# DETERRENCE OF DRINKING AND DRIVING IN FRANCE: AN EVALUATION OF THE LAW OF JULY 12, 1978

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This report describes a study of the effects of a Scandinavian-style law concerning alcohol-impaired driving. The past two decades have seen the adoption throughout most of the developed world of drinking-and-driving laws based on principles originally developed in Norway and Sweden. The French law of July 12, 1978, was a part of this general development. Interrupted time-series analysis of French data on crash-related injuries and fatalities shows that the 1978 law had a notable deterrent effect, but that the effect was not permanent.

## I. INTRODUCTION

The dangerous nature of driving while impaired by alcohol has been acknowledged for a long time in all countries with large concentrations of automobiles. Understanding of the problem and attempts to deal with it have changed dramatically in recent decades, however, partly as a result of experimental and epidemiological research relating blood alcohol concentrations to driver behavior and crash occurrence (Cameron, 1977). In the early years of the automobile, the drinking-and-driving problem was conceived in terms of clinically observable drunkenness; the "classical" law prohibited driving in an "intoxicated condition" or "under the influence of intoxicating liquor." The development of practical devices for measuring blood alcohol concentrations, beginning in the 1920s, provided a scientific basis for characterizing the drinking-and-driving problem. The risk of crash involvement could now be related to the driver's elevated blood alcohol concentration; exclusive dependence on "clinical" evidence of drunkenness was no longer required. With this conception

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there developed a legal approach, termed "Scandinavian" because of its origins in Norwegian legislation of 1936 and Swedish legislation of 1941, which defined the prohibited behavior as driving with a blood alcohol concentration in excess of a standard value, and provided for testing of bodily substances in order to investigate and prove the offense.

The Scandinavian approach is compatible with the deterrence model in social science, which predicts that a threat will be more powerful if it is certain, severe, and closely related in time to its object (Zimring and Hawkins, 1973; Gibbs, 1975; Blumstein *et al.*, 1978). Indeed, the laws of Norway and Sweden, embodying mandatory prison sentences for the first offense, are renowned for their severity; less obvious, but also important, is the impression they give of high probabilities of arrest and prompt conviction for drinking drivers, who are subject to testing by objective instruments under certain conditions.

At their inception, the Scandinavian laws were not based on detailed scientific information (Andenaes, 1978), but research results accumulated over time justified the Scandinavian definition of the problem. Beginning in the 1960s, this accumulated knowledge, along with considerable frustration in applying classical laws, led to the adoption of laws following the Scandinavian model in a wide variety of countries including Britain, the Netherlands, New Zealand, Canada, Australia, and various states in the United States.

Although the effectiveness of the original Norwegian and Swedish laws in deterring drinking and driving was not evaluated when they were instituted, and no subsequent research has been able to demonstrate their effect (Ross, 1973), many of the more recent adoptions of Scandinavian-type laws have been evaluated in terms of reductions in crashes and related injuries and fatalities. Many of these evaluations find evidence of deterrent impact when the laws first take effect, but also that the savings in casualties do not seem to be permanent (Ross, 1981).

The French Law of July 12, 1978 is among the most recent applications of the Scandinavian approach to drinking and driving. Both the nature of the law and its context make it an interesting case study. It was the first adoption of the Scandinavian model abroad to provide for the testing of drivers passing through roadblocks scheduled for this purpose, without the need for the police to suspect that a driver had been drinking or was in any way driving irregularly. Contrary to

popular impression, the Scandinavian law until 1975 required *a priori* suspicion of alcohol influence to justify a police request for a breath test. The most successful recent application of the Scandinavian model—the British Road Safety Act of 1967—permitted such tests only in the event of crashes or serious traffic violations in the absence of *a priori* suspicion of alcohol influence. The alcohol-impaired driver in Norway or England could count on escaping detection if his behavior did not arouse police suspicions. Under the new French law, however, since all drivers passing through a roadblock were required to submit to a breath test, the alcohol-impaired driver would be less likely to escape detection. Furthermore, France is the only major Latin country to have adopted the Scandinavian model, and its per capita alcohol consumption is currently the highest in the world. Wine and other alcoholic beverages occupy a central place in French culture. From the viewpoint of deterrence theory, the threat of roadblock testing suggests the likelihood of an important effect, while the conflict with cultural prescriptions dampens these expectations.

## II. ORIGINS OF THE FRENCH LAW

As in some other countries—e.g., New Zealand—the French Law of 1978 formed part of an evolutionary development. It was preceded by earlier changes from the classical legal model which reflected a developing concern with the problem of alcohol-impaired driving. The testing of breath for alcohol was introduced in 1965 for major traffic law violations; breath testing was made compulsory in injury-producing crashes as well as for these violations in 1970. The 1970 law also created the offense of driving with a blood alcohol concentration in excess of 0.8 grams *pro mille* (80 mg./100 ml. or .08 percent in other notations). Punishment for violating this law could include suspension of the driver's license. Thus, by 1970 France had in some ways already attained the Scandinavian model. But the experience of more than 15,000 crash-related deaths annually did not produce complacency, and a variety of projects to strengthen the law were proposed in Parliament during the ensuing decade.

According to Jean Foyer, President of the Law Commission in the National Assembly, the stimulus for the 1978 law was an accident in which the son of a functionary in the *Département* of Maine-et-Loire was reportedly killed by a drunk driver who fled the scene. The victim's father undertook a personal crusade against drinking drivers and persuaded a Deputy,

Roland Boudet, to propose a stiffer law. If so, the idea fell on the fertile ground of organized government commitment to traffic safety.

In 1972 the French Government created an Interministerial Committee for Road Safety. It was comparable in function to the National Highway Traffic Safety Administration in the United States, but the latter exists as part of a single administrative department while the French Committee linked the Ministries of Transport, Justice, Health, and others with an interest in the problem. It was headed by an ambitious and energetic young civil servant, Christian Gérondeau, whose initial innovations in a field then practically bereft of legal controls seemed to be very effective. Major accomplishments included the institution of general speed limits and mandatory seat belt usage regulations. The effect of these rules was probably magnified by their politically fortunate but scientifically dismaying coincidence with the effects of the 1973 fuel crisis. (However, sophisticated evaluation research has recently confirmed the efficacy of these measures; see Lassarre and Tan, 1980.)

If Gérondeau did not originate the further legislation on drinking and driving, he quickly seized upon the opportunity to guide the enactment of such provisions drawn from a variety of private-member bills then being discussed desultorily in Parliament. He was greatly aided in this task by the Minister of Justice, Alain Peyrefitte. The government supported proposals for measuring drivers' blood alcohol concentrations with breath-testing devices in the context of roadblocks, and proposals for penalties including mandatory license cancellations under some circumstances. Passage of the new legislation did not come smoothly, as might be expected given the interests at stake in both alcohol and automobiles in French society. Parliamentary debates rang with charges that extending compulsory breath testing was an infringement of fundamental liberties. Issues arose concerning whether and how roadblock operations should function, and what sanctions—from merely taking away the keys of the car to mandatory license revocation—should follow apprehension. The use of evidentiary breath tests was attacked on technical grounds. Moreover, during consideration of the legislation major differences developed between the two houses of Parliament, the Assembly and the Senate; the latter was more concerned over the civil liberties implications of the proposed legislation.

One of the problems in getting Parliament to accept new legislation was the existence of police statistics suggesting that alcohol was not a common factor in French traffic casualties. Even though research in many other countries pointed to alcohol as a factor in close to half of all traffic fatalities, the absence of data specifically relevant to France was a major political handicap. This problem was addressed by Professor Claude Got, a physician at the hospital of Garches in suburban Paris, who studied reports of blood alcohol concentrations among those judged responsible for fatal crashes and discovered apparent statistical gaps in existing official figures. Through a series of extrapolations he estimated that illegal blood alcohol concentrations were present among approximately 40 percent of the cases in a sample drawn from the nearby region (Got and Thomas, 1977). Following criticism of this work because of its restricted sample, he broadened the scope of his study to encompass national data, with similar results. In a personal interview, Gérondeau stated that Got's professional status and forthright personality were important in obtaining passage of the legislation.

Passage of the law was also encouraged by public opinion supporting more stringent legislation. The French Institute of Public Opinion, at the request of the Interministerial Committee, sampled public opinion on four occasions between 1975 and early 1978. In the later two polls respondents were asked whether random tests of drivers' blood alcohol, in the absence of accident or violation, would be acceptable; responses were favorable, by a two-to-one margin.

Because of disagreement between the Assembly and the Senate on the proposed legislation, the matter was placed before a joint committee of both houses, which recommended the Assembly bill. The Assembly subsequently passed the legislation by unanimous vote, and the Senate followed, with Communist and Socialist members abstaining. The law was adopted on May 30, 1978, at the very end of the legislative session, and it became effective on July 12.

## III. PROVISIONS OF THE LAW

The main innovations in the Law of July 12, 1978, were:

(1) Any driver could be required to submit to a screening test for blood alcohol, regardless of whether suspicion of alcohol existed, in the context of roadblock operations. These operations (*contrôles*) were to be ordered by the region's chief

judicial official, the *procureur*, and conducted by police or *gendarmes*.

(2) Failure to pass this screening test could result in an order to cease driving then and there, as well as imposition of penalties previously provided for the offense of driving with an elevated blood alcohol concentration.

(3) A driver's license could be revoked—not merely suspended—as a consequence of being found guilty of driving with more than 0.8 *pro mille* blood alcohol concentration. Revocation was mandatory under two circumstances: if the blood alcohol concentration exceeded 1.2 *pro mille* and the accused had caused death or injury, or on a second or further offense where the blood alcohol concentration exceeded 1.2 *pro mille*. The offender would be ineligible to apply for a new license for up to three years.

(4) Provision was made for replacement of the prevailing system of a qualitative screening test of breath followed by a quantitative blood test for evidence, by one depending only on a quantitative breath test, when such a device should be approved by the authorities. (The blood test would remain available to drivers desiring it.)

Thus, the legislation added a new and threatening occasion for testing to an existing law which already provided a wide variety of occasions for blood alcohol concentration tests. Beginning in July, 1978, a screening test was *obligatory* for the driver or other person implicated in causing any injury-producing crash, as well as for the driver accused of any of a large number of traffic law violations (speeding, crossing a solid line, failing to yield right of way, driving without a license, etc.) and for drivers passing through scheduled roadblocks. A screening test was also *permitted* on any driver involved in a crash producing property damage and on any crash victim, driver or not, where the police judged the testing to be useful in their investigation (*Journal Officiel* of the National Assembly, September 29, 1980). Moreover, the probability of a severe sanction—loss of license—was increased by the mandatory provisions of the new law. The certainty and celerity of punishment were to be addressed, in time, by the use of better evidential breath test devices.

## IV. REACTIONS TO THE LAW

In France in 1978, as in Britain in 1967, the officials who helped bring the new law into existence immediately interpreted lower crash figures as being due to the legislation.

The first figures to be cited were those of July, even though the law did not formally become effective until the middle of the month and no roadblock operations were scheduled until August. (Given that the law was passed at the end of May and highly publicized during June, treatment of July as a post-intervention month does seem reasonable to us.) The *Lettre de la Sécurité Routière*, published by the Interministerial Committee, headlined its August issue, "First effects of the law on alcohol—large drop in highway accidents in the month of July—175 fewer killed than in July 1977." In September it was claimed that: "The decline is confirmed—fewer dead and fewer injured in the month of August 1978." In October: "Towards a record year for highway safety—thanks to the Alcotest law, an exceptional summer." These claims were duly promoted in the various French media by the public relations machinery of the Interministerial Committee.

However, as in Britain, there was considerable vocal opposition from interest groups involved in the distribution of alcoholic beverages, from civil libertarians, and from some driver interests. For example, the National Association of the Table Wine Industry issued a press release which claimed:

> These measures aim to produce profound changes in the dietary habits of drivers over time. Everyone knows that the drink habitually consumed during meals by Frenchmen is wine. To wish to transform the dietary habits of Frenchmen, is to incite them to drink something other than wine with their meals. It is to declare war on their traditional beverage.

The president of the National Confederation of French Wines and Spirits charged that the theme of official publicity ("Drink or drive—you have to choose!") was creating "a veritable psychosis of fear . . . incredible terms have been uttered, the word Wine being sometimes associated with 'vice' or a 'plague.' "

The libertarian reaction to the law was exemplified by an open letter which appeared in *Ouest-France* of September 20, addressed to the Minister of Justice from Bernard Landais, director of the University Center in Lorient, Brittany. The letter said, in part:

> It is unthinkable that a State that calls itself liberal can treat Frenchmen as potential criminals, forced to justify themselves before witnesses. It is unthinkable that rulers originating from the people can submit the latter to suspicion joined with the violation of personal integrity which gives the operations their humiliating character. The faults of some people cannot justify the end of liberty for everyone. If this be not the case then, little by little, our entire lives will become the subject of police operations.

The French automobile clubs, unlike their British counterparts, did not take a strong stand on the new law.

However, opposition was focused by the *Auto-Défense* movement, the creation of a traveling salesman from the provinces, Francis Rongier, whose previous battles had included attacks on speed limits and compulsory seat-belt regulations. In a handout, Rongier announced:

> Creation of a National Committee to Fight the Alcotest. . . . It has brought a threat to the physical integrity of the individual taken at random and having committed no crime . . . Adopted legally by Parliament but extorted from Parliamentarians by means of tendentious propaganda and falsified statistics, this law refuses to attack the real problem of alcoholism and has no other ends than to place the state's responsibility in highway matters onto the users of the highways.

Rongier deliberately drove through a pre-announced roadblock and refused to provide the required breath sample on the basis that the legislation was not enforceable due to a technicality in its promulgation. The real basis for his objection, he stated in a personal interview, was the libertarian belief that "only the presumption of guilt warrants the extreme measure of an attack on the driver's body."

The law's opponents, who prior to its passage based their opposition upon the lack of evidence in official statistics concerning the role of alcohol in crashes, continued their opposition based on new findings that the roadblocks produced few positive breath tests. *Auto-Défense* sued Gérondeau personally for diffusion of falsehoods. The complaint disputed claims for the law's effectiveness on the grounds that the decline in deaths was arbitrarily attributed only to the effect of the law rather than to improvements in the highway network. Moreover, it was discovered that the Alcotests used as the screening devices in enforcement of the law were set to show positive results at 0.5 *pro mille*, whereas the law permitted driving with blood alcohol concentrations up to 0.8 *pro mille*. The police and public had not previously been informed of this fact, which suggested a possible lack of good faith on the part of administrators of the law. *Auto-Journal* (April 15, 1979) headlined, "The Alcotest is a cheat!" Rongier unsucessfully demanded the seizure of all Alcotests and cessation of roadblock operations in his jurisdiction. However, although the opposition had martialed some impressive arguments on its side, the Government maintained its position and made no modifications in the administration of the law.

The persistence and *prima facie* reasonableness of the opposition in its criticism, and the doggedness of the Government in resistance and rebuttal, combined with official publicity campaigns, produced a widespread knowledge of the

1978 law and its provisions. As with the British Road Safety Act of 1967, surveys indicated an impressive penetration of the public consciousness. The French Institute of Public Opinion found that 97 percent of a random sample of adults in August, 1978 knew of the law, a record for familiarity with new legislation. In October, 66 percent could state the legal limit in response to a survey question. The law was popular in the abstract; its provisions were favored by nearly four to one in a poll in January of 1979.

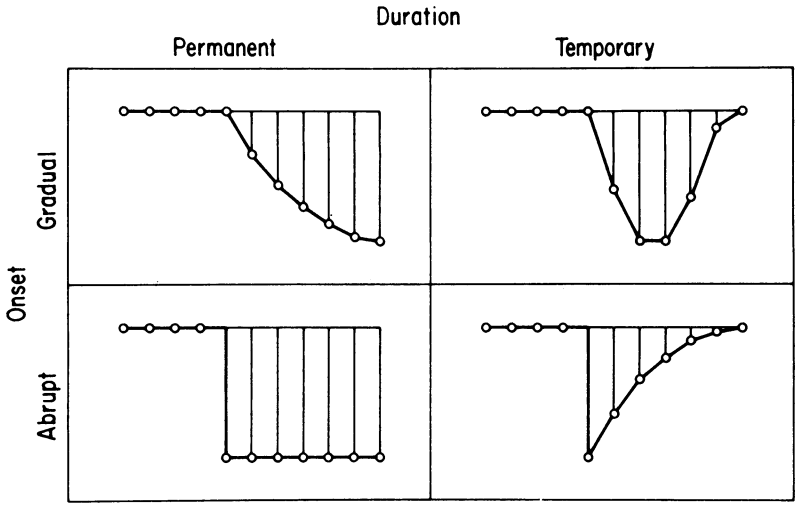
## V. RESULTS OF THE LAW

The opposition was correct in criticizing official claims of effectiveness for the Law of July 12, 1978, based only on comparison of crash statistics immediately following the law's inception with statistics of the previous year. As in many other cases, no attention seems to have been given by officials to the possibility that the change might have been caused by factors other than the effect of the law (cf. Campbell *et al.*, 1970). Other reasonable explanations, for example, included "history"—possible improvements in the roadway network (cited by *Auto-Défense*), changes in vehicles, climatic conditions—and statistical instability (i.e., the possibility that the decline could be explained by chance factors). Moreover, when the picture changed and post-law crash figures no longer clearly accorded with the image of a successful drinking-and-driving law, the government, without much apparent reason, seemed not to consider the possibility that the 1978 law was losing its effectiveness. Instead the bad news was blamed on the failure of drivers to respect the speed limits.

In order to clarify the aftermath of this law, we offer an analysis based upon interrupted time-series methods (McCain and McCleary, 1979). We approached this analysis with the expectation, based upon studies in Great Britain and elsewhere (Ross, 1973; 1981), that the law would elicit an abrupt but temporary change in drinking and driving in France—i.e., that the legislation would have an initial impact, but that matters would gradually return to the *status quo ante*. This expectation is diagrammed schematically in the lower right-hand cell of Figure 1. Alternative models for the analysis are possible, including gradual impact and permanent duration; these are depicted in the other cells of Figure 1. Statistical models have now been developed to determine which of these four impact patterns is most appropriate for a given time series (McCain and McCleary, 1979; McCleary and Hay, 1980; McDowall *et al.*,

1980). We analyzed a variety of available crash data series which were expected to index the extent of drinking and driving in France during the months surrounding July, 1978. We begin our explication with the series of crash-related injuries and crash-related fatalities for the whole of France from 1973 through October, 1980.

Figure 1. Models of Intervention Effects




Injury data are taken as an index of drinking and driving, because the international literature shows alcohol to be present in the blood of a very significant minority of drivers involved in injury-producing crashes. There are no French studies directly on this point, but presumption of a similar relationship seems very reasonable. A weakness of this index is that many factors other than blood alcohol are causally related to automobile accidents, and effective constraints on alcohol will have only a modest effect on injuries. A strength of the index is that the data base is relatively large, yielding a simpler and less "nervous" curve than series more closely related to alcohol, such as fatalities.

The usefulness of crash-related fatalities as an index of alcohol-impaired driving in France is supported by French research demonstrating a major presence of blood alcohol among drivers killed in crashes (Got and Thomas, 1977; Got, n.d.). A completely effective countermeasure to alcohol-

impaired driving might reduce French crash-related fatalities by close to half, and lesser degrees of effectiveness should still be relatively easily perceived in the curve of fatalities.

The results of our investigation of these series are presented in the first two lines of Table 1. The analyses are designed to measure the magnitude and duration of any impact on the assumption that an abrupt, temporary model adequately describes the data. The magnitude of an impact is reported in Table 1 as the simple change (reduction) in series level effected in the first post-intervention month. To allow comparisons among the several effects, the durations are also reported in Table 1 as the amount of time elapsed until the effect is 95 percent dissipated. Finally, for each of the series, a total savings is reported. The total savings, or net impact, is a function of both the magnitude *and* duration of the effect. A relatively trivial impact in terms of magnitude can nevertheless effect a substantial total savings if the impact has a relatively long duration. The formulae for these statistics and for each of the analyses are given in the technical appendix to this article.

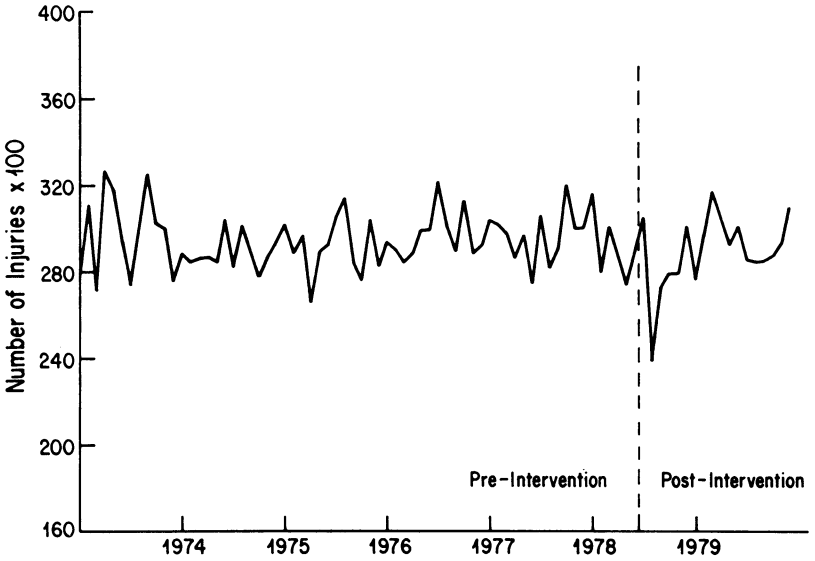
Table 1. Measures of Impact of the Law of July 12, 1978

| Measure | Series mean | Impact[1] | Longevity[2] | Savings[3] |
|---|---|---|---|---|
| Crash-related injuries | 29,468 | −3.684 (−12.5%) | 8.4 mos. | 11,064 |
| Crash-related deaths | 1,111 | −155 (−13.9%) | 12.9 mos. | 694 |
| Crashes producing injury, weekend nights | 518 | −181 (−34.9%) | 9.6 mos. | 613 |
| Crash-related deaths, weekend nights | 78 | −28 (−35.2%) | 8.2 mos. | 81 |
| Crash-related deaths, midweek days* | 248 | +80 (+32.3%) | 3.4 mos. | −62 |
| Crashes producing injury, northern region | 1,338 | −209 (−15.6%) | 30.4 mos. | 2,162 |
| Crashes producing injury, southern region* | 721 | −55 (−7.6%) | 4.8 mos. | 101 |
| Wine sales index* | 3,866 | −1 (−0.0%) | 3.4 mos. | 1 |
| Mileage index* | 1.31 | +0.10 (+7.8%) | 2.3 mos. | −0.15 |

1 Rise or fall in time series following the intervention.
2 Interval to disappearance of 95% of impact.
3 Injuries, deaths, etc., avoided (+) or incurred (−).

* The impact of the intervention on this series is not statistically significant at the $P < .05$ level.

Figure 2. Crash-Related Injuries in France, Seasonal Variation Removed



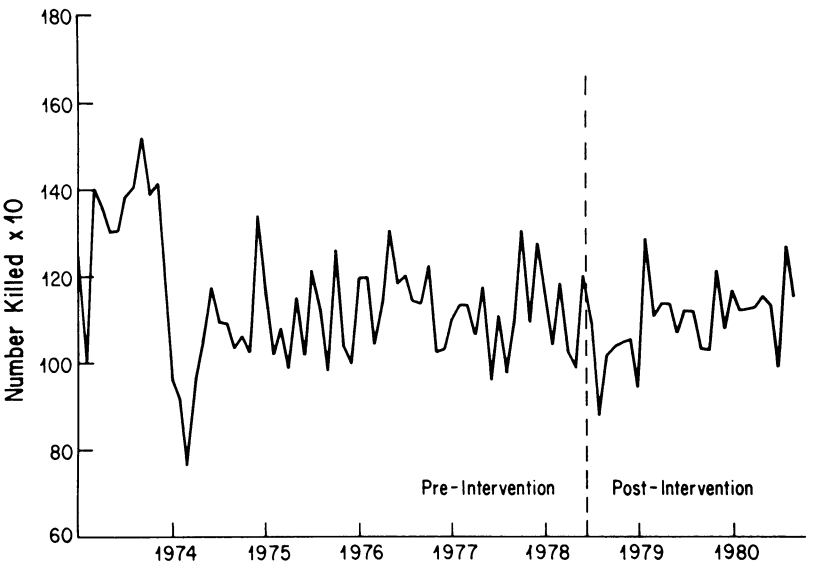

*Crash-Related Injuries*

Figure 2 presents crash-related injury data for all of France, corrected by an averaging process to remove characteristic seasonal variations that might otherwise be confused with or hide the effect of an intervention. The series of available data extends from 1973 to 1980. It shows a slight downward trend from beginning to end, decreasing on the average by about 200 injuries per year. A statistically significant drop from this trend is evident in July, 1978, at the inception of the drinking-and-driving law. The magnitude of the initial decline is 3,684 injuries, or 12.5 percent of the series mean. The effect is temporary, however. Ninety-five percent of the impact had dissipated within 8.4 months of the new law; in other words, the series had returned to within five percent of its pre-intervention "normal" course in less than nine months. Although the series resumed its former shape, the 1978 law produced a cumulative total saving of more than 11,000 injuries.

*Crash-Related Fatalities*

The deseasonalized monthly fatality series for the same period is presented in Figure 3. These data show a general

Figure 3. Crash-Related Deaths in France, Seasonal Variation Removed




declining trend of about 30 deaths per year and a statistically significant drop beginning in July, 1978. The initial reduction is approximately 155 deaths per month, or 13.9 percent of the series mean. Again, the impact is temporary, with 95 percent dissipation after 12.9 months. The total savings in fatalities is 694.

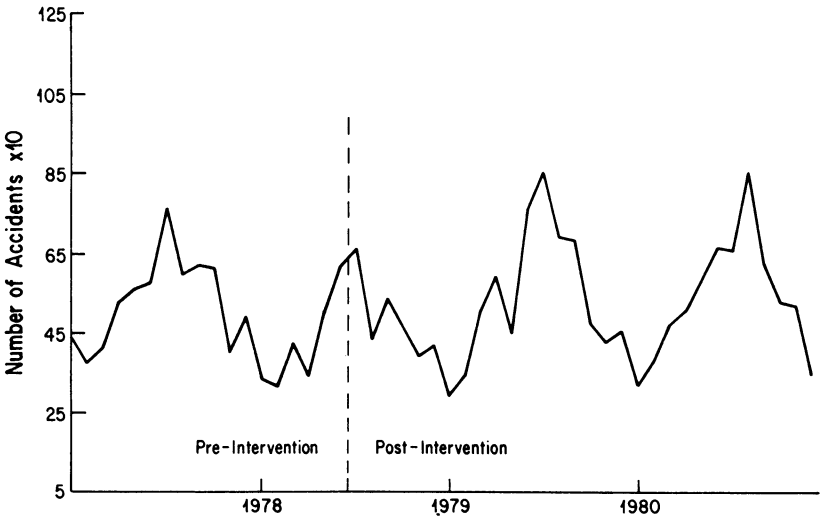
The comparison between the fatalities and the injuries series is of theoretical interest, as we predict that the impact of the law should appear more clearly in the former, where alcohol bulks larger among causal factors. This prediction is fulfilled, both concerning the changes in series levels and the duration of the effect.

*Fatalities by Time of Day and Day of Week*

It is possible to identify more refined measures of drinking and driving, i.e., series of crash-related phenomena which are particularly likely to involve alcohol influence. International epidemiological research suggests that night-time and weekend fatal crashes and single-vehicle fatalities are highly correlated with drinking and driving. Relevant data series were found concerning injury-producing crashes on weekend nights. These series are partial because they are based upon only those data

gathered by the *Gendarmerie Nationale*, a branch of the armed forces comparable to our state police, whose jurisdiction is primarily in rural areas. Statistics concerning urban accidents, which we were unable to obtain, are collected by the *Police Nationale*. The data from both organizations (with a small contribution from yet another source) are aggregated in published statistics to provide the total series analyzed in the previous sections, but the relevant sources do not provide crash data by date, time, and month simultaneously. The *Gendarmerie* provided special tabulations of Friday and Saturday night (9 PM to 3 AM) accidents and fatalities for 1977 through December, 1980. These abbreviated series are presented in Figures 4 and 5. The series were both too short to permit deseasonalization. The impacts are so relatively large, however, that they are visually apparent even in the unadjusted series.

Figure 4. Injury-Producing Crashes, Weekend Nights




The impact of the new law on Friday and Saturday night injury-producing crashes (in areas under jurisdiction of the *Gerdarmerie*) was statistically significant. The initial drop was 181 accidents per month (approximately 34.9 percent of the series mean), with 95 percent dissipation after 9.6 months. The

total effect was a savings of 613 crashes. The impact of the new law on Friday and Saturday night fatalities was also statistically significant. The initial drop was 28 fatalities (approximately 35.2 percent of the series mean), and 95 percent dissipation occurred after 8.2 months.

Figure 5. Crash-Related Deaths, Weekend Nights

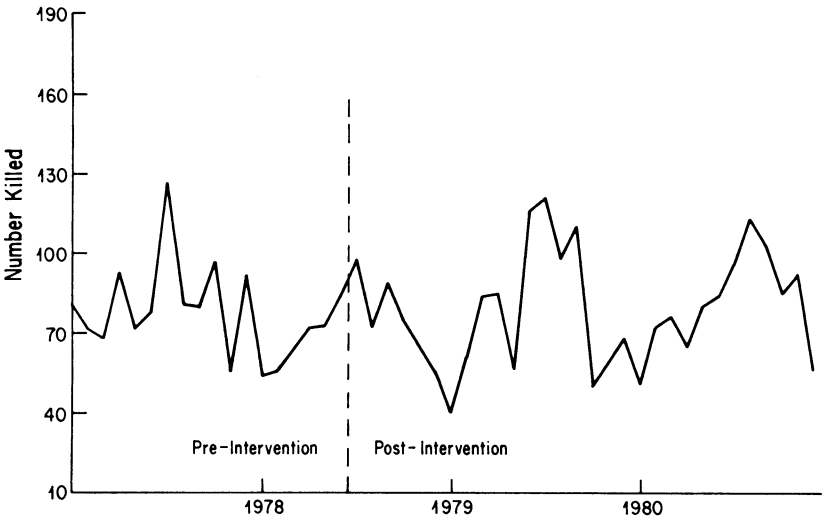
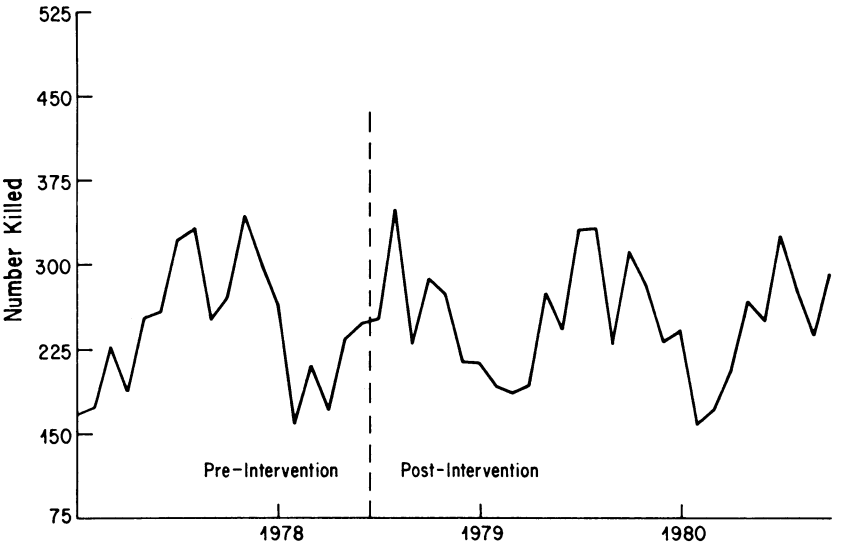



Figure 6 shows a "control" series of midweek (Tuesday, Wednesday, and Thursday) fatalities in areas under jurisdiction of the *Gerdarmerie*. The law was expected to have little impact on this series for the same reason that it was expected to have a dramatic impact on the weekend night series. In fact, the estimated impact of the new law on the midweek fatality series was in the wrong direction and was *not* statistically significant. This analysis confirms our overall interpretation. Had some factor other than the Law of July 12 caused the impacts found in the weekend night series, we would have expected to find an impact in the weekday series also.

*Regional Comparisons*

Another approach to investigating the effect of the drinking-and-driving law is to compare changes at its inception in a region where alcohol consumption is high with changes in a low alcohol-consumption region. Statistics on alcohol

Figure 6. Crash-Related Deaths,
Tuesday through Thursday




consumption and alcoholism suggest that problems involving alcohol use are especially common in the north of France and relatively less common in the agricultural *Midi* (Got, n.d.: Map 2). We therefore gathered data on crashes involving injuries or deaths in the *Départements* of Pas de Calais and Nord, an industrial region on the Straits of Dover in the north of France, and in five *Départements* constituting the agricultural region of Languedoc-Roussillon in the south. (Paradoxically, one of the chief products of the latter region is wine.) The data were available only through the end of 1978. They are presented in Figures 7 and 8.

The impact of the law is shown clearly in Figure 7, which depicts injury-producing crashes, adjusted to remove seasonal variation, in the northern region where alcohol consumption is relatively high. The initial drop is 209 serious crashes, or more than 15 percent of the series mean. The point of 95 percent dissipation occurs at 30.4 months. The impact is remarkably long in duration, resulting in the impressive savings of 2,162 serious crashes overall. In contrast, for the southern region where alcohol consumption is relatively low, the impact is *not* statistically significant. With this understanding, the initial drop of 55 amounts to only 7.6 percent of the series level. Not

Figure 7. Injury-Producing Crashes in Northern France, Seasonal Variation Removed

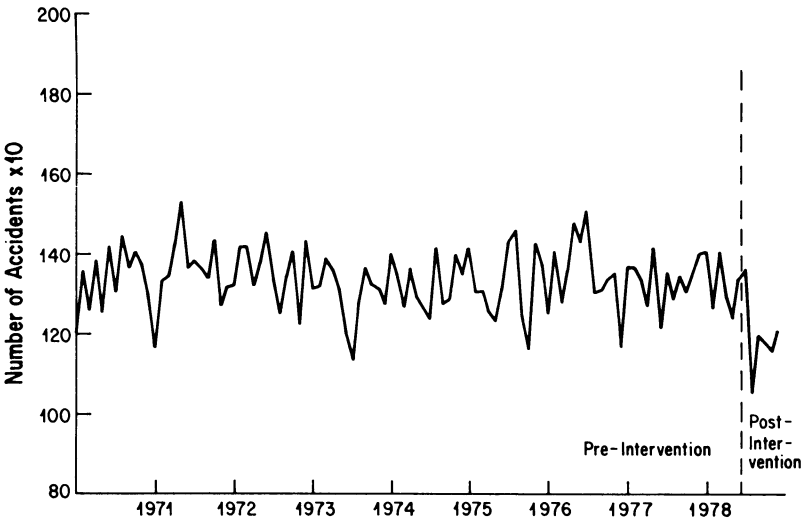
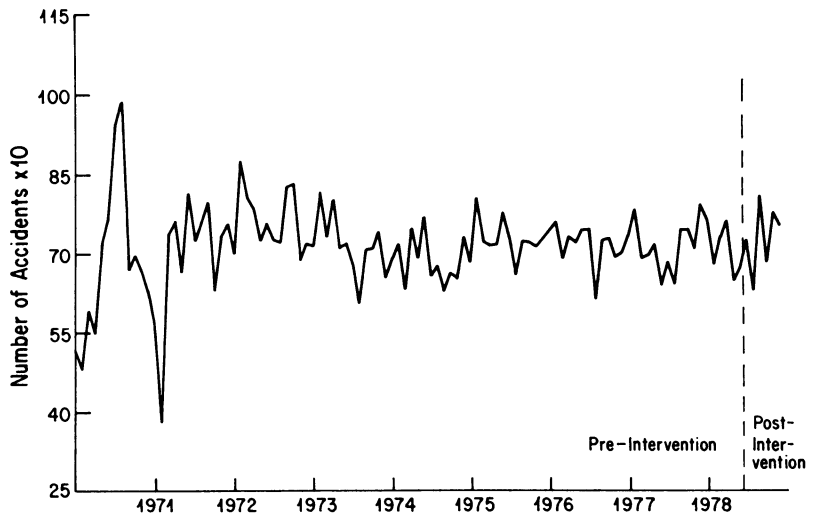



Figure 8. Injury-Producing Crashes in Southern France, Seasonal Variation Removed

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surprisingly, the impact is also short-lived. Ninety-five percent dissipation occurs at 4.8 months, resulting in a net impact of a mere 101 serious crashes saved. This total savings is not significantly different from zero.

These findings again accord with our expectations. Serious crashes were significantly reduced only in the northern region where alcohol consumption was higher and where, presumably, drinking and driving was a more frequent cause of crashes. In the broadest sense, the southern *Départements* serve as a statistical control for the northern *Départements*. The finding of no significant effect in the south gives us more confidence in our finding that the law did indeed have a substantial impact.

## VI. THE MECHANISM OF THE LAW

Our conclusion, based on an interrupted time-series analysis, is that the law produced an abrupt but temporary deterrence of alcohol-impaired driving in France. But how was this accomplished? Three possibilities seem evident: people drove less; they drank less; or they separated occasions of drinking and driving. Interrupted time-series analysis can help in deciding among these possibilities.

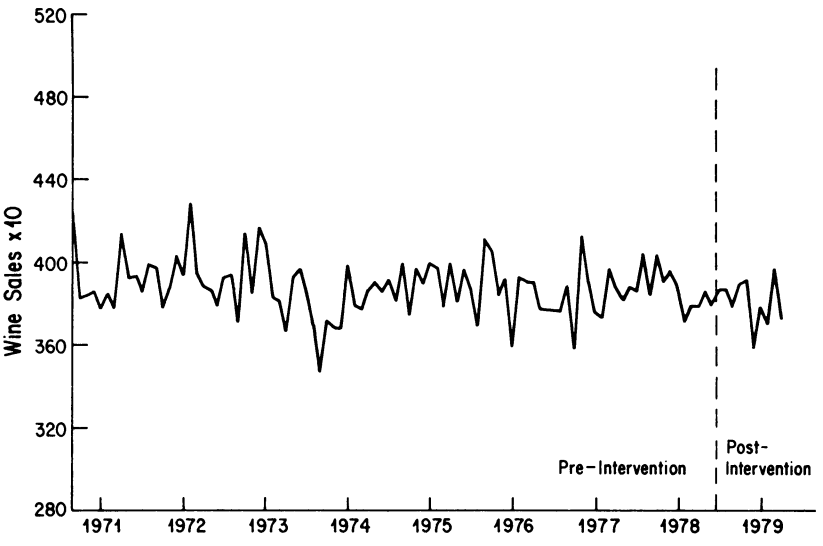
### *Did People Drink Less?*

Claims were frequently made that the 1978 law would lower the consumption of alcohol and thus interfere with the business and sabotage the investments of the cafe and restaurant industry. For instance, "In the Bas-Rhin, restaurateurs and sellers of beverages state that the sale of *aperitifs* and *digestifs* fell by 80 percent" (*Lutte Ouvrière*, August 26, 1978). A 50-percent decline in consumption of alcoholic beverages was claimed in Normandy (*LeHavre Libre*, September 6, 1978). After a few months these claims were heard less often, but it is reasonable to ask whether a perceptible change in alcohol consumption can be demonstrated by the analytical means utilized here.

There are no simple series relating to alcohol consumption in France. Figures are available concerning wine sales at wholesale, however, and they are presented, seasonally adjusted, in Figure 9. As noted in Table 1, we found no statistically significant impact. Interpretation of this finding is not as straightforward as one might wish. Sales at wholesale

and consumption at retail are not strictly linked. However, our analysis gives no hint of even a delayed impact of the law. There are, of course, alcoholic beverages other than wine, including cider, *apéritifs*, beer, brandies, etc., whose sales might have responded to the law. We were unable to locate systematic data concerning sales of these beverages, but representatives of several of the largest producers of alcoholic beverages other than wine stated in interviews that they were unable to distinguish any effects of the 1978 law on their sales. We conclude, therefore, that the deterrent effect of the law was not mediated by a simple decline in consumption of alcoholic beverages.

Figure 9. Wholesale Wine Sales, Seasonal Variation Removed

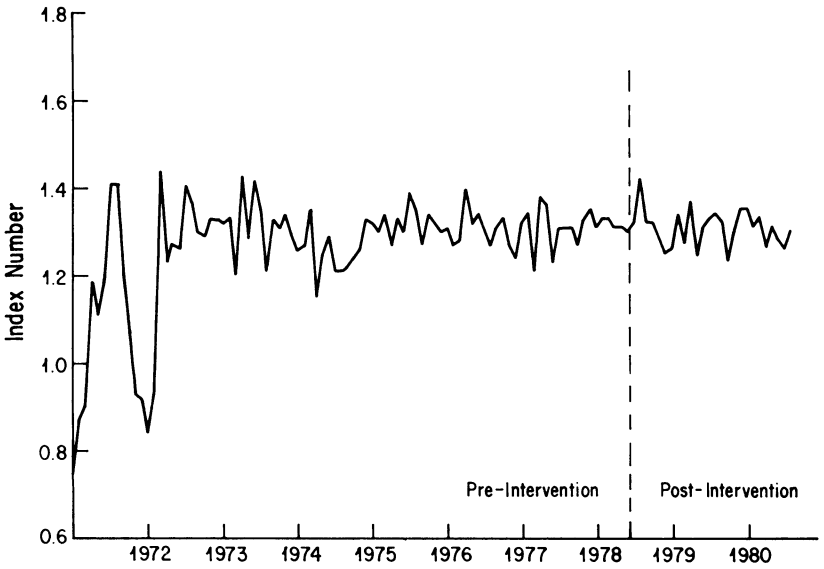



*Did People Drive Less?*

The possibility of reduced driving following the law is directly addressed by official estimates of traffic density. These are available monthly during the entire study period; they are based on a sampling of main roads in all of France. It can be seen from inspection of the seasonally adjusted series in Figure 10, confirmed by the statistical analysis reported in Table 1, that there was no response in traffic density to the 1978 law.

We may conclude with relative confidence that the deterrent effect of the law was not produced by a diminution in driving.

Figure 10. Traffic Mileage Index, Seasonal Variation Removed




*Conclusion*

Given the negative evidence concerning the first two explanations for the achievement of deterrence, we are led to the speculation that it was achieved by the third, separation of drinking and driving. This is the conclusion that was reached in the senior author's prior evaluation (1973) of the British Road Safety Act of 1967.

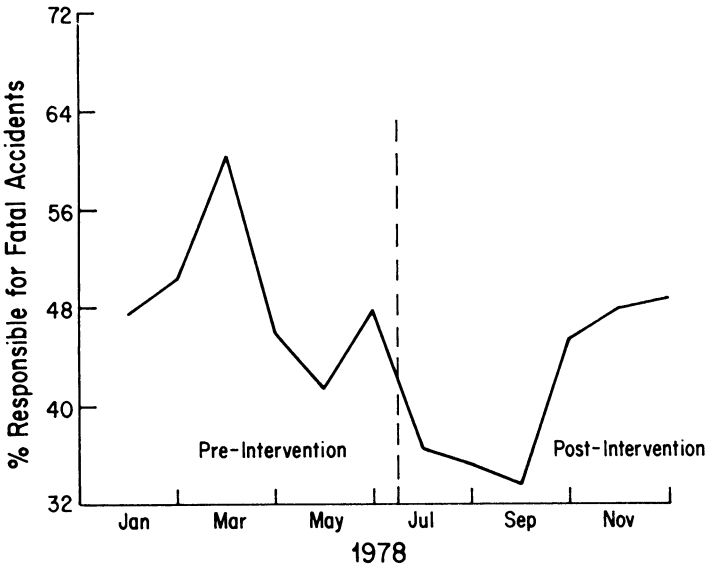
## VII. EVIDENCE FROM OTHER STUDIES

The conclusions we reach about the achievement of a deterrent effect by the law are supported by independent evidence from two French studies, one of which also confirms the disappearance of the law's effect over the course of a year or so.

The French National Organization for Road Safety (ONSER), a private research organization contracting with the government, had undertaken a sample study of blood alcohol concentrations among non-crash-involved drivers in 1977, prior to the new law, and it was able to mount a comparable study

during the months of April through June, 1979, in the year following the new law (DeBuhan and Filou, 1979). Both studies had large samples—approximately 3,000 in the first and 1,600 in the second. The proportion of drivers with illegal blood alcohol concentrations dropped from 3.4 percent in the first period to 1.8 percent in the second. A detailed comparison contains some unexplained paradoxes—for instance, the fact that illegal blood alcohol concentrations were unaffected during night-time hours or on weekend nights, and among certain occupational groups. But the general finding is impressive in magnitude and lends some support to our conclusion that the law had a temporary deterrent effect.

Figure 11. Persons Responsible for Fatal Accidents with Blood Tests Showing Illegal Levels of Alcohol




Also relevant are monthly figures from Claude Got's analysis of blood alcohol concentrations among drivers responsible for fatal crashes throughout France, provided to us by Professor Got. Appropriate data are available concerning only those drivers who were blood-tested but not breath-tested (about one-quarter of the entire sample), and inferences must consequently be very guarded. However, as indicated by the graph in Figure 11, the proportions of illegal blood alcohol concentrations behave as expected according to the interpretation developed in our interrupted time-series analysis of official data for the whole of France. There was an abrupt decline in the proportion of illegal blood alcohol concentrations in this limited sample precisely coinciding with the new law;

but within a few months matters returned to where they were before. The Got and ONSER studies are inconsistent in that the effect measured by Got did not last to the end of 1978, whereas the post-law ONSER study finding a reduction in blood alcohol concentrations was made in the spring of 1979. We are surprised by the shortness of the effect measured by Got and by the length and depth of that measured by ONSER, but given the inadequacies and paradoxes in the studies we find our interpretation of the French experience to be strengthened by these independent data.

## VII. DETERRENCE GAINED AND LOST

Our interpretation is that the source of effect of the Law of July 12, 1978 was in its advertised and notorious threat of apprehension and, possibly, mandatory loss of license for the drinking driver. Although traffic density and wine sales were unaffected, French drivers appear to have responded to their perceptions of threat by separating drinking and driving. Deterrence of alcohol-impaired driving could be accomplished even in a wine-centered culture like that of France.

Our explanation of why the deterrent effect was lost is that the threat was not fulfilled and that people gradually learned this fact. This explanation is drawn from our knowledge of the fate of similar laws in other countries (Ross, 1981) and is supported by evidence concerning the manner in which the French drinking-and-driving law was applied.

In the first place, the risk of being tested under the 1978 law was very low, and the probability of the test being defined as positive was inexplicably minute. The *Journal Officiel* of the National Assembly for April 2, 1979, noted, and the media subsequently publicized, the following statistics: between the inception of the law and the end of January, 1979, there had been in the whole of France 1,091 roadblock operations, with a total of 335,449 Alcotests being administered. Only 1,416 positive tests (0.42 percent) were recorded from this effort. In many *Départements* months went by without any roadblock operations at all, in part because the proportion of positive tests achieved never seemed to validate the effort expended, and in part because law enforcement officials hesitated to enter too avidly into enforcement of the controversial provisions of the law. A high official of the *Gendarmerie*, in a personal interview, noted problems in this area familiar to students of traffic law in other countries: the public fails to understand and resents the police function in this field, and the police have

difficulty in relating to this unfamiliar type of law violator. As stated by this official, "We would much rather chase bandits than speeders."

Another consequence of law-enforcement attitudes appears to have been excessive leniency in reading the results of the qualitative Alcotests. The senior author attended one roadblock operation where between 10 PM and 2 AM on a weekday night 450 drivers were tested by a crew of 18 *gendarmes*, with no positive tests being recorded. However, several tests were defined to be borderline (*juste*), and one driver was told to have his wife take the wheel—though she had not been tested. A personal interpretation of several of these tests was that they were plainly positive but the officers were using discretion to overlook violations where possible. In any event, the validity of such a result is extremely questionable even if, as in the 1979 ONSER study, the proportion of non-crash-involved drivers with illegal blood alcohol concentrations was less than two percent. (Recall that the Alcotests used by law enforcement were calibrated at 0.5 *pro mille* rather than the 0.8 used as basis for the ONSER estimates, and a higher proportion of positives should have been found in the roadblocks for that reason.)

Furthermore, the courts appear to have joined their discretion to that of the police forces in minimizing the infliction of punishment for violating the drinking-driving law. A report of the activities of the French courts during 1979 finds that for blood alcohol concentrations greater than 1.2 *pro mille*, 30,000 licenses were suspended and only 217 were cancelled, though cancellation may occur at the discretion of the courts in these instances. There were 25,721 prison sentences pronounced, but only 3,227 of the defendants actually were imprisoned, the balance of the sentences being suspended or mitigated. There was considerable variation in these matters from court to court, indicative of the use of judicial discretion (Fontaine, 1980).

Survey evidence confirms the public recognition of these facts. The French Institute of Public Opinion asked a national sample in January, 1979, for an estimate of the frequency of roadblocks. Among drivers, only 3 percent thought the operations were "very frequent," and another 26 percent said "rather frequent"; in contrast, 47 percent said "rather rare," and 14 percent declared them to be "very rare." Furthermore, a series of polls in July, 1978, December, 1978, July, 1979, December, 1979, and July, 1980 found the percentage of

respondents who believed that fewer drivers were drinking more than permitted was respectively 17, 49, 32, 31 and 21.

In conclusion, we note that the deterrence model is based upon perceptual variables. From it we predict that people will change their behavior when confronted with a threat that they perceive to possess certainty, severity, and celerity. However, this perception is subject to the influence of daily experience. A well-publicized and controversial threatening rule may have deterrent effects because its threat is exaggerated in the perception of the subject population. If, however, the perception rests on an unrealistic basis, the learning process will undermine it until it becomes, as the French say, a "wooden shoe."

The experience of the French law of 1978 is not unique in this respect. In all competently evaluated adoptions of the Scandinavian-type drinking-and-driving law, where a deterrent effect is evident its subsidence is also remarked. Indeed, with the possible exception of the British Road Safety Act of 1967, the French Law of 1978 may be the most efficacious of the Scandinavian-type laws reported in the literature, for the same reasons that rendered the British law effective: its novelty and notoriety. In saving nearly 750 lives its accomplishment was considerable, and few people accepting this figure would argue that it was not worth the effort. However, if we are right that the deterrent effect of this law is now gone, it is necessary for the French authorities to consider whether it can be retrieved, by what means, at what expense—and whether, among other options for reducing the death and injuries imposed by traffic crashes, the deterrence of alcohol-impaired drivers through legal threat is the most efficient means available to cope with the problem of drinking and driving.

## TECHNICAL APPENDIX

A general model for analysis of the time series quasi-experiment may be written as

$$Y_t = N_t + f(I_t)$$

where $Y_t$ is the $t^{th}$ value of a time series, $N_t$ is the $t^{th}$ value of a stochastic or "noise" component, and $f(I_t)$ is the $t^{th}$ value of an intervention component. The noise components used in our analyses are the *A*uto*R*egressive *I*ntegrated *M*oving *A*verage

(ARIMA) models of Box and Jenkins (1976; Box and Tiao, 1975; 1965). Each $N_t$ is a function of autoregressive and moving average operators,

$$\phi_p(B) = 1 - \phi_1 B - \phi_2 B^2 - \ldots - \phi_p B^p$$

$$\text{and } \theta_q(B) = 1 - \theta_1 B - \theta_2 B^2 - \ldots - \theta_q B^q$$

where B is the backshift operator defined such that $B^n Y_t = Y_{t-n}$. In the general case, $N_t$ will be of the form

$$N_t = \phi_p(B)^{-1}\theta_q(B)a_t$$

where $a_t$ is a white noise random shock. Following the Box-Jenkins philosophy, seasonal variance in the time series is accomodated by multiplicative autoregressive and moving average operators of degree S. Thus,

$$N_t = \phi_p(B)^{-1}\phi_{pS}(B^S)^{-1}\theta_q(B)\theta_{qS}(B^S)$$

for the period S. Since our data are monthly, S = 12.

The iterative identification/estimation/diagnosis model-building strategy whereby a particular ARIMA model is selected for a given time series has been widely reported. We will not describe that strategy here but instead direct the interested reader to McCain and McCleary (1979), McCleary and Hay (1980: 91-103), or McDowall *et al.* (1980) where the model-building strategy is described in detail.

The intervention component, $f(I_t)$, is some transfer function of a dummy variable, $I_t$, which represents the onset of the new law. In this case, the dummy variable is defined as

$I_t = 0$ prior to 7/12/78

$= 1$ beginning on 7/12/78

Unlike the noise component of the model, the intervention component must be specified theoretically. Figure 1 shows a simple theory of impact due to McCleary and Hay (1980: 172). Three of these four impact-types can be ruled out through statistical analysis. In the case of our analyses, the statistical evidence leads overwhelmingly to the conclusion that the July 12, 1978 law had an *abrupt*, *temporary* impact on indicators of alcohol-impaired driving. This impact-type is shown in the lower, right-hand cell of Figure 1. The transfer function of $I_t$ associated with this impact pattern is

$$f(I_t) = (1 - dB)^{-1}w(1 - B)I_t$$

Since $(1 - B)I_t = I_t - I_{t-1}$, this transfer function can be rewritten as

$$f(I_t) = (1 - dB)^{-1}wP_t$$

where $P_t$, the differenced $I_t$, is a *pulse* variable. Expanding this transfer function as a Taylor series, it can be shown that, prior to the onset of the new law, $I_t = P_t = 0$ and

$$f(I_t) = 0$$

But with the onset of the new law, say at t=i, $I_i = P_i = 1$ and

$$f(I_i) = w$$

which is a scalar parameter to be estimated. The transfer function may be interpreted literally as the change in level of a time series coincident with the onset of the new law. In this case, the w-parameter is expected to be negative or zero, indicating either that the new law effected an abrupt *drop* in the level of a time series or that the new law had no effect. This null hypothesis can be tested with a t-statistic for the w-parameter.

In successive postintervention observations of the time series, $P_{i+k} = 0$ and the transfer function takes on the values

$$f(I_{i+1}) = dw$$
$$f(I_{i+2}) = d^2w$$
$$f(I_{i+3}) = d^3w$$
$$\vdots$$
$$f(I_{i+k}) = d^kw$$

Now the parameter d is restricted to the segment

$$-1 < d < +1$$

With this restriction, the term $d^kw$ is very small, approaching zero as a limit. Beginning with the second postintervention observation then, the level of the time series regresses to its preintervention mean.

The d-parameter can be interpreted literally as a *rate*. When d is large, near unity in absolute value, return to the preintervention normal level is slow. When d is small, on the other hand, near zero, return to the preintervention normal level is rapid. this rate interpretation of d permits us to contrast two impacts. For example, the simplest measure of impact longevity in the $k^{th}$ postintervention observation is

$$\text{Percent decay} = 1 - d^{k-1}$$

We have computed the 95% lives shown in Table 1 from the formula

$$95\% \text{ life} = 1 + \text{Ln}(.05)/\text{Ln}(d) \text{ Months}$$

This formula is based on the percent decay measure, of course. Finally, while effects we discovered in the French data are only temporary, it is important to remember that even a temporary effect has permanent consequences: a temporary reduction in

traffic fatalities results in a *total savings* of *net impact*. This statistic is computed as the infinite series of $d^k w$. Since the value of d is constrained to less than unity in absolute value, this infinite series can be evaluated exactly as

Net impact = $w/(1 - d)$ Lives, Injuries, etc.

This statistic will always be given in the metric of the time series (killed, injured, etc.) as shown in Table 1.

In what follows, we present the specific details of our time series analyses. The order of presentation follows the order of Table 1.

(1) *Crash-related injuries*, 1/73 to 12/79: the model identified for this series is

$$Y_t = \frac{\theta_0 + (1 - \theta_{12}B^{12})}{1 - B^{12}} a_t + w(1 - dB)^{-1}P_t$$

Parameter estimates are

$\theta_0 = -192.82$; t = −2.24
$\theta_{12} = .91722$; t = 31.58
d = .66701; t = 3.79
w = −3864.3; t = −2.72

For these estimates, the Residual Standard Error (RSE) = 1438.0 and the Q statistic = 35.79 with 28 degrees of freedom. Both statistics indicate that the model gives an acceptable fit to the data.

(2) *Crash-related deaths*, 1/73 to 9/80: the model identified for this series is

$$Y_t = \frac{\theta_0 + (1 - \theta_1 B - \theta_2 B^2)}{1 - B^{12}} a_t + w(1 - dB)^{-1}P_t$$

Parameter estimates are

$\theta_0 = -28.899$; t = −4.14
$\theta_1 = -.40095$; t = −3.43
$\theta_2 = -.16270$; t = −1.97
$\theta_{12} = .93474$; t = 39.20
d = .77678; t = 3.19
w = −154.88; t = −2.02

For these estimates, RSE = 90.171 and $Q_{26} = 25.82$.

(3) *Injury-producing crashes, Fri/Sat night*, 1/77 to 12/80: the model identified for this series is

$$Y_t = \frac{1 - \theta_{12}B^{12}}{1 - B^{12}} a_t + w(1 - dB)^{-1}P_t$$

Parameter estimates are

$\theta_{12} = .60047$; t = 2.69
d = .70488; t = 4.32

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$w = -181.00;\ t = -2.50$

For these estimates, RSE = 92.096 and $Q_{23} = 17.28$.

(4) *Crash-related deaths, Fri/Sat night*, 1/77 to 12/80: the model identified for this time series is

$$Y_t = \frac{1 - \theta_4 B^4 - \theta_{12} B^{12}}{1 - B^{12}} a_t + w(1 - dB)P_t$$

Parameter estimates are

$\theta_4 = .40950;\ t = 2.37$
$\theta_{12} = .35055;\ t = 1.96$
$d = .66081;\ t = 3.34$
$w = -27.552;\ t = -2.21$

For these estimates, RSE = 16.53 and $Q_{22} = 21.97$.

(5) *Crash-related deaths, midweek days*, 1/77 to 10/80: the model identified for this series is

$$Y_t = \frac{(1 - \theta_7 B^7)(1 - \theta_{12} B^{12})}{1 - B^{12}} a_t + w(1 - dB)^{-1} P_t$$

Parameter estimates are

$\theta_7 = .94174;\ t = 39.75$
$\theta_{12} = .91990;\ t = 20.58$
$d = -.29128;\ t = -1.13$
$w = 79.974;\ t = 3.47$

For these estimates, RSE = 24.13 and $Q_{18} = 13.23$.

(6) *Injury-producing crashes, northern*, 1/70 to 12/78: the model identified for this series is

$$Y_t = \frac{(1 - \theta_4 B^4)(1 - \theta_{12} B^{12})}{(1 - B)(1 - B^{12})(1 - \phi_1 B)(1 - \phi_2 B^2)} a_t + w(1 - dB)^{-1} P_t$$

Parameter estimates are

$\phi_1 = -.69074;\ t = -7.05$
$\phi_2 = -.58813;\ t = -4.81$
$\theta_4 = .27738;\ t = 1.98$
$\theta_{12} = .93462;\ t = 39.90$
$d = .90338;\ t = 7.66$
$w = -208.85;\ t = -2.99$

For these estimates, RSE = 81.34 and $Q_{26} = 28.21$.

(7) *Injury-producing crashes, southern*, 1/70 to 12/78: the model identified for this series is

$$Y_t = \frac{(1 - \theta_1 B)(1 - \theta_{12} B^{12})}{1 - B^{12}} a_t + w(1 - dB)^{-1} P_t$$

Parameter estimates are

$\theta_1 = -.31531;\ t = -3.10$
$\theta_{12} = .62125;\ t = 6.66$

$d = .45870;\ t = .56$
$w = -54.682;\ t = -.84$

For these estimates, RSE = 63.98 and $Q_{28} = 21.21$.

(8) *Wine sales*, 9/70 to 4/79: the model identified for this series is

$$Y_t = \frac{(1 - \theta_3 B^3)(1 - \theta_{12} B^{12})}{(1 - B^3)(1 - B^{12})} a_t + w(1 - dB)^{-1} P_t$$

Parameter estimates are

$\theta_3 = .38123;\ t = 3.68$
$\theta_{12} = .93325;\ t = 43.35$
$d = -.29243;\ t = -.40$
$w = -1.2248;\ t = -1.00$

For these estimates, RSE = 1.48 and $Q_{28} = 32.33$.

(9) *Mileage index*, 1/71 to 8/80: the model identified for this series is

$$Y_t = \frac{(1 - \theta_1 B)(1 - \theta_{12} B^{12})}{(1 - B)(1 - B^{12})} a_t + w(1 - dB)^{-1} P_t$$

Parameter estimates are

$\theta_1 = .87620;\ t = 18.82$
$\theta_{12} = .93511;\ t = 46.88$
$d = .30380;\ t = .70$
$w = .10269;\ t = .18$

For these estimates, RSE = .046 and $Q_{28} = 30.76$.

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