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RAISING THE ALCOHOL PURCHASE AGE: ITS EFFECTS ON FATAL MOTOR VEHICLE CRASHES IN TWENTY-SIX STATES

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BETWEEN 1970 and 1975, over half the states passed legislation reducing the minimum age for the purchase of alcoholic beverages. The minimum purchase age had been twenty-one in most of these states; it was reduced to eighteen, nineteen, or twenty—in most cases to eighteen. All ten Canadian provinces also reduced the legal age for the purchase of alcoholic beverages. Research studies in both countries have indicated that these changes increased fatal crash involvement among drivers under age twenty-one.[1,2,3,4]

Since 1976, there has been a trend toward raising the alcohol purchase age. By the end of 1980, fourteen states that had lowered the minimum age in the early 1970s had raised it again, although not always back to the previous level. Many studies conducted in the early 1980s found that raising the purchase age decreased driver fatal crash involvement in the

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[1] Richard L. Douglass & Lyle D. Filkins, The Effect of the Lower Legal Drinking Ages on Youth Crash Involvement (Final Summary Report, Nat'l Highway Traffic Safety Admin. 1974).

[2] Allan F. Williams *et al.*, The Legal Minimum Drinking Age and Fatal Motor Vehicle Crashes, 4 J. Legal Stud. 219 (1975).

[3] Philip J. Cook & George Tauchen, The Effect of Minimum Drinking Age Legislation on Youthful Auto Fatalities, 1970–1977, 13 J. Legal Stud. 169 (1984).

[4] Reginald G. Smart & Michael S. Goodstadt, Effects of Reducing the Legal Alcohol-purchasing Age on Drinking and Drinking Problems: A Review of Empirical Studies, 38 J. Stud. on Alcohol 1313 (1977).

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affected age groups.[5,6,7,8] Several of these studies were based on the experience of only one state. One study assessed the experience of nine states with increased purchase ages and reported reductions in nighttime fatal crashes among youthful drivers.[9] In that study, the average fatality reduction based on all nine states was 28 percent.

The trend of raising the legal minimum alcohol purchase age has continued to the present. In 1984 a federal law was enacted withholding 5 percent of highway aid from states not having a minimum alcohol purchase age of twenty-one for all alcoholic beverages by October 1, 1986; 10 percent of federal funds would be withheld from states not having a minimum purchase age of twenty-one the following year. This federal initiative has prompted many states to pass additional age change legislation.

The nine-state study conducted in 1981 looked, in most cases, at the early effects of the purchase age law changes. Five of the nine states studied were in the second year of their new law; one was in the first year. The present study was undertaken to assess longer-term effects of raising the alcohol purchase age. It has the advantage of including the experience of additional states that have enacted such legislation. The data available for this study include the years 1975–84, and it was possible to study twenty-six states that changed their laws during this period.

The present study also investigated the effect of the legislation on "beginning" drinkers of different ages. For example, in a state that raises its age from eighteen to nineteen, nineteen-year-olds (after a one-year time lag) will be "beginning" drinkers because they will never have been allowed to purchase alcohol legally prior to age nineteen. In contrast, in states with a minimum age of eighteen, the eighteen-year-olds are the "beginning" drinkers, as the nineteen-year-olds will have been allowed to purchase alcohol for one year. It has been suggested that "beginning" drinkers, whatever their age, are a problem, so that raising the purchase age for alcohol merely postpones their higher driver fatality experience,

[5] Allan F. Williams *et al.*, The Effect of Raising the Legal Minimum Drinking Age on Involvement in Fatal Crashes, 12 J. Legal Stud. 169 (1983).

[6] A. C. Wagenaar, Effects of an Increase in the Legal Minimum Drinking Age, 2 J. Pub. Health Pol'y 206 (1981).

[7] Robert P. Lillis, Timothy P. Williams, & William R. Williford, The Impact of the 19-Year-Old Drinking Age in New York, in Control Issues in Alcohol Abuse Prevention: Strategies for States and Communities (H. Holder ed., in press).

[8] P. M. Maxwell, Impact Analysis of the Raised Legal Drinking Age in Illinois (Nat'l Highway Traffic Safety Admin. 1981).

[9] Williams *et al.*, *supra* note 5.

which negates some or all of the effects of raising the purchase age.[10] However, we believe that the evidence presented thus far in support of this hypothesis is inadequate.[11] It could be argued instead that "beginning" drinkers and even older drivers will be positively affected by raising the legal purchase age because they may drink less than those who have had more prior years of opportunity to purchase alcohol. Also, it is possible that those younger than the affected ages could be positively affected by raising the purchase age because their access to alcohol through their slightly older peers may be reduced.

METHODS

The study was based on drivers aged sixteen through twenty-four who were in crashes in which someone was killed during the years 1975–84 in the forty-eight states that constitute the continental United States. The data were extracted from the Fatal Accident Reporting System (FARS), a computerized data base maintained by the National Highway Traffic Safety Administration. Crashes in which a motorcyclist was killed and crashes involving more than three motor vehicles were excluded. Also excluded were drivers not residing in the state in which the crash occurred. Our results are based on a total of 159,262 driver fatal crash involvements. Population estimates for each state were obtained from the U.S. Bureau of the Census for each of the nine ages, sixteen through twenty-four, for each of the calendar years 1975–84. These were used to control for population-related changes in fatal crashes.

The state-age-year combinations were categorized according to whether alcoholic beverages could legally be purchased; proportional adjustments were made if the minimum legal age was changed during the calendar year. Table 1 indicates the twenty-six states studied, the effective dates of the legal age changes (age changes for short), and the ages and alcoholic beverages affected. By the end of 1984, changes in the minimum purchase age had been in effect for more than two years in nineteen of the twenty-six states and for more than four years in fourteen states.

The new laws in some states contained "grandfather" clauses exempting those who already had reached the prior legal minimum purchase age at the time of the effective date. For example, a law might raise the age from eighteen to twenty-one on January 1, 1980, but exempt those per-

[10] Mike A. Males, The Minimum Purchase Age for Alcohol and Young-Driver Fatal Crashes: A Long-Term View, 15 J. Legal Stud. 181 (1986).

[11] Allan F. Williams, Comment on Males, 15 J. Legal Stud. 213 (1986).

TABLE 1

STATES CHANGING THE LEGAL MINIMUM ALCOHOL PURCHASE AGE, 1975–84

State	Alcohol Purchase Age Changes (From → To)	Effective Date
Connecticut	18 → 19	July 1, 1982
	19 → 20	October 1, 1983
Delaware	20 → 21*	January 1, 1984
Florida	18 → 19	October 1, 1980
Georgia	18 → 19	September 1, 1980
Illinois†	19 → 21	January 1, 1980
Iowa	18 → 19*	July 1, 1978
Maine	18 → 20	October 24, 1977
Maryland†	18 → 21*	July 1, 1982
Massachusetts	18 → 20	April 16, 1979
Michigan	18 → 21	December 21, 1978
Minnesota	18 → 19*	September 1, 1976
Montana	18 → 19	January 1, 1979
Nebraska	19 → 20*	July 19, 1980
New Hampshire	18 → 20	May 24, 1979
New Jersey	18 → 19*	January 2, 1980
	19 → 21*	January 1, 1983
New York	18 → 19	December 4, 1982
North Carolina†	18 → 19	October 1, 1983
Ohio‡	18 → 19	August 19, 1982
Oklahoma‡	18 → 21	September 22, 1983
Rhode Island	18 → 19	July 1, 1980
	19 → 20	July 1, 1981
	20 → 21	July 1, 1984
South Carolina†	18 → 19	January 1, 1984
South Dakota‡	18 → 19	July 1, 1984
Tennessee	18 → 19	June 1, 1979
	19 → 21*	August 1, 1984
Texas	18 → 19	September 1, 1981
Virginia§	18 → 19	July 1, 1983
West Virginia	18 → 19*	July 1, 1983

NOTE.—The law changes apply to all alcoholic beverages except where noted. Alabama changed the minimum alcohol purchase age from twenty-one to nineteen on July 22, 1975. Wisconsin raised the purchase age from eighteen to nineteen on July 1, 1984, but grandfathered eighteen-year-olds, so their effective date did not occur in the 1975–84 period.

* Grandfather clause.

† Applied to beer and wine only.

‡ Applied to beer only.

§ Applied to on-premise beer purchase only.

sons already eighteen to twenty years old before that date. Such laws were treated as if the age change had occurred whenever half the population at any age was first prohibited from purchasing alcohol. Thus, in the above example, the law would be classified as raising the purchase age from eighteen to nineteen on July 1, 1980, another raise from nineteen to twenty on July 1, 1981, and a third raise from twenty to twenty-one on July 1, 1982.

We devised methods of analysis to estimate the effects of the law changes on fatal crash involvement of law-affected drivers, while controlling for the effects of population and other age-related factors on fatalities. In summary, we compared changes in fatal crash involvement among affected drivers before and after the age changes with the experience of drivers not affected by the age change in those same states. The fatal crash involvements among affected drivers were also compared with those of same-age and other-age drivers in states that did not change their laws in the years 1975–84. These comparisons were made for all forty-eight continental states and separately for twelve four-state geographical regions of the country. The states composing these regions are presented in Table 2. None of the four states in the Southwest region changed its purchase age during the study period, so the effect of age changes could not be estimated in this region.

Data accurately indicating whether drivers had been drinking prior to their crash, or their blood alcohol concentration (BAC), are not sufficiently available for all states. It is known, however, on the basis of fifteen states that report BACs of virtually all fatally injured drivers, that about half are legally intoxicated (BACs of 0.10 percent or greater).[12] Drivers fatally injured in nighttime (8:00 P.M.–5:00 A.M.) crashes are especially likely to have been drinking; about two-thirds have BACs of 0.10 percent or greater.[13] This subset of crashes was therefore given special attention in the present study. The results were also analyzed for each separate age and for male and female drivers.

The statistical analysis used produces regression coefficients that estimate the proportional reduction in driver fatal crash involvement rates associated with the prohibition of alcohol from drivers in particular state-age-year combinations. It also provides a quantitative measure of uncertainty for these coefficients. In a slightly modified version, the same method was also used to estimate the combined variation in the crash

[12] James C. Fell, Alcohol Involvement in Fatal Accidents, 1980–1984 (Nat'l Center for Statistics and Analysis, U.S. Dep't Transp. 1985).

[13] James C. Fell, Alcohol Involvement in United States Traffic Accidents: Where It Is Changing (Nat'l Center for Statistics and Analysis, U.S. Dep't Transp. 1983).

TABLE 2

TWELVE FOUR-STATE GEOGRAPHICAL REGIONS STUDIED

Region	States
Northwest	Washington, Oregon, Idaho, Montana
North Midwest	Wyoming, North Dakota, South Dakota, Nebraska
North Central	Minnesota, Iowa, Wisconsin, Illinois
North Mideast	Michigan, Indiana, Ohio, Pennsylvania
Northeast	New York, New Jersey, Connecticut, Rhode Island
New England	Vermont, New Hampshire, Maine, Massachusetts
Mideast	West Virginia, Virginia, Maryland, Delaware
Southeast	North Carolina, South Carolina, Georgia, Florida
East Central	Kentucky, Tennessee, Missouri, Arkansas
South Central	Alabama, Mississippi, Louisiana, Texas
West Central	Kansas, Oklahoma, Colorado, New Mexico
Southwest	Utah, Arizona, Nevada, California

experience of cohorts (ages seventeen through twenty-one) as a function of the number of years the cohort was permitted legal access to alcohol. The methods of analysis are described in Appendix A.

RESULTS

Based on the 87,153 nighttime driver fatal crash involvements that occurred during 1975–84, raising the minimum legal alcohol purchase age was estimated to produce a 13 percent reduction in nighttime driver fatal crash involvements. At the 95 percent confidence level, there were between 8 and 18 percent fewer nighttime driver fatal crash involvements than would otherwise have occurred for the state, age, and year combinations in which the legal right to purchase alcohol was removed (see Figure 1). Results are reported in the present study at the 95 percent confidence level. A change is statistically significant at the conventional level if the confidence interval excludes zero. The effect for daytime crashes, which involve alcohol much less often, was negligible (3 percent ± 6 percent). The effect of increasing the purchase age for both nighttime and daytime, based on 159,262 fatal driver crash involvements, was estimated to be 9 percent ± 4 percent.

Effects by Region

The results of the same analyses, when repeated for the eleven four-state regions in which one or more states changed their laws, were also positive for all fatal crash involvements and for nighttime involvements in ten regions. The estimated effects ranged from −4 percent to 27 percent for nighttime crashes, but there were large standard errors in most cases,

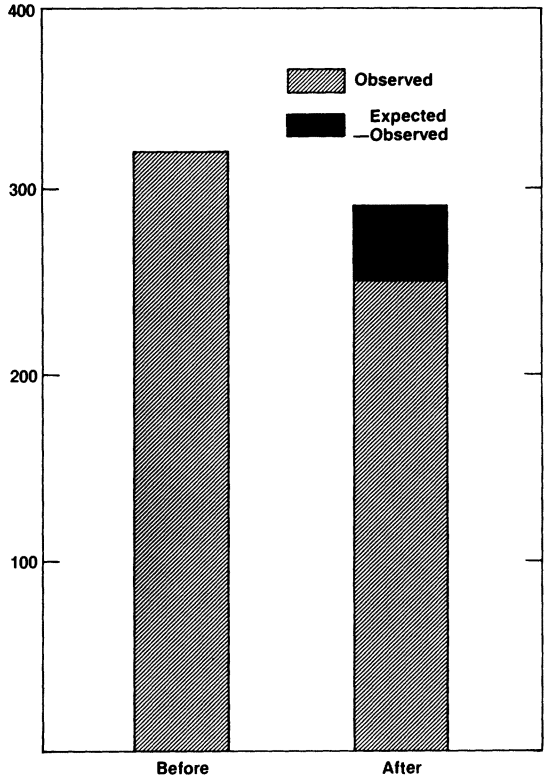

FIGURE 1.—Estimated nighttime fatal crash involvement rates per million person-years before and after increases in minimum legal purchase age.

introducing uncertainties. Considering these uncertainties, there is no evidence that the true effects of raising the purchase age vary by region, which suggests that the overall estimate is the best estimate available for every state.

Effects over Time

To address the important question whether the effects of age changes persist over time, a modified regression model was constructed to provide separate estimates of the relative effect of age changes, depending on the number of years the law had been in effect. In those states with several years' experience with the raised purchase age law, no significant differences in the effects of the age change were observed after the first years of the change. For example, using the sample of all nighttime driver crash involvements, raising the purchase age was estimated to reduce fatalities

13 percent during the first two years of a new law's taking effect and 12 percent during subsequent years. Similarly, there was no evidence of erosion in effects when comparing fatal crash experience after one year and after three years of the age changes.

Effects on "Beginning" Drinkers

To determine whether the first year of legal alcohol purchase, regardless of age, was especially hazardous, a variable was added to the regression model that represented the proportion of "beginning" drinkers ages nineteen through twenty-one (those reaching the age when they could first legally purchase alcohol) in the cells state, age, and year. The effect was negligible, 2 percent ± 6 percent.

Similar analyses were conducted to assess the effects of first-year drinking among nineteen-, twenty-, and twenty-one-year-olds separately. Estimated effects were −1 percent ± 8 percent at age nineteen, 14 percent ± nineteen percent at age twenty, and −8 percent ± 14 percent at age twenty-one. Finally, these analyses were rerun with the potential effects of first-year legal purchase restricted to only the law-change states. The results were equally nonsignificant.

Cohort Effects

The estimated change in the overall involvement in fatal crashes by five-year cohorts (ages seventeen through twenty-one) takes into account effects of the legislation on drivers of these ages, some of whom are directly affected by the law changes and some who are not. In this analysis, the response was a proportional increase or decrease in a crash involvement, while the regressor was the number of years of legal permission to purchase alcohol.

For nighttime crashes, the reduction of driver fatal crash involvement was estimated to be 5 percent ± 4 percent. This estimate implies that a single additional year of alcohol purchase is associated with an increase in fatal nighttime crash involvements of 1–9 percent over an entire five-year period. The 5 percent per year reduction in a cohort's experience in nighttime fatal crashes yields a somewhat larger result than the previously estimated 13 percent reduction based on single age-groups. That is, prorating the 13 percent reduction for a single age-group produces a crude estimate of an approximate 3 percent reduction for each cohort for each additional year of prohibition. The difference between the two estimates is not statistically significant; however, the possibility exists that the results of the cohort analysis indicate a positive spillover effect for drivers not affected by the age changes.

The corresponding estimated reduction for all crashes, including both daytime and nighttime involvements, is 4 percent ± 3 percent. The cohort analyses estimates have greater uncertainty associated with them because information is lost when the five years of data for each cohort are grouped together. In addition, not all the data available were usable because the earliest and latest cohorts could not be followed for the required five years.

Gender and Age

Most of the drivers involved in fatal crashes (81 percent in the present study) are male. However, the effect of raising the purchase age is proportionately greater for females. The estimates for nighttime fatal crashes were 10 percent ± 6 percent for males and 26 percent ± 11 percent for females. (See Appendix B for a discussion of the graphs of the adjusted crash counts by gender.)

The age analysis showed a lessening of the effect at age twenty. For nighttime fatal crashes, the estimated effects were 14 percent ± 6 percent for age eighteen, 15 percent ± 10 percent for age 19, and 1 percent ± 13 percent for age twenty. However, the uncertainties in these estimates are such that the differential effects by age may be statistical artifacts.

Reductions in Driver Fatal Crash Involvements

In the twenty-six states in which the alcohol purchase age was raised, there were forty state-by-age groups affected: twenty-three groups for age eighteen, eleven for age nineteen, and six for age twenty.[14] By cumulating the number of fatal crash involvements of these forty groups separately for the years preceding each age change and for the years following each age change, and using the estimates of the derived percentage reductions in fatal involvements, the numbers of fatal involvements prevented by the age changes were calculated. During the 1975–84 period, the age changes resulted in an estimated 586 fewer fatal involvements of eighteen- to twenty-year-old drivers in crashes (370 males, 216 females).

Discussion

The present study confirms the results of earlier work indicating that raising the legal minimum age for purchasing alcoholic beverages reduces fatal crash involvement among youthful drivers. The study was based on

[14] Because the analyses delayed the effective dates for law changes with "grandfather" clauses by six months, twenty-year-olds in Maryland and nineteen- and twenty-year-olds in Tennessee were treated as not affected by the changes.

a much larger number of states than the earlier work, and it clearly indicates that substantial reductions in fatal crashes occur as a result of the age changes and that the reductions that occur during initial years of the laws are undiminished over time. Larger relative reductions were found for females than for males. Some evidence was found in the present study that the major positive effects are achieved by raising the purchase age to twenty and that raising it from twenty to twenty-one has a smaller effect; however, the evidence is not conclusive.

The cohort analysis and the other analyses conducted indicated that the positive effects of the age changes are not negated by increases among those just attaining legal age to purchase alcohol in law-change states. The results of the cohort analysis took into account the experience of drivers ages seventeen through twenty-one. One possible interpretation of these results is that the law changes had the effect of reducing fatality rates not only for drivers whose legal ability to purchase alcohol is affected by the laws but for younger and older drivers (including "beginning" drinkers) as well.

The reductions in driver fatal crash involvement estimated in the present study are generally consistent with those found in previous work. For example, in the earlier study based on nine states, the reduction in nighttime fatal crash involvement was estimated to be 28 percent ($\pm$ 17 percent with 95 percent confidence),[15] whereas the estimate in the present study was 13 percent $\pm$ 5 percent. Similarly, Cook and Tauchen estimate the effect on all fatal crashes to be 7 percent $\pm$ 6 percent, and the present study estimates this to be 9 percent $\pm$ 4 percent.[16] The agreement with the Cook and Tauchen study is especially noteworthy because it covered the years 1970–77, which were largely outside the period of the present study and were years when the minimum alcohol purchase age was generally decreasing rather than increasing.

Overall, the results of the present study strongly indicate that raising the alcohol purchase age has had, and will continue to have, an important effect on reducing the fatal crash involvement of youthful drivers.

APPENDIX A

Statistical Analysis

Notation

The analyses depended on the deviations of driver involvement counts, N_{say}, from certain baseline counts, denoted by B_{say}. The following notation was used in the analyses:

[15] Williams *et al.*, *supra* note 2.

[16] Cook & Tauchen, *supra* note 3.

B = baseline frequency of driver fatal involvements;
N = observed number of driver fatal involvements;
s = state (forty-eight values);
a = age (nine values: 16, . . . , 24);
y = year (ten values: 1975, . . . , 1984);
Z = standardized driver fatal involvements;
$X1$ = fractional dummy variable for proportion allowed to purchase alcohol;
$X2$ = derived variable for relative population size (P = population size);
$X3$ = derived variable for age-by-year interaction; and
$X4$, $X5$ = estimated proportion of age-group first allowed to purchase alcohol.

Model

The present analysis uses the following model whose assumptions are:

i) The N_{say} have independent distributions with means λ_{say} for all $48 \times 9 \times 10 = 4{,}320$ combinations of (s,a,y).
ii) The means follow a loglinear model of the following form:

$$\log \lambda_{say} = \mu + \alpha_{sa} + \delta_{sy} + \beta_1 X1_{say} + \beta_2 X2_{say} + \beta_3 X3_{ay}.$$

iii) The variance of each N_{say} is proportional to its mean: for some constant κ^2, $V[N_{say}] = \kappa^2 \lambda_{say}$.

The variables $X1$, $X2$, and $X3$ measure prohibition, demographic shifts, and age-year interaction, respectively, as defined below. The parameters μ, $\{\alpha_{sa}\}$, $\{\delta_{sy}\}$, β_1, β_2, and β_3 are constants that need to be estimated from the data. If the counts N_{say} were distributed like Poisson variables, their variances would equal their means. To allow for the possibility of extraneous variation in the data, the parameter κ^2 is defined as in iii. The value $\kappa = 1$ corresponds to the assumption of Poisson-distributed crash frequencies, while larger values of κ would reflect a lack of fit to a Poisson model.

This model is similar in spirit to that of Cook and Tauchen,[17] who fit models of the form

$$\log(N_{sy}/P_{sy}) = \mu + \alpha_s + \delta_y + \beta_1 X1_{sy} + \text{error}$$

to data covering the years 1970–77. Their models did not include age as a factor since their counts N_{sy} were restricted to the small age range actually affected by the law changes. The more complex model used here is intended to protect against several potential sources of bias. For example, the present model uses the population data as a covariate rather than in the definition of the response rate. Because the census population variable is not necessarily a true measure of "drivers at risk" for each state, age, and year, it is safer to use it as a covariate, allowing its estimated coefficient to determine how much weight it gets in the analysis. This model integrates data for all ages sixteen through twenty-four, allows the state effects to vary by age, allows the year effects to vary by state, and incorporates an age-by-year interaction term.

This method will not be biased by statewide crash trends over time, even if these trends differ from state to state, because they are fit by the δ_{sy}. Similarly, no assumption is made about age effects, and they may differ from state to state, as

[17] *Id.*

estimated by the α_{sa}. All information about the effects of the covariates X is taken from the age-by-year interactions within each state. The model provides a way to partition this variation, pooled over all states, into four components: due to prohibition, measured by β_1; due to demographic shifts, measured by β_2; due to any national linear trend, measured by β_3; and a parameter representing the remaining variation, measured by the excess of κ over unity. The method is based on the standard statistical techniques of loglinear models, regression, and analysis of variance, so that standard ways of testing hypotheses, measuring goodness of fit, and assessing the uncertainty of coefficients are available.

Estimation

Cook and Tauchen's method of parameter estimation involved the use of weighted and unweighted regressions in which the forty-eight state effects and the eight year effects are estimated using dummy variables for each state and year.[18] This method is not computationally feasible for the present model because a total of 867 predictor variables, each involving 4,320 cases, would be needed to estimate all the parameters $\{\mu, \alpha_{sa}, \delta_{sy}, \beta_\kappa\}$. Similarly, the standard maximum-likelihood Poisson regression methods for estimating such parameters, as described, for example, in Bishop, Fienberg, and Holland,[19] when applied to the present data and statistical model, require computations that strain even a high-speed computer. Such methods are also lengthy to explain and make it difficult for others to repeat the analysis. Preliminary analyses made in the course of the present investigation did fit Poisson regression models similar to the above model by maximum-likelihood methods. But it was discovered that the estimates of the β's and their standard errors could be almost exactly duplicated by a two-step method, in which the simple model corresponding to setting the β's to zero is first fit and then the deviations of the data from this model are used in weighted regressions, enabling the estimation of the β's and of κ. A summary of the estimation procedure is provided in the following.

The model like i–iii above in which the β's are set to zero is easy to fit to the counts N_{say} because it is the model in which the effects of age and year are independent within each state. The fitted counts for this model will be called baseline driver involvement counts, defined as

$$B_{say} = N_{sa} + N_{s+y} \div N_{s++},$$

where subscript plus sign ($_+$) denotes summation over the missing indices. Next a standardized driver involvement count, Z_{say}, was defined as the relative deviation of N_{say} from B_{say}:

$$Z_{say} + (N_{say} - B_{say}) \div B_{say}.$$

The analyses of effects were based on treating Z_{say} as a response variable in weighted regression analyses using the values of B_{say} as weights. The regression models should be interpreted as if they were multiplicative models for the original counts, N_{say}. For example, because each Z is a relative deviation of the observed frequency, N, from a baseline, B, an increase in Z of .1 due to some factor is

[18] *Id.*

[19] Yvonne M. M. Bishop, Stephen E. Fienberg, & Paul W. Holland, Discrete Multivariate Analysis: Theory and Practice (1975).

interpreted as a 10 percent increase in N due to that factor. In fact, this is an approximation valid when Z is near zero. As discussed below, a more accurate expression for the percentage effect on N of an increase of Z from Z to Z' is $100(Z' - Z)/(1 + Z)$ percent.

From the definition of B_{say}, it can be seen that the Z_{say} satisfied the constraints

$$\Sigma_a B_{sa} + Z_{say} = 0,$$

for every s and y, and

$$\Sigma_y B_{s+y} Z_{say} = 0,$$

for every s and a. This fact was utilized in the computation of regression coefficients.

Weighted Analysis of Covariance

The regression model states that for some variables $X1, X2, \ldots$, which may depend on (s,a,y), the expectation of Z_{say} is

$$E\{Z_{say}\} = \beta_1 X1_{say} + \beta_2 X2_{say} + \ldots.$$

The analyses produce estimates and standard deviations for the parameters $\beta_1, \beta_2, \ldots$. Because of the constraints that the Z's obey, the analyses are technically three-way analyses of covariance. To estimate the β parameters by the usual regression formulas, it was necessary to replace each X by its residual from the three-way analysis of variance, weighted by the B's. That is, for each X, a U is defined by the formula

$$U_{say} = X_{say} - X_{sa.} - X_{s.y} + X_{s..},$$
$$X_{sa.} = (\Sigma_y B_{s+y} X_{say}) \div B_{s++},$$
$$X_{s.y} = (\Sigma_a B_{sa+} X_{say}) \div B_{s++},$$
$$X_{s..} = (\Sigma_a B_{sa+} X_{sa.}) \div B_{s++}.$$

Then the β's are estimated by the regression of Z on the U's.

In the case where there is only one regressor variable, the estimation of β is an especially simple computation:

$$\beta = (\Sigma B_{say} U_{say} Z_{say}) \div \Sigma B_{say} (U_{say})^2.$$

There is a small inaccuracy in interpreting β as the proportional decrease in expected N when $X1$ goes from one to zero (that is, prohibiting an age group from purchasing alcohol). Such an interpretation is strictly correct only if the expected value of Z is zero when $X1 = 1$. Suppose that the expected values of Z are Z_0 and Z_1 when $X1$ is zero and one, respectively. Then, using the formulas $E[Z] = \beta U$ and $N = B(1 + Z)$, the exact proportional change in $E[N]$ is

$$(Z_0 - Z_1)/(1 + Z_1) = -\beta/(1 + \beta U_1),$$

where U_1 is the value of $U = X1 - X1_{sa.} - X1_{s.y} + X1_{x..}$ corresponding to $X1 = 1$. In this report, the typical value of $U_1 = .5$ was used so that the effect of prohibition was always computed as: percentage reduction due to prohibition $= 100\,\beta/(1 + \beta/2)$ percent. Percentage reductions are reported as decimals, that is, $0.05 = 5$ percent.

Adjusting for Confounding Variables

The estimated effect of raising the alcohol purchase age could be due to some other cause that resulted in a relative drop in the counts of fatal crash involvements of age-affected drivers in the last decade. For example, the proportion of drivers under twenty-five years of age who are affected by the legislation may be falling, or some other trend may be producing a similar effect. To rule out these alternative explanations, adjustments were made for these different effects. This was done by creating two more variables and including them as covariates in the weighted analysis of covariances.

First, the U.S. census estimates P_{say} were standardized by a method similar to that used for N. The following expression was defined:

$$X2_{say} = (P_{say}P_{s++} \div P_{sa+}P_{s+y}) - 1.$$

Then, $X2$ is the relative difference between the population size at each age and at each year and the estimate of population size produced by a multiplicative model. For example, if $X2 = .1$ for nineteen-year-olds in 1982, then there are about 10 percent more nineteen-year-olds in the given state in 1982 than in the average year, compared with the other ages sixteen through twenty-four. They might be expected to be involved in roughly 10 percent more crashes than in the average year as well, irrespective of any changes in the drinking law. Therefore, if the variable $X2$ is used as a covariate in the regressions, its slope is expected to be near unity. In practice, the slope of $X2$ tends to be less than unity because the census data are only approximate, and, even if the data were exact, they would not provide a perfect proxy variable for the amount of driving performed by each age-group during each year. However, the use of $X2$ as a covariate does allow for a reasonable adjustment for the effects of shifts in the age distribution of each state's population over time.

Second, to allow for any other secular trends that might differentially affect the crash rate of drivers of different ages, another covariate was added to the model that explicitly allowed for an age-by-year interaction. For each age, a, and each year, y, the variable $X3$ was defined as

$$X3_{ay} = \frac{(a - 20)(y - 1979.5)}{100}.$$

For nighttime involvements, the coefficient of the relative deviation of the census data was estimated to be 0.67 with a standard error of 0.20. Thus a given increase of the Census Bureau's estimate of the proportional representation of an age-group in a given state tends to be accompanied by an increase in proportional crash involvement of just two-thirds as much. The coefficient of the age-by-year interaction variable is 0.38 with a standard error of 0.12. (The value of $X3$ ranges from -0.18 to 0.18.) Although this coefficient is significantly different from zero at the 0.1 percent level, the inclusion or omission of this covariate does not much influence the conclusions of the study regarding the effect of changes in the minimum purchase age law. Because this variable is highly correlated with the census population estimates in most states, dropping the age-by-year interaction variable tends to produce an increase in the coefficient of the census population variable without much of an effect on the estimate of the effect of prohibition. However, dropping both of the covariates does produce a serious bias in the purchase age coefficient, which rises to 0.21 in that case.

Another analysis attempted to discover whether the first year of being legally

allowed to purchase alcohol is especially hazardous. To test this hypothesis, a new predictor variable $X4$ was constructed that should be sensitive to such an effect. Let

$$X4_{say} = \max\{0, X1_{say} - X1_{s,a-1,y-1}\}.$$

Thus, $X4_{say}$ is approximately the proportion at age a, who can purchase alcohol during year y, but not during year $y - 1$, when they were a year younger. Therefore, if the first year of legal alcohol purchase is especially hazardous, the coefficient of $X4$ would be large. Variations of this approach were used in order to test modifications of this hypothesis. Whether the first year of being able to purchase alcohol is more dangerous only at certain ages was investigated by using new predictors defined, for example, as $X5_{say} = X4_{say}$, if $a = 19$ and $X5_{say} = 0$, otherwise, with correspondingly defined predictors focusing on other ages. Similarly, whether any effect of being a "beginning" drinker is concentrated in the law-change states was tested by constructing another predictor equal to $X4_{say}$ during all years y, after a law change in state s, and equal to zero otherwise. An alternative method for taking into account crash involvement by drivers not directly affected by the changes is provided by cohort analysis.

Measuring Goodness of Fit of the Model

According to assumption iii in the preceding, the variance of each N_{say} is approximately equal to $\kappa^2 B_{say}$ (actually, the model described above uses λ_{say} instead of B_{say}, but the more easily computed baseline values are close enough for our purposes), and the weighted sum of the squared residuals, defined by

$$\chi^2 = \Sigma B_{say}(Z_{say} - \beta_1 U1_{say} - \beta_2 U2_{say} - \ldots)^2,$$

would have expectation κ^2 df, where df is the degrees of freedom in the residuals of the weighted regression. If every state has at least one count for every age and for every year, then the degrees of freedom are defined as $48(9 - 1)(10 - 1) -$ (no. parameters β estimated), otherwise the value of df is reduced somewhat. Therefore, the value of χ^2/df is an estimate of κ^2.

This analysis has assumed that the ratio $\mathrm{var}[N_{say}]/E[N_{say}] = \kappa^2$ is about the same for all values of (s,a,y). This assumption can be checked by computing the weighted sum

$$\Sigma_{ay} B_{say}(Z_{say} - \beta_1 U1_{say} - \beta_2 U2_{say} - \ldots)^2$$

separately for each state. The resulting forty-eight values (one for each state) are all expected to be the same, within the limits of sampling error, if the variances of the N's are proportional to the B's. If the variance of the N's are equal to the B's, then the forty-eight subtotals should look like a sample of size forty-eight from a chi-squared distribution with $(9 - 1)(10 - 1) = 72$ df. When these calculations were performed using the counts of the 87,153 nighttime crash involvements, one of the state subtotals lies below the first percentile of the chi-squared (72 df) distribution, forty-six lie within the first and ninety-ninth percentiles, and one lies above the ninety-ninth percentile. A plot of the forty-eight subtotals versus the number of crashes recorded in each state shows no indication of a trend. The total over all forty-eight states is $\chi^2 = 3{,}607.3$, with 3,453 df (three β's were estimated), so that the estimate of κ^2 is $3{,}607.3/3{,}453 = 1.04$. These results support the assumption of very little variation besides that expected from the Poisson distribution. If the states are grouped into three sets of sixteen, with low, medium, and

TABLE A1

TEST FOR POISSON DISTRIBUTION OF CRASHES

Crash Frequency	χ^2	No. of Crash Involvements
Low	1,146.7	7,251
Medium	1,266.4	21,916
High	1,194.2	57,986
Total	3,607.3	87,153

high numbers of crash involvements over the study period, the contribution to χ^2 for each group is as presented in Table A1. The near equality of the three partial sums indicates that the use of the B's to weight the Z's is an effective adjustment for heteroscedasticity. Similar results were observed when these calculations were repeated using all crash involvements, rather than just nighttime involvements.

Discussion of the Method

The question arises whether this estimation procedure corresponds to using internal controls for the law-change states and, if not, just how the no-change or comparison states enter into this analysis. If the analysis were based on $X1$ alone, it would depend solely on the experience within the law-change states. This is because the adjusted variable, $U1$, has no variation ($U1_{say} = 0$) in a state that did not experience a law change. The only use of the no-change states was to allow a better estimate of the effects of $X2$ and $X3$, which are the proper adjustments to allow for the effects of variation of the age structure of the population and for any other age-related trends in crash experience.

When the data were analyzed separately by region of the country, all three coefficients were estimated for each block of four states. This analysis was very similar to a matched-pairs analysis, except that, instead of each block consisting of a pair of states (one with and one without a law change), each block consisted of four states (some with and some without a law change) with an integrated analysis of the effects of prohibition and of possibly confounding trends separately for each block.

Analyses by Cohorts

In a cohort analysis, the focus is not on the experience of drivers at particular state-age-year combinations but on the longitudinal history of drivers over several years. In this study, the cohorts were defined in such a way that, regardless of law changes, the youngest age allowed to purchase was always included. Although the same drivers cannot actually be followed over several years, this approach can be approximated by summing fatal crash involvement counts for approximately the same cohort of drivers. For example, the sum of counts for drivers age seventeen in 1980, age eighteen in 1981, age nineteen in 1982, age twenty in 1983, and age twenty-one in 1984 in a particular state is approximately the total of crash involvement during ages seventeen through twenty-one for a single cohort of drivers born in 1963 and residing in that state. (The exact date of birth and residential mobility of drivers were not known.) This total can be formed for all cohorts by varying

state and birth date. For each of the forty-eight states, there are six such cohorts corresponding to drivers who are aged seventeen in the years 1975–80.

This forty-eight-state by six-birth-year table of counts was adjusted for the marginal effects of confounding variables and then compared with each cohort's history of restrictions regarding alcohol purchase. The primary variable of interest is the number of years of legal permission to purchase alcohol that the cohort experienced during ages seventeen through twenty-one. This varies from one year, for cohorts with a constant minimum legal purchase age of twenty-one, to four years, for cohorts with a constant minimum legal purchase age of eighteen.

The cohort analyses were performed by pooling the values of the response variable Z and the covariate $X1$ in the age-year cells that were pooled for the construction of each cohort's experience. The values of $X2$ and $X3$ were not used in the cohort analysis, but other adjustments were made after the data were pooled. Within each region, the pooled values of Z and each X were adjusted for independent effects of state and birth-year.

APPENDIX B

Graphing Adjusted Crash Counts

A simple plot of crash rate versus proportion drinking over the 4,320 state-age-year cells will not illustrate the tendency for crash rates to go down when permis-

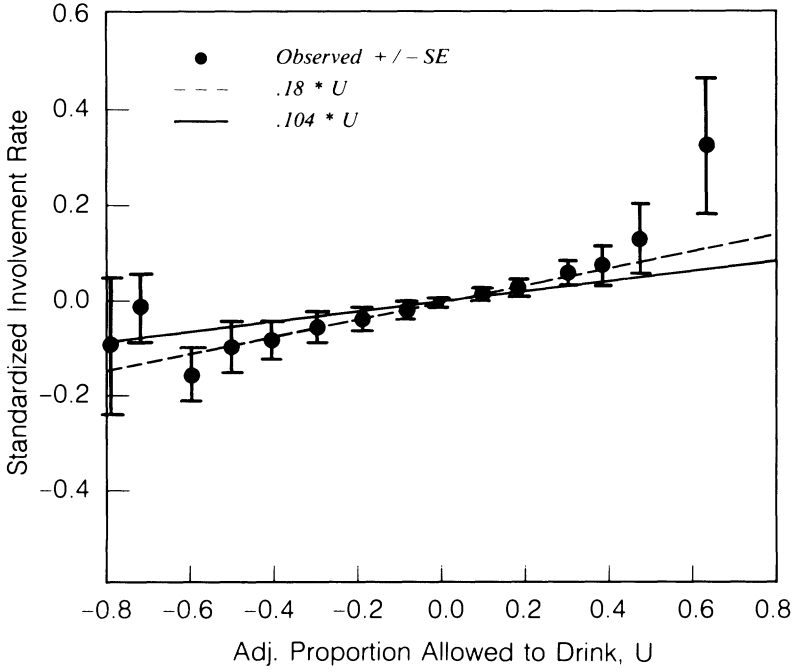

FIGURE B1.—Fatal involvements (nighttime crashes, males) versus proportion drinking. Dashed line = weighted least squares fit to the points. Solid line = relation derived after adjusting for the two additional covariates discussed in the text.

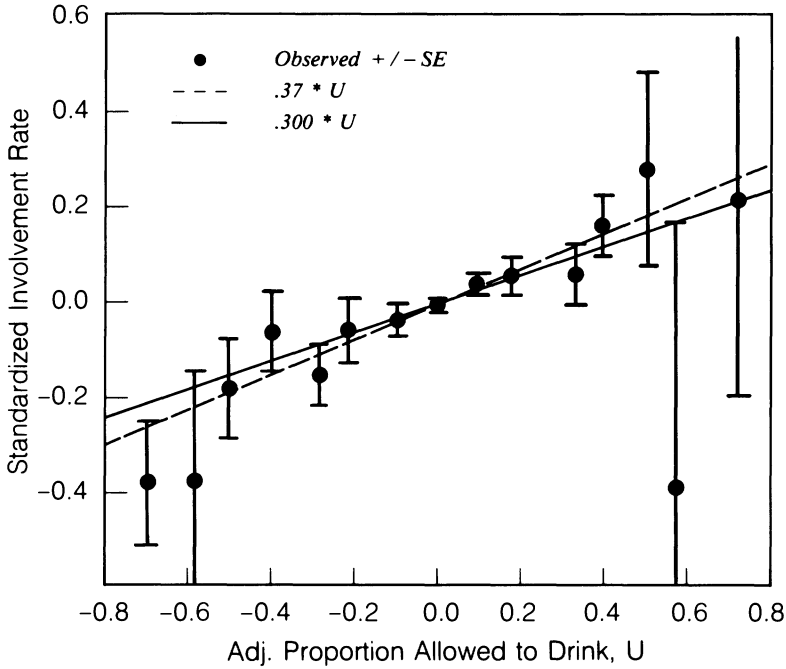

FIGURE B2.—Fatal involvements (nighttime crashes, females) versus proportion drinking

sion to drink is removed. The sample sizes are too small within each cell and the state, age, and year effects are so large that any trend in the graph would be obscured by the other effects. However, it is possible to remove the effects of age and year within each state by plotting the standardized involvement rate, Z, defined in Appendix A, on the vertical scale versus the adjusted proportion allowed to drink, $U_{say} = X1_{say} - X1_{sa.} - X1_{say} + X1_{s..}$, as defined in Appendix A. The 4,320 points (U_{say}, Z_{say}) are almost all based on too few crash counts to be reliable. Therefore, these points are grouped by values of U_{say} rounded to the nearest 0.1, and their corresponding values of Z_{say} are pooled (that is, Z pooled $= (\Sigma N - \Sigma B)/\Sigma B$), where the summation is taken over cells with nearly the same value of U_{say}. The resulting graphs of nighttime crash involvements are given for males in Figure B1 and for females in Figure B2. Vertical end bars are drawn on the graph representing $\pm$ one standard error for each of the pooled Z-values. A definite upward trend is obvious on each of the graphs; the dashed line is the weighted least squares fit to the points, and the solid line is the relationship derived after adjusting for the additional covariates.