Implied Drinking Factor (percent)</td>
<td>4.9</td>
<td>----</td>
<td>----</td>
<td>0.19</td>
</tr>
</tbody>
</table>

Note: All observed and latent variables are measured (or presumed measured) at the state*year level, in deviations from state and year fixed effects (and, for fatalities, scaled by the log of vehicle miles traveled). Spatial correlations are calculated across matched state pairs. Using postal codes, the pairs are as follows: ME/MA, VT/NH, CT/RI, NY/NJ, TX/OK, KS/NE, ND/SD, WA/OR, CA/NV, UT/CO, ID/MT, MN/WI, AZ/NM, MI/Oh, IL/IN, IA/OO, AR/LA, AL/MS, TN/KY, GA/FL, NC/SC, VA/WV, MD/PA, DC/DE, AK/HI. “Adjusted” means that the effects of sampling variance have been removed. Cross correlations are the correlation of HBD and log fatalities, and the general risk factor with the implied drinking factor. There are 1173 observations (51 states * 23 years).
Table 2. GLMM Regression Results (100*law coefficients, with 100*standard errors in parentheses).

<table>
<thead>
<tr>
<th>Law</th>
<th>Sampling Unit</th>
<th>count, any BAC (log specification)</th>
<th>count, BAC &gt; 0 (log specification)</th>
<th>HBD implied % change in counts</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.10 Per Se Laws</td>
<td>Fatalities</td>
<td>0.96 (1.17)</td>
<td>2.34 (1.89)</td>
<td>0.54 (0.53)</td>
</tr>
<tr>
<td></td>
<td>Accidents</td>
<td>2.06 (1.18)</td>
<td>2.56 (1.85)</td>
<td>0.36 (0.51)</td>
</tr>
<tr>
<td></td>
<td>Single Vehicle Accidents</td>
<td>1.37 (1.36)</td>
<td>1.98 (2.07)</td>
<td>-0.10 (0.63)</td>
</tr>
<tr>
<td>0.08 Per Se Laws</td>
<td>Fatalities</td>
<td>0.93 (1.04)</td>
<td>-1.57 (1.71)</td>
<td>-1.08* (0.47)</td>
</tr>
<tr>
<td></td>
<td>Accidents</td>
<td>1.02 (1.05)</td>
<td>-1.33 (1.68)</td>
<td>-1.05* (0.45)</td>
</tr>
<tr>
<td></td>
<td>Single Vehicle Accidents</td>
<td>1.53 (1.20)</td>
<td>-1.91 (1.87)</td>
<td>-1.62* (0.56)</td>
</tr>
<tr>
<td>ALR Laws</td>
<td>Fatalities</td>
<td>-2.65* (0.96)</td>
<td>-3.78* (1.54)</td>
<td>-0.39 (0.43)</td>
</tr>
<tr>
<td></td>
<td>Accidents</td>
<td>-3.62* (0.95)</td>
<td>-3.60* (1.51)</td>
<td>-0.39 (0.41)</td>
</tr>
<tr>
<td></td>
<td>Single Vehicle Accidents</td>
<td>-1.71* (1.11)</td>
<td>-3.55* (1.68)</td>
<td>-0.42 (0.51)</td>
</tr>
<tr>
<td>MLDA (0 = 18 yrs., 1 = 21 yrs.)</td>
<td>Fatalities</td>
<td>-2.24 (3.16)</td>
<td>-8.13 (4.74)</td>
<td>-3.11* (1.30)</td>
</tr>
<tr>
<td></td>
<td>Accidents</td>
<td>-1.98 (2.91)</td>
<td>-7.67 (4.40)</td>
<td>-2.88* (1.23)</td>
</tr>
<tr>
<td></td>
<td>Single Vehicle Accidents</td>
<td>2.20 (3.67)</td>
<td>-3.09 (5.03)</td>
<td>-3.61* (1.62)</td>
</tr>
<tr>
<td>ZT Laws</td>
<td>Fatalities</td>
<td>-1.77 (2.39)</td>
<td>-5.46 (3.95)</td>
<td>-1.18 (0.95)</td>
</tr>
<tr>
<td></td>
<td>Accidents</td>
<td>-1.62 (2.22)</td>
<td>-6.75 (3.81)</td>
<td>-1.58 (0.89)</td>
</tr>
<tr>
<td></td>
<td>Single Vehicle Accidents</td>
<td>-1.07 (2.88)</td>
<td>-5.32 (4.34)</td>
<td>-1.69 (1.28)</td>
</tr>
</tbody>
</table>

Note: see text for description of controls. N = 1058, 48 states (not AK, DC, HI) for 23 years, excluding years prior to discrete jumps in BAC reporting in twelve states. * indicates p < 0.05.
Table 3. Robustness Checks (coefficient estimates, with standard errors in parentheses).

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Estimator / Specification</th>
<th>.10 Per Se</th>
<th>.08 Per Se</th>
<th>ALR</th>
<th>MLDA</th>
<th>ZT</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log(Fatal Accidents)*100</td>
<td>Weighted Least Squares</td>
<td>3.17 (1.15)</td>
<td>1.60 (0.92)</td>
<td>-1.71 (0.87)</td>
<td>-1.40 (2.70)</td>
<td>-2.49 (2.13)</td>
</tr>
<tr>
<td></td>
<td>Unweighted Least Squares</td>
<td>0.55 (1.34)</td>
<td>0.53 (1.19)</td>
<td>-4.90* (1.09)</td>
<td>-2.93 (3.98)</td>
<td>-2.45 (2.90)</td>
</tr>
<tr>
<td></td>
<td>GLMM (from Table 2)</td>
<td>2.06 (1.18)</td>
<td>1.02 (1.05)</td>
<td>-3.62* (0.95)</td>
<td>-1.98 (2.91)</td>
<td>-1.62 (2.22)</td>
</tr>
<tr>
<td></td>
<td>GLMM with Extended Controls</td>
<td>1.60 (1.14)</td>
<td>1.27 (1.00)</td>
<td>-2.41* (0.93)</td>
<td>-6.06* (2.89)</td>
<td>-1.37 (2.17)</td>
</tr>
<tr>
<td>HBD – implied % change in fatal accidents presented</td>
<td>Weighted Least Squares</td>
<td>0.57 (0.83)</td>
<td>-2.93* (0.67)</td>
<td>-1.43* (0.63)</td>
<td>-5.15* (1.71)</td>
<td>-2.52 (1.35)</td>
</tr>
<tr>
<td></td>
<td>Unweighted Least Squares</td>
<td>1.29 (1.03)</td>
<td>-1.77 (0.92)</td>
<td>-0.98 (0.85)</td>
<td>-4.43 (2.46)</td>
<td>-1.39 (1.82)</td>
</tr>
<tr>
<td></td>
<td>GLMM (from Table 2)</td>
<td>0.33 (0.88)</td>
<td>-2.02* (0.78)</td>
<td>-0.80 (0.71)</td>
<td>-4.40* (1.86)</td>
<td>-2.47 (1.35)</td>
</tr>
<tr>
<td></td>
<td>GLMM with Extended Controls</td>
<td>0.45 (0.87)</td>
<td>-1.79* (0.76)</td>
<td>-0.33 (0.71)</td>
<td>-3.74* (1.91)</td>
<td>-2.59 (1.36)</td>
</tr>
<tr>
<td>GLMM Logit with Extended Controls (see below)</td>
<td>GLMM Logit with Extended Controls (see below)</td>
<td>-0.41 (1.15)</td>
<td>-3.34* (1.00)</td>
<td>-0.09 (1.05)</td>
<td>-3.02 (2.03)</td>
<td>-3.60 (2.04)</td>
</tr>
<tr>
<td>Also Add Driver and Accident Controls</td>
<td>Also Add Driver and Accident Controls</td>
<td>-0.03 (1.03)</td>
<td>-1.83* (0.88)</td>
<td>-0.08 (0.96)</td>
<td>-1.80 (1.73)</td>
<td>-2.81 (1.83)</td>
</tr>
<tr>
<td>Dummy for Driver BAC &gt; 0 – logit coefficients presented</td>
<td>Plain Logit, All Drivers in All Accidents</td>
<td>0.015 (0.012)</td>
<td>-0.074* (0.010)</td>
<td>-0.024* (0.010)</td>
<td>-0.085* (0.030)</td>
<td>-0.090* (0.033)</td>
</tr>
<tr>
<td></td>
<td>Single Vehicle Accidents</td>
<td>0.002 (0.017)</td>
<td>-0.095* (0.017)</td>
<td>-0.033* (0.014)</td>
<td>-0.088* (0.041)</td>
<td>-0.083* (0.043)</td>
</tr>
<tr>
<td></td>
<td>Plain Logit</td>
<td>-0.009 (0.023)</td>
<td>-0.071* (0.020)</td>
<td>-0.014 (0.021)</td>
<td>-0.075 (0.044)</td>
<td>-0.080 (0.047)</td>
</tr>
<tr>
<td></td>
<td>GLMM Logit (includes state*year random effects)</td>
<td>-0.008 (0.023)</td>
<td>-0.067* (0.020)</td>
<td>-0.002 (0.021)</td>
<td>-0.067 (0.045)</td>
<td>-0.083 (0.047)</td>
</tr>
<tr>
<td></td>
<td>GLMM with Extended Controls</td>
<td>-0.001 (0.026)</td>
<td>-0.046* (0.022)</td>
<td>-0.002 (0.023)</td>
<td>-0.052 (0.050)</td>
<td>-0.080 (0.052)</td>
</tr>
</tbody>
</table>

Note: In the top two panels, N = 1058 state*year cells. In the bottom panel, 872,289 ≥ N ≥ 59,135 individual observations. See the text for description of the controls. * = p < .05.
Table 4. Variation of Estimates by Sample Period (100*WLS coefficient estimates, with 100*standard errors in parentheses).

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>End of Sample Period</th>
<th>.10 Per Se</th>
<th>.08 Per Se</th>
<th>ALR</th>
<th>MLDA</th>
<th>ZT</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log(Fatalities)</td>
<td>1992</td>
<td>-3.99*</td>
<td>4.06*</td>
<td>-5.23*</td>
<td>-5.97*</td>
<td>6.97</td>
</tr>
<tr>
<td></td>
<td>1995</td>
<td>-1.95</td>
<td>4.31</td>
<td>-4.70*</td>
<td>-3.95</td>
<td>-2.54</td>
</tr>
<tr>
<td></td>
<td>1998</td>
<td>0.28</td>
<td>0.08</td>
<td>-2.01</td>
<td>-2.85</td>
<td>-5.24*</td>
</tr>
<tr>
<td></td>
<td>2001</td>
<td>1.85</td>
<td>0.50</td>
<td>-1.68</td>
<td>-1.04</td>
<td>-3.62</td>
</tr>
<tr>
<td></td>
<td>2004</td>
<td>2.62</td>
<td>1.66</td>
<td>-1.70</td>
<td>-1.04</td>
<td>-3.06</td>
</tr>
<tr>
<td>HBD</td>
<td>1992</td>
<td>-0.12</td>
<td>-2.18*</td>
<td>-0.33</td>
<td>-2.48</td>
<td>-4.54</td>
</tr>
<tr>
<td>[implied % effect on fatalities]</td>
<td></td>
<td>[-0.23]</td>
<td>[-4.16]</td>
<td>[-0.63]</td>
<td>[-4.22]</td>
<td>[-7.72]</td>
</tr>
<tr>
<td></td>
<td>1995</td>
<td>-0.39</td>
<td>-1.81*</td>
<td>-1.02*</td>
<td>-2.92*</td>
<td>-1.17</td>
</tr>
<tr>
<td></td>
<td>1998</td>
<td>-0.45</td>
<td>-1.68*</td>
<td>-1.31*</td>
<td>-3.26*</td>
<td>-0.91</td>
</tr>
<tr>
<td></td>
<td>2001</td>
<td>-0.25</td>
<td>-1.85*</td>
<td>-0.95*</td>
<td>-3.61*</td>
<td>-1.13</td>
</tr>
<tr>
<td></td>
<td>2004</td>
<td>0.45</td>
<td>-1.72*</td>
<td>-0.98*</td>
<td>-3.84*</td>
<td>-1.26</td>
</tr>
</tbody>
</table>

Note: All samples begin in 1982. Least squares is conducted using the number of fatalities per state as weights. * = p < .05.
Figure 1. BAC Conditional on Driving after Drinking, Drivers Involved in Fatal Accidents, Nationwide: with Imputed Data (on left) and without.
Figure 2. Decomposition of the Reduction in HBD in Single-Vehicle Accidents Involving Drivers Aged 15-60, 1982-2004.
Figure 3. Evolution of Driver Alcohol Involvement in Fatal Accidents the U.S.: Profiles by Age, with Imputed Data (on left) and without.

(a) Using Imputed Data

(b) Omitting Imputed Data
(c) Change Relative to 1975-79, Omitting Imputed Data

Deviation of HBD from its 1975-1979 Value

Driver Age

Figure 4. Breakdown of the Change in Traffic Fatalities into Components Associated with Drinking, General Risk, and Miles Travelled.

- **Phase I**
  - Fatality Reductions Due To: Less Drunk Driving

- **Phase II**
  - Fatality Reductions Due To: Less Drunk Driving, Less General Risk

- **Phase III**
  - Fatality Reductions Due To: Less General Risk

- Deviation from Value in 1982 (in logs)

- **Component Descriptions**
  - Mileage Component
  - Relative Sobriety Component
  - General Risk Component
  - Realized Fatalities