

# **JAIL SENTENCES FOR DRIVING WHILE INTOXICATED IN CHICAGO: A JUDICIAL POLICY THAT FAILED**

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Excessive drinking is involved in over one-half of the motor vehicle associated fatalities in the United States (Department of Transportation, 1968). Various means have been suggested, and some tried, to curb these losses. In only a few cases have countermeasures against driving while intoxicated been subjected to scientifically acceptable evaluation as to effectiveness.

In one such study, Barmack and Payne (1964b) used an information program with administrative review and psychiatric referral in an attempt to reduce alcohol-related crashes by airmen at an Air Force base. Based on their earlier studies (1964a) showing various types of stress-related and deviant behavior associated with driving while intoxicated, the image of the intoxicated driver as "sick" was communicated in meetings, on bulletin boards, and in the base newspaper. Airmen who lost time because of injury in a privately-owned vehicle had their service records reviewed, and evidence of "ineffective behavior" could result in discharge or psychiatric referral. The psychiatrist could then recommend a medical discharge, suggest psychiatric therapy, or take no action.

Comparison of the injury rate in the year preceding and the year of the program showed a significantly lower injury rate after three months of the program, and for the remainder of the year of the study. Comparison of the injury rate over the year of the study with national data and with comparable data from another Air Force base during the same time period

as the program showed a significantly lower rate on the Air Force base where the program was conducted.

These findings suggest that alcohol countermeasures can be effective. However, it is undoubtedly more difficult to initiate and administer a program such as this where people are not under as wide an administrative control as is the case in the military. The most commonly employed approach in civilian jurisdictions has been the imposition of legal sanctions through the police and court systems.

An example of good research to evaluate the effects of legal countermeasures is the use of the interrupted time series (Ross, *et al.*, 1970) to show successfully that alcohol countermeasures adopted by the British Government significantly reduced the incidence of fatalities during certain times of the day and week. The British Road Safety Act, effective October 9, 1967, authorized the police to subject a suspect to a breath test for alcohol *prior* to arrest. (The pre-arrest test has not been legislated in most U.S. jurisdictions although every state now has an "implied consent" law which is uniformly applied subsequent to arrest.) Those drivers with blood alcohol concentration above 0.08% by weight as indicated by a breath test were taken to a police station for further tests. If the first test was corroborated by a second breath test and, then, a blood or urine test, the driver was arrested and was subject to a one-year driving license suspension, and a fine of £100 or imprisonment for up to four months or both. The Act was widely publicized before it was put into effect. Ross, Campbell, and Glass showed that the level of the time series of fatalities and "serious casualties" was reduced significantly in England beginning in October 1967, particularly in late night hours when alcohol is most often involved in crashes (Department of Transportation, 1968). These findings have led to legislation providing for pre-arrest breath testing in a few jurisdictions in the United States (Insurance Institute for Highway Safety, 1971).

The purpose of this paper is to use the interrupted time series to study the effect on fatalities in Chicago of the highly publicized use of 7-day jail sentences as a countermeasure against driving while intoxicated during the winter and spring of 1971. Arrests and processing of cases are also examined. We emphasize that we were not involved in the planning or implementation of the Chicago "crackdown." This study is based on data collected from police, coroner, and court records.

## JAIL SENTENCES IN CHICAGO

In late December 1970 and the first six months of 1971, magistrates in Chicago's traffic courts were directed by the supervising judge to sentence persons convicted of driving while intoxicated (DWI) to seven days in jail and to recommend to the Secretary of State's office that such drivers' licenses be suspended for one year. This policy was publicly announced and widely publicized on December 15 and implemented beginning on December 18, 1970. The policy was at first announced as ending January 2, 1971, but subsequent extensions through mid-summer were announced based on claims that fatalities had been greatly reduced (Field, 1971). These claims were based on a percentage change in fatalities during the holiday period from the previous year.

The rationale for the Chicago crackdown was based on a study of convictions for DWI conducted by the Circuit Court's Psychiatric Institute. From April 6, 1970 to the end of July 1970, the first ten persons convicted of DWI each day were placed on probation and referred to the Circuit Court's Psychiatric Institute for examination and diagnosis. Of the cases examined, 8% admitted being "alcoholics" and an additional 12% were diagnosed as alcoholics. On the basis of the psychiatric examinations, it was concluded that "80% of the new arrests will involve social drinkers *only*" (Kelleher, 1970).<sup>1</sup> Chicago officials concluded that the fatalities involving alcohol resulted mainly from social drinking and that 7-day jail sentences for persons convicted of DWI would deter social drinkers from drinking an amount likely to involve them in fatal crashes (Field, 1971). Note that no attempt was made to change the method of detecting impaired drivers by police, as was provided in the British law. Indeed, Illinois at that time had no implied consent law providing penalties if a driver refused to be tested for alcohol after he was arrested.<sup>2</sup>

## INTERRUPTED TIME-SERIES ANALYSIS

The interrupted time-series model has been revised since its use on the British data to test for a change in upward or downward slope of the curve in addition to the changes in its general level as illustrated in the British data (Ross, *et al.*, 1970). Using the revised model, it is possible to test whether either of these types of changes in the time series can be attributed to chance.

A short-term change in a time series, such as the reduction

in fatalities observed by Chicago officials, may occur for a number of reasons. Events simultaneous with those intended to evoke the change, *e.g.*, favorable weather conditions, may actually produce the outcome. A long-term trend, unrelated to the action taken, may have developed and the short-term change may be only a part of it. The methods used to measure the outcome, *i.e.*, police statistics, may have changed or initial measurement, *e.g.*, publicity regarding a high crash rate prior to the action, may have produced the change. The assumed change may only be a chance variation or, in the case where the "before" observation was an extreme case, the change could reflect a statistical phenomenon called regression toward the mean (Ross, *et al.*, 1970). To attribute change to some action, such as jail sentences, one must rule out the possibility that one or more of these explanations account for the observed change.

To measure the possible effect of simultaneous events, we collected data from Milwaukee as well as Chicago to see if comparable trends were found in a neighboring city where there was no special program beyond the usual effort to curb driving while intoxicated in the period under study. Milwaukee is comparable to Chicago in weather conditions and police-court systems. Milwaukee, of course, has a smaller population, but this was controlled in part by calculating the fatality rate per 100,000 population.<sup>3</sup> There was no evidence of a change in the method of compiling the data in either city during the period studied.

### FATALITIES IN CHICAGO AND MILWAUKEE

Motor vehicle related fatalities per 100,000 population per month and various subcategories (pedestrians, etc.) were calculated for all such fatalities recorded by the police from January 1966 through June 1971. Changes in population from the 1960 and 1970 censuses were prorated monthly over the decade. Corrections for variation in length of month were made by multiplying the rate for a given month by 31 divided by the number of days in the month. A Fourier analysis, which reveals seasonal variation (Tintner, 1952: 217-22), was performed and no statistically significant seasonal variation was found in any of the data to be presented. Therefore, no seasonal corrections were made.

Total motor vehicle related fatalities per 100,000 population for Chicago are shown monthly by the darker curve in Figure 1. The vertical line indicates the time of the crackdown. Although the rates in the early months of 1971 are somewhat lower than the same months in previous years the statistical

FIGURE 1 CHICAGO FATALITY RATE

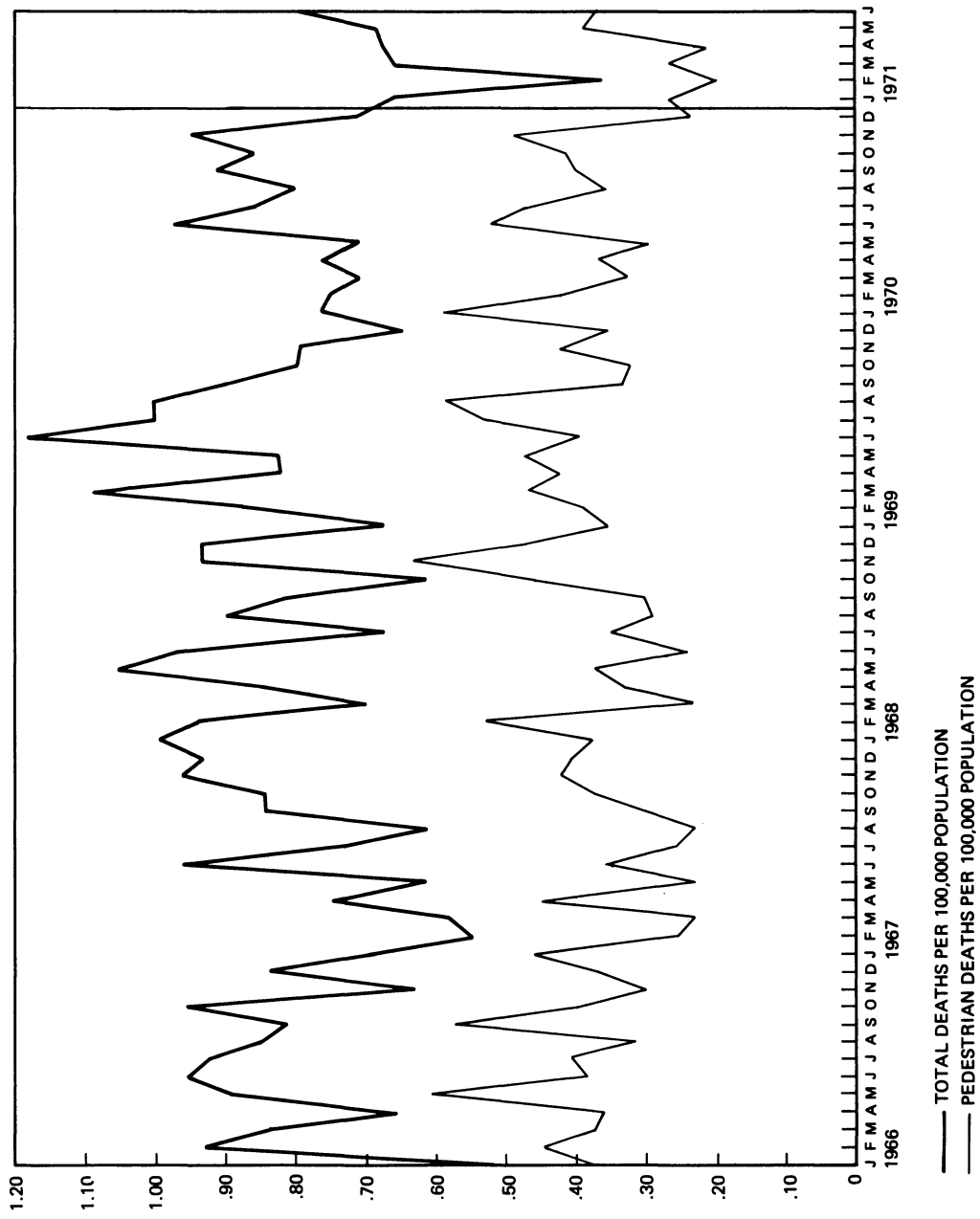
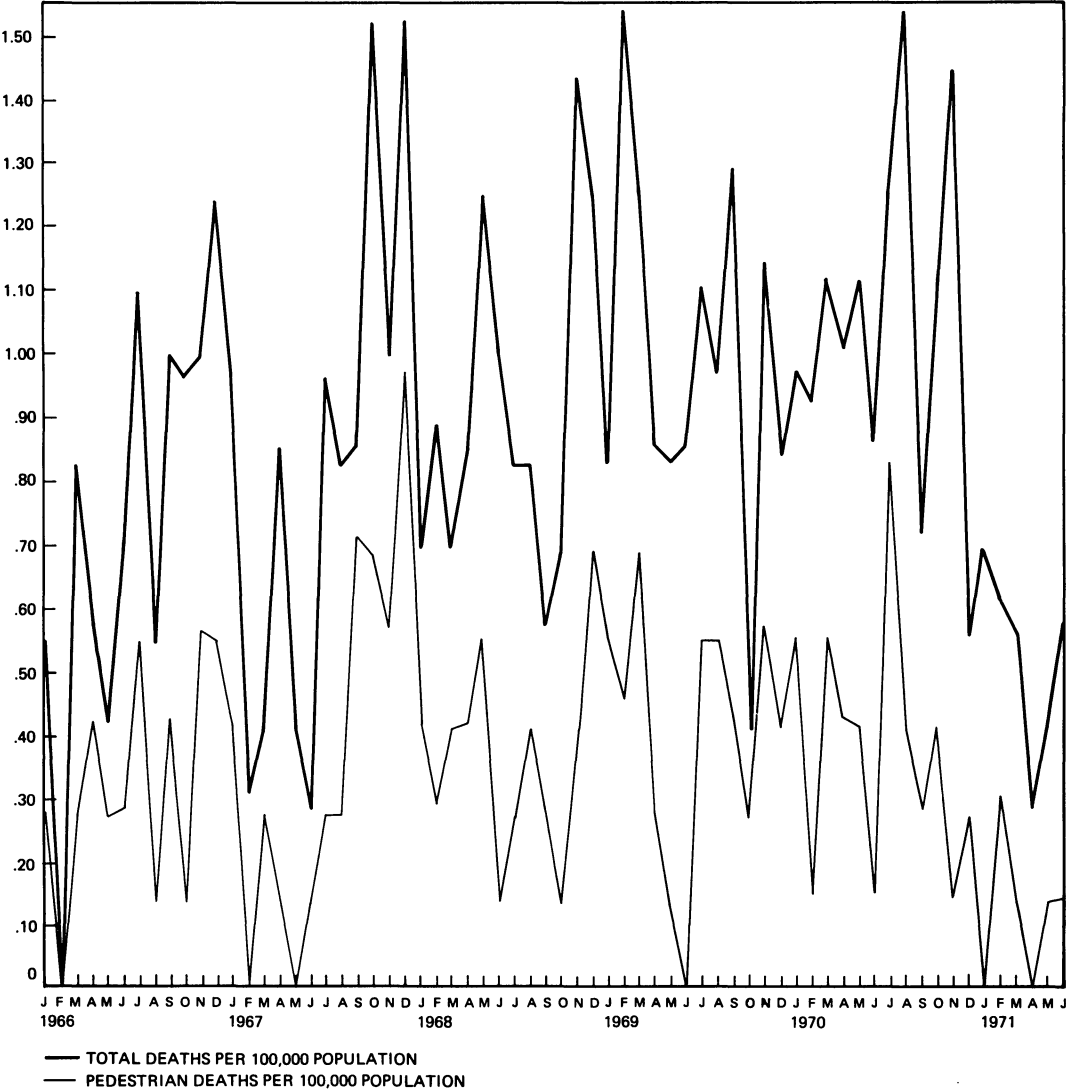


FIGURE 2 MILWAUKEE FATALITY RATE



analysis indicates that the difference between the series from December 1970 through June 1971, and that for the previous years could very commonly have occurred by chance. The statistical significance test shows that  $t$  for change in level was 0.87 ( $df = 62$ ,  $p > 0.20$ ) and the  $t$  for change in slope was 0.79 ( $df = 62$ ,  $p > 0.40$ ), clearly offering no reason to reject the null hypothesis that the change was a chance variation. The Milwaukee data, shown in Figure 2, indicate a much sharper drop in rate of fatalities after November 1970 than that in Chicago during the same period but the statistical test showed that this change, too, could very commonly have occurred by chance;  $t$  for change in level was 0.58 ( $df = 62$ ,  $p > 0.40$ ) and  $t$  for change in slope was 0.70 ( $df = 62$ ,  $p > 0.40$ ).

There appears to the eye to be a downward drift in the fatality rates in both cities beginning in 1969, some two years before the "crackdown." Comparison of the time series before and after January 1969 does reveal a statistically significant reduction in fatalities in both Chicago and Milwaukee after January 1969. However, examination of categories of types of fatalities showed that this reduction occurred for pedestrians only. The lighter curve in Figures 1 and 2 indicate pedestrian fatalities per 100,000 population in Chicago and Milwaukee respectively. There was a significant change in level of pedestrian fatalities in both cities after December 1968 ( $t=1.99$ ,  $df=62$ ,  $p < .05$  in Chicago;  $t=2.86$ ,  $df=62$ ,  $p < .01$  in Milwaukee) and a significant change in slope in Milwaukee ( $t=2.89$ ,  $df=62$ ,  $p < .01$ ) but not Chicago ( $t=1.71$ ,  $df=62$ ,  $p > .05$ ). The changes in nonpedestrian fatalities were within the bounds of usually accepted standards for chance variation.

### OTHER CHANGES DURING THE CRACKDOWN

There were at least 17 articles in Chicago newspapers and eight television newsfilms during the six months of the crackdown that were studied. However, there was no survey of public awareness or behavior during the crackdown so we cannot assess the degree to which the public knew about it or modified drinking or driving behavior. In light of the lack of demonstrated effect of the crackdown, one may question the degree to which a crackdown occurred and what repercussions, if any, the crackdown had on arrests and processing of cases. One study found that a crackdown on speeding in Connecticut had less effect on fatalities than on arrests and dispositions of cases (Campbell and Ross, 1968). Although data on actual sentences for DWI are not available, the number of 7-day sentences

FIGURE 3 CHICAGO ARRESTS FOR DWI

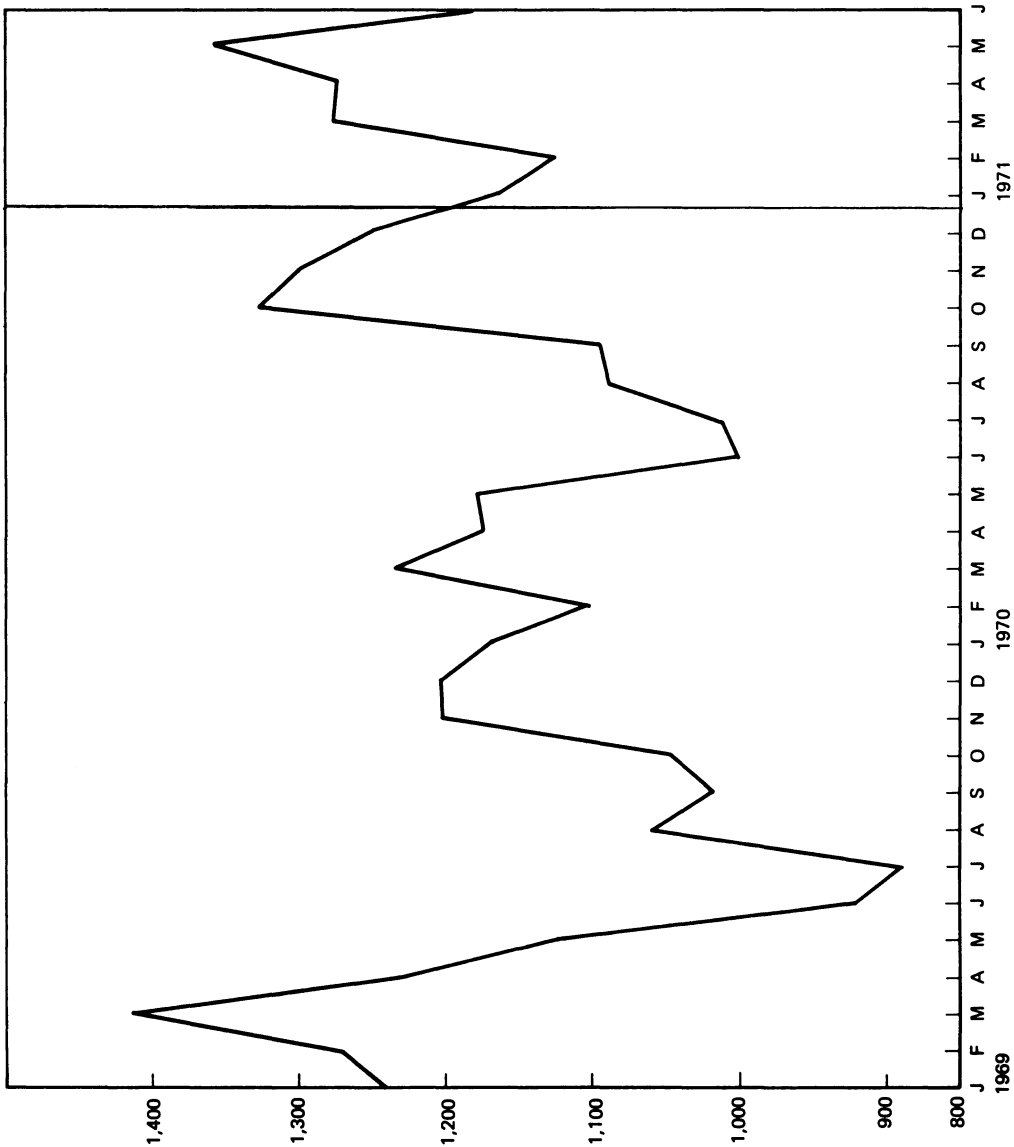
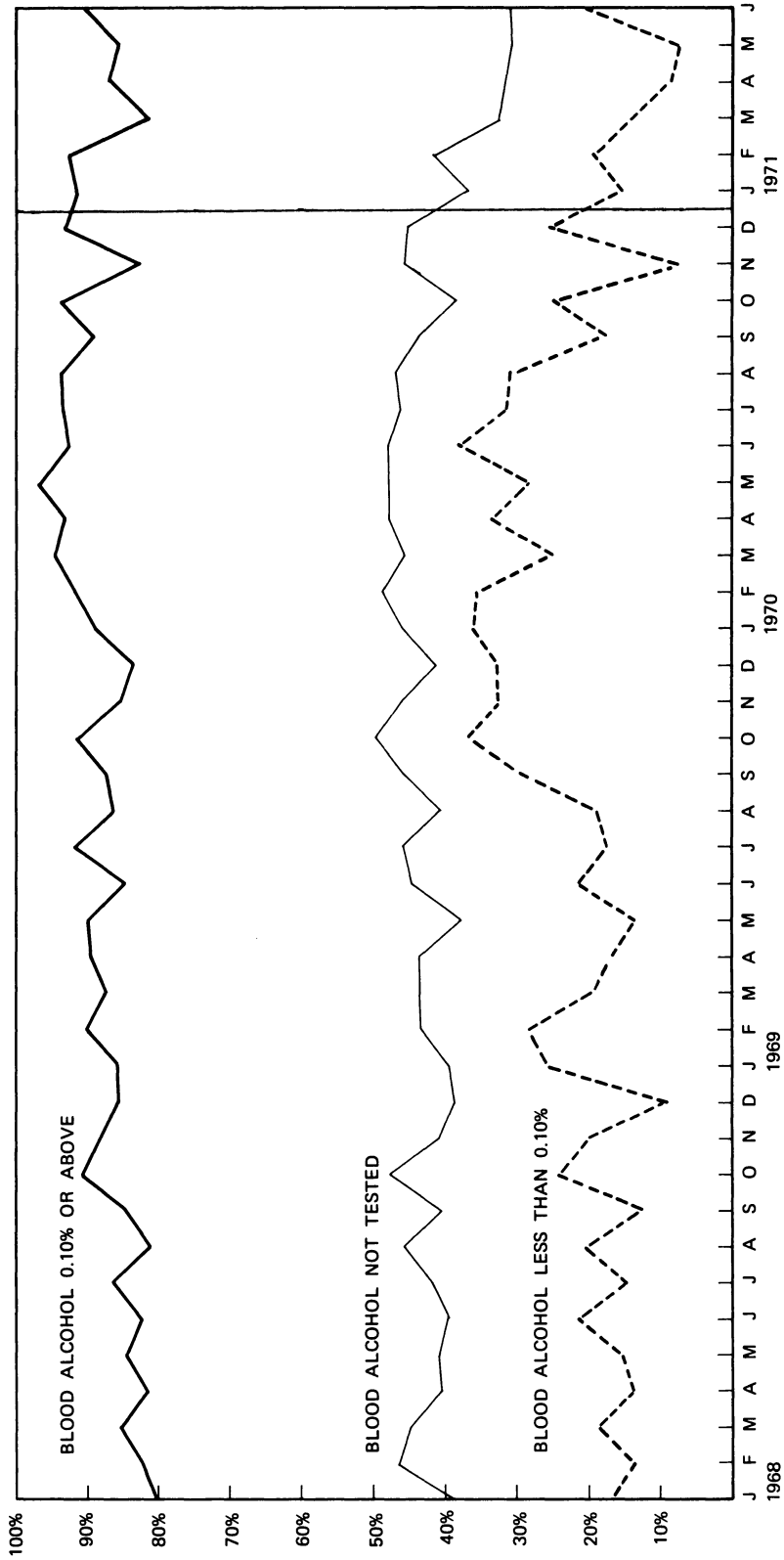




FIGURE 4 PERCENT OF ARRESTS THAT RESULTED IN CONVICTIONS FOR DWI BY BLOOD ALCOHOL CONCENTRATION



for all traffic-related offenses were 317, 357, and 557 for the first six months of 1969, 1970, and 1971 respectively. We were told by Chicago officials that "most" of these sentences were for DWI.

The time series of number of total arrests for DWI is shown in Figure 3. There is no statistically significant difference in level ( $t = 0.38$ ,  $df = 26$ ,  $p > 0.50$ ) or slope ( $t = 0.42$ ,  $df = 26$ ,  $p > 0.50$ ) between the pre- and post-crackdown periods. The time series of percentage of arrestees tested for blood alcohol concentration (not shown) was almost a flat line varying between 26% and 37%. Again, comparison of the series before and after the crackdown reveals no change in level ( $t = 0.52$ ,  $df = 26$ ,  $p > 0.50$ ) or slope ( $t = 0.57$ ,  $df = 26$ ,  $p > 0.50$ ). Figure 4 displays the percentage of those arrested who were found guilty categorized by blood alcohol concentration. The percent found guilty of those whose blood alcohol was less than 0.10% is shown by the bottom curve. The top curve represents the percent found guilty of those whose blood alcohol was 0.10% or greater. The percent found guilty of those not tested is shown by the middle curve. There was no significant change in the time series for those tested, whether below 0.10% ( $t$  for level = 1.65,  $df = 26$ ,  $p > 0.10$ ;  $t$  for slope = 1.76,  $df = 36$ ,  $p > 0.05$ ) or 0.10% or above ( $t$  for level = 1.87,  $df = 26$ ,  $p > 0.05$ ;  $t$  for slope = 1.94,  $df = 26$ ,  $p > 0.05$ ). There was a significant decrease in level ( $t = 4.02$ ,  $df = 26$ ,  $p < .01$ ) and slope ( $t = 4.20$ ,  $df = 26$ ,  $p < .01$ ) of convictions for those who were arrested but not tested.

## DISCUSSION

We conclude from the analysis that the change in motor vehicle fatalities that occurred during the Chicago crackdown on drivers convicted of driving while intoxicated was only a chance variation from the fatality rate over the preceding five years. However, there was a significant decrease in pedestrian fatalities beginning in 1969 both in Chicago and Milwaukee. By comparing 1971 with 1970 figures, Chicago officials mistakenly concluded that the decrease occurred because of the crackdown rather than because it was part of a more general downward trend which occurred outside Chicago as well.

The reason for the more general decrease in fatalities is unknown. There is a high correlation between various economic indicators and fatality rates (Joksch and Wuerdemann, 1970). Since there was an economic recession during the period of the decrease in pedestrian fatalities seen here, we can speculate that the search for the concomitants of economic indicators

that affect fatality rates should concentrate on pedestrian fatalities.

Why did the Chicago crackdown fail to influence the fatality rate significantly? The threat of 7-day jail sentences as a countermeasure against DWI involves a number of assumptions, any one of which may be incorrect. On the basis of a study of convicted DWIs, Chicago officials assumed that most drinking-related fatalities are caused by social drinkers. They also assumed, at least implicitly, that their sample was valid and that convicted DWIs are the same type of drinkers as those who get into fatal crashes.

The validity of the Chicago court's study of convicted DWIs is questionable because of the sampling employed and the oversimplification of its conclusion that the convicted DWIs can be divided into alcoholics and social drinkers. The sample may have been biased by taking the first 10 cases each day rather than sampling at random. Even if the sample were representative of convictions, the convicted drivers in the period sampled included 2% whose blood alcohol was below 0.10% and 45% whose blood alcohol was not tested. There is no way of knowing the degree to which such a sample statistically represents drivers who are involved in alcohol-related fatalities. In light of studies which indicate that there are a number of different types of problem drinkers (Calahan, 1970), and that problem drinkers are a major factor in fatalities (Department of Transportation, 1968), the simple classification of the sample into alcoholics and social drinkers suggests that either the sample was unrepresentative or the diagnoses were invalid.

Assuming that the publicity received by the crackdown was effective in reaching those who subsequently were involved in fatal crashes, it appears that the punitive threat of 7-day jail sentences did not deter them from getting into crashes. Ideally we would like to present the degree of alcohol involvement in these fatalities, but many of the fatally injured drivers involved in these crashes were not tested and some who survived up to 24 hours were tested more than six hours after the crash, which may result in a low estimate of alcohol involvement. Therefore, adequate data on alcohol involvement are not available. We can only speculate that, since the fatality rate did not change significantly during the crackdown, alcohol involvement remained the same as before the crackdown.

There are a number of points in the processing of violators of the law which may change as a result of changes at

other points. Arrest may increase or decrease. Plea bargaining may change the disposition of cases. Judges or juries may become more or less lenient in convictions and sentencing. Penal, rehabilitative, probation and parole arrangements may or may not change. At any one of these points the effect of a given change may be undermined (Campbell and Ross, 1968). If a particular effort fails, it is necessary to ask whether the effort itself is ineffective or whether it failed because of a change somewhere else in the system.

We found no evidence of a significant change in arrests for DWI. The number of drivers tested for blood alcohol after arrests also did not change significantly, and the proportion convicted of those tested did not change significantly. There was a significant decrease in convictions of those who were not tested. Since the number tested did not decrease, this change appears to be a result of changes in plea bargaining or reluctance of judges or juries to convict and sentence to seven days in jail those drivers for whom objective evidence of impairment was not available. If the drivers had perceived that they were less likely to be convicted by refusing the test, there should have been a decrease in the number tested, which did not occur.

Whether or not the Chicago crackdown would have been effective in reducing fatalities had it been given more publicity, been accompanied by more arrests, or included more convictions, is problematic. A major difference between the Chicago crackdown and the Lackland and British efforts is the greater emphasis on apprehension of the drinking driver in the latter two cases. It is reasonable to hypothesize that countermeasures which increase the probability of apprehension may deter a subset of drinkers from driving while impaired but that strictly punitive countermeasures will have little, if any, effect.

The general pattern of stress, deviancy, and multiple convictions in the backgrounds of a large proportion of persons convicted of driving while intoxicated and those involved in crashes after excessive drinking (Waller, 1967) suggests that a strictly punitive approach is not likely to be successful in deterring many of these persons from repeat performances. Most of these persons are known to the police and other community agencies. The collaboration and cooperation of the police, courts, social agencies in the community, and scientists knowledgeable in the multiple facets of the alcohol-impaired driver problem

will be required if effective countermeasures directed to identified subgroups of drinkers are to be developed.

If we, as a society, are to develop effective reforms and avoid repeating our mistakes, those who administer attempts at social reform must be convinced of the value of careful evaluation of the effort, from basic assumptions through final outcomes (Campbell, 1969). If we are to reduce the tragic losses incurred from drivers impaired by drinking, it is necessary that various countermeasures be tried and evaluated as to effectiveness. However, if we are not judicious in careful analysis of assumptions and the evidence on which they are based, we shall continue to mount campaigns which are costly in money, time, and effort and which preclude development and application of effective ones.

### FOOTNOTES

- <sup>1</sup> We reserve critique of this study and its conclusions for the discussion.
- <sup>2</sup> Such laws are required in order to meet the Highway Safety Program Standards issued in 1967 by the Secretary of Transportation as authorized by the Federal Highway Safety Act of 1966.
- <sup>3</sup> The product moment correlation between fatality rate and population size of cities is low, for example, it was  $-0.29$  for the 39 largest cities of the U.S. in 1969.

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