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traffic accidents. Accident report forms in many states include an indication of the officer's judgement concerning whether or not the driver had been drinking at the time of the crash. Some states, for example Michigan, include a separate dichotomous forced-choice item on the statewide standardized accident report requiring the investigating officer to identify the driver as non-drinking or drinking. The data resulting from such an item is a reasonably good indicator of the involvement of alcohol in a crash. In contrast, other states, such as New York, have police officers identify factors contributing to the crash, selected from a list of possible factors, of which alcohol consumption is one of 40 possible choices. The resulting data are a less adequate indicator of alcohol-related crash involvement because, although alcohol use interacts with many other crash causes, an officer must select one factor as the primary cause of the crash. For example, an officer might select "driver fell asleep" or "unsafe speed" as factors contributing to a crash, rather than "alcohol involvement," when in fact heavy drinking may have been the prior cause of falling asleep or speeding.

To provide a consistent indicator of alcohol-related crashes for comparison across states, and to control for reliability and validity problems in police-reported alcohol-involvement in some states, an indirect indicator of alcohol-related crashes was also analyzed. The alternative indicator involved separate analyses of male drivers involved in single-vehicle nighttime crashes. Previous research has shown that a majority of single-vehicle nighttime male crashes involve drinking drivers (Douglass, 1974). Daytime crashes were used as an indirect indicator of non-alcohol-related crashes for comparison with single-vehicle nighttime male crashes.

4.2.2 Crash File Construction. Data collection efforts were aimed at the acquisition of complete crash records for all crash-involved drivers reported to police authorities in each of the four states between January 1972 and December 1979. A census of all reported crashes was successfully obtained for Maine, Michigan, and Pennsylvania. Files for the State of New York were subset prior to acquisition. Two subset files were obtained; the first included all crashes occurring between January 1975 and December 1979 in which alcohol was reported (by the investigating police officer) to have been a contributing factor; the second New York subset file contained all single-vehicle crashes involving male drivers, also for the 1975-1979 period. Thus, both indicators of alcohol-related crashes were available, but no indicators of the frequency of non-alcohol-related crashes were analyzed for the State of New York.

Variables used from the original crash data files acquired from the states included: (1) police officer's judgement concerning whether or not the driver had been drinking; (2) type of vehicle (i.e. automobile, pickup truck, motorcycle, heavy truck, etc.); (3) age of driver; (4) sex of driver; (5) time of day the crash occurred; (6) severity of the crash (i.e., seriousness of injuries and property damage resulting from the crash); (7) and number of vehicles involved in the crash. The operational definition of most of these variables, for example age/sex of driver and time of crash occurrence, is straightforward and comparable across states. However, there were some differences in the definition of type of vehicle, number of vehicles involved in the crash, and reported alcohol-involvement. The definitions desired for this investigation are briefly discussed here, and specific items in each state's datasets are discussed in sections 4.2.2.1 through 4.2.2.4.

The vehicle type variable was simply used to exclude a variety of miscellaneous traffic units included in comprehensive crash files. The goal was to include drivers of automobiles, pickup trucks, and motorcycles, but exclude from the dependent variables drivers of a variety of miscellaneous traffic units such as farm tractors, snowmobiles, busses, and heavy trucks.

A single-vehicle crash for the purposes of this study was defined as a crash involving one vehicle in transport. Thus, one moving vehicle striking an automobile stopped in traffic is not a single-vehicle crash, while one vehicle striking a parked vehicle is a single-vehicle crash. Moving vehicles striking pedestrians or bicyclists were also not considered single-vehicle crashes, since pedestrians and bicyclists are moving traffic units and frequently cause the crash.

The desired operational definition of police-reported drinking behavior was the officer's simple judgement whether or not the driver had been drinking at the time of the crash, not a much more complex judgement by the officer concerning the extent to which alcohol consumption was a primary or contributory cause of the crash. Furthermore, results of chemical tests for the presence of alcohol in a drivers body were not used as an indicator for the incidence of alcohol-related crashes because only a small fraction of all drinking drivers involved in crashes are chemically tested for alcohol.

The datasets acquired from the several states had a variety of dat structures and file formats, and therefore required different processin protocols. Details for each state follow.

4.2.2.1 Michigan. The State of Michigan, Department of Stat Police has routinely supplied The University of Michigan Highway Safet Research Institute (HSRI) with comprehensive crash data files sinc 1964. The data were originally formatted in a hierarchical structure and were reformatted into rectangular files, with one record for eac motor vehicle occupant, for use on the HSRI Automated Data Access an Analysis System (ADAAS). Because almost three quarters of a millic drivers are involved in reported motor vehicle crashes in Michigan eac year, 20% random samples of all crashes were selected for each year frc 1972 through 1977, for use in HSRI's earlier research on Michigan' legal drinking age. To reduce data processing costs, these availabl files were used for the Michigan baseline time period for the preser investigation. For 1978 and 1979, census files of all reported crashe in Michigan were constructed and used as the basis for the depender variables used in this study. Because the 1972-1977 files containe only 20% of all crashes, the resulting crash frequencies for these year were multiplied by five to make them comparable to the 1978 and 197 census data.

All of the Michigan datasets were filtered to include only driver of passenger cars, trucks, and motorcycles. Excluded from analysis wer the drivers of busses, farm and construction machinery, and othe miscellaneous vehicles.

The Michigan crash files included nominal missing data rates c such variables as police-reported drinking, age of driver, and type c vehicle. Missing data rates for the 1978 Michigan file are shown in Table 4.1.

TABLE 4.1
Missing Data Rates for State of Michigan: 1978

Variable	Percentage Missing
Police-reported Had Been Drinking	8.7%
Driver Sex	0.0%
Driver Age	2.9%
Vehicle Type	4.7%
Time of Crash	0.0%
Month of Crash	0.0%

Driver records with missing data on any of the variables required by the research design were excluded from analysis. As noted earlier, the drinking involvement item on the statewide Michigan accident report form requires the investigating officer to make a decision about drinking for every driver of every crash-involved vehicle. As a result, the missing data rate is low compared to other states. The exact phrasing of the "had been drinking" item on the Michigan accident report form is: "Driver had taken alcohol or drugs" or "Driver had not taken alcohol or drugs". "Unknown" is coded in the data files only when the investigating officer leaves this item blank.

4.2.2.2 Maine. The State of Maine, Department of Transportation provided state accident data in a hierarchical format which was reformatted for use on the HSRI system. Unfortunately, the original Maine files did not contain the necessary information to link specific driver records with the corresponding record containing information on the vehicle they were driving at the time of the crash. As a result, miscellaneous vehicles could not be filtered out, and the Maine data used in the time-series analyses included all types of vehicles recorded in the original files.

The condition-of-driver variable was coded using the following six categories: (1) Apparently Normal, (2) Had Been Drinking, (3) Under Influence-Liquor, (4) Under Influence-Drugs, (5) Asleep, and (6) Fatigued. Any driver identified and "had been drinking" or "under influence-liquor" was considered a drinking driver for this study.

No unusual missing data problems were identified for the variables required for this research (see Table 4.2). Those cases missing information or with code values undefined for any of the required variables were excluded from subsequent analyses.

TABLE 4.2
Missing Data Rates for State of Maine: 1979

Variable	Percentage Missing
Condition of Driver	0.0%
Driver Sex	0.6%
Driver Age	1.2%
Time of Crash	0.0%
Month of Crash	0.0%

4.2.2.3 New York. New York motor vehicle crash involvement data were obtained from the New York Division of Alcohol Abuse and Alcoholism. Two sets of files were obtained for the 1975 through 1979 period. The first contained all crash involved drivers for which the investigating police officer indicated that alcohol was a contributing causal factor in the crash. The second set of files included all single-vehicle crashes involving male drivers. As a result, time series of police-reported alcohol-related crashes and single-vehicle nighttime male crashes were analyzed, but the frequency of all daytime crashes or crashes with no police-reported drinking were not available. The frequency of daytime single-vehicle male crashes was used as an indicator of non-alcohol-related crashes.

The frequency of police-reported alcohol-related crashes was relatively low in these New York data. About 3% of all 1979 New York crash-involved drivers were coded with alcohol as an apparent factor in the accident, while about 9.5% of all 1979 Michigan crash-involved drivers were coded as "had been drinking." Part of the difference is due to the nature of the item used to code alcohol involvement. As noted earlier, in Michigan the question put to the officer filling out the accident report is simply whether the driver had been drinking at all. In New York the investigating officer must make the more complex judgement that alcohol involvement was a contributory cause of the crash. Alcohol-involvement is one of 40 possible causative factors from which the officer must select two which he/she believes are the most important causative factors in the crash. Frequently immediate causes such as "excess speed" or "failure to yield" are coded rather than the alcohol-impaired condition of the driver, which often is the underlying cause of immediate driver errors that result in crashes.

Information on the type of vehicle involved in reported crashes was not included in the data files provided by New York. As a result, all vehicle types were included in the final time-series variables. Inclusion of all vehicle types in the analyses was not expected to affect the results since over 92% of all crashed vehicles in New Yorl were automobiles, light trucks, or motorcycles (vehicle types which wer the focus of this investigation).

An important change in crash reporting procedures in New Yor affected interpretation of the data. In September 1978 the minimu dollar amount of property damage for mandatory crash reporting wa increased from $250 to $400. As a result, the number of recorde property damage crashes decreased.

Missing data rates for the New York crash variables are shown i Figure 4.3. Note that the lack of any missing data on the drinkin variable was due to the nature of the variable. The choice "None (i.e., no apparent causative factors) precludes the need for a missin data code. About 65% of all crash-involved drivers in New York had n apparent causal factor recorded.

TABLE 4.3
Missing Data Rates for State of New York: 1979

Variable	Percentage Missing
Police-reported Alcohol as a Causative Factor	0.0%
Driver Sex	4.5%
Driver Age	4.5%
Time of Crash	0.0%
Month of Crash	0.0%

Finally, since New York City is an unusual crash reportin jurisdiction, the final time-series variables were constructed in tw sets. The first set of variables consisted of crashes occurring in th entire state, as was done for the other three states. The second set c variables were constructed excluding all crash reports from New York City.

4.2.2.4 <u>Pennsylvania</u>. A census of all reported motor vehicle accidents in the state of Pennsylvania from January 1972 through December 1979 was obtained from the Pennsylvania Department of Transportation. An important characteristic of the Pennsylvania data was changes in the criteria for reporting a crash which occurred between 1972 and 1979. Prior to June 30, 1977, local police departments were not required to use a statewide standardized accident report form, nor were they required to submit the reports to the State Department of Transportation. Beginning July 1, 1977, a uniform statewide reporting form was adopted and local police departments were required to submit these reports to the Pennsylvania Department of Transportation. At the same time the minimum reporting criteria for drivers involved in an accident also changed. Before June 30, 1977, drivers involved in any crash resulting in damage amounting to $200 or more had to report the crash to the local police department. After this date only accidents resulting in a vehicle being towed from the scene of the crash required reporting. Personal injury accidents always required reporting, both before and after these changes.

As a result of these data collection system changes, the frequency of reported injury crashes increased (all injury crashes were now reported to the state office on standard forms), and the frequency of reported property damage crashes decreased substantially (because property damage crashes were only reported if a vehicle was towed from the scene). Such effects of reporting system changes were controlled in the time-series analyses reported in Chapter 5.

Since the data received from Pennsylvania contained all reported accidents, the data were subset to include only drivers of passenger cars, light trucks (pickups, vans, etc.), and motorcycles. Due to incompatibilities between computer systems approximately 5 crashes per year were deleted from analyses. Since this very small data loss was a random occurrence it was not expected to affect the results.

As was the case in the other states, rates of missing data for Pennsylvania were quite low; the rates for calendar 1979 are shown in Table 4.4.

TABLE 4.4
Missing Data Rates for State of Pennsylvania: 1979

Variable	Percentage Missing
Alcohol as Causative Factor	0.6%
Driver Sex	1.3%
Driver Age	4.6%
Vehicle Type	3.6%
Time of Crash	0.7%
Month of Crash	0.0%

The police-reported drinking variable in Pennsylvania was coded much like New York. The investigating officer must identify alcohol as a causative factor in the accident, selected from a list of 91 possible causes. As a result, the percentage of all crashes reported as alcohol-involved was very low (2.6% in 1979). Also, note that the alcohol item in the Pennsylvania data indicated that the officer judged alcohol to be a causal factor in the accident; this alcohol item could not be linked to a particular driver involved in the accident. As a result, time-series were constructed of the frequency of Pennsylvania drivers involved in alcohol-related accidents, not the frequency of drinking drivers involved in crashes, as was done for the other three states in the present investigation.

4.2.3 Beverage Sales and Beverage Control Law Enforcement. In addition to the primary emphasis on traffic crash dependent variables, additional analyses were conducted of aggregate alcoholic beverage sales and data on the enforcement of the higher drinking age. Data on aggregate monthly beer, wine, and distilled spirits wholesale distribution in the State of Maine were obtained from the Maine Bureau of Alcoholic Beverages. The figures for monthly wholesale distribution of wine, draft beer, and package beer in the State of Michigan were obtained from the Michigan Beer and Wine Wholesalers Association. Data on wholesale monthly draft and package beer distribution in the State of New Hampshire and the United States as a whole, used for comparison with the Maine and Michigan results, were obtained from the U. S. Brewers Association. In addition to the analyses of aggregate beverage sales, annual frequencies of citations for selling or allowing minors to consume alcoholic beverages were briefly examined. Annual frequencies of such citations brought before the Administrative Court in the State of Maine from 1971 through 1979 were provided by the Maine Department of Public Safety, Bureau of Liquor Enforcement. Similar data for the State of Michigan for the period from 1970 through 1980 were provided by the Michigan Liquor Control Commission.

4.3 Research Design Validity

There are numerous potential threats to the validity of conclusions reached in any research. These can be categorized in a number of ways, the most frequent being the dichotomization of internal and external validity originally presented by Campbell and Stanley (1966). However, the present discussion is structured after the more comprehensive discussion of validity presented in Cook and Campbell's (1979) recent volume. Cook and Campbell present four major categories of research design validity: (1) statistical conclusion validity, (2) internal validity, (3) construct validity, and (4) external validity.

4.3.1 Statistical Conclusion Validity. Statistical conclusion validity is concerned with the possibility that random error and/or the inappropriate use of statistical tests may invalidate research conclusions. Statistical conclusion validity is essential to establish that there is in fact a covariation between the operationalizations of the concepts under investigation. Since covariation is the most basic prerequisite for establishing a causal relationship, one must first establish a valid covariation or statistical relationship prior to conducting a causal analysis.

There are a variety of threats to statistical conclusion validity. First, inadequate power of the statistical tests used may invalidate one's conclusion that no covariation is present. This threat to statistical conclusion validity was minimized by a number of design features in the present investigation. Since there is a direct relationship between sample size and power, a large number of observations over an extended period of time surrounding the intervention point were used in estimating the statistical relationships. Power was also increased by refraining from the use of very low levels of Type I error probability as the criterion for a statistically significant relationship, since power is directly related to the level of Type I error probability chosen.

Statistical conclusion validity was strengthened by the use of the most sensitive statistical methods available that could be appropriately applied to the data. For this reason, the present study was designed to meet the requirements of the recently developed Box-Jenkins transfer function methods (Box and Tiao, 1975; Box and Jenkins, 1976).

Finally, statistical conclusion validity can often be substantially increased by explicitly taking into account in the data analyses as many systematic components of the total variance in the dependent measures as possible, and thus reducing the error variance. As is discussed in Section 4.4 on the data analysis methods, extensive effort was expended to identify systematic components of the total variance in each dependent time series prior to an assessment of the statistical significance or magnitude of drinking age effects.

A second threat to statistical conclusion validity is the violation of the assumptions of the procedures used. This threat to validity is minimized by explicitly noting the assumptions accompanying the statistical procedures, the robustness of the procedures to a violation of those assumptions, and an assessment of the extent to which the assumptions are violated. Further discussion of the assumptions underlying the procedures used in this investigation, and an analysis of the extent to which the assumptions were met, can be found in Section 4.4.

A third threat to statistical conclusion validity is the analysis of multiple tests. Examining multiple tests increases the probability of making a Type I error; that is, it increases the probability of falsely concluding that covariation exists.[10] This threat to validity can be avoided either by explicitly making adjustments in the critical significance levels to account for the number of tests conducted (for example, using Bonferroni multiple t-tests; Dunn and Clark, 1974), or by concluding that true covariation exists only on the basis of a pattern of results rather than on the basis of one or two "significant" findings among a large number of tests conducted. In the present investigation, conclusions were made on the basis of the pattern of results over a number of tests, rather than one or two isolated statistically significant results. Furthermore, the significance criterion chosen for this study was the more conservative .01 probability level, rather than .05.

A low level of reliability in the measures constitutes a fourth threat to statistical conclusion validity. The result of low levels of reliability is an inflation of standard errors and a consequent reduction in the ability to detect covariations that may exist. In other words, low reliability reduces the power of the statistical procedures. The main control over this threat in the present study was the use of aggregate outcome measures, rather than measures based on particular drivers, accidents, or data collection sub-systems (such as a single community or county). The impact of random irregularities over time in the data collection systems of particular local jurisdictions was decreased when the data were aggregated at the state level. The result of statewide aggregation was a reduction in the effect of numerous random measurement errors occurring at the local level;

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[10] For example, if one sets the critical significance level at .05, one would expect to find five "significant" results in any 100 tests conducted, simply as a result of chance.

consequently, systematic patterns in the series were more easily discernable in the aggregated data.

A fifth threat to statistical conclusion validity identified by Cook and Campbell (1979:44) is "random irrelevancies in the experimental setting," that is, the random error in the observations due to all of the other influences upon the frequency of accidents that are not explicitly brought into the analyses.[11] It should be noted that a large number of other causes of crash frequency, although not explicitly identified, were controlled in the analyses by specification of systematic trend, seasonal, and other autocorrelation components in the dependent variables. In addition to the components of the series that reflect causal influences, part of the random component in each series is due to other omitted causal influences. The differential operation of these other factors across jurisdictions is suppressed by using aggregate data across a large number of jurisdictions. As in any research, there always remains, however, a random component due to omitted causes of the phenomenon under study. This random error over time, along with other random error due to measurement error, provides the basis for an assessment of the statistical significance of the effects of the legal interventions.[12]

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[11] Major exogenous factors with known effects on reported frequency of crashes include the fuel shortage and national maximum legal speed limit reduction of early 1974, and changes in criteria for reporting crashes. Known effects of such factors were explicitly controlled in the time-series models before assessing the drinking age impact.

[12] The model of statistical inference used in this research was an econometric or time-series model, where statistical significance of an intervention parameter is assessed by comparing its size with the size of the total random component in the dependent variable. The purpose of statistical inference was to separate the systematic effects from the random component, not generalizations to a specified population. For additional discussion of this issue, see Berk and Brewer (1978).

4.3.2 Internal Validity. After a high degree of statistical conclusion validity has been achieved, that is, after the existence of covariation between operationalizations of the concepts of interest has been established, the question as to whether the covariation is plausibly indicative of a truly causal relationship has to be addressed. Establishing the causal nature of observed covariations between operationalizations is the domain of internal validity. There are a large number of potential threats to the internal validity of an investigation and each threat should be explicitly considered and ruled out as a plausible explanation of the observed covariation. Through the successive ruling out of potential alternative explanations for observed covariations, one's confidence in inferring a causal relationship on the basis of the observed covariation is strengthened. Although from an epistemological point of view one can never actually prove the existence of a causal relationship, demonstrating the implausibility of potential alternative explanations, for all practical purposes, functions to establish the causal hypothesis as true until it can be disproved by new evidence. For these reasons, as many potential alternative explanations of observed relationships between the measure of beverage alcohol availability (i.e., legal drinking age) and measures of motor vehicle accidents as possible were analyzed before dismissing them as implausible explanations of observed covariations.

One potential alternative explanation of an observed relationship between legal drinking age changes and measures of accidents is that the proposed causal relationship is reversed, that is, changes in accident frequency bring about changes in the legal drinking age rather than vice-versa. This threat to internal validity was ruled out in the present design by the time-ordered nature of the measurements. A cause must precede in time its effects, and since the measures of the changes in legal drinking age precede the measures of accidents, the argument that the causal relationship is reversed was discarded.

A second major threat to internal validity is history, a contemporaneous event that may be the true cause of the observed effect. For example, one might argue that any downward shifts in accidents among youth after the increase in legal drinking age were due to the moderate gasoline shortage (and increased gasoline prices) of the late 1970s, and the resultant decrease in miles driven. The plausibility of such explanations of observed shifts in the dependent series was reduced by specific features of the research design. The use of quasi-control groups, consisting of the affected age group's older peers not affected by the drinking age change, and comparison states that have not altered their drinking age, permitted an assessment of the validity of alternative explanations, of which the gasoline shortage is one example. Such contemporaneous historical events would most likely affect all age groups in all four states, not just 18-19 or 18-20-year-olds in Maine and Michigan, and the effects of such factors would be observed in the comparison dependent variables.

A third threat to internal validity is maturation, gradual developmental changes in the dependent variables simply due to the passage of time. The time-series design used in the present research rules out this threat by including a series of observations prior to the intervention, permitting a determination of whether the post-intervention observations were simply the continuation of a pre-intervention maturational trend. It should be noted that a gradual trend in the dependent series can be attributed to maturational effects or to the effects of some omitted variables such as economic or population growth or decline; in any case, observed trends in the dependent variable series were explicitly taken into account in the data analyses.

A fourth potential threat to internal validity is instrumentation, a change in the measuring instrument occurring coincident with the intervention. That is, the process by which accident frequencies are measured may have changed at the same time point as legal changes in drinking age, and may account for any observed shifts in the series at the point of the legal changes. This argument is not a plausible alternative explanation of the proposed causal relationship because multiple comparison groups were included that would also have experienced basic changes in measurement process. Such changes in measurement process cannot be used to explain differential shifts in frequency of accidents for the 18-19 or 18-20 age groups as compared to age groups presumably unaffected by legal drinking age changes. The time-series nature of the design also reduces the plausibility of attributing observed changes in crash involvement at the time the drinking age was raised to instrumentation changes. With a large number of observations over an extended period of time prior to the intervention, a substantial instrumentation change exactly at the point of the intervention is less plausible.

Regression to the mean is another often mentioned threat to internal validity. Regression to the mean is a particular problem if an intervention is implemented exactly at a point at which the dependent series is at a very high or a very low point, since the subsequent observations will tend to be closer to the mean of the series simply by chance, regardless of any intervention effect. For example, if the drinking age was raised at precisely that point in time when alcohol-related traffic accidents among youth were at their highest level in years, one would expect the level of accidents to fall somewhat after that unusually high point. Such a regression effect could be mistaken as the effect of the drinking age change. Such an argument is not a threat to the internal validity of the present design for several reasons. First, a long series of observations was available prior to the interventions, facilitating determination of the exceptionality of the observations immediately prior to the interventions. Second, and perhaps more important, the data analysis techniques used to assess shifts in the series were based on all of the observations in the series, rather than relying only on observations immediately prior to and immediately after the intervention. Furthermore, the analysis methods take into account seasonality and autocorrelation regularities in the series, ensuring that the intervention effect identified was independent of effects due simply to the particular time points at which the interventions were implemented.

The sixth and seventh potential threats to internal validity are selection and mortality. That is, particular characteristics of the subjects selected for study, and particular characteristics of those subjects who drop out of the study, may invalidate the results. One type of selection threat occurs when differences in the kinds of subjects in the experimental and control groups account for differences in the post-intervention measures between the two groups, rather than an impact of the intervention. This alternative explanation was not a threat in the present design because the criterion for establishing an intervention effect was not simply differences in the post-intervention observations between experimental and comparison groups, but rather differences in the shifts found within the experimental and the control dependent series. However, when intervention effects are assessed by examining shifts within each of the series, selection and mortality may threaten internal validity if the composition of the experimental group changes substantially at the point at which the intervention was implemented, thus providing a plausible alternative explanation of observed shifts in the series. The composition of the experimental group does change over time with the addition of new drivers who attain the age of 18 and dropping out of individuals who attain the age of 20 or 21. This change in composition of the group, however, occurs gradually, with only a small proportion of the total experimental population changing from month to month. Furthermore, these changes in composition of the experimental groups are primarily due to a stable aging process that cannot be influenced by the intervention or extraneous factors. Thus, it was highly implausible that changes in the composition of experimental groups accounted for observed shifts in the dependent variables.

There are three threats to internal validity which involve interaction with selection of particular threats already discussed. First, selection-maturation refers to a differential maturational trend across the experimental and control groups. This was not a threat to internal validity for the same reasons that the main effect of maturation was not a threat, namely, the long series of observations available prior to the interventions, and the data analysis methods used, which explicitly take into account any maturational trends in each group's series of observations.

The second interaction threat to internal validity is the interaction of selection and history. It is possible that each experimental group experienced a different "local" history, and this differentially experienced contemporaneous event is actually the cause of shifts observed in the series concomitant with drinking age interventions. For example, two contemporaneous events, the moderate gasoline shortage and price increases of the late 1970s, and the ban on non-returnable beverage containers in Michigan and Maine (increasing the cost of alcoholic beverages), may have had a differential impact on the various age groups. One could conceivably argue that both of these contemporaneous events had an influence upon youth but not adults. Since youth may have less discretionary disposable income available, and since these contemporaneous events increased the cost of both driving and drinking, the ban on non-returnable beverage containers and the 1979 fuel shortage/price increases may explain why there were reduced alcohol-related accidents for youth and no such shifts for older age cohorts during 1979. If it is true that the increased cost of fuel and alcoholic beverages influenced the drinking and driving patterns of youth more than the drinking and driving patterns of older cohorts, the major fuel shortage and price increases of early 1974 should also have had a greater impact on young drivers. However, the time-series analysis results presented in Appendix B reveal that the 1974 fuel shortage/price increases did not affect young drivers more than older drivers. This finding reduces the plausibility of the argument that the increased fuel prices in the late 1970s account for the larger reductions in accidents observed for young drivers than older drivers.

The final interaction with selection that is a potential threat to internal validity is selection-instrumentation. This threat could obtain if alterations in the procedures for reporting alcohol-related accidents occurred only for accidents involving youth. The instrumentation change could then account for shifts in accident frequencies specific to this age group. This threat to internal validity is the argument most frequently used by those who favor lower drinking ages, to discredit observed covariations between drinking age changes and the frequency of collisions among young drivers. The argument is that with a lowered legal age police officers are more vigilant in reporting the presence of alcohol in crashes involving young drivers, and conversely, officers report fewer crash-involved young drivers as "had been drinking" when a high drinking age is in effect. Although the extent of any such police reporting bias has not been documented, the selection-instrumentation challenge to internal validity was controlled through the use of a second, indirect indicator of alcohol-related crashes as discussed in Section 4.2 (i.e., single-vehicle nighttime male crashes). It is highly unlikely that reporting of the driver's sex, the time of the crash, or the number of vehicles involved, would change at the time of the drinking age modifications, either for young drivers or for older cohorts.

Cook and Campbell (1979) also point out the potential threat to internal validity of the "diffusion or imitation of treatments," where there is contamination of comparison groups as a result of their experiencing a portion of the intervention. Diffusion of the interventions was possible in the present design for the 16-17 age group, since a major change in the level of availability of alcoholic beverages for the 18-19 or 18-20 age group indirectly changes the level of alcohol availability for the 16-17 age group. As a result, the interventions may have an impact on the 16-17-year-old cohort as well as the focal 18-19 or 18-20 age groups. Diffusion of the intervention to 16-17-year-olds was no threat to the present investigation since other comparison age groups, whose levels of alcohol availability were not affected by the interventions, were included in the design. Effects of drinking age changes on the 16-17 age group were directly assessed along with the impact upon 18-19 or 18-20-year-olds.

In summary, the goal in designing this research was to obtain valid conclusions as to whether reductions in availability of beverage alcohol, as represented by raising the drinking age, cause substantial reductions in the frequency of alcohol-related accidents. The first step was to establish that there was a true covariation between changes in availability and changes in accident frequency, achieved by assuring the statistical conclusion validity of observed shifts in the accident time series. The second step was to rule out extraneous hypotheses, those other than the causal hypotheses under investigation, that could plausibly explain the covariations observed. The result was high internal validity and a high level of confidence that the covariation observed represents a causal relationship between the particular operationalizations of alcohol availability and alcohol-related accidents. The next validity issue was whether the causal relationship established between the particular measures was, in fact, indicative of a causal relationship between the broader constructs of interest, namely, alcohol availability and alcohol-related traffic accidents. The relationship between the operationalizations or measures used and the theoretical constructs of interest is in the domain of construct validity.

4.3.3 Construct Validity. Construct validity answers the question, given the established causal relationship between the operationalizations used (i.e., high internal validity), do the operationalizations adequately reflect the concepts of interest? The first threat to construct validity is inadequate explication of constructs prior to their operationalization. Clear specification of the concepts of interest is an important aid for obtaining measures that are appropriate to the concepts under study.

A second threat to construct validity is labeled "mono-operation bias" by Cook and Campbell (1979:65). Mono-operation bias refers to the use of only a single operationalized measure of each concept. The use of single indicators prevents an assessment of convergent validity, that is, the extent to which different measures of the same concept produce the same result. Mono-operation bias was reduced in the present research through the use of multiple indicators of each concept. As discussed in Section 4.2, the traffic crash dependent variable measures include frequency of police-reported "had not been drinking" crash-involved drivers, frequency of police-reported accident-involved drivers where the driver "had been drinking," and an indirect measure of alcohol-related accidents based on previous research (i.e., single-vehicle nighttime male crashes). Furthermore, two categories of crash involvement were examined, frequency of property damage crash involvement and frequency of injury or fatal crash involvement. Use of such multiple indicators of traffic accidents and alcohol-related traffic accidents permitted an assessment of convergent validity. The measure of changes in alcohol availability was based on the effective date of the legal changes, and was accepted as a valid measure on the basis of face validity.

A threat to construct validity closely related to mono-operation bias is "mono-method bias" (Cook and Campbell, 1979:66). It refers to the reduction in construct validity that occurs if all the measures of a concept are based on the same data collection technique. The most difficult concept to measure in the present investigation, alcohol-related accidents, was measured using two methods. The "had been drinking" measure is based on judgements of investigating police officers, while the single-vehicle nighttime male indicator was empirically constructed on the basis of demographic characteristics of the driver and circumstances surrounding the collision.

There are three threats to construct validity that are potential reactive effects of the experimental situation. The first threat occurs if subjects within the various experimental conditions guess what the researcher's hypothesis is and act in such a manner to confirm (or contradict) that hypothesis. The second threat is "evaluation apprehension" (Cook and Campbell, 1979:67) on the part of the experimental subjects, where, as a result of the subjects' awareness of being evaluated, behave in a socially desirable manner. The third reactive effect that may threaten construct validity is the expectation of the experimenter. If the experimenter's expectations are communicated to the subjects under investigation or those who collect data, distortions in the subjects' behavior or the data collected may result. The experiment examined here was a natural part of the social environment, not imposed on the social system by outside researchers, and thus was unlikely to create reactive effects. However, a form of the third threat, experimenter's expectations, could threaten construct validity if expectations of police officers, who are responsible for collection of data on traffic accidents, influence reported frequencies of "had been drinking" crashes. This threat is minimized by use of measures over which the police had little control and were thus unlikely to be distorted by such subjective factors (i.e., single-vehicle nighttime male crashes).

Another threat to construct validity is somewhat obscurely labeled "confounding constructs and levels of constructs" by Cook and Campbell (1979:67). This source of invalidity occurs when there is implementation of only a small number of all possible levels of the intervention variable, and/or measurement of only a subset of all possible levels of the outcome variable. Invalid conclusions may result if the effect (or lack of effect) observed is due to the fact that only particular levels of the intervention are administered, or only a portion of the potential range of the outcome variables is measured. In the present design, the full range of possible values for the outcome variables was examined and the independent variable was a dichotomy (beverage alcohol legally available to an age group versus beverage alcohol not legally available to that age group). One could argue that the concept of alcohol availability is continuous and the present design only examines two of many possible levels of alcohol availability. If one accepts this very reasonable argument, it must be noted that the two levels of availability examined are at widely divergent points of the availability continuum. Although a detailed examination of the pattern of impact of marginal changes in availability of beverage alcohol was not possible, conclusions concerning the impact of a major change in availability upon motor vehicle accidents, the purpose of the present investigation, could be validly reached.

4.3.4 External Validity. External validity answers the question, given that one can confidently conclude that there is a causal relationship between the focal constructs, to what extent is this causal relationship generalizable across persons, settings, and times? The first major threat to external validity is the interaction of selection and treatment. That is, the plausibly causal relationship that has been established may only apply to the particular atypical population analyzed. The selection of a target population of all accident involved youth in four states, analyzing a census of all reported crashes, reduced this constraint on generalizability.

There are two major limitations on the populations to which findings can safely be generalized. First, since the analyses were limited to the aggregate of all reported crash involved youth in the four states, no generalizations can be made to particular youth subpopulations. For example, without specific analyses of particular subgroups based on socio-demographic or social-psychological variables, one cannot determine the differential impact of changes in legal availability of beverage alcohol upon particular types of youth. Although the overall impact was determined, this overall impact may be the result of differing impacts on particular subgroups of the total youth population. Second, because the analyses were based solely on the population in Maine, Michigan, New York, and Pennsylvania, the generalizability of the results is, strictly speaking, limited to thes states. It must be recognized that generalizing to other states i based on one's judgement as to the similarity between those states ar the states examined here, rather than based on explicit features of th research design. Since the jurisdictions studied include larg heterogeneous states, generalization to numerous similar states ar provinces can reasonably be made.[13]

The interaction of setting and treatment is a second basic threa to external validity. This limitation on generalizability occurs whe the intervention effects observed are due to implementation of tl interventions in a particular socio-cultural setting. Since the presei investigation assessed the effects of changing legal drinking age only one socio-cultural setting, one cannot necessarily generalize tl results to widely different states or countries. However, t! experimental setting was not substantially atypical of a number industrialized states, and some generalization can plausibly be made, it is done with caution, recognizing that one is generalizing inference, not on the basis of explicit features of the research desig

A third major threat to external validity is the interaction history and treatment. If intervention effects occur only under t particular historical circumstances present when the interventions we

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[13] One important difference between states, potentially limiti generalizability, is the degree to which the state is isolated fr contiguous states with different drinking ages. For example, a rais drinking age is likely to have less impact in a state with a long bord with a state that retains a lower drinking age because young drinke may drive to the neighboring state to obtain alcohol. Such a situati is an example of the operationalized measure, a legal change in drinki age, not reflecting a major change in the construct of interest, alcoh availability. Since only a marginal change in alcohol availabili occurs after a legal drinking age change in such circumstances, le effect on alcohol-related collisions might be expected.

implemented, the generalizability of the findings is limited. Comparing the results of this investigation of the raised drinking age with earlier studies of the lowered drinking age substantially reduced this threat to external validity. Since the differential impact of lowered drinking age in the early 1970s and raised drinking age in the late 1970s was examined, the plausibility of the argument that particular historical circumstances interacted with the drinking age intervention in both cases, bringing about both observed shifts, was greatly reduced. However, the drinking age changes were implemented in a particular historical period, and the extent to which similar results would occur during different time periods is unknown. For example, the Vietnam war, the military draft, and associated youth protest activities of the late 1960s and early 1970s may have facilitated the move to a lower age of majority (including drinking age). The movement to raise the drinking age may be affected by the frequently discussed conservative drift of the United States in the late 1970s and early 1980s. One can only speculate as to the effect of larger socio-historical developments on the interaction between drinking age public policy and motor vehicle accidents.

In summary, it is evident that a number of features of the design of this investigation, such as (1) appropriate use of sensitive statistical procedures, (2) use of long series of observations for multiple measures, and (3) analyses of multiple comparison groups, increase the validity of the findings. The high levels of statistical conclusion validity and internal validity of this study facilitate the establishment of a causal relationship between changes in the legal availability of beverage alcohol, as measured by modifications in the drinking age, and traffic accidents, as measured by the frequency of collisions. The levels of design validity for construct and external validity were somewhat lower, however, and broad generalizations to related concepts and other populations and settings should be made with care. In particular, the conceptualization and measurement of alcohol-involvement in traffic crashes requires further development. As Cook and Campbell (1979) pointed out, construct and external validity are, in the final analysis, matters of replication. Therefore, replication of the present investigation in other states, using various measures of the concepts and using the sensitive design and data analysis features used here, would strengthen the conclusion that there exists a general causal relationship between beverage alcohol availability and the frequency of alcohol-related public health problems.

4.4 Data Analysis Methods

Ordinary least squares regression and other commonly used statistical procedures assume independent observations, that is, no serial correlation between the observations. Since a series of observations on the same unit over time are very likely to be autocorrelated, violating the assumption of independence required for the use of standard statistical procedures, alternative data analysis strategies are necessary. One such approach is the modeling strategy of Box and Jenkins (1976) and Box and Tiao (1975). The Box-Jenkins approach involves modeling the autocorrelation in time-series variables to produce unbiased estimates of error variance in the presence of serially correlated observations.[14] Recent methodological developments

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[14] Reid provides evidence of the superiority of the Box-Jenkins methodology by applying five different time-series analysis and

in the use of transfer functions along with the Auto-Regressive Integrated Moving Average (ARIMA) modeling strategy make these techniques the best currently available for the analysis of time-series quasi-experiments (Box and Tiao, 1975; Hibbs, 1977; McCleary and Hay, 1980). The techniques identify a wide variety of patterns in dependent time-series variables, provide a sensitive test of intervention effects, and allow for the analysis of a variety of intervention effect patterns.[13]

The first step in the Box-Jenkins intervention analysis strategy is the identification or specification of a parsimonious Auto-Regressive Integrated Moving Average (ARIMA) model for each dependent time-series variable. The ARIMA model is commonly called the "noise model" since its purpose is to isolate all of the aspects of the stochastic autocorrelation structure of the series, and provide a benchmark for the assessment of intervention effects. The ARIMA model accounts for the variance in the dependent series that is due to identifiable trend,

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forecasting techniques to 113 different series. In the great majority of the applications, the Box-Jenkins techniques produced the smallest residual error variances. The Box-Jenkins techniques performed especially well with long series characterized by seasonal components (cited in Kendall, 1973:125-127). Other assessments of time-series analysis techniques generally support the superiority of the Box-Jenkins methods (see Vigderhous, 1977 for a brief review).

[13] See Glass, Willson and Gottman (1975:44) for a description of possible intervention effect patterns. The purpose of the data analyses in this investigation was to determine the impact of changes in legal drinking age on each dependent variable. After the effects of legal changes on each variable were determined through the statistical procedures described below, the effects were compared across those measures expected to be influenced by the legal change and those not expected to be influenced by the intervention. The present section discusses procedures used to determine the effect on each isolated dependent time-series; the comparison of these effects across experimental and comparison age groups, as called for in the design, is discussed in Chapter 5.0 on results of the statistical analyses.

function).[18] Using the Box-Jenkins nomenclature, these models are labeled as rsb (1,0,0) and rsb (0,0,0), where r is the order of the auto-regressive component, s is the order of the shift or change in level component, and b is the amount of delay or dead time after the beginning of the intervention before any impact is expected. No dela parameter b was included in the analyses because the initial effects o the legal changes were expected in the month immediately following th change in drinking age.[19] The impact patterns assessed by the models are shown in Figures 4.5 through 4.10. All of the major changes in th dependent time series variables were modeled with simple shift transfer functions (Figures 4.7 and 4.8) except the increase in draft beer sales in Michigan following the 1972 reduction in drinking age, which was bes explained by a temporary impact model (Figure 4.10).

After the order of the ARIMA component was identified on the basi of a plot of the raw data and autocorrelation and partia autocorrelation functions, and the appropriate transfer functio components were added, preliminary estimates of the parameters of th identified model were calculated on the basis of the raw data plot an the estimated autocorrelations. These preliminary estimates were inpu

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[18] Techniques for specifying the form of the transfer function mode on the basis of cross correlations between input and output serie (following similar principles as outlined above for the specification o ARIMA models), have been proposed (Box and Jenkins, 1976; Haugh and Box 1977). However, these procedures require the variance of the inpu series to be similar in magnitude to the variance of the output series Since this investigation involved dummy input variables and outpu accident variables with large variances, such empirical transfe function identification procedures could not be used. Instead, transfe function models were specified a priori on the basis of theoretica expectations, and assessed within an hypothesis testing framework.

[19] Immediate effects of the higher drinking ages were hypothesiz because previous research had identified immediate effects of bot lowered (Douglass, 1974) and raised (Wagenaar, 1980) drinking age.

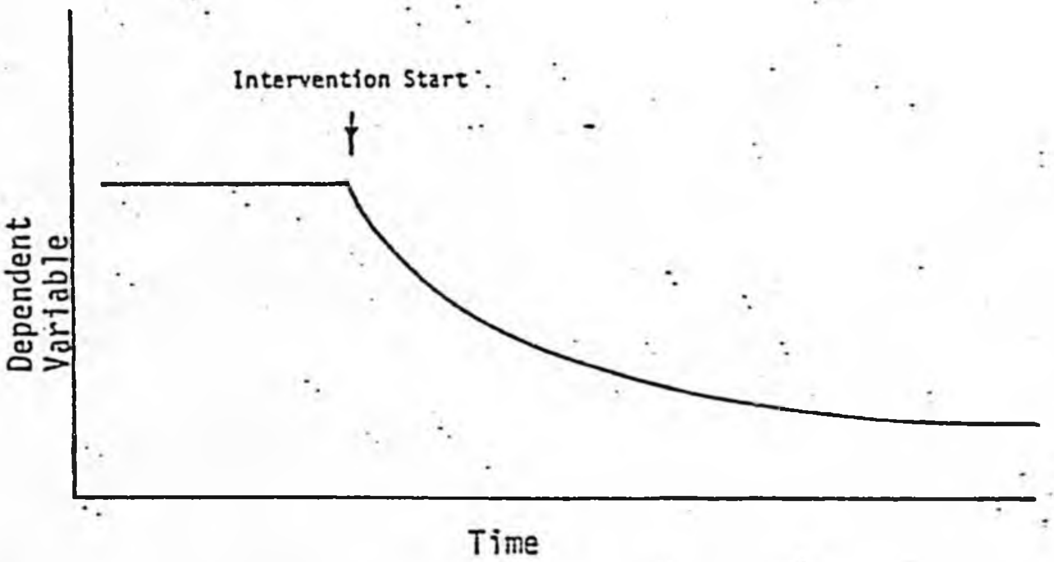

Figure 4.5 Negative Impact Pattern Estimated by the rsb (1,0,0) Transfer Function Model with a Step Function Input

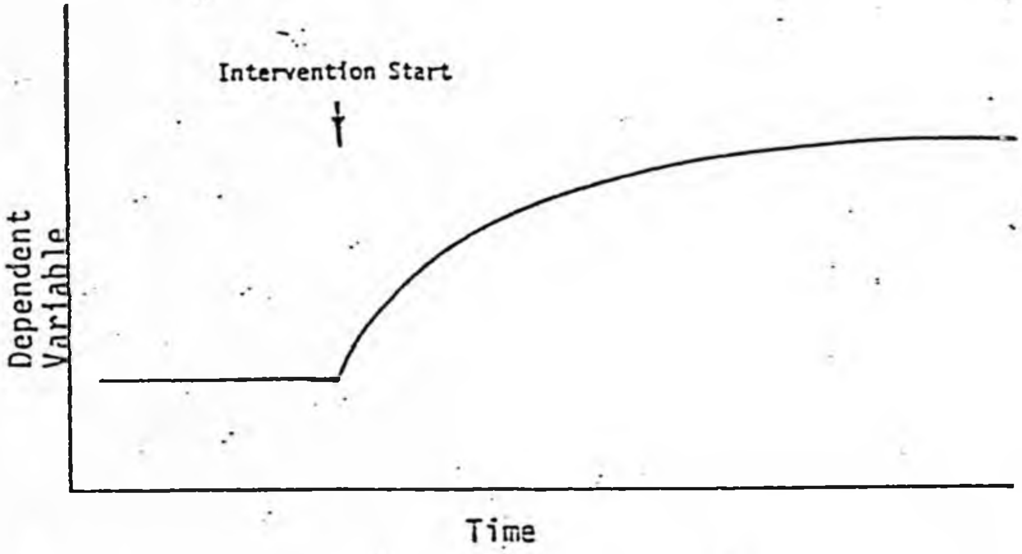

Figure 4.6 Positive Impact Pattern Estimated by the rsb (1,0,0) Transfer Function Model with a Step Function Input

seasonal, and other autocorrelation patterns in the data. The resid "white noise" or random error variance then permits a sensitive test the statistical significance of intervention effects.

Since traffic accident time-series often contain large seaso components, the general multiplicative seasonal model was applied each dependent series. The general seasonal ARIMA model is shown equation one,

$$(1)\qquad z_t = \frac{(1 - \Delta_1 B^s - \dots \Delta_Q B^{sQ})(1 - \theta_1 B - \dots \theta_q B^q)u_t + \alpha}{(1 - \Gamma_1 B^s - \dots \Gamma_P B^{sP})(1 - \phi_1 B - \dots \phi_p B^p)(1 - B^s)^D (1 - B)^d}$$

where p is the order of the auto-regressive process, d is the degree non-seasonal differencing, q is the order of the moving-average proce P is the order of the seasonal auto-regressive process, D is the deg of seasonal differencing, Q is the order of the seasonal moving aver process, s is the seasonal span, Γ_1 to Γ_P are the seasonal au regressive parameters, ϕ_1 to ϕ_p are the regular auto-regress parameters, Δ_1 to Δ_Q are the seasonal moving-average parameters, to θ_q are the regular moving-average parameters, u_t is the ran (white noise) error component, α is a constant, and B is the backsh operator such that $B(z_t)$ equals z_{t-1}. It is important to realize t the ARIMA model is not based on a theory concerning the causes of dependent series. It is a model to describe the nature of the ongo regularities in the series due to any number of (most lik unidentified) causes. The ARIMA model for each variable, therefo must be empirically determined by an examination of a series observations of that particular variable.

Initial specification of an ARIMA model for a particular series was made on the basis of a visual examination of a plot of the raw series and the autocorrelation and partial autocorrelation functions estimated from the series observations. The raw time-series plot provided initial information as to trend and seasonal characteristics of the series, facilitating the identification of differencing factors and the seasonal span. Differencing refers to the calculation of a new series by subtracting each observation from a previous one in the same series. For example, the first difference is $z_t - z_{t-1}$, the first seasonal difference with a seasonal span of 12 is $z_t - z_{t-12}$; the second difference is the first difference of the first differences. Differencing is used to obtain a series with a constant mean or overall level, an assumption of ARMA models. The plot of the raw series was also visually examined for constant variance across the series; if the variance appeared non-constant, appropriate transformations were performed before proceeding.[14]

Theoretical autocorrelation and partial autocorrelation functions corresponding to various ARIMA models have been described by Box and Jenkins ~~(1976)~~. In the present study, a preliminary ARIMA (p,d,q)(P,D,Q)s model was identified for each series on the basis on an examination of the estimated autocorrelations and partial autocorrelations, assessing the degree to which the actual autocorrelations fit one of the theoretically expected patterns. The simplest model that could plausibly account for the behavior of the series was selected.

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[14] Range-mean plots can also be used to check for non-constant variance.

A first-order moving average, first-order seasonal moving average model on the seasonally differenced series was identified for the majority of the dependent variables.[17] In terms of the general model in equation one, the specific baseline model for most of the series is shown in equation two.

$$(2) \qquad z_t = \frac{(1-\Delta_1 B^{12})(1-\theta_1 B)u_t + \alpha}{1 - B^{12}}$$

The variance of several series was roughly proportional to the mean; therefore, they were logarithmically transformed before proceeding with model identification.

After an ARIMA model of the series was identified, transfer functions representing hypothesized effects of the raised drinking age were added to the ARIMA noise model. The general form of the transfer function is shown in equation three,

$$(3) \qquad y_t = \frac{(\omega_0 - \omega_1 B - \ldots \omega_s B^s)}{(1 - \delta_1 B - \ldots \delta_r B^r)}(I_{t-b})$$

where ω_0 to ω_s and δ_1 to δ_r specify the manner in which the "input" or independent variable I_t influences the "output" or dependent variable y_t, B is the backshift operator such that $B(z_t)$ equals z_{t-1}, I_t is either a step function with the value zero before the intervention and

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[17] A plot of each time series and the final identified model is shown in Appendices A, B, C, D, G, I, J, and K.

one thereafter, or a pulse function with the value one for the month in which the intervention begins and zero otherwise, and b is a delay parameter indicating the length of lag or "dead time" between the intervention and the initial effects of the intervention (Hibbs, 1977:149).

The main intervention of interest in the present investigation was the increase in drinking age. In addition, since several reports found that the fuel shortage, national maximum legal speed limit reduction, and related factors of early 1974 resulted in a reduction in motor vehicle crashes (Borg et al., 1976; Burritt et al., 1975; Carpenter; 1974, 1975; Chu and Nunn, 1976; Dart, 1977; Kahane, 1975; Klein et al., 1976; O'Day et al., 1975; Seila et al., 1977; Tofany, 1975; United States Department of Transportation, 1978; Wiorkowski and Heckhard, 1977), a transfer function representing the effects of this major exogenous influence on the frequency of traffic accidents was included in the analyses of those variables exhibiting a substantial decrease in frequency in early 1974. Other major shifts in the dependent variables, due to reporting changes or other historical events, were controlled by including additional transfer functions where appropriate. All of the exogenous factors were modeled with simple forms of the general transfer function model shown in equation three.

Two specific forms of the general transfer function model were used in the present investigation, a temporary impact model ($\frac{\omega}{1 - \delta B} P_t$ where ω is the shift or change in level component, δ is the auto-regressive "memory" component, and P_t is defined as a pulse function), and an abrupt permanent impact model (ωI_t with I_t defined as a step

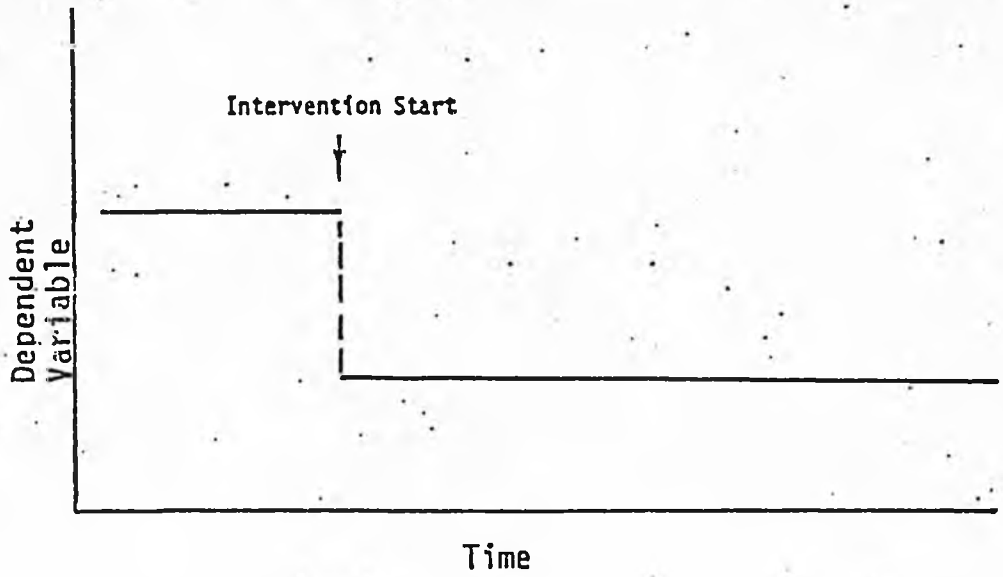

Figure 4.7 Negative Impact Pattern Estimated by the rsb (0,0,0) Transfer Function Model with a Step Function Input

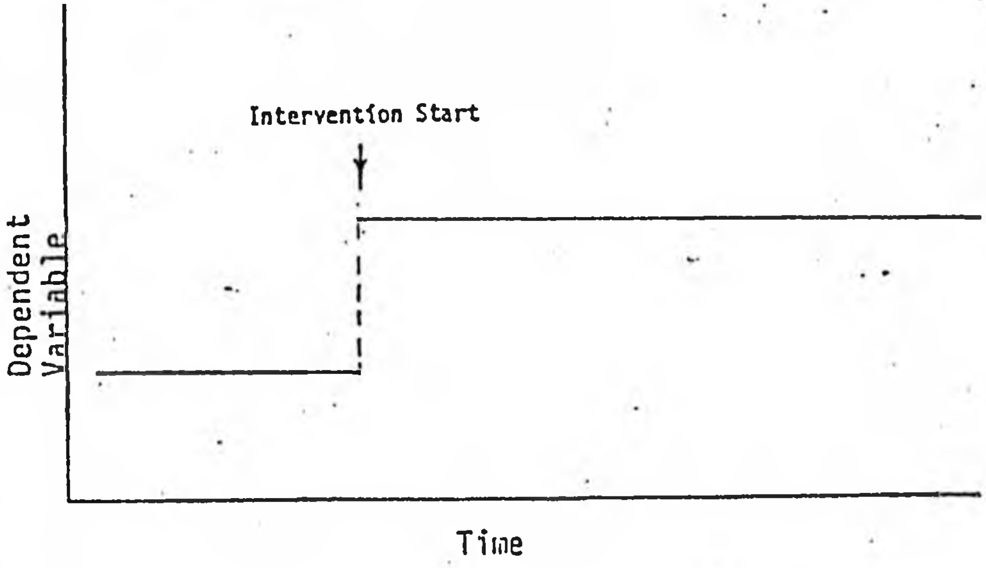

Figure 4.8 Positive Impact Pattern Estimated by the rsb (0,0,0) Transfer Function Model with a Step Function Input

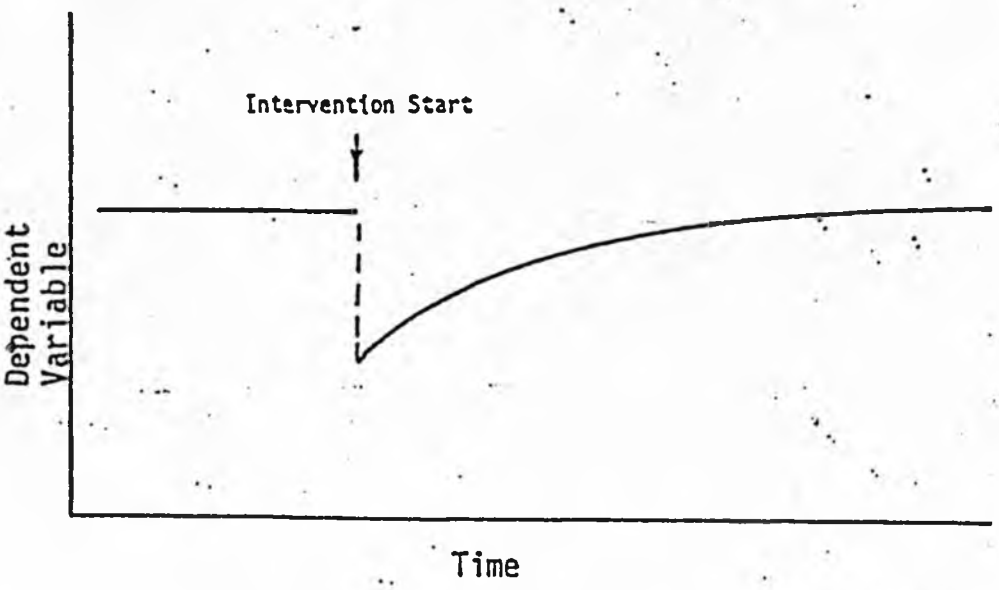

Figure 4.9 Negative Impact Pattern Estimated by the rsb (1,0,0) Transfer Function Model with a Pulse Function Input

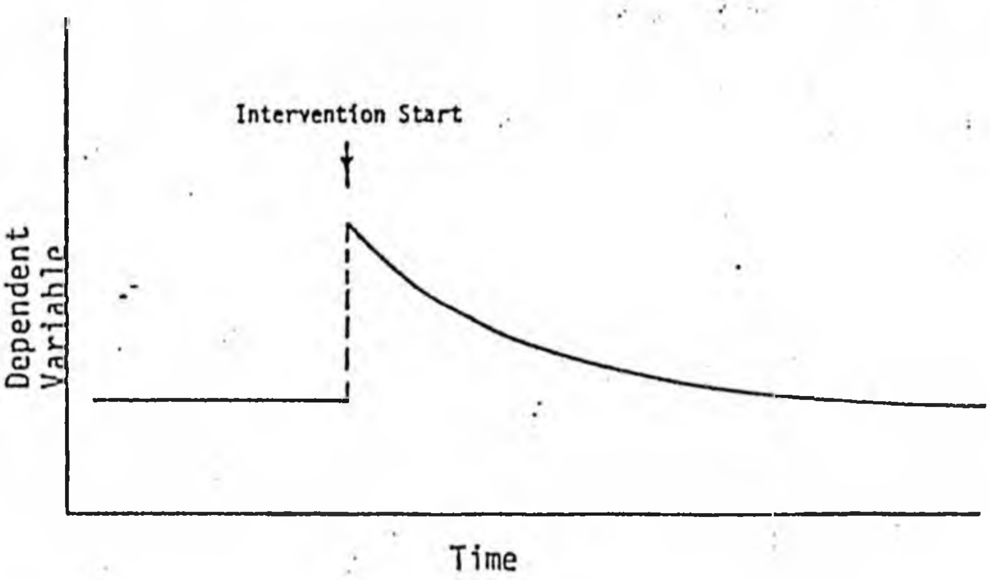

Figure 4.10 Positive Impact Pattern Estimated by the rsb (1,0,0) Transfer Function Model with a Pulse Function Input

as starting values for the computer estimation program BMDQ2T (Liu, 1979). Since the models are intrinsically non-linear, BMDQ2T uses the Gauss-Marquardt method to obtain maximum likelihood estimates of the parameters. The unconditional least squares (i.e. backcasting) estimation algorithm was used rather than conditional least squares because seasonal moving average models were being estimated; Box and Jenkins (1976) recommend unconditional least squares estimation for such models.

Perhaps the most important step in the Box-Jenkins modeling strategy follows the initial estimation of a model. The estimated model must be evaluated with regard to its parsimony and its ability to account for all of the autocorrelation patterns in the original series There are several considerations in assessing model adequacy. First the estimated parameters should meet the conditions of stationarity invertibility required for the particular model under consideration (Bo and Jenkins, 1976). Second, the estimated parameters of the ARIM component should be significantly different from zero. Third, th correlations among the parameters should not be excessive, indicatin redundancy in the model specified. Fourth, the overall "flatness" o the autocorrelation function of the residuals should be documented by non-significant Q-statistic (Box and Pierce, 1970; Ljung and Box, 1976) Fifth, the autocorrelation function should not reveal significan correlations at the first few lags or the first seasonal lag. Sixth parameter estimates should be interpretable in terms of theoretica expectations and known characteristics of the dependent variable.

Inadequacies in the combined ARIMA/transfer function model detected when evaluating the estimation results, were used to guide re specification of the model. After re-specification, maximum likelihood estimates were obtained, and the revised model was evaluated according to the above criteria. If the model was still inadequate, the specification, estimation, and evaluation steps were repeated again; if more than one model was adequate by these criteria, the model with the lowest sum of squared deviations from the fitted model was selected. The values of the transfer function parameter estimates, along with unbiased estimates of their standard errors, were used to determine the existence of effects of the interventions, and where intervention effects were evident, to assess the direction and magnitude of the impact in terms of the number of crashes apparently caused by or prevented by the intervention.

All of the specific models fit to the series were variations of the underlying model which views a particular time series as a realization of a general discrete linear stochastic process (Nelson, 1973:30-33). In modeling a time-series as a realization of a discrete linear stochastic process, one assumes that: (1) the time-series is stationary and characterized by a constant mean, i.e., the series does not exhibit a substantial trend or change in overall level, (2) all random errors (u_t in equation one) are drawn independently from the same distribution over time and thus are characterized by constant variance, and (3) the autocovariances are constant over time, depending only on the extent of lag between the observations. If one adds the assumption of normally distributed errors, what is referred to as strict stationarity is achieved. The assumption of a stationary mean or level in the original series is not strictly required, because the model remains appropriate provided stationarity is obtained after using the appropriate differencing factors on the original series. If a stationary level is obtained after differencing, the series is said to exhibit "homogeneous non-stationary behavior" (Box and Jenkins, 1976:11).

An important strength of the Box-Jenkins modeling strategy is that an assessment of the extent to which the assumptions underlying the analyses are met is explicitly included in the model building process. Thus, after each particular model was specified and estimated, the residuals were examined to ensure that they displayed the characteristics of white noise (i.e., were independently and identically distributed with constant variance).[30] Evidence that the assumptions of the model were met increased the validity of the findings.

In summary, the data analysis strategy was as follows. First, an ARIMA noise model was specified on the basis of a plot of the raw series and the estimated auto- and partial autocorrelation functions. Second, transfer functions for the major exogenous factors were added to the noise model and the combined noise and intervention model was estimated. The combined model was evaluated and the specification, estimation, and evaluation process was repeated until an adequate model was obtained. The statistical significance and magnitude of the transfer function parameter estimates were used to identify the impact of a legal change in drinking age on that particular time series dependent variable. The data analysis strategy was repeated for each dependent variable, and the results, as called for in the research design, were compared across (1) indicators of alcohol-related accidents and indicators of non-alcohol-related accidents, (2) experimental age groups (youth under 20 or 21)

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[30] A more general discussion of the importance of examining residuals to ensure that assumptions are not violated is provided by Draper and Smith (1966).

and comparison age groups (drivers over 20 or 21), and (3) states that changed their drinking age (Michigan and Maine) and states that did not (New York and Pennsylvania).

5.0 FINDINGS

Results are presented in three sections. Section 5.1 presents the findings of the main study, the effect of the drinking age on traffic crashes. Section 5.2 discusses analyses of aggregate alcoholic beverage sales in several states, where the effects of the drinking age and other public policy changes were evaluated. Finally, section 5.3 examines data on beverage control law enforcement activities and the role of enforcement when using the minimum drinking age as an alcohol-related problem prevention strategy. In each of these sections, the main findings are discussed and summarized in tabular form. Additional information on the data and statistical models used is provided in the appendices.

5.1 Effects of the Raised Drinking Age on Motor Vehicle Crash Involvement

Results of the time-series modeling of crash involvement in Michigan, Maine, New York, and Pennsylvania are summarized in Tables 5.1 through 5.12 below. Shown for each dependent variable is the estimated average change in monthly frequency of driver crash involvements after a change in drinking age was implemented. As noted in the tables, some series were logarithmically transformed to reduce heteroscedasticity, that is, variation over time in the series variance. In such cases, the parameter estimates refer to the log of crash frequency, and cannot be directly interpreted as the change in number of crash-involved drivers per month. In addition to the time-series model estimates, the summary tables include the t-ratio for the estimates, and the estimates calculated as percentage change between actual crash involvement frequencies after the implementation of the raised drinking age and frequency expected had there been no change in drinking age.[11]

The reader is encouraged to concentrate on the percent figures, comparing changes in alcohol-related crash involvement with changes in non-alcohol-related crash involvement, and comparing changes in crash involvement for young drivers with the figures for older drivers. The t-ratios identify those shifts in crash involvement that are statistically significant (indicated in the tables with footnotes). Note that the results summarized in Tables 5.1 through 5.12 were extracted from comprehensive models for each time-series dependent variable. The estimates, therefore, represent the net change in crash involvement associated with raising the drinking age, controlling for the effects of long-term trends, seasonal cycles, and other major factors affecting the frequency of reported crash involvement, including motor fuel shortages, maximum speed limit changes, and modifications in criteria for reporting a crash. Taking into account these factors produced models which in most cases explained well over 50% of the variance in the dependent variables. A plot of each time series, the complete model developed for each series, and an estimate of goodness of fit between each model and the data (i.e. the adjusted R-square statistic) are shown in the appendices.

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[11] Percent change figures were based on the first 12 months after an increase in drinking age, and were calculated as follows:

$$(100)\frac{12\omega}{\left(\sum_{i=1}^{12} f_i\right) - 12\omega} = \text{percent change}$$

where ω is the shift in the frequency of driver crash involvement estimated by the time-series model, and f_i is the actual monthly frequency of crash involvement.

5.1.1 Michigan. Net changes in property damage crash involvement associated with the December 1978 increase in drinking age in Michigan are summarized in Table 5.1. The following findings emerge. First, alcohol-related crash involvement decreased significantly for drivers age 18-20, the target of the increase in drinking age (see Appendix B, Figure 10); non-alcohol-related crash involvement also decreased significantly for this age group, but the magnitude of the decrease is only half as large as the decrease in alcohol-related crashes. Subtracting the percent decrease in general non-alcohol-related crash involvement from the percent decrease in alcohol-related crashes produces a net reduction of 11 to 22 percent in alcohol-related property damage crash involvement attributable to implementation of the higher drinking age. The 11 to 22 percent reduction represents 725 to 1,617 fewer young Michigan drivers involved in alcohol-related property damage crashes over the first 12 months with the higher legal age than would have been expected had the drinking age not been increased. The conclusion that the raised drinking age is responsible for these observed crash reductions is strengthened by the results for the older age groups; drivers age 21-23 and 24-45 exhibited no significant change in crash involvement, in contrast to 18-20-year-olds, the target of the drinking age change.

A second major finding was that there was no observed effect of the raised drinking age on the frequency of property damage crashes among 16- and 17-year-old drivers. Although police-reported alcohol-related crashes decreased significantly, crashes with no police-reported drinking decreased by about the same amount; as a result, the decrease

TABLE 5.1
Time-series Model Estimates of Changes in Property-damage-only Crash Involvement Associated With Raising the Legal Minimum Drinking Age in Michigan

Type of Crash	Age of Driver			
	16-17	18-20	21-23	24-45
Alcohol-related Indicators				
Police-reported Drinking				
Estimate	-21.4	-208.2	-7.6	44.9
T-ratio	3.0[1]	7.2[1]	0.5	1.2
Percent	-14.7	-34.0	-1.7	3.8
Single-vehicle Nighttime Male.				
Estimate	-34.9	-129.4	-31.2	-20.7
T-ratio	2.6	5.1[1]	2.3	0.5
Percent	-15.5	-24.0	-8.8	-2.5
Non-alcohol-related Indicators				
No Police-reported Drinking				
Estimate	-400.2	-0.1[2]	-0.1[2]	-0.1[2]
T-ratio	3.7[1]	3.0[1]	2.4	2.3
Percent	-13.3	-12.2	-10.4	-9.5
Daytime				
Estimate	-324.2	-539.9	-315.5	-1186.7
T-ratio	3.4[1]	2.9[1]	2.0	2.4
Percent	-13.3	-12.8	-9.6	-10.0

[1] P<0.01, two-tailed test.

[2] Series logarithmically transformed to reduce heteroscedasticity; estimate based on transformed series.

in alcohol-related crashes cannot be attributed to the drinking age increase.

A third trend evident in Table 5.1 is that the parameter estimates for non-alcohol-related crash indicators for all age groups are negative and have relatively large t-ratios, even if not significant at the .01

level. Furthermore, all of these estimated crash reductions are in the 10 to 13% range. Reduced economic activity in Michigan in 1979 is the likely cause of the uniform reduction in general non-alcohol-related crash frequency across all age groups.

The effect of a raised drinking age was also evident from analyses of injury and fatal crash involvement in Michigan (Table 5.2). Police-reported alcohol-related accidents among 18-20-year-olds decreased 28 percent when the drinking age was raised; single-vehicle nighttime male crashes decreased 22 percent. Daytime injury and fatal accidents also decreased significantly for this age group but the magnitude of the decrease was only half as large as the decrease in nighttime crashes. If one subtracts the 11 percent decrease in daytime accidents from the 22 percent decrease in single-vehicle nighttime male crashes, an 11 percent reduction in alcohol-related accidents remains attributable to the raised drinking age. Since there was no significant change in crashes with no police-reported drinking, the 28 percent reduction in police-reported alcohol-related accidents also can be considered an estimate of the effect of the drinking age change. In short, raising the drinking age resulted in an 11 to 28 percent reduction in alcohol-related injury/fatal motor vehicle crash involvement. This 11 to 28 percent reduction represents 373 to 1,726 fewer young drivers involved in injury/fatal crashes over the first 12 months after the drinking age increase than would have been expected had the law not been changed. The conclusion that these reductions are due to the drinking age is strengthened by finding no significant reductions in alcohol-related or non-alcohol-related crashes among drivers age 21-23 or 24-45. In fact, results for drivers age 24-45 indicate increased alcohol-related crash involvement, in contrast to the decreases evident for 18-20-year-old drivers. Although the increases among drivers age 24-45 were not significant at the .01 level, examination of the time-series plots reveals higher alcohol-related injury/fatal crash involvement in 1979 than previous trends would lead one to expect (see Appendix B, Figures 25 and 27).

TABLE 5.2
Time-series Model Estimates of Changes in Injury/Fatal Crash Involvement Associated With Raising the Legal Minimum Drinking Age in Michigan

Type of Crash	Age of Driver			
	16-17	18-20	21-23	24-45
Alcohol-related Indicators				
Police-reported Drinking				
Estimate	-20.2	-143.8	9.6	88.2
T-ratio	2.5	4.6[1]	0.6	2.3
Percent	-16.8	-27.8	2.5	8.7
Single-vehicle Nighttime Male				
Estimate	-12.7	-63.0	9.3	38.6
T-ratio	2.3	3.6[1]	1.3	2.4
Percent	-12.6	-21.5	5.1	9.8
Non-alcohol-related Indicators				
No Police-reported Drinking				
Estimate	-100.8	-160.4	-60.3	-263.0
T-ratio	2.5	1.8	0.8	1.0
Percent	-8.5	-7.8	-3.8	-4.8
Daytime				
Estimate	-73.2	-187.7	-53.0	-219.9
T-ratio	2.1	3.0[1]	0.8	1.0
Percent	-7.7	-10.9	-4.0	-4.4

[1] P<0.01, two-tailed test.

As was the case for property damage crashes, no significant effect of the raised drinking age on injury and fatal crash involvement among Michigan drivers age 16-17 was found.

5.1.2 <u>Maine</u>. Effects of the raised drinking age on less serious, property damage crashes in Maine are shown in Table 5.3. Drivers age 18-19 experienced a significant 16.8 percent reduction in police-reported alcohol-related property damage crash involvement, and a 21.5 percent reduction in single-vehicle nighttime male crashes (the significant decreases are evident in Appendix A, Figures 10 and 12, where the 12-month moving average levels off after the drinking age was raised, in contrast to the previous long-term upward trend). In contrast, drivers age 18-19 showed no significant change in non-alcohol-related property damage crash involvement, measured either by police reports or daytime crashes. In addition, no significant changes in any of the four crash time series were observed for drivers age 21-22 or 22-45 in Maine. A significant reduction in alcohol-related property damage crashes for young drivers in Maine at the time the drinking age was raised, with no comparable reductions in either non-alcohol-related crashes among young drivers in Maine, or alcohol-related crashes among older drivers in Maine, indicates that raising the drinking age apparently caused the observed reduction in alcohol-related property damage crash involvement. The 16.8 to 21.5 percent reduction represents 61 to 90 fewer young drivers involved in alcohol-related property damage accidents the first year with the higher drinking age than expected on the basis of previous trends.

Although the time-series model estimates indicate reductions in alcohol-related property damage crashes among drivers age 16 and 17, the

TABLE 5.3
Time-Series Model Estimates of Changes in Property-damage-only Crash Involvement Associated With Raising the Legal Minimum Drinking Age in Maine

Type of Crash	Age of Driver			
	16-17	18-19	20-21	22-45
Alcohol-related Indicators				
Police-reported Drinking				
Estimate	-0.3[2]	-5.1	-3.0	-7.0
T-ratio	1.4	3.7[1]	1.5	1.8
Percent	-24.4	-16.8	-10.5	-6.9
Single-vehicle Nighttime Male				
Estimate	-2.8	-7.5	-0.9	1.1
T-ratio	2.2	3.5[1]	0.5	0.2
Percent	-15.9	-21.5	-3.6	14.9
Non-alcohol-related Indicators				
No Police-reported Drinking				
Estimate	5.8	-19.8	-12.5	-28.8
T-ratio	0.5	1.2	0.6	0.3
Percent	3.0	-6.4	-4.8	-1.9
Daytime				
Estimate	3.5	-10.8	-8.0	-31.4
T-ratio	0.3	0.7	0.5	0.3
Percent	2.2	-4.3	-3.6	-2.3

[1] P<0.01, two-tailed test.

[2] Series logarithmically transformed to reduce heteroscedasticity; estimate based on transformed series.

estimates were not significant using the .01 probability level chosen for this study. However, the small monthly crash counts for 16-17-year-olds in Maine, and the resulting large random component in the time series may mask the effect of the raised drinking age on underage drinkers. Examination of the raw frequency plots (Appendix A, Figures 2 and 4) indicated a decline in crash involvement among underage drinkers for the first year after the drinking age was raised; however, the crash frequency series resumed the long-term upward trend in 1979.

Analyses of serious crashes, that is, those involving at least one injury or fatality, revealed no significant shifts in alcohol-related crash involvement among Maine drivers age 18-19 (Table 5.4). Similarly, there were no measurable changes in the frequency of serious crashes for Maine drivers age 22-45, and no changes in non-alcohol-related serious crash involvement among Maine drivers age 20-21. However, 18-19-year-old drivers were involved in 24% more daytime serious crashes after October 1977 than previous trends indicate would be expected.[22] Maine drivers age 20-21 experienced a significant increase in alcohol-related crashes, averaging 4.9 drivers per month (21.4%) for the police-reported drinking series, and 2.4 drivers per month (14.2%) for the single-vehicle nighttime male series.

A significant increase in serious daytime crash involvement among 18-19-year-old drivers in Maine, with no significant change in serious single-vehicle nighttime male crash involvement, might indicate that the raised drinking age prevented an increase in single-vehicle nighttime male crash involvement that would have occurred had there been no legal change. Such a conclusion is strengthened by finding a significant increase in alcohol-related crash involvement for drivers age 20-21, the proximal peers of the focal age group. In addition, although serious single-vehicle nighttime male crash involvement among drivers age 18-19 showed no statistically significant change, the time-series model estimation results indicate an 18.4% decrease when the drinking age was

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[22] Note that daytime crash involvement for 18-19-year-olds and 20-21-year-olds decreased significantly in 1979, as was the case in Michigan (see Appendix A, Figure 15 and 23).

TABLE 5.4
Time-series Model Estimates of Changes in Injury/Fatal Crash Involvement Associated With Raising the Legal Minimum Drinking Age in Maine

Type of Crash	Age of Driver			
	16-17	18-19	20-21	22-45
Alcohol-related Indicators				
Police-reported Drinking				
Estimate	2.7	1.9	4.9	3.2
T-ratio	1.6	0.8	2.8[1]	0.8
Percent	30.5	7.9	21.4	3.5
Single-vehicle Nighttime Male				
Estimate	0.4	-3.2	2.4	2.5
T-ratio	0.3	1.4	2.8[1]	0.9
Percent	2.8	-18.4	14.2	4.4
Non-alcohol-related Indicators				
No Police-reported Drinking				
Estimate	0.8	-1.8	-1.4	-18.3
T-ratio	0.2	0.2	0.2	0.7
Percent	1.1	-1.5	-1.4	-3.5
Daytime				
Estimate	2.1	19.0	5.7	-26.4
T-ratio	0.5	4.4[1]	1.1	1.0
Percent	3.2	24.0	7.2	-5.4

[1] $P<0.01$, two-tailed test.

raised. Because the magnitude and direction of the estimated (non-significant) change in single-vehicle nighttime male crashes among drivers age 18-19 is consistent with hypothesized drinking age effects, one might interpret the results as evidence that Maine's raised drinking age affected serious crash involvement. Given the tenuousness of the argument concerning single-vehicle nighttime male and daytime serious crashes, and no changes in serious police-reported drinking crashes or serious crashes with no police-reported drinking, the safest conclusion is that there was no clearly demonstrable effect of Maine's raised drinking age on injury and fatal alcohol-related crash involvement among 18- and 19-year-old drivers.

As in Michigan, there was also no effect of the raised drinking age on injury/fatal crash involvement among 16-17-year-old Maine drivers.

5.1.3 New York. The same time-series analysis strategy used for Maine and Michigan was applied to New York, to determine whether youthful alcohol-related crash involvement in that state decreased significantly at the time either Maine or Michigan raised the legal minimum drinking age. If New York, which did not change its drinking age, showed decreases in crash involvement similar to Maine or Michigan, one might conclude that observed crash reductions in the experimental states were due to some factor unrelated to the raised drinking age.[23]

Estimated changes in New York property damage crash involvement beginning November 1977 (when Maine raised the drinking age) are summarized in Table 5.5. There were no significant changes in any of the crash involvement categories for 18-20-year-old drivers, in contrast to Maine, where 18-19-year-old drivers experienced significant reductions in alcohol-related property damage crashes beginning in November 1977. Note that the parameter estimates for single-vehicle nighttime male and daytime single-vehicle male crashes were negative for all age groups, and were significant for drivers 21-23 and 24-45. Furthermore, the reported crash reductions were substantial, ranging

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[23] As noted earlier, the New York crash data were analyzed twice, first including the entire state as was done for other study states (Appendix C), and second, excluding New York City from the statewide totals (Appendix K). Exclusion of New York City did not appreciably change the results, and results discussed here are based on the statewide totals excluding New York City.

from 30 to 52%. This pattern of lower than expected reported property damage crash involvement across all age groups for both daytime and nighttime single-vehicle male driver crashes was a result of an important change in crash reporting criteria, mentioned earlier in section 4.2.2.3. After September 1978, any crash causing property damage of $400 or more had to be reported; prior to this date, any crash causing $250 or more in damage had to be reported. This instrumentation change, occurring within the first year after the October 1977 drinking age change in Maine, apparently caused the significant reduction in reported property damage crashes identified in the New York time-series models.

Estimated changes in New York injury and fatal crash involvement, concomitant with Maine's increase in drinking age, are shown in Table 5.6. As with property damage crashes, there were no significant changes in any crash category for 18-20-year-old drivers. The other three age groups experienced significant increases in police-reported alcohol-involved crashes. Because of the nature of the New York reporting system concerning alcohol-related crashes,[24] the frequency of reported alcohol-related crashes using this indicator is probably quite sensitive to increased public interest in the alcohol-related crash problem. In recent years, more media and public policy attention has been given to the alcohol/highway safety issue, and as a result, observed increases in police-reported alcohol-related serious crash involvement may very well be due to improved reporting of the involvement of alcohol in New York crashes.

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[24] Refer to section 4.2.2.3 for details.

TABLE 5.5
Time-series Estimates of Changes in Property-damage-only Crash Involvement in New York After October 1977, When Maine Raised the Legal Minimum Drinking Age

Type of Crash	Age of Driver			
	16-17	18-20	21-23	24-45
Alcohol-related Indicators				
Police-reported Drinking				
Estimate	0.2	3.5	1.6	12.0
T-ratio	0.2	2.1	0.9	2.5
Percent	3.1	12.7	6.9	15.3
Single-vehicle Nighttime Male				
Estimate	-0.7[2]	-0.3[2]	-0.4	-0.7[2]
T-ratio	3.7[1]	1.5	6.7[1]	4.1[1]
Percent	-52.3	-23.7	-29.5	-47.8
Non-alcohol-related Indicator				
Daytime Single-vehicle Male				
Estimate	-0.3[2]	-0.3[2]	-0.4[2]	-0.6[2]
T-ratio	2.5	1.9	3.7[1]	4.7[1]
Percent	-28.8	-22.9	-32.3	-45.1

[1] P<0.01, two-tailed test.

[2] Series logarithmically transformed to reduce heteroscedasticity; estimate based on transformed series.

The New York time-series models also included estimates of shifts in crash involvement beginning January 1979, for comparison with the Michigan findings. Results for property damage crashes are summarized in Table 5.7. No significant reductions in property damage alcohol-related crash involvement occurred among New York 18-20-year-old drivers in 1979, in contrast to significant decreases in Michigan. This finding provided further support for the hypothesis that the crash reductions in Michigan were a direct result of the higher drinking age, not other more

TABLE 5.6
Time-series Model Estimates of Changes in Injury/Fatal Crash Involvement in New York After October 1977, When Maine Raised the Legal Minimum Drinking Age

Type of Crash	Age of Driver			
	16-17	18-20	21-23	24-45
Alcohol-related Indicators				
Police-reported Drinking				
Estimate	5.3	5.4	21.5	27.5
T-ratio	4.0[1]	0.4	5.0[1]	2.8[1]
Percent	18.1	3.1	18.0	7.3
Single-vehicle Nighttime Male				
Estimate	5.6	-3.4	-5.3	7.9
T-ratio	1.7	0.4	0.6	0.5
Percent	10.5	-1.3	-3.0	2.3
Non-alcohol-related Indicator				
Daytime Single-vehicle Male				
Estimate	-3.4	-0.1	-1.0	-32.7
T-ratio	0.7	0.0	0.2	7.1[1]
Percent	-3.9	-0.5	-0.9	-8.9

[1] P<0.01, two-tailed test.

general nationwide factors influencing crash involvement during this period.

The only significant time-series model estimates in Table 5.7 were for single-vehicle nighttime male crashes among drivers age 21-23, and daytime single-vehicle male crashes among drivers age 18-20. In both of these cases, the significant positive time-series model estimates were a result of the leveling off a pre-existing downward trend in crash frequency (see Appendix K, Figures 18 and 12).

Table 5.8 shows estimated changes from pre-existing trends in New York serious crash involvement in 1979. Again, no significant decreases

TABLE 5.7
Time-series Model Estimates of Changes in Property-damage-only Crash Involvement in New York After December 1978, When Michigan Raised the Legal Minimum Drinking Age

Type of Crash	Age of Driver			
	16-17	18-20	21-23	24-45
Alcohol-related Indicators				
Police-reported Drinking				
Estimate	2.0	-1.5	4.2	2.1
T-ratio	1.6	0.8	1.9	0.5
Percent	34.6	-4.7	16.4	2.6
Single-vehicle Nighttime Male				
Estimate	0.0[2]	0.3[2]	0.3[2]	0.1[2]
T-ratio	0.1	1.7	7.2[1]	0.0
Percent	3.0	35.0	35.0	6.2
Non-alcohol-related Indicator				
Daytime Single-vehicle Male				
Estimate	0.0[2]	0.4[2]	0.0[2]	0.1[2]
T-ratio	0.2	3.1[1]	0.0	0.6
Percent	2.0	46.2	4.1	7.2

[1] P<0.01, two-tailed test.

[2] Series logarithmically transformed to reduce heteroscedasticity; estimate based on transformed series.

in injury/fatal crash frequency for 18-20 drivers was found, in contrast to the Michigan results, providing further evidence that Michigan's crash reductions among youth were due to the increase in drinking age.

Significant increases in police-reported alcohol-involved crashes for three of the four age groups might be due to improved police reporting of alcohol involvement, the suggested explanation of the increased frequency of police-reported alcohol-involved property damage crashes in New York identified with the November 1977 time-series model parameter. Unlike the pattern of results in Table 5.6, however, Table

TABLE 5.8
Time-series Model Estimates of Changes in Injury/Fatal Crash Involvement in New York After December 1978, When Michigan Raised the Legal Minimum Drinking Age

Type of Crash	Age of Driver			
	16-17	18-20	21-23	24-45
Alcohol-related Indicators				
Police-reported Drinking				
Estimate	11.2	23.7	31.1	56.8
T-ratio	7.0[1]	2.3	6.6[1]	5.0[1]
T-ratio	7.0[1]	2.3	6.6[1]	5.0[1]
Percent	33.1	11.9	21.2	13.7
Single-vehicle Nighttime Male				
Estimate	3.2	32.7	24.3	59.6
T-ratio	0.9	3.6[1]	2.6	4.6[1]
Percent	5.2	12.2	14.0	17.5
Non-alcohol-related Indicator				
Daytime Single-vehicle Male				
Estimate	1.1	7.0	4.0	7.6
T-ratio	0.2	1.0	1.0	1.5
Percent	1.2	4.7	3.7	2.2

[1] P<0.01, two-tailed test.

5.8 reveals that the more reliable alcohol-related indicator, single-vehicle nighttime male crash involvement, also exhibited significant increases for the 21-23 and 24-45 age groups, and an estimated increase for 18-20-year-olds that was significant at the less conservative .05 level. The pattern of consistently positive point estimates, with five of the eight significant at the .01 level, suggests that a real increase in injury/fatal alcohol-related crash involvement occurred in 1979 among New York drivers.

5.1.4 Pennsylvania. Crash involvement in Pennsylvania from 1972 through 1979 was also analyzed, for comparison with the Michigan and

Maine results. The police-reported alcohol indicator was not well suited for this research. Few crashes were identified as alcohol-related, and for those that were, the alcohol item applied to the accident, and could not be linked to particular drivers. As a result, frequencies of police-reported alcohol-involved crashes were not analyzed in detail, although a plot of each time series is included in Appendix D. Instead of detailed analyses of frequencies of crashes with no police-reported alcohol involvement, the total frequency of crash-involvement was examined.

Results, shown in Table 5.9, indicated that Pennsylvania drivers experienced no change in property damage crash involvement in 1977, at the time Maine raised its drinking age and experienced a significant decrease in alcohol-related property damage crash involvement among young drivers. Again, a state without a drinking age change, Pennsylvania, experienced no significant change in youth crash involvement at the time a state that raised its drinking age, Maine, experienced significant decreases in alcohol-related crashes.

Analyses of Pennsylvania injury crashes revealed no significant changes beginning in 1977 for any of the crash category/age group combinations (Table 5.10).

When Michigan raised its drinking age and experienced significant decreases in alcohol-related property damage crash involvement among young drivers, Pennsylvania showed no change in alcohol-related property damage crash involvement among youth (Table 5.11). Daytime property damage crashes in Pennsylvania among drivers of all ages were down 9 to 17% in 1979, however. Similar reductions in non-alcohol-related crashes across all age groups were found in Michigan, indicating that some more

TABLE 5.9
Time-series Model Estimates of Changes in Property-damage-only Crash Involvement in Pennsylvania After October 1977, When Maine Raised the Legal Minimum Drinking Age

Type of Crash	Age of Driver			
	16-17	18-19	20-21	22-45
Alcohol-related Indicator				
Single-vehicle				
Nighttime Male				
Estimate	0.2	-17.2	-12.7	-77.1
T-ratio	0.0	0.8	0.8	1.6
Percent	0.1	-8.9	-7.3	-12.6
Non-alcohol-related Indicator				
Daytime				
Estimate	-0.1[1]	-0.0[1]	-0.1[1]	-0.0[1]
T-ratio	1.9	0.5	0.7	0.3
Percent	-12.2	-3.4	-4.8	-2.1
Total				
Estimate	-0.1[1]	-0.1[1]	-0.0[1]	-0.0[1]
T-ratio	1.7	0.8	0.5	0.3
Percent	-10.6	-5.0	-2.9	-2.0

[1] Series logarithmically transformed to reduce heteroscedasticity; estimate based on transformed series.

general factor affecting crash frequencies in multiple states, such as reduced economic activity in 1979, may be the cause of reduced non-alcohol-related crash involvement. Further support for such an argument can be found in the Maine results, where non-alcohol-related property damage crash involvement also decreased significantly in 1979 (see Appendix A). Of the four states analyzed here, only New York did not exhibit obvious decreases in non-alcohol-related property damage crash involvement in 1979.

TABLE 5.10
Time-series Model Estimates of Changes in Injury/Fatal Crash Involvement in Pennsylvania After October 1977, When Maine Raised the Legal Minimum Drinking Age

Type of Crash	Age of Driver			
	16-17	18-19	20-21	22-45
Alcohol-related Indicator				
Single-vehicle				
Nighttime Male				
Estimate	0.1[1]	-0.0[1]	-0.2[1]	-0.0[1]
T-ratio	0.7	0.1	2.1	0.5
Percent	-7.5	-0.6	-14.8	-3.4
Non-alcohol-related Indicator				
Daytime				
Estimate	20.8	31.7	-6.0	133.7
T-ratio	0.5	0.5	0.1	0.5
Percent	3.1	3.7	-0.7	2.8
Total				
Estimate	34.5	25.1	-76.5	170.1
T-ratio	0.5	0.3	1.2	0.5
Percent	-3.5	1.9	-5.7	2.6

[1] Series logarithmically transformed to reduce heteroscedasticity; estimate based on transformed series.

Analyses of serious crash involvement in Pennsylvania revealed no significant changes in 1979 for any of the crash category/age group combinations (Table 5.12). Finding no change in alcohol-related serious crash involvement among Pennsylvania youth in 1979, when Michigan had significant decreases in youthful serious alcohol-related crash involvement strengthens a causal interpretation of the drinking age/crash association observed in Michigan.

5.1.5 Discussion of Crash Findings. The time-series model estimates represent the net shift in the frequency of crash involvement

TABLE 5.11
Time-series Model Estimates of Changes in Property-damage-only Crash Involvement in Pennsylvania After December 1978, When Michigan Raised the Legal Minimum Drinking Age

Type of Crash	Age of Driver			
	16-17	18-19	20-21	22-45
Alcohol-related Indicator				
Single-vehicle				
Nighttime Male				
Estimate	-11.8	-20.7	12.7	42.4
T-ratio	0.7	1.0	0.8	0.9
Percent	-11.0	-10.8	8.4	8.4
Non-alcohol-related Indicator				
Daytime				
Estimate	-0.2[2]	-0.2[2]	-0.2[2]	-0.1[2]
T-ratio	3.8[1]	3.1[1]	3.3[1]	2.1
Percent	-16.6	-14.4	-16.5	-9.1
Total				
Estimate	-0.2[2]	-0.1[2]	-0.1[2]	-0.1[2]
T-ratio	3.3[1]	2.6	2.5	1.3
Percent	-13.9	-10.7	-10.2	-5.5

[1] P<0.01, two-tailed test.
[2] Series logarithmically transformed to reduce heteroscedasticity; estimate based on transformed series.

associated with increases in the legal minimum drinking age in Maine and Michigan, independent of the effects of long-term trends, seasonal cycles, and other factors. Alcohol-related property damage crash involvement among young drivers after an increase in drinking age was significantly lower than previous trends would lead one to expect in both Maine and Michigan. Significant reductions in alcohol-related property damage crash involvement were not found for older drivers within either experimental state, or for young drivers in the two comparison states that did not change the drinking age. Furthermore,

TABLE 5.12
Time-series Model Estimates of Changes in Injury/Fatal Crash Involvement in Pennsylvania After December 1978, When Michigan Raised the Legal Minimum Drinking Age

Type of Crash	Age of Driver			
	16-17	18-19	20-21	22-45
Alcohol-related Indicator				
Single-vehicle				
Nighttime Male				
Estimate	0.0[1]	0.1[1]	0.1[1]	0.1[1]
T-ratio	0.5	0.7	1.1	1.9
Percent	3.9	5.1	4.6	9.0
Non-alcohol-related Indicator				
Daytime				
Estimate	-52.6	-20.3	-8.3	64.1
T-ratio	1.2	0.3	0.2	0.2
Percent	-8.5	-2.6	-1.2	1.5
Total				
Estimate	-104.0	-27.7	-91.3	108.0
T-ratio	1.6	0.4	1.4	0.3
Percent	-11.3	-2.2	-7.4	1.7

[1] Series logarithmically transformed to reduce heteroscedasticity; estimate based on transformed series.

identified reductions in non-alcohol-related property damage crashes during this period were less than half the magnitude of the decrease in alcohol-related crashes. The same analyses applied to serious (i.e. injury and fatal) alcohol-related crash involvement demonstrated significant reductions among young drivers associated with the higher drinking age in Michigan, with no clear effect observed in Maine. Analyses of older drivers in Michigan and young drivers in the comparison states revealed no significant reductions in alcohol-related serious crashes. The most plausible explanation of this pattern of findings is that raising the legal drinking age caused significant reductions in alcohol-related property damage crash involvement among young drivers in both Michigan and Maine, and caused significant reductions in serious crash involvement in Michigan.

5.2 Effects of Raised Drinking Age on Beverage Alcohol Sales

In addition to analyses of crash involvement discussed above, aggregate alcoholic beverage sales were examined to identify whether the drinking age has a noticeable effect on the beverage alcohol market in Maine or Michigan. Beverage sales data for the United States as a whole were used as a comparison time-series. Finally, sales data for New Hampshire were also examined to assess whether observed reductions in Maine beverage sales were due to increased cross-border purchases rather than a reduction in actual consumption of alcoholic beverages.

5.2.1 Michigan. Results of analyses of aggregate beverage distribution in Michigan are summarized in Table 5.13 with a plot of each time series and detailed modeling results shown in Appendix H. Application of the iterative model building strategy to wine distribution in Michigan resulted in the model shown in Appendix H, Figure 1. Time-series modeling estimation results show that, controlling for the unexplained significant increase in October 1970, there were no significant changes in wine sales in 1972, after the drinking age was lowered, or 1979, after the drinking age was raised.

The final model for total beer distribution is shown in Figure H.2. From Table 5.13, it is evident that no significant change in total beer sales occurred in 1972, and a significant decrease of 5,384 kiloliters per month in total beer sales occurred beginning in January 1979, when a

TABLE 5.13
Time-series Model Estimates of Changes in Average Monthly Beverage Alcohol Sales in State of Michigan

Type of Beverage	Intervention	
	January 1972: Michigan Lowered Drinking Age	January 1979: Michigan Raised Drinking Age
Draft Beer		
Estimate	1321.4	1600.2
T-ratio	6.3[1]	11.9[1]
Percent	18.9	19.8
Package Beer		
Estimate	1049.2	-7658.7
T-ratio	0.6	4.3[1]
Percent	1.9	-11.5
Total Beer		
Estimate	1462.0	-5383.5
T-ratio	0.8	3.0[1]
Percent	2.4	-7.3
Wine		
Estimate	165.9	86.8
T-ratio	1.2	0.7
Percent	0.2	0.1

[1] $P<0.01$, two-tailed test.

mandatory beverage container deposit law and the raised drinking age went into effect.

Since package beer (Appendix H, Figure 3) accounts for more than three-fourths of all beer distributed in Michigan, the time-series model is very similar to the model for total beer. Again, no significant shift in package beer sales in 1972 was observed. However, a significant decrease in package beer sales of 7,659 kiloliters per month occurred over the January 1979 through October 1980 period.

The large one-month drop in wholesale package beer distribution in October 1978, evident in the time-series plot, was most likely a result of wholesale and retail dealers reducing their purchases in an attempt to eliminate their inventory of nonreturnable containers before the December deadline when the mandatory deposit law went into effect.

The 1979-80 decrease in package beer distribution is substantially larger than the decrease in total beer sales, leading one to suspect that part of the decline in package beer sales was offset by an increase in draft beer sales, which is confirmed by separate analyses of the draft beer series. Time series analyses produced the model of draft beer sales shown in Appendix H, Figure 4. An additional step function for 1980 was added to the model because draft beer sales increased substantially in 1980 over and above the increase in 1979. The results reveal a significant temporary increase in draft beer sales beginning in January 1972, when the drinking age was lowered. The effect dissipated by the end of 1972, however, and no significant permanent shift in draft beer sales associated with the lowered drinking age was observed (note the pattern of the 12-month moving average in Appendix H, Figure 4).

Results for 1979 and 1980 indicate dramatic increases in draft beer sales in Michigan. Distribution in 1979 was 1,600 kiloliters per month higher than expected given baseline patterns; the first 9 months of 1980 exhibited an additional 1,194 kiloliter per month increase. In spite of the large increases in draft beer distribution, total beer distribution was down significantly for 1979-80 because package beer, which decreased in 1979-80, accounts for over three-fourths of all beer distributed in the State of Michigan.

The significant increase in draft beer distribution in 1972, immediately after the drinking age was reduced from 21 to 18, can be interpreted as a result of the sudden expansion in the population of legal drinkers. Why the positive shift in draft beer sales rapidly decayed is not clear. One might speculate that the effect of the reduced drinking age was a sudden experimentation with consumption of draft beer, with the novelty wearing off after the first year, followed by a return to drinking patterns practiced before the drinking age was lowered. This argument seems less plausible, however, given the permanent significant increases in alcohol-related public health problems, such as traffic collisions, following reductions in drinking age.

A plausible hypothesis is that other exogenous factors, unrelated to the lowered drinking age, caused draft beer sales to decrease in 1973. Since youth age 18-20 constitute only a fraction of the total beer drinking population, a substantial increase in consumption by the 18-20 age group could be easily masked by a small reduction in consumption by all other drinkers. Therefore, the significant increase in aggregate draft beer sales in 1972, concomitant with the lowered drinking age, may be offset by a small reduction in consumption among all drinkers in 1973, due to other unidentified factors.

The substantial decrease in package beer distribution in 1979-80, when the drinking age was raised and non-returnable containers were banned, also has multiple explanations. Although the raised drinking age may account for a portion of the decrease, the magnitude of the decrease suggests that other factors, affecting the entire population of package beer consumers, were operating. One such factor is the mandatory container deposit law implemented in Michigan the same month the drinking age was raised. One result of the container law was a 10 percent increase in the real price of package beer.[25] The price jump and the inconvenience of returnable containers may explain the decrease in sales. In addition to reduced consumption of package beer resulting from its increased cost (both direct monetary costs and in terms of inconvenience), some consumers residing near Michigan's borders may have shifted their package beer purchases to retailers in bordering states. However, since most of Michigan's population does not reside near the state's borders, they do not have easy access to adjoining states with non-returnable containers where lower package beer prices prevail. As a result, it would appear that cross-border purchases do not account for the entire decrease in package beer sales in Michigan. This is not to say cross-border purchase of beer does not account for a significant portion of the observed decrease in Michigan package beer sales, since the densely populated metropolitan Detroit area is less than an hour drive from the Ohio border, where non-returnable containers are sold. In 1981 the Michigan Commerce Department announced a crackdown on persons crossing the State's borders to purchase package beer, arguing that a smuggling "epidemic" was harming Michigan business (Detroit Free Press, 1981).[26]

Given substantial increases in draft beer distribution during the same period, it appears that a number of beer drinkers shifted some of

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[25] Real price refers to price in constant dollars, controlling for the effects of inflation.

[26] The political debate surrounding the implementation of the mandatory container law included discussion as to whether the beverage industries were justified in raising prices; it is simply noted here that retail prices did, in fact, increase.

their consumption from package to draft beer. Michigan's economic recession, with associated unemployment and reductions in disposable income, may have contributed to the shift to less expensive draft beer.[27]

Michigan's raised drinking age may have had a larger effect in reducing package beer consumption among youth than draft beer consumption because the law did not prohibit young people age 18-20 from patronizing public drinking establishments; only purchase and consumption of alcoholic beverages were prohibited. In a crowded bar or tavern a legal drinker might easily purchase alcoholic beverages for friends under age 21. As a result, draft beer sales may be higher than expected if those under 21 were not allowed to frequent public drinking establishments. In addition, part of the increased draft beer sales in 1979-80 may be a result of an increased number of "kegger" parties among 18-20-year-olds, where a legal drinker purchases a keg of draft beer for consumption off-premise at a party attended by underage drinkers.

5.2.2 Maine. Results of analyses of aggregate beverage alcohol distribution in Maine are summarized in Table 5.14, and a plot of each time series and detailed modeling results are shown in Appendix G. Application of the iterative model building strategy to the Maine beer distribution time series resulted in the model shown in Appendix G, Figure 2. Controlling for long-term trends and seasonal cycles, there was no significant change in beer sales associated with the reduction in

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[27] A telephone survey of a random sample of Michigan bars, taverns, and restaurants revealed that draft beer has a significantly lower retail price than than any other on-premise alcoholic drink. Depending on region of the state, retail price of draft beer was 17 to 40 percent lower than on-premise package beer, usually the second least expensive drink (Douglass et al., 1980).

drinking age. However, there was a significant decrease in total beer sales averaging 1,114 kiloliters per month (11.8%) after the drinking age was raised. The lower than expected sales after November 1977 are clearly evident in the time series plot.

TABLE 5.14
Time-series Model Estimates of Changes in Average Monthly Beverage Alcohol Sales in State of Maine

Type of Beverage	Intervention	
	June 1972: Maine Lowered Drinking Age	November 1977: Maine Raised Drinking Age
Total Beer		
Estimate	179.4	-1114.2
T-ratio	1.1	6.8[1]
Percent	2.6	-12.9
Wine		
Estimate	-14.2	46.9
T-ratio	0.7	2.5
Percent	-4.1	10.2
Spirits		
Estimate	-23.3	25.7
T-ratio	1.2	1.3
Percent	-3.7	3.6

[1] P<0.01, two-tailed test.

No change in wine sales at the time the drinking age was lowered was identified. As shown in Table 5.14, the model estimation results indicate an increase in wine sales of 10.2% after the drinking age was raised in late 1977; the increase is significant at p<.05, but not at p<.01, the significance level selected for this study. Consistent with the marginal significance of the estimate, examination of the wine sales plot (Appendix G, Figure 1) indicates that wine sales after October 1977 were not dramatically different than expected, given the long-term upward trend in wine sales.

The time series model for spirits sales in Maine (Appendix G, Figure 3) revealed no measurable changes associated with either lowering or raising the legal drinking age.

One might conclude, on the basis of these findings, that the raised drinking age in Maine resulted in significantly reduced alcohol consumption among youth, since sales of beer, the beverage of choice among young drinkers, decreased significantly after the raised drinking age was implemented. Although the interrupted time-series design employed here is characterized by high internal validity, a serious threat to a causal interpretation of the findings remaining is contemporaneous history, that other events occurring at about the same time as the raised drinking age may account for the observed decrease in beer sales. As in Michigan, the major confounding historical factor is a mandatory beverage container deposit law, implemented in Maine in January 1978, just two months after the higher drinking age went into effect (Maine, State of, 1977). Inconvenience of returnable containers, along with substantial increases in the price of canned and bottled beer may have caused the reduction in beer sales. Reduced sales, in turn, may have been due to reduced consumption and/or a shift from purchasing beer in Maine to purchases in neighboring states, particularly New Hampshire, where beer prices are substantially lower. Beer prices have traditionally been lower in New Hampshire than Maine because of lower tax rates. Maine's taxes on beer include excise tax of 25 cents per gallon and general sales tax of 5 percent; in contrast, New Hampshire has a 15 cent per gallon excise tax on beer and no sales tax (Distilled Spirits Council of the United States, 1980). According to the Maine state officials, the beer price differential has widened noticeably since implementation of the mandatory deposit law, with Maine residents increasingly purchasing the lower-priced New Hampshire beer.[28]

5.2.3 New Hampshire and United States as Comparison Jurisdictions.

The only change in alcohol distribution associated with lowered drinking ages in the two focal states of this investigation, Maine and Michigan, was a temporary increase in draft beer sales in Michigan in the first year after the lower drinking age was implemented. When the drinking age was raised, however, there were significant decreases in total beer sales in both Maine and Michigan. In Michigan, where beer sales were also examined separately for draft and package beer, the decrease in total beer sales in 1979-80 was due to a substantial decrease in sales of package beer, while sales of draft beer increased.

Interpretation of the changes in beer sales at the time these two states raised the drinking age is complicated by a major confounding historical factor in both states, implementation of mandatory beverage container deposit laws at about the same time the higher drinking ages went into effect.

In an attempt to separate the effects of the drinking age change and the beverage container deposit law, beverage sales in the State of New Hampshire were analyzed. New Hampshire is the only state bordering

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[28] Beer prices increased "considerably," after the mandatory container law was implemented, and consumers complained "bitterly" according to S. Redfield, Maine Department of Agriculture; personal conversation, June 1, 1981.

with Maine,[29] and Maine state officials have suggested that Maine beer drinkers may have shifted some of their beer purchases to New Hampshire after the container law was implemented and beer prices increased.[30] If such cross-border purchases account for the 1,114 kiloliter per month decrease in Maine beer sales concomitant with the increase in drinking age and implementation of the container law, New Hampshire beer sales should increase by a comparable amount. Time-series modeling of New Hampshire beer sales, however, reveal no significant change at the time the Maine container law was implemented (Table 5.15). As a result, one has more confidence that observed reductions in Maine beer sales are due to the increase in drinking age, and not the mandatory container deposit law.

New Hampshire lowered the legal minimum drinking age from 21 to 18 for all alcoholic beverages in June 1973. Parameters for this legal change were included in the time-series models for beer sales in New Hampshire, and results showed a significant 7.7% increase in package beer sales after the drinking age was lowered (Table 5.15). Draft beer sales were up an estimated 10.7% (significant at p<.05 but not at p<.01, using two-tailed tests). Based on these results, it appears that lowering the drinking age in New Hampshire contributed to an increase in sales of package and draft beer.[31]

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[29] Although there might be beer purchases across Maine's northern border with Canada, the great majority of Maine's population resides in the southern region of the state.

[30] S. Redfield, Maine Department of Agriculture, personal conversation June 1, 1981, and F. Robie, Maine Liquor Control Board, personal conversation, June 1, 1981.

[31] Effects of New Hampshire's increase in drinking age from 18 to 20 in May 1979 could not be evaluated because beer sales data were only available through 1979, providing only seven post-change observations.

TABLE 5.15
Time-series Model Estimates of Changes in Average Monthly Beverage Alcohol Sales in State of New Hampshire

Type of Beverage	Intervention	
	June 1973: New Hampshire Lowered Drinking Age	January 1978: Maine Mandatory Deposit Law
Draft Beer		
Estimate	67.8	11.3
T-ratio	2.5	0.8
Percent	10.7	1.5
Package Beer		
Estimate	539.7	258.6
T-ratio	2.8[1]	1.5
Percent	7.7	3.1

[1] $P<0.01$, two-tailed test.

Beer sales in the entire United States from 1970 through 1980 were analyzed for comparison with the results for Maine, Michigan, and New Hampshire (Table 5.16).[32] Results showed no significant changes in nationwide draft beer sales after January 1972, when Michigan lowered the drinking age and experienced a temporary increase in draft beer sales. Second, there were no changes in nationwide draft or package beer sales after November 1977, when Maine raised the drinking age and

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[32] The time-series models included parameters for January 1972 (Michigan lowered drinking age), November 1977 (Maine raised drinking age), and January 1979 (Michigan raised drinking age). Parameters for Maine and New Hampshire's drinking age reductions were not included because they occurred within a relatively short time. Inclusion of parameters for all of the drinking age reductions would have resulted in multicollinearity problems when estimating the parameters of the full time-series model (see Pindyck and Rubinfeld, 1976:66-68 for an introduction to the multicollinearity issue). In any event, the raw time-series plot with a simple 12-month moving average shown in Appendix J, Figures 2 and 3 can be examined for the exact nature of beer sales trends in the early 1970s.

experienced a 12.9% reduction in beer sales. Third, nationwide package beer sales did not change significantly after January 1979, when Michigan raised the drinking age and experienced a significant 11.5% package beer sales reduction. In each of these cases, nationwide beer sales showed no measurable change at a time when sales in a state that has changed the minimum drinking age changed significantly.

TABLE 5.16
Time-series Model Estimates of Changes in Average Monthly Beverage Alcohol Sales in the United States

Type of Beverage	Intervention		
	January 1972: Michigan Lowered Drinking Age	November 1977: Maine Raised Drinking Age	January 1979: Michigan Raised Drinking Age
Draft Beer			
Estimate	1045.8	-2765.2	11515.2
T-ratio	0.5	1.4	5.8[1]
Percent	0.6	-1.5	5.9
Package Beer			
Estimate	30431.0	17099.6	-13407.8
T-ratio	2.0	1.0	0.9
Percent	2.8	1.2	-0.9

[1] $P<0.01$, two-tailed test.

Total draft beer sales in the United States increased significantly beginning January 1979, when Michigan implemented the higher drinking age and also experienced a significant increase in draft beer sales. The pattern of sales of U.S. total draft beer in recent years was similar to that found in Michigan. That is, draft beer sales increased significantly in 1979 over expected sales given previous trends, with an additional significant increase in 1980 over the already higher than expected 1979 figures. However, nationwide draft beer sales in 1979 was only 5.9% higher than expected (Table 5.16), while Michigan draft beer sales increased 19.8% (Table 5.13). It is clear that part of the increased Michigan draft beer sales in recent years is a reflection of a nationwide trend. Significant increases in draft beer sales nationwide may be a result of such factors as the very successful introduction and marketing of low-calorie ("light") beers. The substantial increases in Michigan draft beer sales over the nationwide increases are likely due to factors unique to Michigan, such as the increase in drinking age, mandatory container deposit law, and unusually severe economic slowdown.[33]

5.2.4 Discussion of Sales Findings. Controlling for the effects of long-term trends and regular seasonal patterns, the present study found significant changes in aggregate sales of alcoholic beverages concomitant with modifications in the legal minimum drinking age. In all states examined, beer, the beverage of choice among young drinkers, was the beverage category most affected by changes in drinking age. In Michigan and New Hampshire, reductions in drinking age from 21 to 18 were associated with significant increases in beer sales. In Maine and Michigan, increases in drinking age from 18 to 20 and from 18 to 21, respectively, were associated with significant decreases in total beer sales. Results of analyses of nationwide beer sales for comparison with the state-specific analyses strengthen the argument that observed

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[33] Subtracting the 5.9% reduction in U.S. total draft beer sales from the 19.8% reduction observed in Michigan leaves a 13.9% reduction in Michigan that cannot be directly attributed to the factors causing the nationwide increase in draft beer sales.

relationships between drinking age and beer sales reflect a causal effect of the drinking age and not some other factors.

However, the results on beverage sales are not as unambiguous as the results of analyses of crash data. Simultaneous implementation of a higher drinking age and a mandatory beverage container deposit law in both Maine and Michigan complicates interpretation of the results. In addition, poor general economic conditions in the late 1970s and early 1980s in states such as Michigan may be influencing the beverage alcohol market in ways that are not yet fully understood. These complications illustrate limitations of analyses of aggregate sales data. Without age-specific consumption data, the differential effects of drinking age changes and other policy changes, such as the mandatory deposit law, cannot be unambiguously determined. Furthermore, detailed information on the drinking practices of various subpopulations, identified by stratification variables such as income level and employment status, is needed to assess the influence of economic conditions and beverage-specific price changes on individual drinking patterns.

5.3 Beverage Control Law Enforcement

In addition to detailed analyses of crash involvement and alcoholic beverage sales, the frequency of enforcement citations for violation of beverage control laws concerning selling or providing alcoholic beverages to underage persons in Michigan and Maine were briefly examined. The objective was simply to provide a preliminary impression of enforcement of the drinking age, focused on the providers of alcoholic beverages. Citations for underage individuals purchasing or consuming alcoholic beverages were not examined.

5.3.1 Michigan. The substantial effect of Michigan's higher drinking age in reducing alcohol-related crashes among youth in the first six months after the law was implemented was apparently not a result of strict enforcement of the law. In the early months after the increase in drinking age, enforcement was minimal, and the new law was "flagrantly" disobeyed, according to an advisory panel established by Michigan's governor (Grand Rapids Press, 1979). Increased enforcement efforts in late 1979 and 1980 were reflected in the frequency of citations issued by the Michigan Liquor Control Commission for selling, serving, or allowing minors to consume alcoholic beverages. Although the number of citations was 68% higher in 1979 than 1978, citation frequency in 1980 was 156% higher than 1978 (see Appendix F, Figure 1). These increased citation frequencies in 1979-80 were in contrast to the relatively constant 300-400 per year citation frequency in the mid-1970s. The effect of higher frequency of citations on alcohol-related crashes among youth in 1980 remains a question for further research.[34]

5.3.2 Maine. The frequency of citations brought before the Administrative Court in Maine for selling or allowing minors to consume alcoholic beverages are shown in Appendix E, Figure 1. Although the pattern of increase in citation frequency the first two years after the higher drinking age was implemented was similar to the Michigan experience, the magnitude of the increases were substantially smaller.

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[34] In spite of the increase in citations in 1980, underage youth were still able to acquire alcoholic beverages from licensed outlets. For example, of 41 underage youth arrested between April and September 1980 by an Oakland County (Michigan) special enforcement team for driving while intoxicated or driving under the influence of liquor, 24% reported they had been at a bar prior to arrest (Wolfe, 1981).

Citations for providing alcoholic beverages to underage persons in Maine increased 17% from 1977 to 1978, and 68% from 1977 to 1979. Based on these data, it appears that Maine also may have had minimal enforcement the first year after the new law was implemented, with increased enforcement efforts in the second year with the higher drinking age. One might also speculate that smaller increases in citation frequency in Maine compared to Michigan after the raised drinking age went into effect indicates less rigorous enforcement of the law in Maine. The lower enforcement efforts may account for the lack of a clear effect of the higher drinking age on injury crashes in Maine, in contrast to the significant effect of the increased drinking age on injury crashes in Michigan.

5.3.3 Discussion of Enforcement Findings. This very brief examination of enforcement of the legal minimum drinking age was based only on enforcement focused on the providers of alcoholic beverages, primarily off-premise retail outlets, bars and taverns, and restaurants. Enforcement of the drinking age focused on the individual underage drinker, and enforcement of laws against drinking-driving all would be expected to influence the frequency of alcohol-related crash involvement, the main dependent variable of the present study. The frequency of citations is also only a gross indicator of the level of enforcement, since information was not available on the number of citations in relation to the total number of citable violations, or the perceived risk of experiencing sanctions for providing alcohol to minors. The role of enforcement of drinking age laws is complex and deserves the attention of a separate study.

6.0 SUMMARY, DISCUSSION, AND RECOMMENDATIONS

6.1 Summary

The core issue of this investigation was whether raising the legal minimum age for purchase of alcoholic beverages has a significant effect in reducing alcohol-related motor vehicle crash involvement among young drivers, the leading cause of death for this age group. The findings are unambiguous; analyses of extended crash time series, comparing (1) alcohol-related with non-alcohol-related crashes, (2) young drivers with older drivers, and (3) states that raised the legal age with those that have not, demonstrate that significant reductions in alcohol-related crash involvement among young drivers result from increases in the minimum drinking age. Taking into account the results from analyses of multiple states, age groups, and indicators of alcohol involvement, the best estimates of the effects of the raised drinking age in Michigan and Maine are as follows. First, Michigan drivers age 18-20 experienced a net reduction of approximately 20% in the frequency of involvement in alcohol-related injury-producing crashes due to the higher drinking age. The 20% reduction means that about 1100 fewer young Michigan drivers were injured in the first 12 months with the higher drinking age than would have been expected had the legal age not been raised. Second, young Michigan drivers were involved in 17% fewer alcohol-related property damage crashes after the drinking age change, representing a reduction of about 1500 crash-involved drivers per year. Third, 18-19-year-old Maine drivers were involved in approximately 20% fewer alcohol-related property damage crashes after the drinking age was raised; that is, 75 fewer young drivers were involved in property damage crashes than one would have expected had the law not been changed. These crash reductions are causally attributable to the higher drinking ages because substantial decreases in crash involvement were limited to alcohol-related crashes among young drivers in states that raised the drinking age, with no comparable reductions in non-alcohol-related crashes among youth, crash involvement of older drivers within the same state, or crash involvement of young drivers in comparison states with unchanged drinking ages.

Although the public health benefits and reduced social costs resulting from injury and property damage reductions identified in this research are large, note that the benefits of higher drinking ages are understated, because reductions in injuries to passengers of young crash involved drivers have not been taken into account.

The conclusion that the legal minimum drinking age affects youth crash involvement is further strengthened by comparing results of this research on the raised drinking age with results of earlier research on effects of lowered drinking age. Douglass and Freedman (1977) analyzed a subset of Michigan jurisdictions with complete accident reporting over the 1968 through 1975 period, using a time-series design. Results revealed a 17% ($p<.06$) increase in total (i.e., property damage and injury producing) single-vehicle nighttime male crash involvement among drivers age 18-20 associated with the lowered drinking age in 1972. Police-reported drinking driver crash involvement increased 35% ($p<.01$) after the drinking age was reduced. Similar analyses for the State of Maine revealed a 29% ($p<.02$) decrease in reported alcohol-related crashes, and a 16% ($p<.10$) decrease in single-vehicle nighttime male crash involvement associated with Maine's reduction in drinking age from 20 to 18 in 1972 (Douglass et al., 1974). Comparisons between these earlier findings and results of the present investigation reveal that raising the drinking age reverses the effect of prior reductions in drinking age. Estimates of the increase in alcohol-related crash involvement among young drivers associated with Michigan and Maine's lowered drinking ages ranged from 16 to 35%, remarkably similar to the 11 to 34% range of estimates obtained in the present study of decreased alcohol-related crash involvement associated with raising the drinking age in these two states.

6.2 Discussion

Although the effect of the raised drinking age in reducing youthful auto crashes is now clearly documented, some caution is warranted before a blanket statement is made that any state raising the drinking age can count on a 20% decrease in youth crash involvement. The effect of higher drinking ages is not necessarily uniform across states. In this research, the effect in Michigan was larger and more obvious that the effect in Maine, particularly for the more serious, injury-producing crashes. As noted in section 3.2.2, two studies of fatal crash involvement in Massachusetts found no significant reductions due to an increase in drinking age from 18 to 20 (Hingson et al., 1981; Williams et al., 1981).

One possible reason for the lack of an effect in Massachusetts is that four of the five state bordering Massachusetts had minimum drinking aged of 18 for all alcoholic beverages after Massachusetts' higher drinking age was implemented.[35] The availability of beverage alcohol to

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[35] Vermont, New York, Connecticut, and Rhode Island permitted 18-year-olds to purchase all types of alcoholic beverages during the period for which the Massachusetts law was evaluated. New Hampshire increased

Massachusetts youth was not reduced as much as in other states that raised the drinking age, since Massachusetts youth had a legal supply of alcohol in contiguous states. Hingson et al.'s (1981) survey results provide some support for this line of reasoning, since underage Massachusetts youth reported little difficulty obtaining alcohol after the drinking age was raised. Evidence that contiguous states with differential minimum drinking ages create problems with cross-border purchases of alcohol by youth was provided by Lillis et al. (1981), who found that 18-20-year-old Pennsylvania residents were over-represented in alcohol-related traffic crashes occurring in New York counties contiguous with Pennsylvania.[36] Taking such cross-border problems into consideration, one might suggest the development of a nationwide consensus for a drinking age at 20 or 21, with uniform effective enforcement of the law across states. In any event, potential cross-border purchase of alcohol must be considered when evaluating effects of state-specific drinking age laws.

Another potential explanation of the lack of an observed effect of Massachusetts' higher drinking age is related to the data analyzed. In both studies where no effect of higher drinking ages was found (Hingson et al., 1981; Williams et al., 1981), the dependent variable, fatal crash involvement, had low frequencies. The number of alcohol-related crash fatalities among a limited age group within one state is relatively small for analysis purposes, and, as a result, the large random variation in the number of fatalities from month to month, or

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its drinking age from 18 to 20 in May 1979, just one month after the Massachusetts increase was implemented.

[36] The minimum drinking age is 21 in Pennsylvania and 18 in New York.

even year to year, makes it difficult to identify a significant effect of a policy change such as the drinking age. Even in Michigan, where substantial reductions in both injury-producing and property damage crash involvement due to the raised drinking age were clearly found, no significant effect of the raised drinking age was discernable when fatal crashes alone were analyzed (Wagenaar, 1980). The problems with low crash counts for analysis might also emerge for non-fatal crashes in less populous states like Maine, making it more difficult to detect any effect of policy changes. As a result, while evidence to date clearly demonstrates an effect of raising the drinking age, reductions in crashes and injuries may not always be clearly evident in less populous jurisdictions.

One implication of these findings for future evaluations of the drinking age or other public policy changes is that analyses should not be limited to fatalities only, but should also include the much larger numbers of injury and property damage crashes. Although the effort and costs associated with analyzing non-fatal crashes is substantially higher, such analyses may avoid incorrect conclusions that a policy change had no effect on the outcome of interest.[37]

In spite of the substantial effect of the raised drinking age in reducing alcohol-related crash involvement among young drivers, it is important to keep in mind that the drinking age does not eliminate this

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[37] Increased cost and effort required for analyses of non-fatal crash involvement is readily apparent when the number of fatally injured drivers is compared with the total number of crash involved drivers. For example, in 1979 about 2,500 Michigan drivers were fatally injured in crashes, while about 625,000 drivers were involved in reported crashes. Analyses of all crash-involved drivers over a multi-year period in several states, as was done in the present investigation, requires the processing of millions of crash records.