# How Dangerous Are Drinking Drivers?

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We present a methodology for measuring the risks posed by drinking drivers that relies solely on readily available data on fatal crashes. The key to our identification strategy is a hidden richness inherent in two-car crashes. Drivers with alcohol in their blood are seven times more likely to cause a fatal crash; legally drunk drivers pose a risk 13 times greater than sober drivers. The externality per mile driven by a drunk driver is at least 30 cents. At current enforcement rates the punishment per arrest for drunk driving that internalizes this externality would be equivalent to a fine of $8,000.

## I. Introduction

Motor vehicle crashes claim over 40,000 lives a year in the United States, approximately the same number of Americans killed over the course of either the Korean or Vietnam wars. The death toll in motor vehicle accidents roughly equals the combined number of suicides and homicides, and motor vehicle deaths are 30 times as frequent as accidental deaths due to firearms. Motor vehicle accidents are the leading cause of death for Americans aged 6–27.

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Alcohol is often implicated in automobile deaths. According to police reports, at least one driver has been drinking (although not necessarily over the legal blood-alcohol limit) in over 30 percent of fatal crashes. During the time periods in which alcohol usage is greatest, that proportion rises to almost 60 percent.[1]

Without knowing the fraction of drivers on the road who have been drinking, however, one cannot possibly draw conclusions about the relative fatal crash risk of drinking versus sober drivers, the externality associated with drinking and driving, or the appropriate public policy response. For instance, if 30 percent of the drivers had been drinking, over half of all two-vehicle crashes would be expected to involve at least one drinking driver, even if drinking drivers were no more dangerous than sober drivers.

Past research has attempted to measure this fraction through the use of random roadblocks and driver stops (Lehman, Wolfe, and Kay 1975; Lund and Wolfe 1991; Hurst, Harte, and Frith 1994).[2] While these studies are extremely valuable, they suffer from a number of important limitations. First, they are costly to undertake and consequently are performed only rarely. Second, in such experiments, drivers who have been stopped cannot be compelled to submit to alcohol tests. In practice, roughly 10 percent of drivers refuse to participate—presumably those most likely to have been drinking (Lund and Wolfe 1991). The assumptions adopted for dealing with this sample selection are critical to the interpretation of the data. Third, even if the estimates obtained are reliable, they reflect the specific circumstances at a particular time and place, and the extent to which the conclusions are broadly generalizable is unknown.

In this paper, we adopt a radically different strategy for estimating the fraction of drinking drivers and the extent to which their likelihood of a fatal crash is elevated. Specifically, we rely exclusively on data from fatal crashes. A priori, it would seem that such an exercise, even if feasible, would require an extremely restrictive set of assumptions and the imposition of an arbitrary functional form. Separately identifying the fraction of drinking drivers on the road and their relative risk of a fatal crash using only the fraction of drinking drivers in fatal crashes is ostensibly equivalent, for instance, to determining the relative free-throw

[1] Because many fatal crashes involve more than one vehicle, the actual fraction of drinking drivers involved in these crashes is lower than the values cited above. Overall, roughly 30 percent of drivers in fatal crashes have been drinking, with that percentage rising to 50 percent during peak times of alcohol usage.

[2] There are also survey data asking drivers whether they have driven when they have "had too much to drink" (Liu et al. 1997). In addition to any question about the accuracy of the responses given, these surveys have not attempted to ask drivers to report the percentage of miles driven with and without the influence of alcohol. Without that number, accurate estimates of the elevated risk of drinking drivers cannot be computed.

shooting ability of two basketball players on the basis of *only* the number of free throws successfully completed by each. Without knowledge of how many free-throw attempts each player had, such an exercise would appear futile. In the realm of economics, our task is equivalent to separately identifying per capita income and population on the basis of only aggregate income data. Despite the apparent difficulty of this exercise, the assumptions required for identification of the model are in actuality quite natural and do not even require the imposition of arbitrary distributional assumptions.

The ability to identify the parameters arises from a hidden richness in the data due to the fact that crashes often involve multiple drivers. For two-car crashes, the relative frequency of accidents involving two drinking drivers, two sober drivers, or one of each provides extremely useful information. Indeed, given the set of assumptions outlined in Section II, this information alone is sufficient to separately identify both the relative likelihood of causing a fatal crash on the part of drinking and sober drivers and the fraction of drivers on the road who have been drinking. The intuition underlying the identification of the model is quite simple. The number of two-car fatal crash *opportunities* is dictated by the binomial distribution. Consequently, the number of fatal two-car crash opportunities involving two drinking (sober) drivers is proportional to the *square* of the number of drinking (sober) drivers on the road. The number of fatal crash opportunities involving exactly one drinking and one sober driver is *linearly* related to the number of both drinking and sober drivers. Identification of the model arises from these intrinsic nonlinearities. These nonlinearities are not artificially imposed on the problem via arbitrary functional form assumptions, but rather are the immediate implication of the binomial distribution, which relies only on the assumptions to be stated in Section II concerning independence of crashes and equal mixing of the different types on the road.

Applying the model to data on fatal accidents in the United States over the period 1983–93, we obtain a number of interesting results. Drivers identified by police as having been drinking (but not necessarily legally drunk) are at least seven times more likely to cause a fatal crash than drivers with no reported alcohol involvement. Drivers above the blood-alcohol limit of 0.10 are at least 13 times more likely to be the cause of fatal crashes. When we apply the model to other observable traits, males, young drivers, and those with bad previous driving records are also more likely to cause crashes. Drinking, however, is far more important than these other characteristics, and much of the apparent impact of gender and past driving record actually reflects differential propensities to drink and drive across groups. The exception is young drivers: sober, young drivers are almost three times as likely to cause a fatal crash as other sober drivers. The peak hours for drinking and driving are between 1:00 A.M. and 3:00 A.M., when as many as 25 percent of drivers are estimated to have been drinking. The proportion of drinking drivers appears to have fallen by about one-quarter over the course of our sample. The relative fatal crash risk of drinking drivers, in contrast, appears to have been stable.

The great majority of alcohol-related driving fatalities occur to the drinking drivers themselves and their passengers. Since these individuals are likely to have willingly accepted the risks associated with their actions, the role for public policy in preventing these deaths is unclear. According to our estimates, roughly 3,000 other people are killed each year by drinking drivers. When standard valuations of a life are used, the externality due to drinking drivers' killing innocent people is 15 cents per mile driven. For legally drunk drivers, we estimate the externality at 30 cents per mile driven. At current arrest rates for drunk driving, the Pigouvian tax that internalizes that externality is $8,000 per arrest.

We also use our estimates of the fraction of drivers who are drinking and the risk that they pose to analyze the impact of various public policies. A separate literature examines the impact of public policies on fatal car crashes (Cook and Tauchen 1982; Asch and Levy 1987; Saffer and Grossman 1987; Homel 1990; Chaloupka, Saffer, and Grossman 1993; Grossman et al. 1993; Ruhm 1996). In contrast to previous reduced-form approaches to measuring the impact of alcohol policies, we are able to differentiate between very different underlying behavioral responses. For instance, we find some evidence that higher beer taxes and stiff punishments for first-time offenders reduce the number of drinking drivers, but no evidence that such policies affect the level of care that drinking drivers exhibit on the roads. On the other hand, harsh penalties for third-time offenders and large numbers of police on the road have little impact on the number of drinking drivers, but drinkers who do drive tend to pose a lower risk. This latter result may occur either because a few chronic drunk drivers are deterred or because the drunks who do drive are more cautious on the roads.

The remainder of the paper is structured as follows. Section II derives the basic model and discusses the sensitivity of the results to alternative modeling assumptions. Section III describes and summarizes the data used. Section IV presents the empirical estimates of the relative crash risk and the number of drinking and sober drivers, as well as a number of extensions to the basic model. Section V computes the externality associated with drunk driving and analyzes the relationship between public policies, the number of drinking drivers, and the risks that they pose. Section VI presents conclusions.

## II. A Model of Fatal Crashes

In this section, we present a simple model of fatal accidents, demonstrating how identification of the underlying structural parameters (the fraction of drivers on the road who have been drinking and the relative likelihood of causing a fatal crash by drinking and sober drivers) naturally emerges from the model. A number of features of the model are worth noting. First, identification relies only on the distribution of crashes in a particular geographic area over a given period of time. Consequently, the model does not necessitate comparisons across times and places that may differ in systematic yet unobservable ways, leading to biased estimates. Second, although the model is identified off nonlinearities, the structural equations that will be estimated follow directly from the restrictions dictated by nature in the form of the binomial distribution. Third, the approach we outline provides a previously unattainable flexibility in measuring drinking and driving. The solution to the model depends only on tallies of fatal crashes, data that are already collected and widely available. Thus parameter estimates can be obtained almost without cost for any geographic area or time period of interest to the researcher, for example, the Chicago metropolitan area, on weekends between 10:00 P.M. and 2:00 A.M. (although standard errors increase as the number of fatal crashes on which the estimate is based shrinks).

### *Assumptions of the Model*

We begin by outlining the five assumptions underlying the model. The first assumption is as follows.

ASSUMPTION 1. There are two driver types, $D$ and $S$.

The terms $D$ and $S$ correspond to drinking and sober drivers, respectively, although other categories of driver types could also be used. Restricting the analysis to two types is done primarily to ease exposition of the model, which readily generalizes to multiple types. In some of our empirical estimation we allow four types. In theory, any number of types could be incorporated if enough data existed. As we demonstrate later, the parameter estimates from a model assuming exactly two types have a straightforward interpretation when there is heterogeneity in driver risk within these two categories of drivers.

The second assumption of the model pertains to "equal mixing" of drinking and sober drivers on the roads. By equal mixing, we mean two things: (1) the number of interactions that a driver has with other cars is independent of the driver's type, and (2) a driver's type does not affect the composition of the driver types with which he or she interacts. The term "interaction" is used here to mean a two-car fatal crash opportunity in which two cars are close enough that a mistake by one of the drivers could cause a fatal accident.[3] A formal statement of this second assumption requires some notation. Let the total number of drivers be $N_{\text{total}}$, and let the total number of drivers of type $i$ be $N_i$. By assumption 1, there are only two types, $N_D + N_S = N_{\text{total}}$. Define $I$ to be an indicator variable equal to one if two cars interact and equal to zero otherwise. Two drivers are denoted as having types $i$ and $j$.

ASSUMPTION 2. (i) $\Pr(i|I = 1) = N_i/(N_D + N_S)$. (ii) $\Pr(i, j|I = 1) = \Pr(i|I = 1)\Pr(j|I = 1)$.

Assumption 2 is essentially a homogeneity requirement. Over a small enough geographic range and time period, assumption 2 is certainly reasonable. For example, on a particular stretch of highway over a 15-minute period, there may be little reason to think that drinking and sober drivers are not equally mixed. As the unit of observation expands with respect to either space or time, this homogeneity condition clearly becomes suspect. We devote a great deal of attention to possible violations of assumption 2 and their impact on the results in the empirical section of the paper.

The third assumption of the model is as follows.

ASSUMPTION 3. A fatal car crash results from a single driver's error.

Assumption 3 rules out the possibility that each of the drivers shares some of the blame for a crash. As discussed later in this section, while assumption 3 is critical to the identification of the model, it is possible to sign the direction of bias introduced by assumption 3 if, in fact, both drivers contribute to fatal crashes.

The fourth modeling assumption is as follows.

ASSUMPTION 4. The composition of driver types in one fatal crash is independent of the composition of driver types in other fatal crashes.

This assumption allows us to move from individual crash probabilities to probabilities involving multiple crashes. Given the level of aggregation used in the empirical analysis (e.g., weekend nights between the hours of midnight and 1:00 A.M. in a given state and year), there is little reason that this assumption should fail, although for very localized observations (a short stretch of road over a 15-minute time period), it may be less applicable.

The final assumption required to solve the model is that drinking (weakly) increases the likelihood that a driver makes an error resulting in a fatal two-car crash. Denote the probability that a driver of type $i$ makes a mistake that causes a fatal two-car crash as $\theta_i$.

[3] Of course, there are different degrees of interactions between vehicles. Two vehicles can meet at an intersection, pass each other on a two-lane highway, or pass one another on a residential street. We abstract from this complexity in our model, but one could imagine treating the degree of interaction between vehicles as a continuous rather than a discrete variable.

ASSUMPTION 5. $\theta_D \geq \theta_S$.

The existing evidence concerning the relative crash risk of drinking and sober drivers overwhelmingly supports this assumption (e.g., Linnoila and Mattila 1973; Borkenstein et al. 1974; Dunbar, Penttila, and Pikkarainen 1987; Zador 1991).

### *Fatal Crash Data and the Parameters of Interest*

Having laid out the assumptions of the model, we derive the link between fatal crash data and the parameters of interest in three steps. First, we derive the formulas corresponding to the likelihood that two cars will interact with one another. Second, we determine the likelihood of a crash conditional on both the drivers' types and an interaction that takes place between two cars. We then back out the probability that a given pair of driver types will be involved conditional on the occurrence of a fatal crash. Third, we derive the likelihood function and discuss the identification issues involved in its estimation.

Assumption 2 gives the joint distribution for a pair of driver types, conditional on an interaction between two drivers:

$$\Pr(i, j|I = 1) = \frac{N_i N_j}{(N_D + N_S)^2}, \tag{1}$$

where $i$ and $j$ are drivers of a particular type, that is, either drinking or sober. So, for example, given that an interaction occurs between two cars, the probability that both are sober drivers is $(N_S)^2/(N_D + N_S)^2$. Interactions between drivers in this model, as reflected in equation (1), are logically equivalent to randomly drawing balls labeled either $S$ or $D$ out of an urn.

Define $A$ to be an indicator variable equal to one if there is a fatal accident and equal to zero otherwise. Assumption 3 implies that the conditional probability of a fatal two-car crash given that two drivers of types $i$ and $j$ pass on the road is

$$\Pr(A = 1|I = 1,\ i,\ j) = \theta_i + \theta_j - \theta_i\theta_j \approx \theta_i + \theta_j. \tag{2}$$

The likelihood of a fatal crash is the sum of the probabilities that either driver makes a fatal error minus the probability that both drivers make a mistake. Given that the chance that either driver makes a fatal mistake is *extremely* small, the chance that both drivers make an error is vanishingly small and can be ignored.[4] So, for example, given that two sober

[4] There are roughly 13,000 fatal two-car crashes in the United States annually. The total number of vehicle miles driven is approximately 2 trillion. If every car interacted with an average of five other cars per mile, then the implied $\theta_i$ is on the order of $10^{-9}$ and the interaction term is on the order of $10^{-18}$.

drivers interact, the probability of a fatal accident is $2\theta_S$. More generally, we could allow for heterogeneity in driver risk within each driver type, as discussed in the extensions to the basic model. In that case, $\theta_i$ and $\theta_j$ in equation (2) represent the *mean* driver risks for the population of drivers of types $i$ and $j$ on the road.

When equations (1) and (2) are multiplied, the joint probability of driver types and a fatal crash conditional on an interaction between two drivers is

$$\Pr(i, j, A = 1 | I = 1) = \frac{N_i N_j(\theta_i + \theta_j)}{(N_D + N_S)^2}. \tag{3}$$

In words, given that two random drivers interact, the probability that a fatal crash occurs and that the drivers involved are of the specified types is simply equal to the likelihood that two drivers passing on the road are of the specified types multiplied by the probability that a fatal crash occurs when these drivers interact.

The key relationship that we seek is the probability of driver types conditional on the occurrence of a fatal accident rather than on an interaction. That value can be obtained from equation (3):

$$\begin{aligned}\Pr(i, j | A = 1) &= \frac{\Pr(i, j, A = 1 | I = 1)}{\Pr(A = 1 | I = 1)} \\ &= \frac{N_i N_j(\theta_i + \theta_j)}{2[\theta_D(N_D)^2 + (\theta_D + \theta_S)N_D N_S + \theta_S(N_S)^2]}.\end{aligned} \tag{4}$$

Although the expression in equation (4) looks somewhat complicated, it is in fact quite straightforward. For each combination of driver types, the numerator is proportional to the number of fatal crashes involving those two types. The denominator is a scaling factor assuring that the probabilities sum to one.

Let $P_{ij}$ represent the probability that the drivers are of types $i$ and $j$ given that a fatal crash occurs. We can explicitly state the values of $P_{ij}$ by simply substituting for $i$ and $j$ in equation (4):

$$\begin{aligned}P_{DD} &= \Pr(i = D, j = D | A = 1) \\ &= \frac{\theta_D(N_D)^2}{\theta_D(N_D)^2 + (\theta_D + \theta_S)N_D N_S + \theta_S(N_S)^2},\end{aligned} \tag{5}$$

$$\begin{aligned}P_{DS} &= \Pr(i = D, j = S | A = 1) + \Pr(i = S, j = D | A = 1) \\ &= \frac{(\theta_D + \theta_S)N_D N_S}{\theta_D(N_D)^2 + (\theta_D + \theta_S)N_D N_S + \theta_S(N_S)^2},\end{aligned} \tag{6}$$

and

$$P_{SS} = \Pr(i = S, j = S|A = 1)$$
$$= \frac{\theta_S(N_S)^2}{\theta_D(N_D)^2 + (\theta_D + \theta_S)N_D N_S + \theta_S(N_S)^2}. \quad (7)$$

Note that the ordering of the driver types does not matter. Consequently, in equation (6), the probability of a mixed drinking-sober crash is the sum of the probability that $i$ is sober and $j$ is drinking plus the probability that $j$ is sober and $i$ is drinking.

Examination of equations (5)–(7) reveals that there are only three equations but four unknown parameters ($\theta_D$, $\theta_S$, $N_D$, and $N_S$). Consequently, all four parameters cannot be separately identified. Closer examination of equations (5)–(7) reveals that only the ratios of the parameters could possibly be identified. Therefore, let $\theta = \theta_D/\theta_S$ and $N = N_D/N_S$. The term $\theta$ is the relative likelihood that a drinking driver will cause a fatal two-car crash compared to a sober driver, and $N$ is the ratio of sober to drinking drivers on the road at a particular place and time.[5] Expressing equations (5)–(7) in terms of $\theta$ and $N$ yields

$$P_{DD}(\theta, N|A) = \frac{\theta N^2}{\theta N^2 + (\theta + 1)N + 1}, \quad (8)$$

$$P_{DS}(\theta, N|A) = \frac{(\theta + 1)N}{\theta N^2 + (\theta + 1)N + 1}, \quad (9)$$

and

$$P_{SS}(\theta, N|A) = \frac{1}{\theta N^2 + (\theta + 1)N + 1}. \quad (10)$$

The final step is deriving the likelihood function. The values in equations (8)–(10) provide the likelihoods of observing the various combinations of driver types conditional on the occurrence of a crash. From assumption 4, which provides independence across fatal crashes, and given the total number of fatal crashes, the joint distribution of driver types involved in fatal accidents is given by the multinomial distribution. Define $A_{ij}$ as the number of fatal crashes involving one driver of type $i$ and one driver of type $j$ and $A_{\text{total}}$ as the total number of fatal crashes. Then

[5] Since we have three observable pieces of data (the number of drinking-drinking, drinking-sober, and sober-sober crashes), one might expect that we might be able to do better than to identify only two parameters, the ratios $\theta$ and $N$. In fact, although there are three equations, the three equations are linearly dependent (i.e., the equations sum to one), so in practice only two parameters can be identified.

$$\Pr(A_{DD}, A_{DS}, A_{SS}|A_{\text{total}}) = \frac{(A_{DD} + A_{DS} + A_{SS})!}{A_{DD}!A_{DS}!A_{SS}!}(P_{DD})^{A_{DD}}(P_{DS})^{A_{DS}}(P_{SS})^{A_{SS}}. \tag{11}$$

Substituting the solutions to $P_{DD}$, $P_{DS}$, and $P_{SS}$ into equation (11) yields the likelihood function for the model. In the empirical work that follows, we perform maximum likelihood estimation of equations (8)–(11), directly linking our empirical estimation strategy to the model. Note that maximization of equation (11) with respect to $P_{DD}$, $P_{DS}$, and $P_{SS}$ yields the following intuitive result:

$$\hat{P}_{DD} = \frac{A_{DD}}{A_{\text{total}}}, \quad \hat{P}_{DS} = \frac{A_{DS}}{A_{\text{total}}}, \quad \hat{P}_{SS} = \frac{A_{SS}}{A_{\text{total}}}. \tag{12}$$

In other words, the maximum likelihood estimate of the fraction of crashes involving two drinking drivers is simply the observed fraction of such crashes in the data.

In order to solve the model for $\theta$, we take the following ratio, in which $N$, the ratio of drinking to sober drivers, cancels out:

$$\frac{(A_{DS})^2}{A_{DD}A_{SS}} = \frac{(\hat{P}_{DS})^2}{\hat{P}_{DD}\hat{P}_{SS}} = \frac{(\theta + 1)^2N^2}{\theta N^2} = 2 + \theta + \frac{1}{\theta}. \tag{13}$$

It is worth pausing here to note the significance of equation (13), which says that it is possible to determine the relative crash risk of drinking and sober drivers ($\theta$) *solely* on the basis of the observed distribution of fatal crashes. The key to this result is the cancellation of the $N$ term. The $N$ disappears from equation (13) because, by the binomial distribution, the squared number of interactions between drinking and sober drivers is in fixed proportion to the product of drinking-drinking and sober-sober interactions. Therefore, information on the relative number of drinking and sober drivers on the road is not needed to identify the model.

Defining the value in the left-hand side of equation (13) as $R$,

$$R \equiv \frac{(A_{DS})^2}{A_{DD}A_{SS}}, \tag{14}$$

then rearranging equation (13) and multiplying through both sides by $\theta$ yields an equation that is quadratic in $\theta$:

$$\theta^2 + (2 - R)\theta + 1 = 0, \tag{15}$$

the solutions to which are

$$\theta = \frac{(R-2) \pm \sqrt{R^2 - 4R}}{2}. \tag{16}$$

Ignore for the time being values of $R < 4$, which do not yield a real-valued solution for $\theta$. When $R = 4$, the only solution is $\theta = 1$, a limiting case in which drinking drivers pose no greater risk of causing two-car crashes than sober drivers. Note that for $R = 4$, the distribution of fatal crashes precisely matches the distribution of crash opportunities as given by the binomial distribution. This occurs only when the crash likelihoods are equal. For all $R > 4$, there are two real solutions: one with $\theta > 1$ and the other with $\theta < 1$. By assumption 5, which requires drinking drivers to be at least as dangerous as sober drivers, we select the first of those two solutions. When a solution for $\theta$ is obtained, it is straightforward to back out the relative number of drinking and sober drivers. Standard errors for both $\theta$ and $N$ are readily attainable from the Hessian of the likelihood function.[6]

Now consider the case in which $R < 4$. A non-real-valued solution to equation (16) emerges. Values of $R < 4$ are not consistent with the binomial distribution; that is, there is no combination of $\theta$ and $N$ that can generate this outcome. From equation (14), low values of $R$ result when there are too few drinking-sober crashes. Such values of $R$ may arise in practice either because of small numbers of observed crashes or because of a violation of the equal mixing assumption, as will be discussed below. It is important to note, however, that observed values of $R < 4$ do not invalidate the maximum likelihood estimation. When $R < 4$, the maximum likelihood estimate of $\theta$ is one and the maximum likelihood estimate of $N$ is the observed ratio of sober and drinking drivers involved in two-car crashes. Note that regardless of the value of $R$, standard errors for both $\theta$ and $N$ can be computed.

Figure 1 provides a sense of how the estimates of $\theta$ and $N$ vary with the distribution of two-car crashes. The *y*-axis in figure 1 reflects $\theta$, the relative crash likelihood of drinking drivers. The *x*-axis is the number of mixed drinking-sober crashes ($A_{DS}$). The two curves plotted in the

[6] In particular, the first-order condition from maximum likelihood estimation of eq. (11) provides a solution for $N$ in terms of $\theta$:

$$\theta N = \left[A_{DS}\left(\frac{\theta}{1+\theta}\right) + A_{DD}\right] \Big/ \left[A_{DS}\left(\frac{1}{1+\theta}\right) + A_{SS}\right].$$

From the definitions of $N$ and $\theta$, the left-hand side of this equation is $N_D\theta_D/N_S\theta_S$. This ratio is the number of two-car crashes caused by drinking drivers relative to the number caused by sober drivers. The right-hand side of the equation above expresses that ratio solely in terms of $\theta$ and the observed distribution of crashes. Drinking drivers cause $\theta/(1+\theta)$ of the crashes between drinking and sober drivers and all crashes involving two drinking drivers; sober drivers cause $1/(1+\theta)$ of the crashes between drinking and sober drivers and all crashes involving two sober drivers.

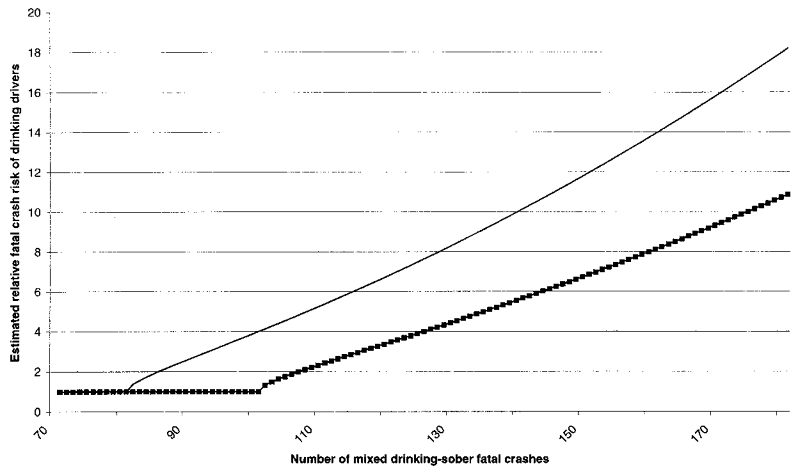


FIG. 1.—Estimated relative risk of drinking drivers as a function of observed crash mix. Solid line: $A_{DD}=20$, $A_{SS}=80$; dotted line: $A_{DD}=50$, $A_{SS}=50$.

figure correspond to alternative numbers of drinking-drinking and sober-sober crashes. The top line is the case in which $A_{DD}$ and $A_{SS}$ (the number of fatal crashes involving two drinking and two sober drivers) are 20 and 80, respectively. In the bottom line, both $A_{DD}$ and $A_{SS}$ are held constant at 50. When the number of drinking-drinking and sober-sober crashes is held constant, the estimated $\theta$ increases roughly in proportion to the square of the number of crashes involving one sober and one drinking driver, although over the relevant range the relationship appears almost linear since the constant of proportionality ($1/A_{DD}A_{SS}$) is so small. The flat portions of the curves correspond to values of $R < 4$ described in the preceding paragraph. While the shapes of the curves are similar, note that for a given number of mixed drinking-sober crashes, the implied $\theta$ is much higher when the number of drinking-drinking crashes is lower. The intuition for this result is that when drinking-sober crashes are held constant, a low number of drinking-drinking crashes implies fewer drinking drivers on the road since the number of drinking-drinking crashes is a function of the square of the number of drinking drivers. In order for a small number of drinking drivers to crash into sober drivers so frequently, the drinking drivers must have a high risk of causing a crash.

*Incorporating One-Car Crashes*

The model above is derived without reference to one-car crashes. Intuition might suggest that there would be useful information provided by such crashes. In fact, that intuition turns out to be correct only to a limited degree. Since one-car crashes lack the interactive nature of two-car crashes, which provides identification of the model, there is relatively little to be gained by adding one-car crashes to the model.

Let $\lambda_D$ and $\lambda_S$ denote the probabilities that drinking and sober drivers make an error resulting in a fatal one-car crash, paralleling the $\theta_D$ and $\theta_S$ terms in the two-car case. Let $C$ be an indicator for the presence of a one-car crash; that is, $C = 1$ if a one-car crash occurs and equals zero otherwise. Let $Q_D$ and $Q_S$ denote the respective probabilities that a drinking or sober driver is involved in a given one-car crash. Then

$$Q_D = \Pr(i = D|C = 1) = \frac{\lambda_D N_D}{\lambda_D N_D + \lambda_S N_S} \tag{17}$$

and

$$Q_S = \Pr(i = S|C = 1) = \frac{\lambda_S N_S}{\lambda_D N_D + \lambda_S N_S}. \tag{18}$$

Defining $\lambda = \lambda_D/\lambda_S$, we can rewrite the ratio of equations (17) and (18) as

$$\frac{Q_D}{Q_S} = \lambda N. \tag{19}$$

Thus, in contrast to the two-car case, it is impossible to separately identify the parameters of the model using only one-car crashes. Algebraically, adding equation (19) to the two-car crash model provides one additional equation and one extra unknown. Since $N$ is identified from two-car crashes, it is possible nonetheless to back out estimates of $\lambda$. Note, however, that it is the identification coming from the two-car crashes that is critical to obtaining that parameter, and adding the one-car crashes does not affect the solution to the two-car case.[7]

*Relaxing the Assumptions*

Before we proceed to the data, it is worth considering the way in which each of the assumptions made influences the solution to the model,

[7] In the empirical estimation, we include one-car crashes for two reasons. First, we are interested in estimating $\lambda$, even if identification hinges on two-car crashes. Second, in our empirical estimates, we shall generally impose equality restrictions across $\theta$ for different geographic areas or time periods. Once such restrictions are imposed, one-car crashes are useful in estimating the parameters of the model.

possible alternative assumptions, as well as the likely direction of bias in the estimation induced by violation of the assumptions.

The assumption of exactly two types is relatively innocuous. The introduction of more types allows for greater differentiation of estimates for particular subgroups of the population (e.g., drinking teenagers or sober motorists with clean driving records). From a practical perspective, however, the number of two-car crashes is not great enough to support more than a small number of categories. In the presence of heterogeneity within categories (i.e., there is a distribution of driving abilities among drinking and sober drivers), the estimates obtained from the two-type model are nonetheless readily interpretable. The estimates obtained are weighted averages across drivers in the category, with weights determined by the number of drivers of each ability *on the road*.[8] This is a surprising and useful result. Given that we observe drivers only in fatal crashes, intuition might suggest that the weights would be based on the distribution of abilities among crashers rather than drivers as a whole. If our coefficients reflected only the abilities of those in crashes, then the results would be much less useful for public policy since the distribution of crashers is likely to be very different from the underlying distribution of drivers.[9]

The second assumption, equal mixing/homogeneity on the road, is much more critical to the results. This assumption will likely be violated through spatial or temporal clumping of similar types of drivers. For instance, if the unit of observation were all crashes in the United States in a given year, then it is clear that drinking drivers would not be randomly distributed, but rather concentrated during nighttime hours and especially weekend nights. Even within a smaller unit of analysis (e.g., weekends between midnight and 1:00 A.M. in a particular state and year) there may be clumping. Roads near bars may contain a higher fraction of drinking drivers, or the proportion of drinking drivers may rise

[8] Suppose that driver types $D$ and $S$ have distributions of driver risk given by $f_D(\theta)$ and $f_S(\theta)$. Then the probability of a fatal accident given only the types $i$ and $j$ of two interacting drivers is

$$\begin{aligned}\Pr(A = 1 \mid i, j, I = 1) &= \int (\theta_i + \theta_j - \theta_i\theta_j) f_{i,j}(\theta_i, \theta_j)\, d\theta_i d\theta_j \\ &= E(\theta_i) + E(\theta_j) - E(\theta_i\theta_j) \approx E(\theta_i) + E(\theta_j).\end{aligned}$$

Thus with heterogeneity we can simply reinterpret $\theta$ as the mean driver risk for the given types.

[9] In the presence of heterogeneity, our simple model can do no better than to identify the ratio of the means of the distributions. Identification of higher moments of the distributions would require imposing arbitrary parametric assumptions.

sharply immediately following bar closings.[10] Such a nonrandom distribution of drivers will result in a greater number of drinking-drinking and sober-sober interactions than predicted by the binomial distribution, with correspondingly fewer drinking-sober interactions. From equation (14), nonrandom mixing will lead to smaller values of $R$ and consequently a downwardly biased estimate of $\theta$, which is an increasing function of $R$. Conversely, $N$, the ratio of drinking to sober drivers, will be biased upward. Violations of this assumption are likely to be less extreme as the geographic and temporal units of analysis shrink. In fact, that is precisely the pattern revealed by the empirical estimates in Section IV.

The third assumption requires that one driver be wholly at fault in a fatal crash, rather than that both drivers share some fraction of the blame. A simple alternative model would allow both drivers to play a role in the crash: if one driver makes a fatal error, the second driver has an opportunity to take an action to avoid the crash. Let $\mu$ denote the relative inability of drinking drivers to avoid a fatal crash that another driver initiates, with $\mu \geq 1$. It is straightforward to demonstrate that this more general model yields a solution $R = 2 + (\theta/\mu) + (\mu/\theta)$, paralleling equation (13), but with $\theta/\mu$ replacing $\theta$. In our basic model, $\mu$ is implicitly set equal to one because there is no scope for crash avoidance. If in actuality drinking drivers are less proficient in avoiding crashes initiated by the other driver, then the parameter identified in our model is actually $\theta/\mu < \theta$. Allowing for sober drivers to be more skilled in averting potential fatal crashes yields a larger estimated value of $\theta$ for any observed distribution of crashes, implying that the estimates for $\theta$ obtained in this paper are lower bounds on the true value of how dangerous drinking drivers are.[11]

[10] It is at least theoretically possible that this assumption could be violated in the opposite way; i.e., drinking drivers would be less likely to interact with other drinking drivers. For instance, if all drinking drivers are traveling north on a two-lane highway and all sober drivers are traveling south, then drinking and sober drivers will disproportionately interact with the opposite type.

[11] An even more general model would also allow the seriousness of mistakes made by drivers to vary by type. Let $\delta > 1$ capture the additional difficulty of avoiding a mistake made by a drinking driver. The parameters that we identify in two-car and one-car crashes in this expanded model are, respectively, $\theta\delta/\mu$ and $\lambda/\mu$. The term $\theta\delta$ is the seriousness-weighted ratio of drinking to sober driver mistakes—precisely what we are attempting to capture in the model; thus this further generalization does not pose any problems to our estimation. It is worth noting that $\delta$ does not appear in the solution for one-car crashes. If $\delta$ were large, one would expect to see big differences in the empirical estimates for one-car and two-car crashes. In practice, the two sets of estimates are close, perhaps suggesting that $\delta \approx 1$. One could also imagine other possible models of fatal crashes. For instance, the probability of a crash (conditional on the occurrence of an interaction) might be modeled as $\theta_i\theta_j$. In other words, each driver has to make a mistake in order for a crash to occur. In such a model, the solution to eq. (13) is $R = 4$ for all observed crash distributions (a result strongly rejected by the data), and only the composite parameter $\theta N$ is identified.

The final two assumptions of the model, independence of crashes and the higher risk that drinking drivers pose, are unlikely to impose any important biases on the empirical estimates. The existing evidence overwhelmingly confirms the assumption that drinking drivers have greater likelihoods of involvement in fatal crashes. Given the unit of analysis of the paper, the independence assumption appears quite reasonable. Even if this assumption were to fail (i.e., the presence or absence of one crash influences other potential crashes), there is no reason to expect that violation of this assumption should systematically bias the estimates in one direction. For bias to occur, the presence of one crash has to differentially affect drinking-sober crashes relative to drinking-drinking or sober-sober crashes.

## III. Data on Fatal Crashes

The primary source of data on fatal motor vehicle accidents in the United States is the Fatality Analysis Reporting System (FARS) administered by the National Highway Transportation Safety Administration. Local police departments are required by federal law to submit detailed information on each automobile crash involving a fatality. Compliance with this law was uneven until 1983; thus we restrict the analysis of the paper to the years 1983–93. Because our primary interest is the impact of alcohol on driver risk, we limit our sample to those hours (8:00 P.M.–5:00 A.M.) in which drinking and driving is most common.[12] Our sample includes over 100,000 one-car crashes and over 40,000 two-car crashes. During these hours, almost 60 percent of drivers involved in fatal crashes have been drinking, compared to less than 20 percent of drivers at all other times of the day. Crashes involving three or more drivers, which represent less than 6 percent of fatal crashes, are dropped from the sample. All crashes from a handful of state-year pairs with obvious data problems are also eliminated.

Among the variables collected in each fatal crash are information on the time and location of the accident and whether the drivers involved were under the influence of alcohol. In extensions to the basic model, we also utilize information on the age, sex, and past driving record of those involved in fatal crashes. Therefore, we exclude from the sample any crash in which one or more of the drivers are missing information about the time or location of the accident, police-reported drinking status, age, sex, or past driving record. Combined, these missing data lead to the exclusion of roughly 8 percent of all crashes.

[12] Also the lack of drinking-drinking crashes during the daytime period makes estimation difficult. During daytime hours, there are typically only a total of about 30 two-car crashes a year in the entire United States in which both drivers have been drinking.

Two measures of alcohol involvement are included in FARS. The first of these is the police officer's evaluation of whether or not a driver had been drinking. The officer's assessment may be based on formal breath, blood, or urine test results or other available evidence such as a driver's behavior (for those drivers not killed in the crash) or alcohol on the driver's breath. The primary advantage of this measure is that it is available for virtually every driver involved in a fatal crash. There are at least two drawbacks of this variable. First, it does not differentiate between varying levels of alcohol involvement. In particular, no distinction is made between those drivers who are legally drunk and those who have been drinking but are below the legal limit. Second, the measure is often subjective and relies on the discretion of the police at the scene of the accident.

In spite of these shortcomings, the police officer's assessment of whether or not a driver has been drinking serves as our primary measure of alcohol involvement. As a consequence, the coefficients we obtain with respect to the elevated risk associated with drinking and driving are based on the entire population of drinking drivers, not just the subgroup of legally drunk drivers. As a check on the results obtained, we also examine a second measure of alcohol involvement, which is measured blood-alcohol content (BAC). The major problem with this variable is the frequent failure to conduct such tests, despite federal law mandating that all drivers involved in fatal crashes be tested. Evidence suggests that the likelihood of BAC testing is an increasing function of actual blood-alcohol levels, suggesting that this measure will be most flawed for low-BAC motorists. Thus, for the purpose of analysis, we compare two groups of drivers: (1) those with measured BAC greater than 0.10 percent (the legal limit in most states for most of the sample period) and (2) those who test free of alcohol or who are not tested but whom police describe as not having been drinking. We eliminate those who test positive for alcohol but are below the 0.10 threshold. Furthermore, because sample selection in the pool of drivers who are tested for BAC is a major concern when this measure is used, we exclude all crashes occurring in states that do not test at least 95 percent of those judged to have been drinking by the police in our sample in that year (regardless of whether the motorist in question was tested). This requirement excludes more than 80 percent of the fatal crashes in the sample.

The presence of classification error in each of our measures of drinking, like the violations of the model's assumptions in the previous section, will almost certainly lead to an underestimate of the relative fatal crash risk of drinking drivers, making our estimates conservative. This point is discussed in detail in the next section.

The time series of fatalities in all motor vehicle crashes rises from an

TABLE 1
SUMMARY STATISTICS FOR FATAL CRASHES IN THE SAMPLE: ONE- AND TWO-CAR CRASHES BETWEEN 8:00 P.M. AND 5:00 A.M., 1983–93

| Variable | Mean |
|---|---|
| Total number of fatal one-car crashes | 103,077 |
| Total number of fatal two-car crashes | 39,470 |
| Percentage of all drivers in fatal crashes: | |
| Reported to be drinking by police | 53.4 |
| Male | 82.2 |
| Under age 25 | 44.0 |
| Bad previous driving record | 37.2 |
| Reported to be drinking and male | 45.2 |
| Reported to be drinking and under age 25 | 24.6 |
| Reported to be drinking and bad previous driving record | 23.5 |
| Percentage of fatal one-car crashes with: | |
| One drinking driver | 63.0 |
| One sober driver | 37.0 |
| Percentage of fatal two-car crashes with: | |
| Two drinking drivers | 14.2 |
| One drinking, one sober driver | 53.2 |
| Two sober drivers | 32.6 |
| Percentage of fatal one-car crashes in restricted sample with high BAC reporting with: | |
| One legally drunk driver | 55.9 |
| One sober driver | 44.1 |
| Percentage of fatal two-car crashes in restricted sample with high BAC reporting with: | |
| Two legally drunk drivers | 6.2 |
| One legally drunk, one sober driver | 47.8 |
| Two sober drivers | 46.0 |

NOTE—Means are based on one- and two-car fatal crashes in FARS data for the years 1983–93 between the hours of 8:00 P.M. and 5:00 A.M. Drinking status is based on the police classification of drivers as drinking and includes drivers who were not legally drunk, except in the bottom portion of the table, where the sample is restricted to crashes in state-year pairs in which at least 95 percent of the drivers whom police report to have been drinking were given blood-alcohol tests. A bad driving record is defined as two or more minor blemishes (a moving violation or previous accident) or one or more major blemishes (previous DUI conviction, license suspension, or license revocation) in the last five years. Values in the table are based on the same data that are used in estimation.

initial value of 42,589 in 1983 to a peak of just over 47,000 in 1988 and then declines to roughly 40,000 by 1993. The percentage of deaths occurring in crashes with at least one drinking driver steadily falls over the sample from 55.5 percent in the beginning to 43.5 percent in the end.

Table 1 presents means for the data in our sample.[13] Slightly more

[13] Because the degree of aggregation in our analysis varies from hour × year to hour × year × state × weekend, standard deviations, minimums, and maximums are not particularly meaningful.

than half of all drivers involved in fatal crashes are reported to be drinking by police. Drivers in fatal crashes are mostly male (82.2 percent), somewhat less than half are under the age of 25, and about one-third qualify as having bad previous driving records under our definition: two or more minor blemishes in the last three years (any combination of moving violations and reported accidents) or at least one major blemish (conviction for driving while intoxicated or license suspension/revocation). The fraction of drivers who are both male and drinking (45.2 percent) is higher than would be expected on the basis of the marginal distributions if gender and drinking were uncorrelated (i.e., $.534 \times .822 = .439$). This implies that males involved in fatal crashes are more likely to have been drinking than females. Young drivers and those with bad previous driving records are also more likely to have been drinking.

In one-car crashes, 63 percent of drivers are classified as drinking by the police. Of two-car crashes, 14.2 percent involve two drinking drivers, 53.2 percent have exactly one drinking driver, and in the remaining 32.6 percent of cases, neither driver was drinking. When we restrict our sample to states with good BAC reporting practices, 55.9 percent of drivers in one-car fatal crashes have a BAC over 0.10. In two-car crashes, there are two drivers with BACs over the legal limit 6.2 percent of the time and exactly one driver over the limit in 47.8 percent of the crashes.

## IV. Estimation of the Model

Table 2 presents maximum likelihood estimates of equation (11), focusing on how the relative fatal crash risks for drinking drivers in two-car crashes ($\theta$) and one-car crashes ($\lambda$) are affected as we increasingly disaggregate the data. Each column represents a different specification, with the distinction between columns being the unit of observation over which “equal mixing” of drivers is assumed. Column 1 is the most restrictive, imposing equal mixing of all drivers in all years, locations, and hours of the day. Equal mixing is unlikely to hold at such a high level of aggregation. This restriction is continually relaxed as one moves from left to right in the table. In column 3, for instance, equal mixing is imposed for crashes in each hour-year pair in the sample (e.g., it is assumed that between 2:00 A.M. and 3:00 A.M. in 1991, drinking and sober drivers are equally mixed across all locations in the United States). By column 8, equal mixing is assumed only within a given hour and weekend status (equal to one on Friday or Saturday night and zero otherwise) for a particular state and year.[14] Theory predicts that all the

[14] Stated more formally, in maximum likelihood estimation of eq. (11) in col. 8, we restrict $\theta$ and $\lambda$ to each be the same for all observations but allow $N$ to vary by state × year × hour × weekend.

TABLE 2

Relative Likelihood of Causing a Fatal Crash: Drinking vs. Sober Drivers (Allowing for Differing Restrictions on the Unit of Observation over Which "Equal Mixing" Is Imposed)

| Variable | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Relative two-car fatal crash risk for drinking drivers ($\theta$) | 3.79<br>(.14) | 4.87<br>(.16) | 4.92<br>(.16) | 5.14<br>(.16) | 5.35<br>(.17) | 5.74<br>(.18) | 6.48<br>(.20) | 7.51<br>(.22) |
| Relative one-car fatal crash risk for drinking drivers ($\lambda$) | 5.04<br>(.11) | 5.46<br>(.12) | 5.50<br>(.12) | 5.67<br>(.12) | 5.83<br>(.12) | 6.13<br>(.13) | 6.72<br>(.14) | 7.45<br>(.15) |
| Unit of observation over which "equal mixing" of drivers is imposed | all data | hour | hour × year | hour × year × weekend | hour × region × year | hour × region × year × weekend | hour × state × year | hour × state × year × weekend |
| Degrees of freedom used in estimation | 3 | 11 | 101 | 200 | 884 | 1,764 | 3,427 | 6,668 |
| Log likelihood | −106,961 | −103,795 | −103,658 | −103,068 | −102,537 | −101,534 | −99,690 | −97,307 |

Note.—Estimates in the table are the estimated relative one- and two-car crash risks for drinking vs. sober drivers. The reported values are maximum likelihood estimates of eq. (11). Drinking status is based on police assessments of the presence of alcohol; those who have been drinking but are not legally drunk are included in the drinking category. The unit of observation is state × year × day × hour. Data pertain to the years 1983–93 between the hours of 8:00 p.m. and 5:00 a.m., with some state-year observations omitted because of data problems. For a number of reasons presented in the paper, these estimates are likely to be lower bounds on the increased risk of drinking drivers. As one moves from left to right in the table, the unit of observation over which the assumption of "equal mixing" of drinking and sober drivers is imposed is relaxed. As this restriction is relaxed, theory predicts that the estimated coefficients should increase but are still likely to reflect a lower bound on the true values. Standard errors are in parentheses.

estimates presented in the table are likely to be lower bounds on the true parameters but that the downward bias will be mitigated as one moves from column 1 to column 8. In fact, that is precisely the pattern observed in the table. The two-car fatal crash risk of drinking drivers rises monotonically from four times greater than that of sober drivers to over seven times greater.[15] For one-car crashes, the value goes from five to more than seven times larger. In all cases, the parameters are precisely estimated, and the null hypothesis of equality between drinking and sober drivers (i.e., $\theta = 1$, $\lambda = 1$) is resoundingly rejected. The restrictions implied by the specifications in columns 1–7 relative to column 8 are each rejected with the relevant likelihood ratio test. For this reason, and because theory predicts that less restrictive specifications should minimize the downward bias, we take column 8 as our preferred specification.[16] All results presented in the remaining tables correspond to column 8.

Although the parameters are not directly comparable to past estimates in the literature, it is nonetheless useful to consider relative magnitudes. Zylman (1973) finds that the relative crash risk of those with positive levels of alcohol is only 2.2 times that of sober drivers, much smaller than our estimates. The reason underlying his estimate of a low impact is that drinking drivers in his sample are disproportionately made up of low-BAC drivers. More informative are the estimates of Borkenstein et al. (1974), who find the relative likelihood of causing a fatal crash to be two times higher for drivers with BACs between 0.05 and 0.099, 10.1 times higher for BACs between 0.10 and 0.149, and 30–40 times greater for BACs over 0.15. For reasonable distributions of BACs across drinking drivers, the results of table 2 are consistent with the Borkenstein et al. estimates. Zador (1991), using FARS data in conjunction with the results of the national roadside testing survey of Lund and Wolfe (1991), estimates that BACs of 0.05–0.09 are associated with nine times greater risk, BACs of 0.10–0.15 are associated with 50 times greater risk, and BACs above 0.15 have 300–600 times greater risk. These estimates are far greater than those that we obtain. Zador's study is subject to the critiques of roadside surveys discussed earlier in this paper. His high-risk estimates, especially at the highest BAC levels, can be explained by

[15] To the extent that the drinking drivers, when they are themselves sober, are systematically safer/more dangerous than other sober drivers, this coefficient gives a biased estimate of the incremental effect of alcohol on driver risk. Previous research (Hurst et al. 1994) suggests that drinking drivers, when sober, are actually safer than the typical sober driver. If that is the case, then the coefficients in table 2 understate the true impact of alcohol on driver risk. Estimates that further disaggregate drivers by type, presented below, will also shed light on this issue.

[16] One could imagine further disaggregating the data. The main stumbling block is lack of data. By col. 8, we are allowing over 6,000 different cells in which there are approximately 43,000 two-car crashes.

TABLE 3
ESTIMATES OF THE RELATIVE CRASH RISK OF DRINKING VS. SOBER DRIVERS (Allowing for Risk to Vary across Years)

| Year | Relative Two-Car Fatal Crash Risk for Drinking Drivers ($\theta$) (1) | Relative One-Car Fatal Crash Risk for Drinking Drivers ($\lambda$) (2) | Implied Fraction of Drivers That Have Been Drinking (8:00 P.M. to 5:00 A.M.) (3) |
|---|---|---|---|
| 1983 | 6.74 | 6.83 | .205 |
| | (.80) | (.56) | (.016) |
| 1984 | 6.34 | 6.25 | .221 |
| | (.62) | (.41) | (.013) |
| 1985 | 8.40 | 7.54 | .185 |
| | (.78) | (.50) | (.012) |
| 1986 | 6.82 | 7.07 | .206 |
| | (.63) | (.45) | (.012) |
| 1987 | 8.29 | 7.26 | .198 |
| | (.71) | (.44) | (.011) |
| 1988 | 7.16 | 7.57 | .188 |
| | (.66) | (.49) | (.012) |
| 1989 | 8.01 | 7.77 | .177 |
| | (.74) | (.51) | (.011) |
| 1990 | 7.69 | 7.72 | .182 |
| | (.73) | (.52) | (.012) |
| 1991 | 8.89 | 8.60 | .161 |
| | (.88) | (.63) | (.012) |
| 1992 | 7.34 | 8.08 | .164 |
| | (.80) | (.64) | (.013) |
| 1993 | 7.59 | 7.98 | .153 |
| | (.85) | (.66) | (.014) |
| *F*-test: equality of coefficients across years | 6.78 | 10.93 | 26.21 |

NOTE.—Values are maximum likelihood estimates of eq. (11), allowing the relative crash risks of drinking and sober drivers to vary by year. The implied fraction of drinking drivers (col. 3) is calculated on the basis of the estimates in cols. 1 and 2 and the means of the data in our sample (between the hours of 8:00 P.M. and 5:00 A.M., 1983–93). A driver does not have to be legally drunk to be categorized as drinking. Note that in the estimation, the fraction of drinking drivers is allowed to vary by state × year × weekend × hour. Thus the estimates in this table are comparable to col. 8 of table 2. Coefficients for each year are estimated separately. For the reasons stated in the paper, the values in cols. 1 and 2 are likely to be lower bounds, and the value in col. 3 is likely to be an upper bound. Standard errors are in parentheses. The reported *F*-test is asymptotically distributed $\chi^2$ with 10 degrees of freedom. The .05 critical value for this statistic is 18.3.

noting that the highest BAC range is likely to be most underrepresented in the voluntary roadside survey used. When this underestimate of the fraction of high-BAC drivers on the road is compared to the true involvement rate of high-BAC drivers in fatal accidents, the result is sure to overestimate the relative risk at this BAC level.

Table 3 presents separate estimates of relative crash risk across the years of our sample. For both one-car and two-car crashes, the parameter estimates are fairly stable. A test of the null hypothesis of equality across all of the years, reported in the bottom of the table, cannot be rejected in either case. Given that each year's estimates are derived independently, the stability of the parameters suggests that the estimation approach is robust.

Column 3 of table 3 presents the implied fraction of drivers in the sample who have been drinking.[17] To the extent that estimates of the relative risks of drinking drivers are downward biased in columns 1 and 2, the fraction of drinking drivers will be biased upward. The estimates of drinking drivers range from a high of 22.1 percent in 1984 down to a low of 15.3 percent in 1993. There is a discernible downward trend in the fraction of drinking drivers over the period. The null hypothesis of a constant share of drinking drivers across all years is rejected at the .01 level.[18]

Table 4 breaks down the estimates by hour of the day. Once again, the relative risk for two-car fatal crashes is stable, and equality across all hours cannot be rejected. One-car crash risks vary enough that the test of equality is rejected. The relative risk of drinking drivers appears to be somewhat lower between 8:00 P.M. and 10:00 P.M., perhaps as a result of a less lethal composition of drinking drivers on the roads during these early hours. The peak hours for drinking and driving are between 1:00 A.M. and 3:00 A.M., as reported in column 3. More than one-quarter of drivers appear to have some alcohol in their system during these hours. This is more than twice as high as before 10:00 P.M. and after 4:00 A.M.

Table 5 reports the sensitivity of the basic results on drinking and driving to the alternative assumptions discussed in Section III as well as to various forms of measurement error. The first row of the table contains the baseline estimates for comparison purposes. Each of the remaining rows reflects a different violation of the assumptions. If crash avoidance matters in two-car crashes (violating assumption 3) and drinking drivers are 25 percent less successful in getting out of the way ($\mu = 1.25$), then the estimated impact of alcohol rises above nine in both two-car and one-car crashes and 11 in one-car crashes.[19]

[17] These estimates are derived in two steps. First, $\theta$ and $\lambda$ are estimated, allowing the ratio of sober to drinking drivers to vary by state × year × hour × weekend. Then $\theta$ and $\lambda$ are fixed at the maximum likelihood estimates from the first step and the optimal $N$ is estimated. Then $N$ is transformed into the percentage of drinking drivers, with the appropriate standard errors calculated using the delta method.

[18] The estimated fraction of drinking drivers is broadly consistent with numbers obtained from other approaches. In a national roadside survey, Lund and Wolfe (1991) report that 8.3 percent of those who provide BACs had alcohol levels greater than 0.05. If all of those who refused to provide BACs had been drinking, then the total fraction of drinkers in their sample could be as high as 16.3 percent.

[19] Since the identified parameters are $\theta/\mu$, $\lambda/\mu$, and $\mu N$, the risks of drivers in one- and two-car crashes are simply scaled up by the relative crash avoidance factor $\mu$. Although one-car crashes are not directly affected by violations in this assumption, the estimates of one-car crashes rely on the fraction of drinking drivers on the road, which is identified through the two-car crashes. As a result, $\lambda/\mu$ is identified rather than $\lambda$, paralleling $\theta/\mu$'s identification rather than $\theta$.

TABLE 4
ESTIMATES OF THE RELATIVE CRASH RISK OF DRINKING VS. SOBER DRIVERS (Allowing for Risk to Vary by Time of Day)

| Time of Day | Relative Two-Car Fatal Crash Risk for Drinking Drivers ($\theta$) (1) | Relative One-Car Fatal Crash Risk for Drinking Drivers ($\lambda$) (2) | Implied Fraction of Drivers That Have Been Drinking (8:00 P.M. to 5:00 A.M.) (3) |
|---|---|---|---|
| 8:00 P.M.–9:00 P.M. | 6.33<br>(.55) | 5.55<br>(.36) | .136<br>(.008) |
| 9:00 P.M.–10:00 P.M. | 6.82<br>(.57) | 5.95<br>(.37) | .145<br>(.008) |
| 10:00 P.M.–11:00 P.M. | 7.16<br>(.58) | 7.00<br>(.42) | .157<br>(.009) |
| 11:00 P.M.–midnight | 7.60<br>(.59) | 7.96<br>(.45) | .180<br>(.009) |
| Midnight–1:00 A.M. | 8.36<br>(.66) | 8.80<br>(.48) | .209<br>(.009) |
| 1:00 A.M.–2:00 A.M. | 7.33<br>(.64) | 7.68<br>(.42) | .275<br>(.011) |
| 2:00 A.M.–3:00 A.M. | 6.48<br>(.61) | 7.28<br>(.42) | .296<br>(.012) |
| 3:00 A.M.–4:00 A.M. | 7.28<br>(.90) | 8.85<br>(.72) | .222<br>(.016) |
| 4:00 A.M.–5:00 A.M. | 7.68<br>(1.17) | 9.74<br>(1.10) | .137<br>(.019) |
| *F*-test: equality of coefficients across hours | 6.62 | 42.51 | 236.63 |

NOTE.—Values are maximum likelihood estimates of eq. (11), allowing the relative crash risks of drinking and sober drivers to vary by hours of the day. The implied fraction of drinking drivers (col. 3) is calculated on the basis of the estimates in cols. 1 and 2 and the means of the data in our sample (fatal crashes between the hours of 8:00 P.M. and 5:00 A.M., 1983–93). A driver does not have to be legally drunk to be categorized as drinking. Note that in the actual estimation, the fraction of drinking drivers is allowed to vary by state × year × weekend × hour. Thus the estimates in this table are comparable to col. 8 of table 2. Coefficients for each hour of the day are estimated separately. For the reasons stated in the paper, the values in cols. 1 and 2 are likely to be lower bounds, and the value in col. 3 is likely to be an upper bound. Standard errors are in parentheses. The reported *F*-test is asymptotically distributed $\chi^2$ with eight degrees of freedom. The .05 critical value for this statistic is 15.5.

Violations of equal mixing (assumption 2) might likely occur through an increased probability of same type interactions beyond that given by the binomial distribution. To consider such violations, we augment the interaction stage of the model. If $N_D/N_{\text{total}}$ is the fraction of drinking drivers on the road, then in the augmented model the probability that two drivers passing are both drinking drivers is $(1 + \Delta)(N_D/N_{\text{total}})^2$ rather than $(N_D/N_{\text{total}})^2$; that is, drinking drivers are $\Delta$ times more likely to interact with one another than would be suggested by the binomial distribution. Sober drivers are assumed to be precisely enough more likely to interact with sober drivers to maintain the assumption that the overall fraction of interactions by driver type reflects the fraction of drivers on the road. Given any value of $\Delta$ corresponding to some degree of unequal mixing, we can estimate this expanded model in order to assess the sensitivity of our estimates to violations of assumption 2. In-

TABLE 5

Sensitivity of the Estimates to Violations of the Modeling Assumptions and the Presence of Measurement Error

| | Relative Two-Car Fatal Crash Risk for Drinking Drivers ($\theta$) (1) | Relative One-Car Fatal Crash Risk for Drinking Drivers ($\lambda$) (2) | Implied Fraction of Drivers Who Have Been Drinking (8:00 P.M. to 5:00 A.M.) (3) |
|---|---|---|---|
| Baseline | 7.51 | 7.45 | .186 |
| | (.22) | (.15) | (.004) |
| Drinking drivers 25 percent less efficient at avoiding crashes initiated by other drivers | 9.38 | 9.31 | .155 |
| | (.28) | (.19) | (.003) |
| Drinking drivers 10 percent more likely to interact with drinking drivers than with sober drivers | 9.60 | 9.00 | .160 |
| | (.26) | (.18) | (.003) |
| 5 percent of drivers misclassified because of random measurement error | 11.99 | 11.71 | .135 |
| | (.19) | (.05) | (.001) |
| 5 percent of drinking drivers misclassified as sober | 9.73 | 9.31 | .177 |
| | (.14) | (.04) | (.001) |
| In 5 percent of *crashes* all drivers are reported as sober, regardless of true drinking status | 11.23 | 10.12 | .166 |
| | (.34) | (.23) | (.004) |

Note.—The baseline specification in the first row corresponds to col. 8 of table 2. The other values reported in the table are estimates of the true parameters implied by the coefficients in the baseline specification if that baseline specification is contaminated in the named manner. Crash avoidance represents a violation of assumption 3. Non–equal mixing violates assumption 2. The last three rows of the table report three different types of measurement error.

creased clustering of same-type drivers corresponds to lower predicted values of $R$ in the model. To offset this effect and explain the observed $R$ in the data, $\theta$ must increase (and $N$ decrease). The result in the third row of table 5 shows that a 10 percent increase in drinking-drinking interactions would lead to an approximately 25 percent increase in the estimates of one- and two-car relative risks.

An implicit assumption of the model is that driver types are known. In fact, as discussed in Section III, we use the police officers' assessment of the drinking status of involved drivers, which is a potentially imperfect measure. The remaining rows of table 5 examine the sensitivity of our estimates to three alternative forms of driver misclassification. The fourth row of the table corresponds to a case in which 5 percent of the observations are randomly misrecorded; that is, a drinking driver is as likely to be mistakenly reported as sober as vice versa. In this case, the estimated risk of drinking drivers will be biased toward zero by approximately 40 percent. Our results are sensitive to this type of measurement error because the few drinking-drinking crashes have the greatest impact on the estimates. Given the rarity of drinking-drinking crashes relative to drinking-sober crashes, such measurement error will exaggerate the number of drinking-drinking crashes; that is, the true number of drinking-drinking crashes is even smaller than that observed in the data. Previous research suggests, however, that this type of measurement error is not the most likely scenario. Lund and Wolfe (1991) present evidence that police officers systematically report drinking drivers to be sober, but not vice versa. If 5 percent of drinking drivers are misreported in this manner, then the true $\theta$, although still biased, is not as sensitive, as is the case with classical measurement error. A final misclassification story assumes that reporting errors are correlated within a particular crash. Different police officers may have divergent standards for classifying an individual as drinking because of the methods used to determine drinking status or the officer's skill in identifying drinking drivers. From equations (13) and (14), it is clear that such systematic measurement error will impart a large downward bias on $\theta$ because any misclassification of this type will shift crashes from the numerator to the denominator of $R$. For instance, if 5 percent of police officers filling out crash reports always report both drivers sober, even if they are drinking, then the true $\theta$ is almost 35 percent higher than our baseline estimate.

### *Examining Other Driver Traits*

Although the comparisons presented thus far are limited to drinking versus sober drivers, the model is equally applicable to other comparisons. Table 6 presents results for a range of other dimensions. The first

TABLE 6
Estimating Relative Fatal Crash Risks on the Basis of Other Driver Characteristics

| Comparison Groups | Relative Two-Car Fatal Crash Risk for First Category Named (1) | Relative One-Car Fatal Crash Risk for First Category Named (2) | Implied Fraction of Drivers in the First Category Named (8:00 P.M. to 5:00 A.M.) (3) |
|---|---|---|---|
| All drinking vs. sober | 7.51 | 7.45 | .186 |
| | (.22) | (.15) | (.004) |
| Legally drunk vs. sober | 13.24 | 14.39 | .080 |
| | (1.17) | (1.05) | (.011) |
| Under age 25 vs. all others | 1.39 | 1.66 | .353 |
| | (.18) | (.11) | (.009) |
| Male vs. female | 3.38 | 1.95 | .719 |
| | (.21) | (.05) | (.005) |
| Bad driving record vs. clean driving record | 2.41 | 1.85 | .254 |
| | (.13) | (.06) | (.006) |

Note.—Values in cols. 1 and 2 are maximum likelihood estimates of the fatal crash risk of the first group named relative to the second group named. All specifications assume equal mixing by hour × state × year and therefore are comparable to col. 8 of table 2. Estimates are based on the same data sample used in table 2, except in the comparison of legally drunk to sober drivers, where only those state-year pairs that report blood-alcohol levels for 95 percent of those drivers identified as drinking by the police are included in the sample. Standard errors are in parentheses.

row of table 6 presents as a baseline the results from drinking and sober drivers. The second row presents a comparison of drivers with BACs greater than 0.10 (as opposed to all drinking drivers) relative to sober drivers. As noted earlier, owing to sample selection concerns, we include only accidents in state-year pairs in which a high fraction of drivers are tested. As would be expected, the results for legally drunk drivers are even stronger than for all drinking drivers. Drivers over the legal BAC limit of 0.10 have a relative risk for fatal crashes that is 13–14 times higher than that of sober drivers. This number is almost twice as great as for all drinkers, including those who are not legally drunk. Approximately 8 percent of the drivers on the road during the hours we examine appear to be over the 0.10 limit, where this number is again most appropriately interpreted as an upper bound. The estimates are relatively imprecise because many states have poor records of BAC testing, leading them to be excluded from the sample.

Rows 3–5 consider other categories into which drivers can be divided. Drivers under the age of 25 are 40–70 percent more likely to cause fatal crashes. The estimate of the fraction of young drivers on the road (35 percent) appears reasonable. Grossman et al. (1993) report that those under the age of 25 accounted for 20 percent of drivers in 1984, but younger drivers are likely to make up a disproportionate fraction of nighttime drivers. Males are over three times as dangerous as female drivers for two-car crashes and are at two times higher risk for one-car fatal crashes. As will be demonstrated below, however, a large fraction of this gap is due to more frequent drinking and driving among males. Finally, those with bad driving records (either two or more minor blemishes on their driving record in the past three years or one or more major blemishes) appear to be more than twice as dangerous on the roads. Although table 6 reports significant increases in risk associated with other driver characteristics, it should be stressed that alcohol usage is far and away the best predictor of increased crash risk.[20]

The results presented thus far fail to take into account possible interactions between the various risk factors such as drinking status, gender, and age. To the extent that risk factors are correlated with one another, the results of table 6 may be misleading. Table 7 reports estimates that allow for interactions between drinking status and the other risk factors. In order to do so, we expand our model to allow for four driver types rather than two. All of the intuition from the two-type model presented in Section II continues to hold in the $n$-type case. Panel A of table 7 allows for differential fatal crash risk for young and old drivers who have or have not been drinking. Sober drivers over the age of 25 are the lowest-risk drivers and serve as a baseline. Interestingly, young sober drivers are almost three times as likely to cause fatal crashes as older sober drivers, but age has little impact on fatal crash risk among drinking drivers. Most likely, this reflects the fact that young drinking drivers tend to have low BACs relative to older drinking drivers. In column 3, we estimate that roughly one in four young drivers has been drinking in our sample, compared to one in six older drivers.

Panel B of table 7 reports the results of interacting drinking status with driver gender. Sober female drivers are the safest. Sober male drivers are 36 percent more likely to cause fatal two-car crashes and 10 percent more likely to cause fatal one-car crashes. Males who have been drinking are almost nine times more dangerous than sober females and are a 60 percent greater risk than drinking females. The gap between male and female drivers shrinks substantially when drinking status is taken into account.

Panel C of table 7 shows interactions between drinking status and past driving record. Sober drivers with bad past records are almost twice as likely to cause two-car fatal crashes as sober drivers with clean records. Interestingly, however, the impact of driving record shrinks substantially in percentage terms among those who have been drinking. Those with

[20] Consistent with our results, insurance premiums tend to be higher for young drivers, those with bad previous driving records, and male drivers. Auto insurance is highly regulated, however, so it is difficult to know if the magnitude of differences in premiums in an unregulated market would correspond to our estimates. A further complication is that insurance premiums take into account not only fatal crashes but also nonfatal accidents and potential property damage.

TABLE 7
Relative Fatal Crash Risk (Allowing for Interactions between Drinking and Other Driver Characteristics)

| Driver Classification | Two-Car Fatal Crash Risk Relative to Baseline Category (1) | One-Car Fatal Crash Risk Relative to Baseline Category (2) | Implied Fraction of Drivers in the First Category Named (8:00 p.m. to 5:00 a.m.) (3) |
|---|---|---|---|
| | A. Age | | |
| Under 25 × drinking | 10.88 | 10.79 | .083 |
| | (.57) | (.40) | (.003) |
| Over 25 × drinking | 10.13 | 8.56 | .115 |
| | (.46) | (.27) | (.003) |
| Under 25 × not drinking | 2.78 | 2.30 | .231 |
| | (.13) | (.06) | (.004) |
| Over 25 × not drinking | 1.00 | 1.00 | .572 |
| | | | (.004) |
| | B. Gender | | |
| Male × drinking | 8.57 | 6.93 | .169 |
| | (.94) | (.36) | (.005) |
| Female × drinking | 5.18 | 4.37 | .044 |
| | (.77) | (.38) | (.004) |
| Male × sober | 1.36 | 1.09 | .594 |
| | (.17) | (.05) | (.007) |
| Female × sober | 1.00 | 1.00 | .194 |
| | | | (.006) |
| | C. Past Driving Record | | |
| Bad driving record × drinking | 9.47 | 8.11 | .080 |
| | (.55) | (.35) | (.003) |
| Clean driving record × drinking | 7.52 | 6.91 | .118 |
| | (.38) | (.24) | (.003) |
| Bad driving record × sober | 1.92 | 1.37 | .212 |
| | (.11) | (.04) | (.004) |
| Clean driving record × sober | 1.00 | 1.00 | .590 |
| | | | (.004) |

Note.—Values in cols. 1 and 2 are maximum likelihood estimates of the fatal crash risk of the named group relative to the named baseline group (i.e., the group with a relative risk defined to be equal to one). All specifications assume equal mixing by state × year × day × hour and therefore are comparable to col. 8 of table 2. Estimates are based on the same data sample used in table 2. Standard errors are in parentheses.

bad driving records who have been drinking are roughly 25 percent more likely to cause fatal two-car crashes than drinking drivers with good records.

## V. Externalities and Public Policy

In this section we examine the public policy implications of the estimates obtained above. We focus on two sets of questions: (1) How big is the externality associated with drinking and driving, and (2) what impact do current public policies have on both the number of drinking drivers and the risk that these drivers pose?

We begin with the question of externalities.[21] We make a number of assumptions in what follows, all of which have the effect of understating the true negative externality. First, the analysis here is limited strictly to deaths in fatal crashes. If injuries, property damage, and lost welfare due to behavioral distortions (e.g., sober drivers being afraid to drive at night for fear of being hit by a drinking driver) were included, the estimates would be substantially higher. Second, we assume that if a drinking driver dies as a result of a crash that he causes, then he bears the cost of his actions. Presumably, when choosing to drive after drinking, he took the risk into account. Any suffering of friends and family that the driver does not internalize is left out of the calculation. Third, and perhaps more questionable, we assume that a parallel logic applies to passengers in the drinking driver's vehicle; that is, they are willing participants and thus have chosen to accept the risk associated with riding with a drinking driver.[22] Thus, in the externality calculations that follow, we include only the deaths of pedestrians and occupants of vehicles who die in crashes caused by a driver in another vehicle who has been drinking. We also present estimates under alternative assumptions for comparison purposes.

In 1994, 40,716 people died in motor vehicle crashes in the United States. Of these, 27,023 were killed in crashes that involved no drivers reported by the police to have been drinking.[23] Another 8,234 fatalities occurred in one-vehicle crashes in which the driver was drinking. On

[21] There are a substantial number of papers that attempt to measure the costs that alcohol imposes on society (e.g., Rice, Kelman, and Miller 1991; Harwood, Fountain, and Livermore 1998), with the typical estimate roughly $150 billion annually in 1998 dollars. Much of this research fails to distinguish between costs borne by alcohol users and costs borne by society, and all these papers exclude the utility that individuals obtain from drinking. Manning et al. (1989) draw the distinction between internalized and externalized costs, arguing that the optimal Pigouvian tax on alcohol is much higher than the current tax level.

[22] Passengers who bear increased risk of death because they are riding with drinking drivers are worse off. In that sense, drinking drivers do impose an externality on their passengers. Given the ability of passengers and drivers to negotiate jointly optimal consumption and travel patterns, however, it is not clear that there is a market failure between driver and passengers that should be corrected through government intervention. This is less likely to be true of children of the drinking drivers. Only 7 percent of passengers dying in vehicles driven by drinking drivers were under the age of 15. To the extent that drivers or passengers are systematically misinformed about the risks of drinking and driving, there may be an information dissemination role for public policy. In any specific instance, passengers are likely to have less precise information about the state of intoxication of the driver and the associated risk, although there is no reason to believe that passengers should systematically underestimate this risk.

[23] A substantial fraction of pedestrians involved in fatal crashes have been drinking. They are not included here.

the basis of the logic of the preceding paragraph, these deaths are also excluded from our externality estimate.

There are a number of different kinds of deaths that qualify as an externality. In two-vehicle crashes in which both drivers were drinking, 583 people died. We categorize half of these deaths (292) as externalities under the assumption that fatalities are evenly split between the vehicle that caused the crash and the other vehicle. Although the drivers of both vehicles were drinking, in our model only one of the drivers makes a fatal error. An additional 2,306 fatalities occurred among passengers of vehicles driven by sober drivers who died in two-vehicle crashes with drinking drivers. On the basis of our model, $(\theta - 1)/(\theta + 1)$ or 76.5 percent of these deaths would have been avoided if the driver had not been drinking. This translates into an additional 1,764 lives. Extrapolating our estimates to crashes with more than two vehicles and applying the same set of rules yields 308 more deaths that are classified as externalities. Finally, 631 pedestrians were killed by drinking drivers. Under the assumption that the relative risk of drinking drivers for killing pedestrians mirrors the estimates for one-vehicle crashes, $(\lambda - 1)/\lambda$ or 86.6 percent of these pedestrian deaths are attributable to alcohol. Combining all these categories, we estimate a total of 2,910 traffic fatalities in 1994 that without question should be classified as externalities as a result of drinking drivers. If passengers in vehicles driven by drinking drivers are also included in this calculation, the number would rise to about 5,000.

Value of life analyses such as Viscusi (1992) typically assign dollar amounts between $1 million and $5 million per life lost. With a value of $3 million, the externality associated with lost lives due to drinking and driving in 1993 was almost $9 billion. This calculation represents a lower bound because our estimates of how dangerous drinking drivers are is likely to be biased downward because injuries and property loss are excluded, as are deaths of all passengers in the car of the driver at fault. The Federal Highway Administration (1996) estimates that there are 2.3 trillion vehicle miles traveled each year. According to Festin (1996), 370 billion of these miles are driven during the hours covered by our sample, 8:00 P.M. to 5:00 A.M. Our estimates suggest that 15.3 percent of drivers in this time period were drinking, which translates into 56.6 billion miles driven by drinking drivers.[24] This implies a negative externality of 16 cents per mile driven by drinking drivers (including those who have been drinking but are not legally drunk). If we replicate the analysis above but instead use our estimates of the risk and

[24] The parameter $N$ gives the ratio of interactions by drinking drivers to those by sober drivers. Under assumption 2, $N$ can be used to compute the fraction of drinking drivers on the road. To translate this fraction into miles driven, we are assuming that drinking and sober drivers have approximately the same number of interactions per mile.

fraction of legally drunk drivers alone (see table 6), the externality almost doubles to 30 cents per mile driven.[25] This calculation implies that almost all the costs of drinking drivers are concentrated among those who are legally drunk.

One way to correct this externality is to tax alcohol consumption directly. If the typical drunk driver consumes eight drinks prior to driving and makes a 10-mile trip, then the necessary tax per drink to internalize this externality would be 37.5 cents. An alcohol tax is an extremely blunt instrument, however, because most alcohol consumption is not followed by motor vehicle operation. Taxing alcohol consumption to reduce drinking and driving introduces distortions into consumption decisions of those who do not drink and drive.

A more direct means of correcting the problem is to enforce drunk driving laws. In 1994, there were approximately 1.1 million arrests for driving under the influence (DUI), or roughly one arrest for every 27,000 miles driven by drunk drivers. In terms of internalizing the externality, the appropriate punishment for those arrested for driving drunk would be $8,000 per arrest, or the utility equivalent of this in license suspensions, increased insurance premiums, lost wages, embarrassment, or jail time. For most first-time offenders, the likely punishment is at or below this level: a small fine, perhaps a 30-day license suspension, and perhaps one night in jail. For many third-time offenders, however, the punishment can be substantial: a three-year license suspension and 30 days in jail would not be uncommon. Thus it appears that existing punishments are not radically out of line with what might be optimal.

### *The Impact of Public Policies*

We now switch attention to the impact of various state policies on the number of drinking drivers and the risks that they pose. A number of studies have examined the link between drunk driving laws, alcohol taxes, and motor vehicle fatalities (e.g., Cook and Tauchen 1982; Asch and Levy 1987; Saffer and Grossman 1987; Homel 1990; Chaloupka et al. 1993; Grossman et al. 1993; Ruhm 1996). Previous research is “reduced form” in the sense that the estimates obtained capture the total impact of policy changes on fatalities but do not shed light on the underlying behavioral parameters. For instance, if the number of alcohol-involved motor vehicle fatalities is found to fall when the punishment for repeat DUI offenders increases, existing research would not shed light on whether there were fewer drunk drivers on the roads or

[25] The internalized cost of fatal crash deaths per mile driven is about three times larger than the magnitude of the externality, or almost $1 per mile for legally drunk drivers.

whether drunk drivers choose to drive more carefully after the laws change. Learning about these behavioral parameters is critical to the development of optimal public policy (Heckman 2000).

Our estimation approach allows us to begin to shed light on these behavioral parameters. Using the estimates of Section IV as *dependent* variables, we are able to separately examine the number of drinking drivers and their relative risk of causing a fatal crash. We caution, however, that the results presented below represent only a first step in this direction.

Our regression specification takes the following form:

$$\text{depvar}_{st} = \text{policy}_{st}\Gamma + X'_{st}\Phi + \eta_t + \epsilon_{st}, \qquad (20)$$

where $s$ indexes states, $t$ reflects years, and depvar is any one of a number of measures of interest: fraction of drinking drivers on the road, relative two-car fatal crash risk ($\theta$), and relative one-car fatal crash risk ($\lambda$). These measures are constructed in a manner identical to those presented in table 4, except that separate estimates are obtained for each state-year pair.[26] The explanatory variables of primary interest are measures of state alcohol policies discussed below. Other control variables are the fraction of the state population between the ages of 15 and 24, between 25 and 54, or over age 55; the percentage of state residents in metropolitan areas; percentage black; and state unemployment rates. Year dummies are also included. In some specifications, state random effects or region fixed effects corresponding to the nine census regions are also added.[27]

Our measures of state alcohol policies are admittedly crude. We include the state beer excise tax per case (measured in 1993 dollars). We include two variables for sentence severity corresponding to first- and third-time offender punishments. Drunk driving penalties have two primary components: license suspension and jail time. States with both license suspensions of 30 or more days and mandatory jail time are assigned two points for first-offense severity. If only one of the conditions applies, one point is given. If neither applies, a zero is assigned. For third-time offenders, one point each is given for license suspensions of three or more years and mandatory jail sentences of 30 or more days.[28] The final state policy variable is the number of police per capita, which

[26] In contrast to estimated right-hand-side variables, which lead to errors-in-variables bias and also require standard error corrections (Murphy and Topel 1985), estimated left-hand-side variables do not pose any special econometric problems as long as this estimation error is uncorrelated with the explanatory variables.

[27] Because there is so little within-state variation in alcohol policies over this time period, models with state fixed effects are too imprecisely estimated to be useful.

[28] We have experimented with a variety of other approaches to capturing the severity of punishments, all of which yield categorically similar results.

is a (relatively crude) measure of the certainty of punishment for driving while intoxicated.

Table 8 reports the results of the analysis. Each column of the table presents results for a different dependent variable. Column 1 is the overall fraction of drinking drivers in fatal crashes. This measure mirrors the reduced-form approach of previous studies. Columns 2–4 show results for our three estimated parameters from the model ($\theta$, $\lambda$, and, $N$). Panel A of the table is estimated using weighted least squares, with weights based on the number of two-car crashes in the state-year. Panels B and C of the table are identical to panel A except that state-level random effects or region dummies are added. For simplicity, we present only the coefficients on the state policy variables. The other controls appear plausibly estimated; full results are available on request from the authors.

Column 1 presents the "reduced-form" results. The fraction of drinking drivers in fatal crashes is generally negatively related to the beer tax, punishment measures, and police per capita across all three specifications, although in most instances the parameter estimates are not statistically significant. The magnitudes of the policy coefficients are generally small. A one-standard-deviation increase in the state beer tax reduces the fraction of drinking drivers in fatal crashes by 0.2 percentage points (the mean fraction of drinking drivers in fatal crashes is approximately 55 percent). A one-standard-deviation increase in punishments for both first- and third-time offenders lowers the proportion of drinking drivers in fatal crashes by roughly one percentage point in the first two specifications but has no impact when region fixed effects are included. A one-standard-deviation increase in police per capita has the largest impact: approximately a three- to six-percentage-point decline.

The other columns of table 8, however, reveal a much richer behavioral story. Columns 2 and 3 provide estimates of the impact of public policies on how dangerous drinking drivers are. Interestingly, higher beer taxes and tougher punishments for first-time offenders are generally associated with the greater danger posed by drinking drivers on average (although sometimes with only weak statistical significance). These estimates are consistent with a scenario in which such policies have the greatest impact on the least dangerous/most marginal drinking drivers. If these individuals leave the pool of drinking drivers, the average riskiness of the remaining drinking drivers increases. Supporting this interpretation are the coefficients in column 4 corresponding to the estimated fraction of drinking drivers on the roads. Both the beer tax and first-offense severity are negatively related to the number of drinking drivers. As the reduced-form estimates demonstrate, however, removing these relatively benign drinking drivers from the roads has only a small impact on overall fatalities.

TABLE 8
RELATIONSHIP BETWEEN STATE POLICIES, THE NUMBER OF DRINKING DRIVERS, AND THEIR RELATIVE CRASH RISK

| | WEIGHTED LEAST SQUARES REGRESSIONS | | | |
|---|---|---|---|---|
| VARIABLE | Fraction of Drinking Drivers in Fatal Crashes (1) | Relative Fatal Crash Risk for Drinking Drivers: Two-Car Crashes ($\theta$) (2) | Relative Fatal Crash Risk for Drinking Drivers: One-Car Crashes ($\lambda$) (3) | Fraction of Drivers Who Have Been Drinking (4) |
| | A. Weighted Least Squares | | | |
| Beer tax | −.00008 | .101 | .105 | −.00049 |
| | (.00028) | (.077) | (.079) | (.00016) |
| First-offense punishment | −.007 | 3.23 | 3.43 | −.022 |
| | (.015) | (1.83) | (1.93) | (.012) |
| Third-offense punishment | −.007 | −4.65 | −5.28 | .023 |
| | (.015) | (1.93) | (2.00) | (.012) |
| Police per capita | −.021 | −2.68 | −1.84 | .008 |
| | (.015) | (1.37) | (1.18) | (.010) |
| | B. State Random Effects Included | | | |
| Beer tax | −.00012 | .321 | .329 | −.00028 |
| | (.00023) | (.054) | (.081) | (.00028) |
| First-offense punishment | −.002 | 5.71 | 3.65 | −.002 |
| | (.008) | (2.79) | (4.13) | (.013) |
| Third-offense punishment | −.014 | −8.97 | −10.06 | .009 |
| | (.007) | (2.37) | (3.48) | (.011) |
| Police per capita | −.018 | −1.11 | −0.57 | −.003 |
| | (.010) | (3.01) | (4.54) | (.015) |
| | C. Region Fixed Effects Included | | | |
| Beer tax | −.00008 | .091 | .117 | −.00039 |
| | (.00029) | (.105) | (.105) | (.00022) |

| | | | | |
|---|---|---|---|---|
| First-offense punishment | −.008 | 4.67 | 4.50 | −.037 |
| | (.015) | (1.98) | (2.22) | (.019) |
| Third-offense punishment | .007 | −4.89 | −5.59 | .034 |
| | (.014) | (2.17) | (2.31) | (.013) |
| Police per capita | −.046 | −2.08 | −1.42 | −.002 |
| | (.015) | (1.46) | (1.57) | (.011) |

Note.—The dependent variable is listed at the top of each column. The unit of observation is a state-year pair. With the exception of col. 1, the dependent variables are estimates from the model of Sec. IV. In addition to the four right-hand-side variables listed in the table, all regressions also include the fraction of the state population aged 15–24, 25–54, and over 55, percentage metropolitan, percentage black, state unemployment rates, and year dummies. In panels A and C, estimation is done with weighted least squares, with weights corresponding to the number of two-car fatal crashes in the state-year. In panels A and C, reported standard errors are correct for within-state, across-time correlation of the error term. The number of observations is equal to 381 in all cases. The means (standard deviations) of the policy variables are as follows: beer tax: 46.3 (34.4); first-offense punishment: .69 (.61); third-offense punishment: 1.05 (.75); police per capita: 3.00 (1.50). The means and standard deviations of the dependent variables are as follows: fraction of drinking drivers in fatal crashes: .55 (.08); relative fatal crash risk for drinking drivers in two-car crashes: 10.6 (15.9); relative fatal crash risk for drinking drivers in one-car crashes: 10.4 (19.8); and fraction of drivers who have been drinking: .20 (.10).

The coefficients on third-offense punishments reveal a very different pattern. These penalties would apply only to a relatively small fraction of hard-core drunk drivers.[29] As one would expect, the primary impact of harsh penalties for repeat offenders is to reduce the average risk posed by drinking drivers. Evaluated at the sample mean, a standard deviation change in the harshness of third-offense punishments lowers the predicted riskiness of drinking drivers in a state from eight times as risky for two-car crashes to approximately five times as risky. A similar magnitude of decline is observed in one-car crash risks of drinking drivers. From these estimates alone, it is impossible to know whether the decrease in drinking driver risk with harsher laws is due to a reduction in the frequency of drinking and driving among repeat offenders or whether the laws lead such individuals to drive with greater care when intoxicated. The fact that the estimated fraction of drivers that have been drinking does not fall with harsh laws for repeat offenders provides some circumstantial evidence in favor of the hypothesis that such laws do not keep hard-core drunk drivers off the roads, but rather cause them to drive more carefully to avoid detection. The coefficients on the police per capita variable further substantiate this claim. More police are associated with drinking drivers' appearing less risky, but do not systematically influence the number of drinking drivers.

## VI. Conclusions

This paper presents a new methodology for measuring the relative risk of drinking drivers. Unlike previous approaches, our strategy requires only data on the observed number of fatal crashes. Although a priori it would seem impossible to separately identify relative crash risk from the fraction of drivers on the road who have been drinking using such limited data, we demonstrate that there is a hidden richness in the information contained in two-car crashes. As a consequence, with only the most skeletal assumptions we are able to identify the key parameters of interest. Drinking drivers (including those not legally drunk) are at least seven times more likely to cause fatal crashes than sober drivers, although sensitivity tests suggest that this lower bound could nontrivially understate the true value. For drivers with BACs greater than 0.10, that ratio is at least 13 to one. A great majority of the victims of drinking drivers are the drivers themselves and their passengers. We estimate that of the 12,000 people killed in alcohol-involved fatal crashes in 1994, only 3,000 are accurately classified as externalities. The implied Pigouvian tax per mile driven by drinking miles is about 16 cents. For legally

[29] Harsh punishments for repeat offenders may also indirectly affect the behavior of forward-looking drivers with clean records.

drunk drivers the tax should be about 30 cents. Given current rates of enforcement for drunk driving, the appropriate loss per drunk driving arrest is approximately $8,000.

The methodology we present provides a simple, flexible, inexpensive tool for analyzing driver risk. Unlike past survey methods that are expensive to administer and subject to important nonparticipation biases, our approach is easily implemented using only data on fatal accidents that are readily available. Our method can be applied to any driver characteristic, any localized geographic area, and any time period. The variety of specifications presented suggests that the method is robust.

The estimates we obtain can be used to inform public policy debates on road safety. In contrast to reduced-form estimates, which can reveal only what policies have worked in the past, the more structural approach we adopt may suggest better policies for the future that take into account behavioral responses of drinking drivers. For instance, random roadblocks work solely on the margin of reducing the number of drinking drivers, ignoring the possibility that the care such drivers take might be affected. Our results suggest that policies focused on stopping erratic drivers with greater frequency might be more successful. For instance, rewards could be provided to motorists who use cellular phones to help police identify reckless drivers, or dedicated police patrols could be created whose only responsibility is the identification of drunk drivers. Employing the latter strategy, a pilot program in Stockton, California, reduced involvement in drunk driving crashes by 10–15 percent by dedicating the equivalent of four full-time-equivalent police officers exclusively to drunk driving patrols (Voas and Hause 1987).

More generally, our approach to addressing the issue of drinking and driving may provide insight to researchers on topics far afield. By exploiting the hidden richness of the data arising from interactions between participants, we are able to identify relationships in the data that may have seemed beyond reach ex ante. Variations on our approach may be useful in examining other issues in which agents interact, for example, search models, the transmission of AIDS, oligopoly pricing, and the relationship between special-interest groups and politicians. The parallel between our model and these more complicated economic settings is not exact because the random matching in our traffic model is less plausible in other cases (e.g., Flinn and Heckman 1982). Nonetheless, with the adoption of an appropriate alternative matching model, extensions of this model may prove relevant.

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