

UNDERSTANDING THE DECLINE IN DRINKING AND DRIVING DURING  
“THE OTHER GREAT MODERATION”: THE EFFECTS OF LAW, DEMOGRAPHICS,  
ALCOHOL CONSUMPTION, AND SOCIAL FORCES\*

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Abstract: We show that the dynamics of drinking and driving can be adequately described using simply the fraction of accidents involving drinking drivers. 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, and allowing estimation on microdata that incorporates individual-level controls. 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 that of “social forces.”

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

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For two decades, beginning in the 1980s, the American macroeconomy entered a period of relative quiescence—the Great Moderation. Less well known, however, is the other moderation that accompanied it: a large reduction in the number and fraction of traffic fatalities involving drivers who had been drinking. Like its counterpart, our understanding of The Other Great Moderation remains piecemeal and incomplete. A large literature studying various drunk driving laws shows that at least some have been effective. But the aggregate contribution of such laws to the reduction in fatalities has not been estimated, nor the contribution of other major factors, nor the dynamics by which this reduction occurred. Doing this is the goal of this paper. Employing detailed descriptive statistics, a battery of panel regressions, and a Oaxaca-style regression decomposition, we describe how drunk driving has evolved in the past forty years, and reveal the footprint of the social processes shaping this evolution.

In order to accomplish this task, we must account not only for tangible factors that can be measured directly, but also for intangibles that cannot. Our list of tangibles includes all influences on drinking and driving that are indicated by the literature, including every drunk driving law with even tepid academic support. 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 toward drunk driving. Though the importance of such attitudes permeates the broader traffic safety literature, they remain largely unquantified in the U.S. Because of the magnitude of both intangibles and their interplay with drinking and driving, accounting for their influence is essential.

The vehicle 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 reveals the role of general risk in traffic safety and supports a novel, bias-reducing estimation approach that can be applied to microdata. This, in turn, allows us to decompose the nationwide decline in drinking involvement in fatal accidents into components associated with all major determinants that are recognized in the literature: laws, demographics, economic factors and alcohol consumption, and social forces and other residual factors. Each contributes to The Other Great Moderation, some more than others.

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. This, and the occasional fiat of the federal government, then encourages the diffusion of these laws throughout the country, to gently diminishing effect. 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 now nearing two full decades of stasis. This stasis does not coincide with the end of legislation, but could reflect static social attitudes. Even during the heyday of deterrence, during the 1980s, social forces appear to explain as much of the decline in drinking and driving as laws do.

In Section I, we introduce the latent variable model and use it to conduct a comprehensive descriptive analysis of the dynamics of drinking and driving. Coupling this model to panel regressions, we then investigate the effects of laws, in Section II. Section III adapts the model to individual-level estimation using microdata, a first for this literature, and presents the decomposition with which the paper culminates. Section IV concludes.

## I. The Basic Dynamics of Drinking and Driving.

We focus our analysis on a simple statistic: the fraction of accidents involving drivers who Had Been Drinking, called HBD. This choice could easily be justified informally. This natural measure of the extent of drunk driving is regularly reported by the National Highway Traffic Safety Administration (NHTSA) and occasionally analyzed in the literature, as discussed below. Previously unrecognized, however, is that it also has a sound descriptive and theoretical basis, adequately characterizing the dynamics of drunk driving and, via the latent variable model introduced herein, usefully disentangling these dynamics from those of general risk. We demonstrate this in this section.

This measure is calculated using NHTSA's Fatality Analysis Reporting System (FARS), which records accident, vehicle, and driver characteristics for all fatal traffic accidents on U.S. 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 main analysis uses data through 2004. This period comfortably spans The Other Great Moderation and aligns with the periods analyzed in comparison studies discussed below. 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 infrequent, following a Poisson process around their expected value.<sup>1</sup>

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<sup>1</sup> 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

The Dominance of the Extensive Margin. The most fundamental justification for focusing on HBD is this: in the aggregate, the dynamics of drinking and driving 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 25<sup>th</sup>, 50<sup>th</sup>, and 75<sup>th</sup> 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 50<sup>th</sup> 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 25<sup>th</sup> and 75<sup>th</sup> percentiles yielded very similar results.

Thus changes in drinking and driving can be tracked using simple measures of alcohol involvement such as HBD. 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

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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.

chance an accident will occur in the first place, making the accident the most natural unit of analysis.

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. (Consistent with the finding above, the fraction of accidents involving drivers with a BAC of at least .08, the current per se illegal threshold in all states, exhibits the same trend or lack thereof.) This decrease is depicted below for a subset of accidents that is analyzed later, and will be explained in the decomposition that culminates 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 2 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.)

During the early and mid-1970s alcohol-related accidents surged, partly due to the lowering of the minimum legal drinking age (MLDA) in many states. This began to reverse around the turn of the decade with the onset of The Other Great Moderation.<sup>2</sup> Its vanguard, apparent in the second panel of Figure 2, was a 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

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<sup>2</sup>As alcohol imputations are not present in the FARS prior to 1982, it is hard to give a precise date. Anecdotal evidence strongly suggests that alcohol-related accidents increased during most of the 1970s, in part because many states lowered the drinking age to 18. In the unimputed FARS data, alcohol involvement rose somewhat in the waning years of this decade, then turned down decisively beginning in 1981.

tolerance for it, both of which are well-documented (see below), and both of which were political precursors to the legislation 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 (see below). 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.

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,
- $f$  = the expected number of fatal accidents, so that  $F \sim \text{Poisson}(f)$ ,
- $H$  = HBD, the fraction of fatal accidents involving drinking drivers, and

- $h =$  the expected value of  $H$  given  $d$  and  $s$ , 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  $h = kd / (s + kd)$ .

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,  $k \gg 1$  and almost all collisions between sober and drinking drivers are the drinking driver's fault (see also 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:  $F = F_{\text{SOBER}} + F_{\text{DRINKING}}$ . In expectation, the latent variable equivalent is  $f = f_{\text{SOBER}} + f_{\text{DRINKING}}$ . Without any loss of generality, let  $f_{\text{SOBER}} = rs$  and  $f_{\text{DRINKING}} = rkd$ . Then:

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

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

$$\log(f) - \log(M) \approx \log(r) - \log(1 - h) \quad (2)$$

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.”<sup>3</sup> The same relation applies to fatalities, where  $h$  is the expected fraction of fatalities

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<sup>3</sup> 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



occurring in accidents involving drinking drivers; one can show 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. Ditto for the finding, discussed at length in Grant (2016), that  $k$  has changed little in decades. Both findings support the “natural” interpretation that changes in the relative sobriety component represent changes in the incidence of drinking and driving.

At the national level there are so many accidents that 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 annually, treating 1982 as the base year, yields the decomposition in Figure 3, which depicts the effects of general risk and relative sobriety on total U.S. traffic fatalities from 1982-2014. The upper line denotes the projected growth in log fatalities, relative to the base year, that would be required to “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 attributed to reductions in drinking and driving. The bottom shaded area indicates the shortfall attributed 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 fatality reductions in stem from declines in drinking and driving. While improvements in vehicle technology and road quality 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 Morrissey, 2004, and Burke and Nishitaten, 2015). After real gas prices stabilized in the late 1980s, steady declines

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of the model introduced below, which does not mandate that fatalities be proportional to miles.

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, which concluded The Other Great Moderation. 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 with the decreased economic activity and increased gas prices of the Great Recession.<sup>4</sup> According to this decomposition, drinking and driving has changed little in nearly twenty years. The continued decline in the number of alcohol-related traffic fatalities can be attributed to reductions in general risk, which have improved safety for both sober and drinking drivers.

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.

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<sup>4</sup> The value of this underappreciated improvement in traffic safety amounted to 1% of annual GDP. Fatalities fell by 10,000 people annually; multiplying this by Department of Transportation's preferred estimate of the Value of Statistical Life, \$9.1 million, then by two to account for damages in non-fatal accidents (Blincoe et al., 2002) yields a total of \$182 billion, or roughly 1% of GDP.

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,<sup>5</sup> 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  $\log(f/M)$  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 remains strong at a three year lag, then drops off sharply.

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

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<sup>5</sup> 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 for the law in question; this is contradicted by the low values reported.

components are only weakly correlated, probably from local economic activity, which decreases alcohol involvement while increasing general risk (through more aggressive driving). During The Other Great Moderation drinking and driving evolved along the extensive margin, non-uniformly by age, and was characterized by a large national component punctuated by brief (three to four year), local (state-specific) innovations that were 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 general risk could affect estimates of laws' effects. This problem is eliminated using an estimation method based on our latent variable model, which we now introduce.

Estimation Method. Classify all independent variables into three types: general risk adjusters,  $G$ ; factors affecting drinking and driving but not general risk,  $X$ ; and factors that affect both,  $Z$ . Then, from Section I:

$$F \sim \text{Poisson}(f) \tag{3}$$

$$\log(f) = \mu \log(M) + \log(r(G,Z)) - \log(1 - h(X,Z))$$

where we have relaxed the elasticity between miles and fatalities to be a parameter,  $\mu$ , that need not equal one. The marginal effect of X on log fatalities is:

$$\frac{\partial \log(f)}{\partial X} = \frac{1}{1-h} \frac{\partial h}{\partial X} \quad (4)$$

This estimation method requires the safety effects of X variables to be mediated through  $h$ —that is, drunk driving laws reduce fatalities because they reduce drunk driving.<sup>6</sup>

Using the identity in equation (2), we can compare this “restricted estimate” of  $\partial \log(f)/\partial X$  to two alternative estimates of the same quantity: the more traditional “unrestricted” estimate formed by relating  $\log(F)$  directly to X, and a “partially restricted” estimate formed by relating  $\log(F_{\text{DRINKING}})$  on X, then multiplying the result by  $h$ . Temporarily setting aside the theoretical independence of  $r$  from X, these yield the following:

$$\begin{aligned} \text{Unrestricted:} & \quad \frac{1}{r} \frac{\partial r}{\partial X} + \frac{1}{1-h} \frac{\partial h}{\partial X} \\ \text{Partially Restricted:} & \quad \frac{h}{r} \frac{\partial r}{\partial X} + \frac{1}{1-h} \frac{\partial h}{\partial X} \\ \text{Restricted:} & \quad \frac{1}{1-h} \frac{\partial h}{\partial X} \end{aligned} \quad (5)$$

All three estimates treat the relative sobriety component identically, but not so for general risk, whose effect is not eliminated a priori in the unrestricted and partially restricted estimates. This is

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<sup>6</sup> Compensating variation, in which drivers become less careful because they expect fewer drunks to be on the roads, would imply  $\partial r/\partial X \geq 0$  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.

problematic, as general risk is identified from accidents involving sober drivers only.

In theory, these are distinctions without a difference: X variables have no causal effect on  $r$ , ceteris paribus, so  $\partial r/\partial X$  is zero. In practice, however, this requires general risk to be adequately controlled for by G and Z, which is doubtful given the limited controls available for use. If this is correct, the restricted estimate will eliminate bias through unmeasured elements of G that correlate with X in the population, reduce bias through similar unmeasured elements of Z (to the extent they influence  $r$ ), and reduce variability arising from “incidental” (sample) correlations of unmeasured risk influences with X. (These can easily arise in the 10-20 year panels common in the literature, as some elements of G, X, and Z evolve slowly.) Then the three estimates would differ systematically.

Each of these estimates can be formed with state panel data. To calculate the restricted estimate, first estimate  $\Delta h/\Delta X \equiv \beta$  as follows:

$$F_{s,t} \cdot H_{s,t} \sim \text{Binomial}(F_{s,t}, h_{s,t}) \tag{6}$$

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

where  $s$  indexes states and  $t$  time;  $\gamma$  is a coefficient vector; and  $\sigma$  and  $\tau$  are state and year fixed effects.<sup>7</sup> 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 (6) is used because it follows directly from the latent variable model and has a natural form,

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<sup>7</sup> 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.

that of a generalized linear mixed model, or GLMM (McCulloch, 2006).<sup>8</sup> 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  $H$ .

The unrestricted estimate is calculated using GLMM as follows:

$$F_{s,t} \sim \text{Poisson}(f_{s,t}) \tag{7}$$

$$\log(f_{s,t}) = \alpha + \beta X_{s,t} + \gamma Z_{s,t} + \delta G_{s,t} + \mu M_{s,t} + \sigma_s + \tau_t + \epsilon_{s,t}$$

where  $\hat{\beta}$  directly estimates the population change in log(fatalities). The partially restricted estimate utilizes the same specification, replacing all fatalities with fatalities involving drinking drivers. The estimated population change in log(fatalities) is then  $\hat{\beta}H_M$ .

Implementation. Our initial estimations focus on three drunk driving laws: the 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. The signature legislation passed during each of the three phases in Figure 3, respectively, these three laws are now universal within the U.S., partly due to federal legislation withdrawing highway funds from “non-adopters.” Thirty-five states and the District of Columbia adopted all three during our sample period, 1982-2004; the others adopted two of the three. And each has a mature, reasonably convergent literature, amounting to a total of over one hundred

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<sup>8</sup> 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. 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. In addition, in the micro specification introduced in Section III, these random effects account for state\*year clustering in the calculation of the standard errors.

published studies. All law variables range in value from zero (nonexistent) to one (full coverage in that state all year).<sup>9</sup> Following Dee (1999), Freeman (2007), and others, regressions are conducted on the 48 continental states.<sup>10</sup>

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).<sup>11</sup> The *X* and *Z* vectors are included in fatality and HBD regressions, while *G* is included in the fatality regressions only (and is generally insignificant in the HBD regressions, as expected). All variables are measured at the state\*year level.

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<sup>9</sup> 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. (In the logit models, a value of zero or one can be assigned, based on month, year, and driver age, as noted in the text.) All laws are coded from the *Digest of State Alcohol-Highway Safety Related Legislation*, supplemented occasionally with Dang (2008) or Grant (2010). Of the seven laws included in the expanded specification, described below, all but one (dram shop laws) were adopted by the majority of states during the sample period.

<sup>10</sup> 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 omit from the sample those few years prior to the jump in reporting in those states. The affected states and 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.

<sup>11</sup> 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).



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 2). 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.

Results. Tables 2 presents the restricted, partially restricted, and unrestricted estimates of the effect of each law. Each is multiplied by one hundred, so that it predicts the percentage change in fatal accidents resulting from that law's implementation. Note that the 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.

Looking vertically within the panel corresponding to any given law, one can compare the results for three alternative fatality measures: 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. As expected, given the dominance of the extensive margin documented above, the estimates are not sensitive to the measure used.

Looking horizontally, across columns, one can compare the unrestricted, partially restricted, and restricted estimates. The unrestricted estimates are unusual, implying that .10 and .08 laws raise fatalities and that the drinking age is ineffectual; only ALR is consistently significant. These inauspicious findings reflect the sample period, which is on the long side for these literatures (see Freeman, 2007 and Grant, 2015), and, for .08 and .10 laws, the fact that the GLMM does not weight all states equally. (Higher-profile studies of these laws, such as Dee, 2001, do not use weights.)

Moving to the partially restricted and then fully restricted estimates, the results change substantially. The restricted estimates, especially, are more credible: .08 laws now lower fatalities by 2%, while raised MLDA's reduce them by 4-6%; ALR, ZT, and .10 laws are impotent. They are also more precise: the standard errors of the restricted estimates are perhaps three-fourths as large as those of the unrestricted estimates. (The partially restricted estimates, as would be expected, are partway between the two extremes.)

Turning to Table 3, we find that the restricted estimates are more robust as well. This table is also organized by law, not regression, and now presents unrestricted and restricted estimates of each law's effect using a variety of different estimators and regression specifications. The

unrestricted estimates in the first row are relatively sensitive, responding to the weighting applied to the observations and to the inclusion of additional controls. In contrast, the restricted estimates, in the second row, are remarkably stable, rarely varying by more than one percentage point across specifications. (The remaining columns will be discussed below.) Overall, the findings in Tables 2 and 3 are consistent with the presence of unmeasured general risk factors that substantially impact unrestricted fatality regressions, but not the restricted regressions based on HBD.

Altogether, the fatality reductions implied by the restricted regressions are 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%, but 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 double-digit 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 material effect. (Grant, 2011, extensively reviews all three literatures.) Our nil findings for .10 laws and ALR are also somewhat milder than is typical in the literature (Eisenberg, 2003; McArthur and Kraus, 1999).

A few other studies in the literature also use the equivalent of HBD regressions, though this has been obscured by the fact many such studies analyze instead a transformation of HBD, simply scaling  $F_{\text{DRINKING}}$  by  $F_{\text{SOBER}}$ . Their findings, which are typically very favorable, differ from ours more

than panel fatality analyses do.<sup>12</sup> But this difference is more illusory than real. These favorable findings always come from studies that omit state fixed effects. This allows 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.

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

We now extend the HBD analysis to an individual-level specification that can be applied to microdata, to control for driver and accident-specific factors that correlate with drinking. Using this, we then conduct a decomposition of the decline in HBD during The Other Great Moderation.

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:

$$P(\eta_{i,s,t} = 1) = \Lambda(\phi D_{i,s,t} + \gamma L_{i,s,t} + \phi S_{s,t} + \sigma_s + \tau_t + \epsilon_{s,t}) \quad (8)$$

where  $i$  indexes individuals,  $\phi$ ,  $\gamma$ , and  $\psi$  are coefficient vectors, and  $\epsilon$  is a normally distributed

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<sup>12</sup> See 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.

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  $\Delta h/\Delta X$ , and from this implied percentage reduction in fatal accidents can be calculated as before. 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 two columns of Table 3 present the results from this specification, for one and two-vehicle accidents and just single-vehicle accidents, using the basic set of controls identified above, supplemented only with age dummies and a dummy for two-vehicle accidents when necessary.<sup>13</sup> The one and two-vehicle estimates should be regarded only as illustrative: the logit specification is most conceptually appropriate for single-vehicle accidents, and one could argue that average marginal effects should be calculated at the level of the accident, not the driver, and that the standard errors are downward-biased because inter-accident clustering is ignored.

Nonetheless, the two sets of estimates are generally comparable, and the single-vehicle

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<sup>13</sup> The GLMM model had trouble converging with the full set of accidents, but 94% of the accidents in the data had at most two vehicles. The age dummies affect the estimates little except for the MLDA coefficient. 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.

estimates closely resemble those in the comparable panel analysis in Table 2—which themselves resemble those for all accidents. Similarly, the estimates are little affected by the inclusion of state\*year random effects or the extended controls. In the remaining rows we add demographic and accident-specific factors: dummies for driver age and gender and for the hour, day, and month of the accident. These matter more: the law coefficient estimates always weaken, though not dramatically.

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: laws, demographics, alcohol consumption, and economic variables, the last two grouped together in the vector of state factors. The remainder then contains the nationwide effect of the one unmeasurable influence, social forces, along with that of any other residual factors. An equivalent explanation for fatalities remains elusive: too many general risk factors, such as improvements in vehicles and roads, cannot be quantified.

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

$$H_t = \bar{\eta}_t = \hat{\eta}_t = \overline{\Lambda([\phi D_{i,s,t} + \sigma_s] + \gamma L_{i,s,t} + \psi S_{s,t} + \tau_t)} \quad (9)$$

$$E(H_t | L = L_0) = \overline{\Lambda([\phi D_{i,s,t} + \sigma_s] + \gamma L_{i,s,0} + \psi S_{s,t} + \tau_t)} \quad (10)$$

$$E(H_t | L = L_0, S = S_0) = \overline{\Lambda([\phi D_{i,s,t} + \sigma_s] + \gamma L_{i,s,0} + \psi S_{s,0} + \tau_t)} \quad (11)$$

$$E(H_t | L = L_0, S = S_0, t = 0) = \overline{\Lambda([\phi D_{i,s,t} + \sigma_s] + \gamma L_{i,s,0} + \psi S_{s,0} + \tau_0)} \quad (12)$$

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 (12) 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 broader literature on traffic safety (e.g., Borkenstein, 1985; Vereeck and Vrolix, 2007) 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, 2015) 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. This approach is necessitated by the lack of U.S. data on social attitudes towards traffic safety and drunk driving. In Europe, where the SARTRE project regularly surveys these attitudes, one could estimate a social forces component directly. It would be instructive to do this and compare the two approaches.

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 dummies for driver age and gender and for the hour, day, and month of the accident. The vector  $S$  includes per capita alcohol consumption and the unemployment rate. (Alcohol consumption 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 used as controls 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, NHTSA's *Alcohol and Highway Safety* (2006) deems these laws five of the six "most important pieces of alcohol safety legislation in the last quarter century"—that is, during The Other Great Moderation.<sup>14</sup>

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. Figure 4 presents the trend in HBD among this group and its decomposition. HBD falls

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<sup>14</sup> The remaining piece of legislation, increased sanctions for repeat drunk driving offenders, has been studied little 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).



nearly fifteen percentage points over the sample period, all by 1997, and remains constant afterwards, analogous to the change in relative sobriety in Figure 3. Concomitantly, the contribution of each factor is largest in 1982.

The largest component is associated with social forces and other residual factors, and accounts for one-third of the reduction in HBD, five percentage points. Unlike some other components, its effects are concentrated in the early years of the sample, the period in which social forces are believed to be strongest. From 1992 forward this component is quite small.

The next largest component, which explains four percentage points of the reduction in HBD, is associated with demographic and accident-specific factors. It is shaped by a gradual aging and “feminizing” of the set of accident-involved drivers, along with a slight decline in weekend accidents. Its size suggests that the relative inattention to these factors in the literature could be consequential, and reinforces the value of our microdata estimation approach that captures these factors best.

Next come laws, which explain three percentage points of the reduction in HBD, one-fifth of the total. This component builds gradually across the sample period, as increasing amounts of drunk driving legislation took effect, first raised MLDA and .10 per se limits, then ALR and ZT laws, followed toward the end by .08 per se limits, with dram shop and open container laws interspersed throughout. Here, at least, formal instruments—legal sanctions—diffuse more gradually than social attitudes do.

The smallest and final component, associated with state factors, explains two percentage points of the reduction in HBD. Its interpretation can be clarified by additional results that we now report. First, it mostly reflects changes in per-capita alcohol consumption; controlling for this, 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). Its timing, concentrated during the early years of the sample, is coincident with that of social forces.

These estimates are reasonably robust. Following some recent traffic safety studies (e.g., Dills, 2010; Adams, Blackburn, and Cotti, 2012), we added state-specific linear time trends to the specification, then replicated the decomposition above, incorporating the effects of these trends into the component associated with social forces and other residual factors.<sup>15</sup> The results, found in Figure 5, resemble those in Figure 4, with slightly smaller effects for laws, slightly larger effects for social forces and other residual factors, and the other two components little changed. This reinforces our finding, from Section I, that the dynamics of relative sobriety are predominantly national, not local.

Our findings are also robust to the inclusion of a different set of trends, motivated by Figure 2, which showed that declines in drinking occurred at different times for different ages. To account for this, we broadened the original specification to include a full set of decade\*year dummies, where decade is determined by driver age (teens, 20s, 30s, etc.) The components, not reported here, are hardly changed from those in Figure 4.

The results are also robust to the period used for estimation. As noted in Section II, early studies of the raised MLDA, zero tolerance laws, and .08 laws all find larger effects, which dissipate

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<sup>15</sup> This is not by choice: these trends and the year dummies are collinear. These state trends could pick up state-level variation in rate of change in social attitudes, unmeasured time-varying omitted variables, or both.

in later studies with longer sample periods. As our sample period is on the long side for these literatures, it is reasonable to wonder this holds for the restricted estimates as well. It does not. While our unrestricted estimates reproduce this same progression, the restricted estimates remain remarkably stable if the sample period is cut by one quarter or even by one half. These estimates' robustness, demonstrated in Table 3, extend to the length of the sample period as well.

Finally, we can ask what would happen if we "reverse engineered" the process, and took estimates of laws' effects from fatality regressions, calculated the implied effects of these laws on HBD, and used those to compute the laws component. Though a direct comparison is not possible, since fatality regressions cannot control for demographics and accident-specific factors as we do, the evidence suggests that it wouldn't change much. As noted in Section II, the prevailing panel estimates in the literature only slightly exceed our restricted estimates, which in turn slightly exceed our own unrestricted estimates.

Our findings do differ from those of the only other study that attempts to quantify the aggregate effects of laws on HBD, Dang (2008). But, once again, these differences are more illusory than real. Dang's data and methods resemble those used here, except that she uses a pooled regression model, common in traffic safety studies outside of economics, that omits state and year fixed effects. As in the studies discussed previously, this strongly 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. Discussion and Conclusion.**

To synthesize the findings in this paper, it is best to harness that most un-dynamic of dynamic concepts: the steady state. The Other Great Moderation is best understood as a long, socially complex convergence to a new drunk driving steady state, in which alcohol is present in about 36% of all accidents.

This process begins in the late 1970s and early 1980s, with changed social attitudes towards traffic safety in general and drunk driving in particular. Zimring (1988, p. 380) captured the moment well:

The most substantial change in the status of drunk driving in the United States and throughout the Western world is not a matter of either law or technology, but one of social psychology. Driving while intoxicated, always a crime in the statute books, has come to be regarded in society as more of a “real crime” worthy of condemnation by the general public and punishment by the criminal justice system.

This affected drinking and driving directly, first among older drivers, and indirectly, by inspiring the passage of raised drinking ages and drunk driving laws in many states; it may have contributed to a decline in alcohol consumption as well. These, along with changed demographics, helped decrease drinking and driving substantially during the 1980s. This decrease accounts for most of this decade’s decline in per-mile traffic fatalities.

But this was only the beginning. The diffusion of drunk driving legislation across the states, like most changes in policy, was ponderous, requiring a decade (raised MLDA, ZT laws) or more (ALR, .08 per se limits, open container laws) to consummate. Similarly, the secular shift in alcohol involvement played out gradually across a sequence of birth cohorts. These factors, abetted significantly by demographic shifts, caused further reductions in drinking and driving throughout most of the 1990s.

Absent further shifts in public sentiment, however, it would be unrealistic to expect this

decline to continue unabated. Indeed, it did not. By the end of the 1990s, continued reductions in per-mile traffic fatalities came solely from decreases in general risk. The Other Great Moderation had ended. This denouement can be observed in the rate of alcohol involvement in fatal accidents, which is the same today as it was twenty years ago; in the social forces component in our decompositions, which has been static since the early-1990s; and in the rate at which significant new laws are adopted, which has slowed from a 1980s torrent to a post-2004 trickle.

The focus of academics and policymakers on legislation was not pre-ordained. During the run-up to The Other Great Moderation, laws and social suasion were viewed as alternative ways to expend political and social capital in order to reduce drunk driving, and a debate raged over the relative efficacy of these two approaches (see, for example, Whitehead, 1975, and Ross, 1992). This tradeoff has since been obscured, by the subsequent dominance of deterrence in U.S. policy (Ross, 1992; Grant, 2015) and by the academic literature, which has repeatedly quantified the effects of laws, but never those of social forces. Our decompositions suggest that social suasion is, indeed, a potential source of gains in traffic safety, at a time when the deterrence approach has long encountered diminishing returns.

Nonetheless, this debate looked at the problem too narrowly. The narrative we have constructed treats social attitudes as the firmament upon which are grounded many sources of gains in relative sobriety: laws, alcohol consumption, possibly, and the decision to drink and drive conditional on these two, every component in our decompositions but demographics. This narrative indicates that if this firmament were once again to move, further gains in alcohol-related traffic safety would not just be possible, but probable.

## 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:

$$\begin{aligned}\Sigma(H - H^*)^2 &= \Sigma(H - h)^2 + \Sigma(h - H^*)^2 = \Sigma \frac{h(1-h)}{F} + \Sigma(h - H^*)^2 \\ &\approx \Sigma \frac{H^* \cdot (1 - H^*)}{F} + \Sigma(h - H^*)^2\end{aligned}\tag{13}$$

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:

$$\Sigma \frac{(h - H^*)^2}{C} \approx \Sigma \frac{(H - H^*)^2}{C} - \Sigma \frac{H^* \cdot (1 - H^*)}{C \cdot F}\tag{14}$$

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:

$$\begin{aligned}\Sigma \left(\frac{F - F^*}{F^*}\right)^2 &= \Sigma \left(\frac{F - f}{F^*}\right)^2 + \Sigma \left(\frac{f - F^*}{F^*}\right)^2 = \Sigma \frac{f}{F^{*2}} + \Sigma \left(\frac{f}{F^*} - 1\right)^2 \approx \\ \Sigma \frac{1}{F^*} + \Sigma \log^2\left(\frac{f}{F^*}\right) &= \Sigma \frac{1}{F^*} + \Sigma (\log(f) - \log(F^*))^2 \equiv \Sigma \frac{1}{F^*} + \Sigma (\log(f/M) - \log(F^*/M))^2\end{aligned}\tag{15}$$

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:

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

$$\begin{aligned} \text{cov}(\log(F) - \log(F^*), \log(N) - \log(N^*)) &= \text{cov}(\log(f'), \log(n')) = \\ \text{cov}(\log(r') - \log(n'), \log(n')) &= \text{cov}(\log(r'), \log(n')) - \text{var}(\log(n')) \end{aligned}$$

$$\begin{aligned} \Sigma(\log(f/M) - \log(F^*/M))^2 &= \Sigma\left(\log\frac{r/n}{r^*/N^*}\right)^2 = \Sigma([\log(r) - \log(r^*)] - [\log(n) - \log(N^*)])^2 \\ &\rightarrow C \cdot [\text{var}(\log(r')) + \text{var}(\log(n')) - 2\text{cov}(\log(r'), \log(n'))] \end{aligned}$$

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 (15)—the variance of  $\log(r')$ .

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Table 1. Standard Deviations and Various Correlations of HBD, Log Fatalities, and More.

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

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/MO, 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. Three Different GLMM Estimates of the Percentage Effect of Laws on Traffic Accidents or Fatalities (standard errors in parentheses).

<i>Law</i> Sampling Unit	<i>Estimate</i>			Grand Mean of HBD
	<i>Unrestricted</i>	<i>Partially Restricted</i>	<i>Restricted</i>	
<i>0.10 Per Se Laws</i> Fatalities	1.39 (1.21)	0.99 (0.80)	0.57 (0.92)	0.424
Accidents	2.06 (1.18)	1.08 (0.78)	0.33 (0.88)	0.422
Single Vehicle Accidents	2.08 (1.43)	1.04 (1.08)	-0.69 (1.32)	0.523
<i>0.08 Per Se Laws</i> Fatalities	0.75 (1.08)	-0.67 (0.72)	-2.12* (0.82)	0.424
Accidents	1.02 (1.05)	-0.56 (0.71)	-2.02* (0.78)	0.422
Single Vehicle Accidents	0.98 (1.27)	-1.00 (0.98)	-3.17* (1.17)	0.523
<i>ALR Laws</i> Fatalities	-3.81* (0.98)	-1.60* (0.65)	-0.83 (0.75)	0.424
Accidents	-3.62* (0.95)	-1.52* (0.64)	-0.80 (0.71)	0.422
Single Vehicle Accidents	-3.18* (1.16)	-1.86* (0.88)	-1.05 (1.07)	0.523
<i>MLDA (0 = 18 yrs., 1 = 21 yrs.)</i> Fatalities	-2.24 (3.16)	-2.82 (1.64)	-4.78* (1.99)	0.347
Accidents	-1.98 (2.91)	-2.60 (1.49)	-4.40* (1.86)	0.339
Single Vehicle Accidents	2.20 (3.67)	-1.47 (2.39)	-6.87* (3.09)	0.475
<i>Zero Tolerance Laws</i> Fatalities	-1.77 (2.39)	-1.89 (1.37)	-1.90 (1.45)	0.347
Accidents	-1.62 (2.22)	-2.29 (1.29)	-2.47 (1.35)	0.339
Single Vehicle Accidents	-1.07 (2.88)	-2.53 (2.06)	-3.37 (2.44)	0.475

Note: N = 1058, 48 states (not AK, DC, HI) for 23 years, excluding years prior to discrete jumps in BAC reporting in twelve states. Separate regressions are conducted for adults aged 21-60 (the first three laws) and youth aged 18-20 (the last two laws). The restricted estimates include controls for the unemployment rate and (for youth) .08 and .10 per se laws and ALR. The other estimates also include controls for seat belt laws and speed limits, as described in the text. \* indicates  $p < 0.05$ .

Table 3. Robustness Checks (implied % change in fatal accidents, with standard errors in parentheses).

<i>Law</i> Estimator / Controls	<i>Specification</i>			
	<i>Unrestricted</i>	<i>Restricted</i>	<i>Logit, 1 &amp; 2 Vehicle Accidents</i>	<i>Logit, Single Vehicle Accidents</i>
<i>.10 Per Se Laws</i>				
Weighted Least Squares	3.17 (1.15)	0.57 (0.83)	----	----
Unweighted Least Squares / Plain Logit	0.55 (1.34)	1.29 (1.03)	0.58 (0.39)	0.10 (0.84)
GLMM	2.06 (1.18)	0.33 (0.88)	0.37 (0.58)	-0.45 (1.14)
Add Extended Controls	1.60 (1.14)	0.45 (0.87)	0.50 (0.58)	-0.41 (1.15)
Also Add Driver and Accident Controls	----	----	0.24 (0.51)	-0.03 (1.03)
<i>.08 Per Se Laws</i>				
Weighted Least Squares	1.60 (0.92)	-2.93* (0.67)	----	----
Unweighted Least Squares / Plain Logit	0.53 (1.19)	-1.77 (0.92)	-2.27* (0.32)	-4.72* (0.84)
GLMM	1.02 (1.05)	-2.02* (0.78)	-1.50* (0.50)	-3.53* (0.99)
Add Extended Controls	1.27 (1.00)	-1.79* (0.76)	-1.43* (0.50)	-3.34* (1.00)
Also Add Driver and Accident Controls	----	----	-0.95* (0.44)	-1.83* (0.88)
<i>ALR Laws</i>				
Weighted Least Squares	-1.71 (0.87)	-1.43* (0.63)	----	----
Unweighted Least Squares / Plain Logit	-4.90* (1.09)	-0.98 (0.85)	-1.05* (0.32)	-1.64* (0.70)
GLMM	-3.62* (0.95)	-0.80 (0.71)	-0.57 (0.54)	-0.70 (1.04)
Add Extended Controls	-2.41* (0.93)	-0.33 (0.71)	-0.27 (0.93)	-0.09 (1.05)
Also Add Driver and Accident Controls	----	----	-0.28 (0.47)	-0.08 (0.96)

<i>MLDA (0 = 18 yrs., 1 = 21 yrs.)</i>	-1.40	-5.15*	----	----
Weighted Least Squares	(2.70)	(1.71)		
Unweighted Least Squares / Plain Logit	-2.93	-4.43	-2.90*	-3.98*
	(3.98)	(2.46)	(0.92)	(1.85)
GLMM	-1.98	-4.40*	-2.24*	-3.39
	(2.91)	(1.86)	(1.05)	(1.99)
Add Extended Controls	-6.06*	-3.74*	-1.95	-3.02
	(2.89)	(1.91)	(1.06)	(2.03)
Also Add Driver and Accident Controls	----	----	-0.75	-1.80
			(0.95)	(1.73)
<i>Zero Tolerance Laws</i>	-2.49	-2.52	----	----
Weighted Least Squares	(2.13)	(1.35)		
Unweighted Least Squares / Plain Logit	-2.45	-1.39	-3.08*	-3.76*
	(2.90)	(1.82)	(1.05)	(1.95)
GLMM	-1.62	-2.47	-3.06*	-3.62
	(2.22)	(1.35)	(1.17)	(2.13)
Add Extended Controls	-1.37	-2.59	-3.15*	-3.60
	(2.17)	(1.36)	(1.18)	(2.04)
Also Add Driver and Accident Controls	----	----	-2.67*	-2.81
			(1.05)	(1.83)

Note: In the leftmost two columns, N = 1058 state\*year cells. In rightmost two columns, 872,289  $\geq$  N  $\geq$  59,135 individual observations. See Table 2 for the controls in the basic specifications. The extended controls include per capita alcohol consumption and dram shop and open container laws. Driver and accident controls include dummies for driver age and sex, accident hour, day, and month, and the number of vehicles in the accident (when appropriate). In the WLS estimates, weights are the number of accidents in each state\*year cell. \* = p < .05.

Figure 1. BAC Conditional on Driving after Drinking, Drivers Involved in Fatal Accidents, Nationwide: with Imputed Data (on left) and without.

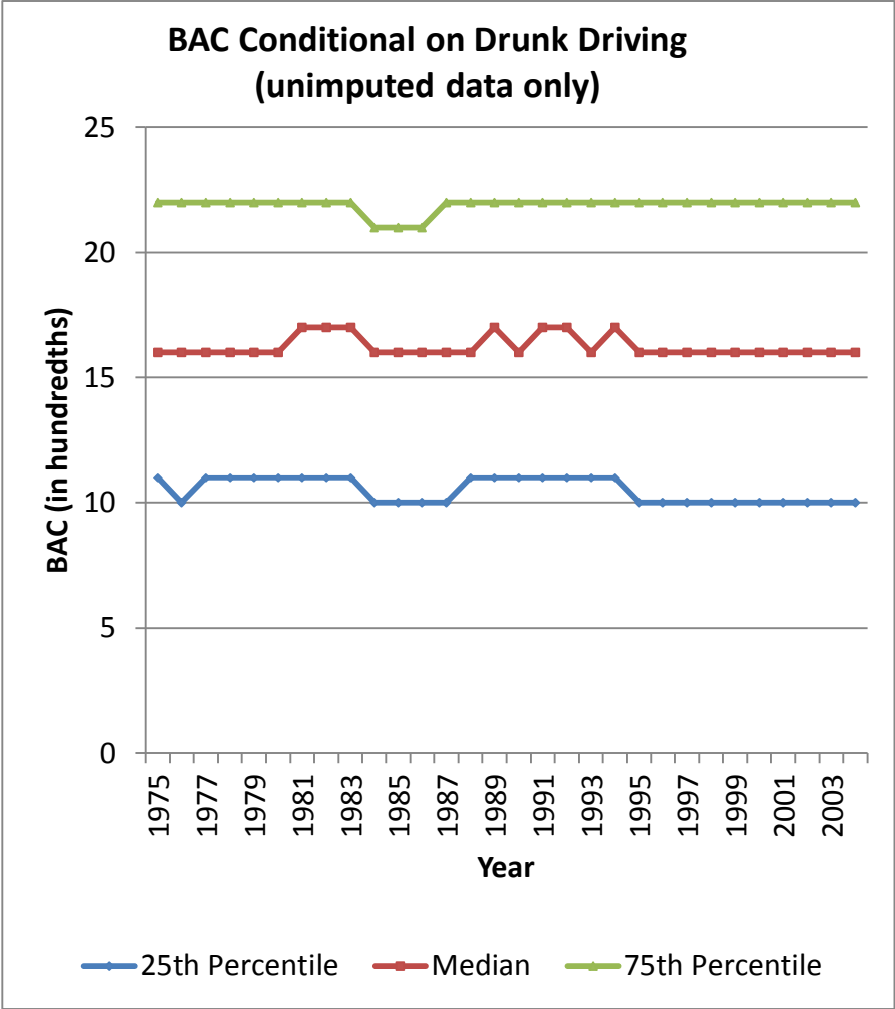
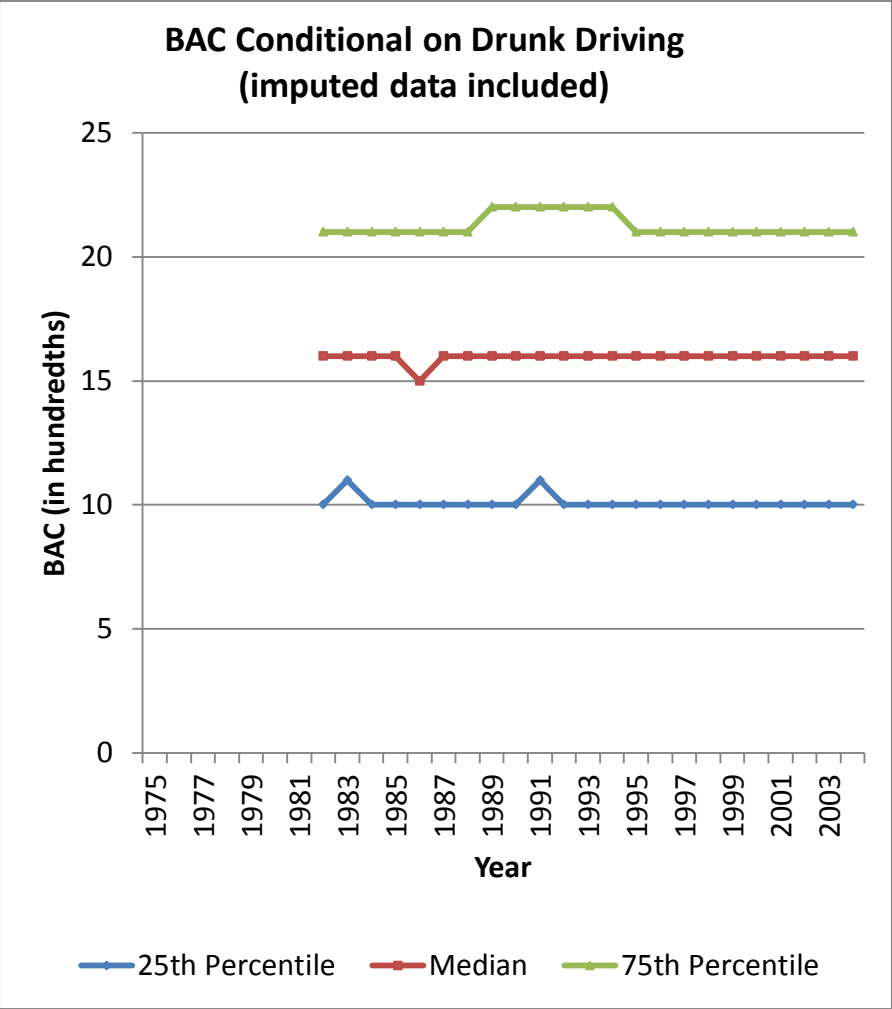




Figure 2. Evolution of HBD in the U.S.: Profiles by Age, with Imputed Data (on left) and without.

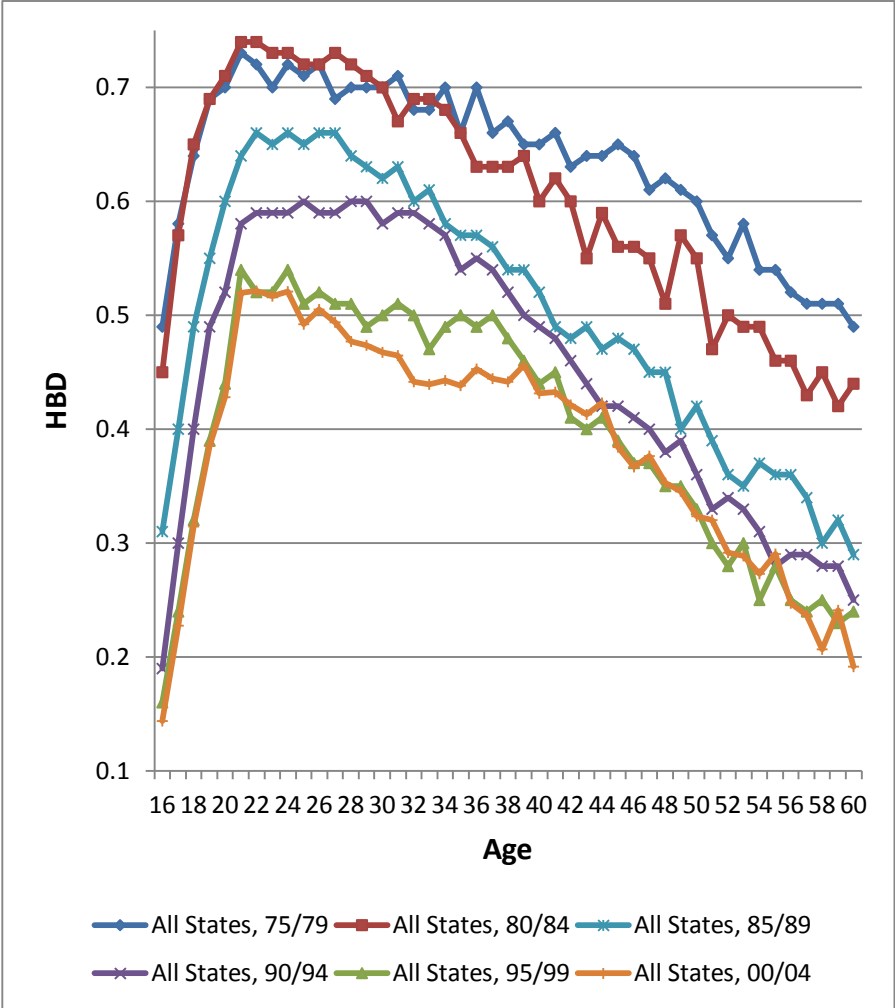
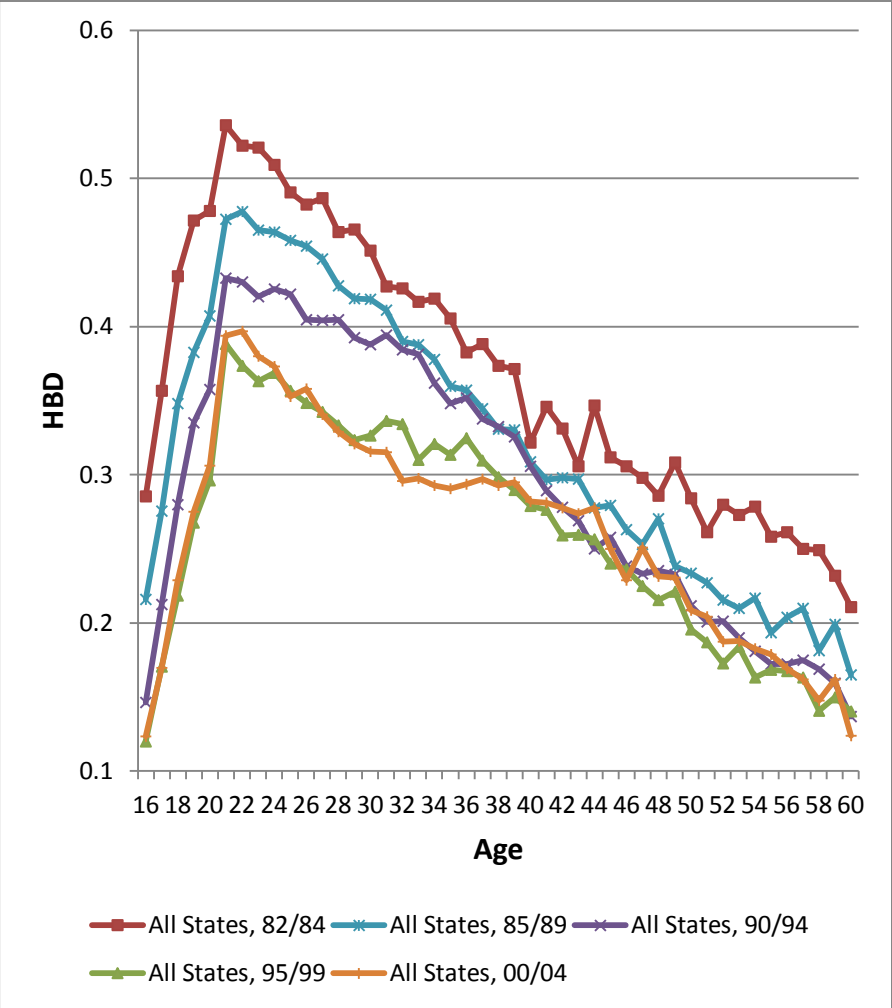


Figure 3. Breakdown of the Change in Traffic Fatalities into Components Associated with Drinking, General Risk, and Miles Travelled.

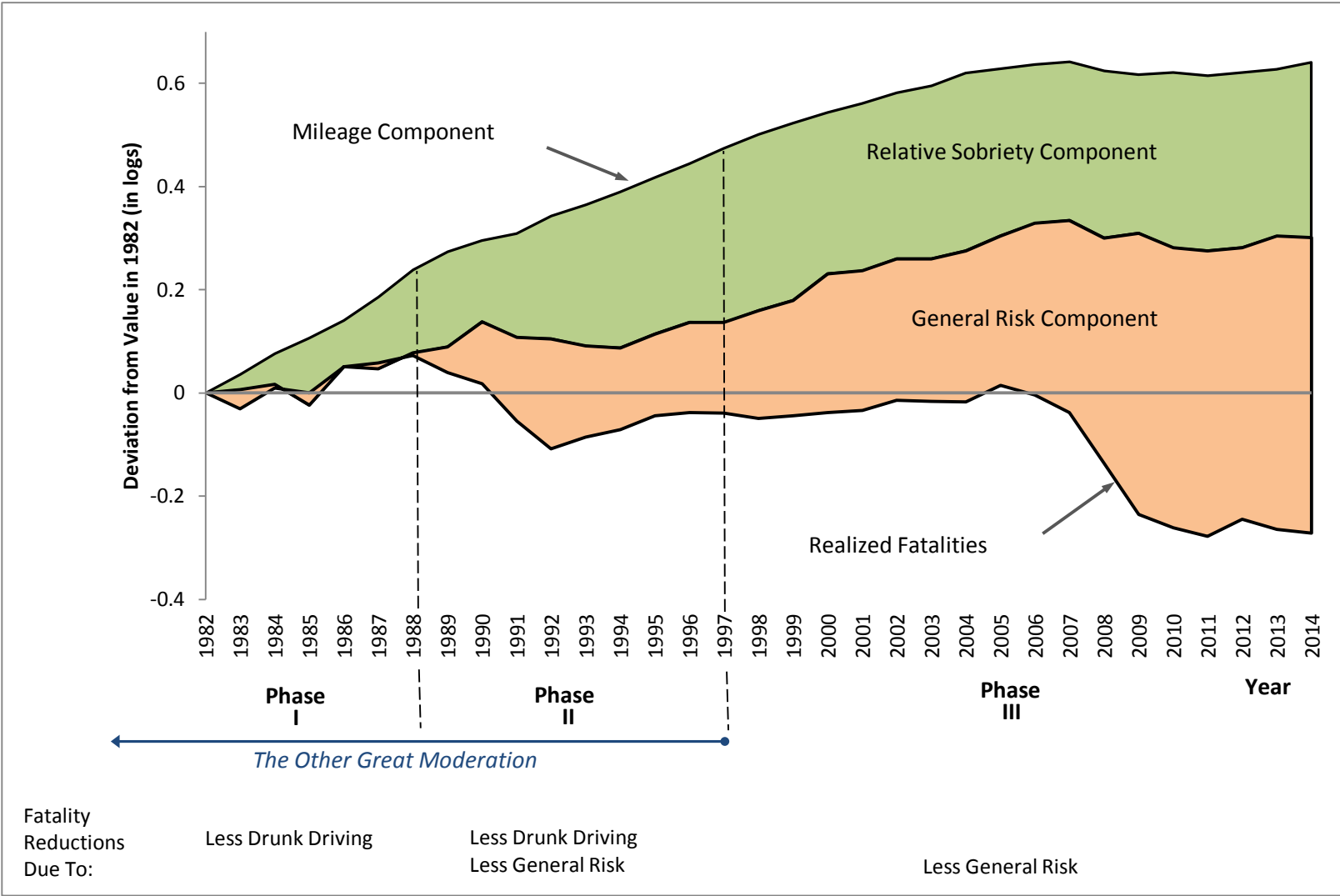


Figure 4. Decomposition of the Reduction in HBD in Single-Vehicle Accidents, 1982-2004.

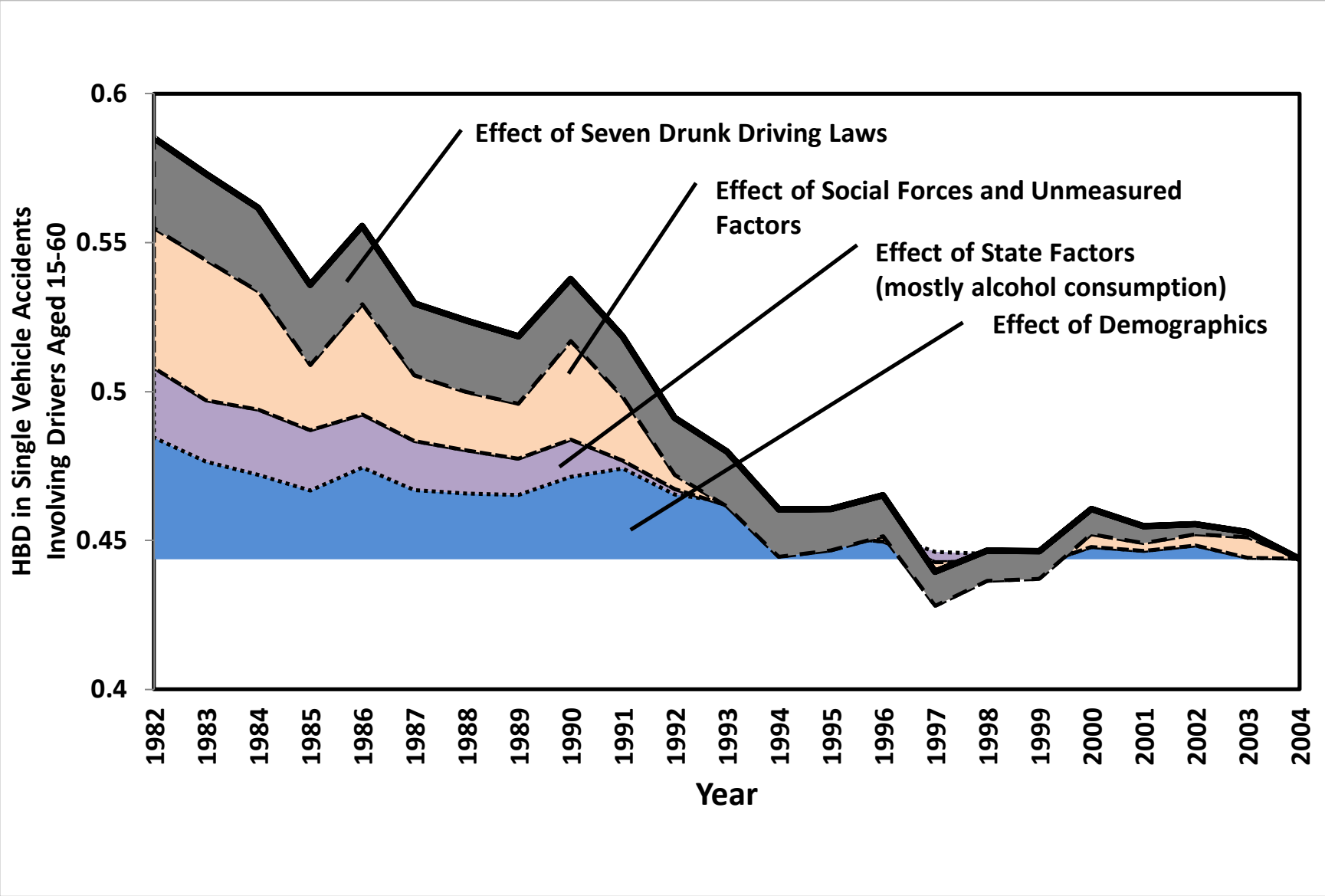


Figure 5. Decomposition of the Reduction in HBD in Single-Vehicle Accidents, 1982-2004, State Trends Added.

