

Spring 1990

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Recommended Citation

H. Laurence Ross, Richard McCleary, Gary LaFree, Can Mandatory Jail Laws Deter Drunk Driving--The Arizona Case, 81 J. Crim. L. & Criminology 156 (1990-1991)

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CRIMINOLOGY

CAN MANDATORY JAIL LAWS DETER DRUNK DRIVING? THE ARIZONA CASE*

H. Laurence Ross,**
Richard McCleary,***
and Gary LaFree****

I. INTRODUCTION

The political climate of the 1980s in America has led to the view that drunk driving is a violent crime, properly punishable with time served in jail. This evolving viewpoint has led to a little-recognized confrontation between the branches of government. Legislatures, influenced by groups like Mothers Against Drunk Driving (MADD) to view the behavior as extremely dangerous, have tried to force this perception on a resistant judiciary. The judicial belief that in most cases drunk driving is a none-too-dangerous traffic infraction results from the experience of processing numerous cases in which no harm resulted. Furthermore, judges face imperative demands to move cases through the system by agreeing to reduced penalties for guilty pleas. One outcome of this confrontation has been for legislatures to mandate jail sentences for convicted drunk drivers and to prohibit plea bargaining as a means to avoid these sentences.

As of late 1988, mandatory jail for convicted drunk drivers had become law in forty-two states in the case of repeat offenders and in fourteen states for first offenders.¹ Typically, the mandatory incarceration is for relatively brief periods, and some statutes permit sub-

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¹ Nichols & Ross, *The Effectiveness of Legal Sanctions in Dealing with Drinking Drivers*, in OFFICE OF THE ATTORNEY GENERAL, U.S. PUBLIC HEALTH SERVICE, THE SURGEON GENERAL'S WORKSHOP ON DRUNK DRIVING: BACKGROUND PAPERS 93-112 (1989).

stitution of community service.² Despite the brevity of the jail sentences and the occasional possibility of substituting community service, these laws have been strongly supported as providing the severity necessary for effective deterrence of drunk driving.

This support relies on the conventional belief in deterrence that the threat of swift, certain, and severe punishment can reduce undesirable behavior. The empirical evidence for the deterrent effectiveness of severe penalties such as jail is, however, inconsistent at best. In Norway and Sweden, where drunk driving laws carrying jail penalties have been in place for a half-century, there appears to be less impaired driving than in many other Western countries,³ but Ross⁴ has shown that the causal connection between these laws and low rates of impaired driving is unproved in Scandinavia. Votey and Shapiro,⁵ in work intended to criticize Ross's position, conclude that fines and license actions have been more effective than jail in producing the Scandinavian results. In the United States, early studies of severity-based drunk driving interventions all produced negative findings.⁶ More recently, an evaluation of mandatory jail for first offenders in the state of Tennessee found that, although implementation was relatively good, there was no significant change in either awareness of the law or measures of the extent of drunk driving.⁷ Similarly, a case study of an Ohio jurisdiction where the judge routinely sentences first-offense drunk drivers to jail found no evidence of reduced drinking and driving even though the penalties were well known among the citizens.⁸

Two recent United States studies claim to have found evidence

² See, e.g., IND. CODE ANN. § 9-11-3-4 (West 1983).

³ J. ANDENAES, *THE SCANDINAVIAN EXPERIENCE IN SOCIAL CONTROL OF THE DRINKING DRIVER* 43-63 (M.D. Laurence, J.R. Snortum & F.E. Zimring eds. 1988); Ross, *The Scandinavian Myth: The Effectiveness of Drinking-and-Driving Legislation in Sweden and Norway*, 4 J. LEGAL STUD. 285 (1975).

⁴ Ross, *supra* note 3.

⁵ Votey & Shapiro, *Highway Accidents in Sweden: Modeling the Process of Drunken Driving Behavior and Control*, 15 ACCIDENT ANALYSIS & PREVENTION 523 (1983).

⁶ See J. GRUBE & K. KEARNEY, *THE YAKIMA COUNTY DRINKING AND DRIVING PROJECT: AN EVALUATION OF A MANDATORY JAIL SENTENCE AND A PUBLIC AWARENESS CAMPAIGN* (1980); Robertson, Rich, & Ross, *Jail Sentences for Driving While Intoxicated in Chicago: A Judicial Policy that Failed*, 8 LAW & SOC'Y REV. 55 (1973); Voas, *A Systems Approach to the Development and Evaluation of Countermeasures Programs for the Drinking Driver*, in PROCEEDINGS OF THE FOURTH ANNUAL ALCOHOLISM CONFERENCE OF THE NATIONAL INSTITUTE ON ALCOHOL ABUSE AND ALCOHOLISM 38-44 (Dep't of Health, Education and Welfare 1975).

⁷ R.K. JONES, H.C. JOKSCH, J.H. LACEY, & H.J. SCHMIDT, *FINAL REPORT, FIELD EVALUATION OF JAIL SANCTIONS FOR DWI* (National Highway Traffic Safety Administration 1988).

⁸ H.L. ROSS & R. VOAS, *THE NEW PHILADELPHIA STORY: THE EFFECTS OF SEVERE PUNISHMENT FOR DRUNK DRIVING* (AAA Foundation for Traffic Safety 1989).

for a deterrent effect of mandatory jail laws. One instance involved two independent analyses by Falkowski and by Cleary and Rodgers of the impact of a policy by judges in Minneapolis which led to the brief jailing of the vast majority of convicted drunk drivers in that city.⁹ The evaluations found that nighttime fatal crashes—a generally accepted indicator of drunk driving—declined significantly near the time of implementation of the jailing policy. Although reductions were also found statewide and in neighboring Ramsey County (St. Paul), these were smaller than those in Hennepin County, where the jailing policy was in effect. Confidence in these results must be tempered by the fact that numerous other changes were made in the drunk driving law at about the same time, which could have caused the change. Moreover, in the Falkowski study, the crash reductions appeared following an unexplained two-month delay beyond the policy's inception.

A different kind of evidence favoring the deterrent effectiveness of jail comes from a cross-sectional study of state laws.¹⁰ The types of law found effective by the research team included "illegal per se" laws,¹¹ administrative license-revocation laws, and mandatory-jail laws.¹² The laws providing for mandatory jail terms were credited with producing a six percent reduction in late-night fatal crashes.¹³ Although negative results were found in a similarly designed study by Joksch,¹⁴ the study by Zador and his colleagues seems to have utilized more appropriate comparisons, and its results are more credible.

In sum, the question of whether the threat of a mandatory brief jail sentence can be effective in deterring drunk driving is an open one. A majority of the individual case studies failed to find evidence for effectiveness of jail, but two independent teams concluded that

⁹ C. FALKOWSKI, *THE IMPACT OF TWO DAY JAIL SENTENCES FOR DRUNK DRIVERS IN HENNEPIN COUNTY, MINNESOTA* (National Highway Traffic Safety Administration 1984); J. CLEARY & A. RODGERS, *ANALYSIS OF THE EFFECTS OF RECENT CHANGES IN MINNESOTA'S DWI LAWS, PART III: LONGITUDINAL ANALYSIS OF THE POLICY IMPACTS* (Minnesota House of Representatives Research Department 1986).

¹⁰ P. ZADOR, A. LUND, M. FIELDS & K. WEINBERG, *FATAL CRASH INVOLVEMENT AND LAWS AGAINST ALCOHOL IMPAIRED DRIVING* (Insurance Institute for Highway Safety 1988).

¹¹ Such laws criminalize not merely impaired driving, but driving while the level of alcohol in the blood exceeds an arbitrary standard such as .10%. The blood alcohol level is determined by a chemical test of blood or breath, such as the Breathalyzer test. Such a law is intended to simplify and render more certain the prosecution of drunk drivers.

¹² P. ZADOR, A. LUND, M. FIELDS & K. WEINBERG, *supra* note 10.

¹³ *Id.*

¹⁴ H. JOKSCH, *THE IMPACT OF SEVERE PENALTIES ON DRINKING AND DRIVING* (1988) (final report to MidAmerica Research Institute of New England).

the Minneapolis judges may have achieved their goal, and the better-designed of the national correlational analyses supports the deterrence predictions. This report adds to the literature evidence from experience in another mandatory-jail jurisdiction, the state of Arizona, since 1982.

II. THE ARIZONA CASE

In the context of drunk-driving laws prevailing at the time, the Arizona law of 1982¹⁵ appears to have been exceptionally severe. A first-time offender, whether guilty of impaired driving (Section A) or driving with a blood-alcohol level concentration (BAC) of more than .10% (Section B, the illegal per se provision), was subject to a sentence including a mandatory twenty-four hours in jail as well as a \$250 fine and a ninety-day license suspension.¹⁶ Further, second-time offenders within three years faced sixty days in jail along with fines and license actions, and third-time offenders faced six months in jail along with other penalties.¹⁷ The law also prohibited plea bargaining: if convicted, persons accused of drunk driving were to be found guilty on the original charges.¹⁸ This would prevent an offender's being charged merely with a first offense if arrested for drunk driving on a subsequent occasion.

One formally trivial exception to the mandatory jail provision eventually proved important. Judges were authorized upon the prosecutor's recommendation to impose an alternative sentence for first-time offenders who had inflicted no injury and whose BAC did not exceed .20%.¹⁹ This alternative avoided a jail commitment, and although it included a fine and license suspension, the suspension was fully imposed (or "hard") for only thirty days, following which the offender had permission to drive for work-related purposes. In order to limit resort to the alternative sentence, the legislature stipulated that its use in any given case required the prosecutor's assent.

In 1984, the Arizona Supreme Court ruled on the constitutionality of the legislation in *State v. Jones*.²⁰ The court held that the portion of the 1982 law which made alternative sentencing dependent upon the prosecutor's recommendation violated the principle of the separation of powers in the Arizona constitution.²¹ After *Jones*,

¹⁵ The Act was approved April 26, 1982. 1982 Ariz. Sess. Laws 716, ch. 234.

¹⁶ *Id.* at 723.

¹⁷ *Id.* at 724.

¹⁸ *Id.*

¹⁹ *Id.* at 723.

²⁰ 689 P.2d 561 (Ariz. Ct. App. 1984).

²¹ *Id.* at 563.

judges could actively use their discretion to pronounce alternative sentences without prosecutorial assent. During the field work for this study in the summer of 1986, the alternative sentence had become routine for first-time-offender drunk drivers. Prosecutors and defenders in the Phoenix courts estimated that up to 99% of convicted drunk drivers were avoiding jail sentences. Although the statute limited the alternative sentence to drivers with BACs less than .20%, the courts reportedly widely violated this limitation and apparently jailed only those offenders who were found to have been "smart in the street" or whose cases involved "aggravating circumstances" such as reckless behavior.

The eventual failure to jail most offenders was not suspected at the law's inception, however, and extensive publicity campaigns preceding and accompanying its enactment prominently highlighted the mandatory jail sentence.²² Such media attention ought to have increased the law's deterrent value, as deterrence theory premises its prediction of behavior modification on the perceived severity, certainty, and swiftness of the threatened punishment. If the government was able to convince the relevant public that a new law would sharply increase punishment for drunk drivers, the legal change should have produced a notable decline in the threatened behavior. Although in time the courts extensively vitiated the mandatory jail requirement, the Arizona law still furnishes an appropriate case for investigating the ability of such threats to deter drunk driving.

III. DATA AND ANALYSIS

As police know, the behavior of driving under the influence of alcohol is not easy for persons outside the car to observe. Therefore, the extent of drunk driving is difficult to measure directly. Most research on drunk driving employs indirect or surrogate measures, such as nighttime fatalities. In many circumstances these are the best available data. However, in recent years the federally-sponsored Fatal Accident Reporting System (FARS), based on blood-alcohol measures taken in fatal crashes, has provided a more valid index. In areas where the testing of crash victims is fairly complete, FARS data are the most useful measure for investigating the impact of drunk driving countermeasures.²³

²² Epperlein, *Initial Deterrent Effects of the Crackdown on Drinking Drivers in the State of Arizona*, 19 ACCIDENT ANALYSIS & PREVENTION 285-86, 289-90 (1987).

²³ These data may be obtained, usually without charge, from the National Highway Traffic Safety Administration, Washington, D.C.

We obtained FARS data for Maricopa County, Arizona—the county including Phoenix and its suburbs—from the inception of the system in 1980 through 1986. Nearly half the state's population resides within this county, and the Phoenix courts had the reputation of being vigorous enforcers of the drunk driving legislation from its inception. We also obtained data on the coverage of the law by the principal Phoenix newspaper. We analyzed the data by applying an interrupted time-series quasi-experimental research design in order to verify whether alcohol-related fatalities had decreased significantly at the time of the law's inception. Support for this hypothesis would lend credibility to the idea that the mandatory jail threat can deter drunk driving.

Table 1 summarizes our analysis. To measure the law's effect,

TABLE 1
SUMMARY OF INTERRUPTED TIME SERIES ANALYSIS OF THE
EFFECTS OF THE 1982 ARIZONA LAW AND ATTENDANT PUBLICITY

<u>Model</u>	<u>Law Effect</u>	<u>Media Effect</u>	<u>Interaction Effect</u>	<u>Residual Variance</u>
<u>Model 1</u>				35.16
<u>Model 2</u>	-5.21			34.04
<u>Model 3</u>		-.08		34.99
<u>Model 4</u>	5.74*	-.11		33.68
<u>Model 5</u>	9.88*	-.37	.34	33.09

* Statistically significant at $p=0.05$.

we created a model (Model 1)²⁴ of the monthly time series of Maricopa County fatalities. Model 1 accounts for approximately 23% of the variance in monthly fatalities as a function of seasonal variation and stochastic drift. In other words, the observed data reflect the fact that crashes are more frequent at some times of the year than at others, and that there seems to be a long term decline in Maricopa County fatalities, which is not uncommon in FARS data nationally. Model 1, incorporating these phenomena, serves as a benchmark to assess the impact of the 1982 law and its attendant publicity.

Model 2²⁵ incorporates a simple before-after variable to accommodate the hypothetical effect of the 1982 law. We found an apparent intervention effect in the form of a reduction of slightly more

²⁴ Parameter estimates and detailed statistics are given in the Technical Appendix. See *infra* appendix.

²⁵ See *infra* appendix.

than five fatalities per month in the county, or a 17% reduction on the base of approximately thirty fatalities per month before the law. However, this reduction was not statistically significant; that is, we could not say with assurance that the decline was anything other than a random fluctuation in the series, like numerous other monthly declines observed in its course. Put differently, Model 2 does not significantly improve over the knowledge base established in Model 1, and cannot vouch for the effectiveness of the law.

Model 3²⁶ incorporates a measure of publicity—the number of relevant articles published in the chief newspaper in the area, the *Arizona Republic*. Surprisingly to us, the publicity effect was a reduction of less than one fatality per month, an effect which is neither statistically nor substantively significant. Again, the information accounted for in Model 3 provides no improvement over that contained in Model 1: there is no evidence that publicity about the mandatory jail law affected alcohol-related deaths.

Model 4²⁷ incorporates both the legal variable of Model 2 and the publicity variable of Model 3. Controlling for publicity renders the change associated with the law statistically significant, but the improvement of Model 4 as a whole (including both the law and publicity) over Model 1 (with no information about either) is not statistically significant. Therefore, we cannot conclude that the data provide any support for the proposition that even well-publicized mandatory jail sentences are capable of reducing alcohol-related crash fatalities. Model 5²⁸ incorporates the variables used in the preceding models, adding an interaction term to allow for the possibility of differences in their interrelations depending on background variables. It leads to the same conclusions as the balance of the analysis.

The analysis is handicapped because Maricopa County produces only a small number of fatalities.²⁹ This leads to a problem in statistical power: if the law produced a real but small effect, it would be hard to detect with our methods. A longer time series prior to the law would help resolve this problem, but FARS data collection began only in 1980. However, we can say confidently that if a deterrent effect sufficiently important to warrant the investment of major legal resources had been present, the chances are good that we would have seen evidence of it. Our best estimates do not support

²⁶ See *infra* appendix.

²⁷ See *infra* appendix.

²⁸ See *infra* appendix.

²⁹ The monthly number of vehicle-related fatalities in the entire state averages around 70, and fewer than half of these occur in Maricopa County.

the hopes and expectations on the part of citizen activists and policymakers that led to the 1982 Arizona law.

The conclusion that the law very likely had no important deterrent effect is supported by evidence from an independent study, published after our field work was done, which used state accident files rather than FARS data and covered all of Arizona rather than just Maricopa County.³⁰ A score of judges, prosecutors, defense attorneys, court administrators, and other legal system personnel whom we interviewed also concurred in the conclusion that no important benefits were achieved by the 1982 mandatory jail law.

IV. DISCUSSION

In our opinion, the failure of the courts to implement the law as intended is not a convincing explanation of its impotence. The legislature indeed meant business. In the words of the Arizona drivers license manual: "Get caught driving drunk in Arizona and you're going to spend at least 24 hours in jail . . . You'll have your license suspended for 90 days, too." Initial news stories assumed that jail would be the routine penalty for drunk drivers, and we based our theoretical expectation of deterrence on perceptions prevailing at the inception of the law. We expected no delay for the deterrent effect, if an effect did occur. The *Jones* decision came two years after the law's enactment. Moreover, even at the time of our field work in 1986, people we spoke with in the Phoenix legal community believed that most members of the public expected to be treated severely when convicted of drunk driving. As one person put it: "It's pitiful to hear them beg and plead. . . . They believe they've had it—they're going to jail."

The failure to incarcerate, though notorious among informed people, is probably not well known by ordinary drivers in Arizona even today, and it was certainly not suspected in the early days of the 1982 law. It cannot explain the law's failure to deter.

A more reasonable explanation lies in the possibility that the public disregards threats when it perceives a negligible likelihood of these threats being applied. The actual chances of apprehension for a drunk driver in an American jurisdiction are estimated to range between 1 in 200 and 1 in 2000.³¹ Hence, drivers who perceive a severe punishment if caught, but a near-zero chance of being

³⁰ Epperlein, *supra* note 22.

³¹ I. L. SUMMERS & D. HARRIS, NATIONAL HIGHWAY TRAFFIC SAFETY ADMINISTRATION, *THE GENERAL DETERRENCE OF DRIVING WHILE INTOXICATED* (1978) (Technical Report DOT-HS 803 582).

caught, are being rational in ignoring the threat. We were unable to find Maricopa County data directly on point, but a survey taken in two rural Arizona counties in November 1982, shortly after the legislature enacted the law, found that 25% of the respondents described the probability of their being caught by the police when driving drunk as either low or very low, and 15% thought that the probability of being punished even if caught was low or very low.³²

Although the majority professed other beliefs, it is possible that the optimistic (and realistic) minority seeing relatively little risk might ignore the new law. Moreover, this minority may be comprised of those people who are most in need of having their impulses controlled by deterrent threats, yet who are at the same time the most immune to the threats. Some research has found that drunk drivers are disproportionately likely to be problem drinkers and alcoholics, as well as young working-class men whose attitudes towards compliance with law in general may be casual. As one state safety official noted: "The thinking class drinks less, but the blue-collar areas are untouched now." A traffic court judge added: "The major effect has been on people who don't cause that many accidents anyway [F]or the real problem drinker, nothing deters."

Another possible explanation for the failure of the law is that members of the target population did not perceive it as severe. Jail may be symbolically important to middle-class people, including the vast bulk of members of citizen activist groups like MADD. However, defense attorneys in Phoenix often told us that the punishment most threatening to their clients was not jail, but license suspension. The effect of the 1982 law on this component of punishment for drunk drivers is unclear, and it was not highlighted in publicity. Many habitual drunk drivers may have viewed the 1982 law as constituting less of a change than did the law's initiators and supporters.

V. CRIMINAL JUSTICE SYSTEM EFFECTS

Our research also addressed the impact of a law mandating extensive use of jail on the operation of the Arizona criminal justice system. Although the intent of the law was to increase sanction severity, we expected defendants to increase their resistance to accusations by obtaining counsel and demanding jury trials. We also expected to find increasing burdens on prosecutors, courts, and jails, due to an increasing case load.

³² M. Mykyta, Governor's Office of Highway Safety, *Attitudes of Coconino and Yavapai County Residents Toward Driving and Drinking (1982)* (unpublished report).

Had the law worked as planned, our initial expectations might have been fulfilled amply. Indeed, effects were noted in the city of Phoenix, where authorities enforced the law most strictly. The Phoenix courts experienced a significant increase in trials, which stretched available resources and resulted in lengthy delays for processing offenders, thus paradoxically reducing the swiftness of punishment for drunk driving in the attempt to increase its severity. The "durability" or time to conclusion of Phoenix drunk driving cases doubled in the first two years of the law. By November, 1984, 1,500 cases were awaiting trial in the city. Four additional temporary courtrooms were added in November and December of that year, and two of these were made permanent in the following summer. Although the claim of a state official that "the Phoenix courts are destroyed" seems excessive, the future remains hostage to the investment decisions of defendants and their lawyers, whose cases and fees dramatically increased after 1982.

In other communities, where enthusiasm for the law appears to have been less vigorous, prosecutors allegedly offered to drop drunk driving charges on grounds of insufficient evidence and to accept guilty pleas to collateral offenses that would not result in the loss of a license. Such plea bargains would also conceal repeat-offender status in the event of a rearrest. We heard no reports of increased numbers of trials outside of Phoenix.

Delay in the Phoenix courts threatened Arizona's compliance with federal regulations requiring swiftness in handling drunk driving cases. The average time from arrest to conviction increased from fifty-one days before the law to 108 days at the time of our study, exceeding the United States Department of Transportation guideline of ninety days and incurring the possibility of reduced federal highway subsidies. In order to counter the backlog, the state legislature passed administrative *per se* legislation, which had the effect of shifting the most feared sanctions from the criminal process to the administrative one, and diminishing the usefulness of demanding a jury trial in the vast majority of cases where a possible jail sentence was not at issue.³³

In light of the near-universal use of the alternative sentence, it is hardly surprising that the 1982 law had little impact on Arizona jails. Officials of the Phoenix jail had been warned to expect large numbers of new, short-term inmates on top of the 3000 prisoners normally in custody each day, but these expectations diminished with the passage of time. According to a spokesman for the Mari-

³³ ARIZ. REV. STAT. ANN. § 28-694 (1987).

copa County Sheriff: "Initially we were told to expect an increase in jail population of 1000 a day. This was modified as things settled down to a prediction of 100 a day. We think the impact has been much less, but the public is probably not aware of this." If drunk drivers face jail in Phoenix or elsewhere in Arizona today, it is most likely to be in the hours immediately following arrest, during booking, rather than as a consequence of a post-trial sentence.

VI. CONCLUSION

Increasing the severity of threatened punishment for drunk drivers through mandatory jail sentences does not appear to have been a successful deterrent in Arizona. We found no significant reduction in the measure of drunk driving when the law was implemented. It is possible that the threatened punishment lacked credibility because of a low perceived probability of its implementation. Another possibility is that a day in jail was not regarded as severe punishment by the relevant population, those who drive while impaired, especially as compared with the alternative of license actions. The problems of obtaining deterrent effects using jail have been confirmed in other research,³⁴ and these results, though unexpected by some people, are not surprising.

The relatively restricted consequences of the 1982 Arizona law for the criminal justice system of the state testify to the lack of implementation. In Phoenix, where commitment to applying the penalties was strongest, the need for additional resources for prosecuting offenders led to unacceptable reductions in the swiftness of punishment, and eventually to the need for legislation shifting the locus of important penalties from the criminal to the administrative process.

We have tried to speculate about the reasons for the failure of the Arizona law, with its attendant publicity, to reduce drunk driving. Whether or not we have succeeded in our explanation, it is clear that the evidence does not support the validity of deterrent expectations for a policy of mandatory jail for drunk drivers. This finding confirms those of several other studies and reinforces the balance of evidence against deterrence in this kind of situation. It may serve to caution those who would further extend the use of jail sentences for drunk drivers. A jailing policy can perhaps be defended on the grounds that drunk drivers deserve punishment. It cannot be defended on the grounds that punishing them severely will save the lives of others.

³⁴ Nichols & Ross, *supra* note 1.

TECHNICAL APPENDIX
AUTOCORRELATIONS

No Differences

1-12	.34	.27	.44	.44	.16	.25	.39	.15	-.01	.29	.15	-.04
ST.E.	.12	.13	.14	.16	.17	.17	.18	.19	.19	.19	.20	.20
Q	8.5	13.9	28.9	44.3	46.4	51.4	64.1	66.1	66.1	73.4	75.3	75.5
13-24	.08	.02	-.14	-.08	-.04	-.18	-.24	-.05	-.21	-.35	-.19	-.08
ST.E.	.20	.20	.20	.20	.20	.20	.21	.21	.21	.21	.22	.22
Q	76.1	76.2	77.9	78.5	78.6	81.9	87.9	88.1	92.9	106	110	110

First Differences

1-12	-.46	-.18	.13	.22	-.28	-.03	.29	-.07	-.35	.34	.02	-.24
ST.E.	.12	.14	.14	.15	.15	.16	.16	.17	.17	.18	.18	.18
Q	15.5	17.9	19.2	22.9	29.2	29.3	35.9	36.3	46.3	56.3	56.4	61.6
13-24	.15	.08	-.17	.02	.14	-.07	-.20	.29	-.03	-.22	.04	.28
ST.E.	.19	.19	.19	.19	.19	.19	.20	.20	.20	.20	.21	.21
Q	63.5	64.1	66.8	66.9	68.8	69.2	73.3	82.1	82.2	87.2	87.3	96.1

Third Differences

1-12	-.06	.02	-.28	.19	-.04	.07	-.00	-.03	-.25	.09	.06	.03
ST.E.	.12	.12	.12	.13	.13	.13	.13	.13	.13	.14	.14	.14
Q	.3	.3	6.2	9.0	9.1	9.5	9.5	9.5	14.7	15.4	15.7	15.8
13-24	-.03	-.03	-.02	.03	-.06	-.01	-.06	.18	-.10	-.07	-.07	.25
ST.E.	.14	.14	.14	.14	.14	.14	.14	.14	.15	.15	.15	.15
Q	15.9	15.9	16.0	16.1	16.4	16.4	16.7	19.9	21.0	21.4	22.0	28.7

Fourth Differences

1-12	.05	.00	.18	-.16	.02	-.02	.15	-.12	-.31	.24	.01	-.21
ST.E.	.12	.12	.12	.13	.13	.13	.13	.13	.13	.14	.15	.15
Q	.2	.2	2.6	4.4	4.4	4.4	6.2	7.4	15.1	19.8	19.8	23.5
13-24	.21	-.04	-.14	-.04	.05	-.02	-.12	.11	.04	-.20	.10	.18
ST.E.	.15	.16	.16	.16	.16	.16	.16	.16	.16	.16	.17	.17
Q	27.3	27.5	29.3	29.5	29.7	29.7	31.2	32.3	32.4	36.4	37.4	41.1

Sixth Differences

1-12	.08	-.07	.32	.28	-.09	-.26	.20	.02	-.25	.08	.05	-.14
ST.E.	.12	.12	.12	.14	.14	.15	.15	.16	.16	.16	.16	.16
Q	.5	.8	7.9	13.6	14.2	19.3	22.4	22.4	27.3	27.8	28.0	29.6
13-24	.04	-.08	-.02	-.02	-.00	-.14	-.11	.15	-.01	-.23	.08	.19
ST.E.	.16	.17	.17	.17	.17	.17	.17	.17	.17	.17	.18	.18
Q	29.7	30.3	30.3	30.3	30.4	32.3	33.5	35.8	35.9	41.1	41.8	45.6

Twelfth Differences

1-12	.23	.12	.55	.39	.03	.26	.38	-.08	-.11	.31	-.00	-.41
ST.E.	.13	.14	.14	.17	.18	.19	.19	.20	.20	.20	.21	.21
Q	3.3	4.2	24.1	34.4	34.4	39.0	48.9	49.4	50.2	57.4	57.4	70.2
13-24	.10	-.01	-.24	-.14	.04	-.25	-.31	.04	-.12	-.36	-.14	-.05
ST.E.	.22	.23	.23	.23	.23	.23	.24	.24	.24	.24	.25	.25
Q	70.9	71.0	75.5	77.2	77.3	82.7	91.2	91.4	92.9	105	108	108

MODEL (1) — NO EFFECT

PARAMETER LABEL	VARIABLE NAME	NUM./DENOM.	FACTOR	ORDER	VALUE	STD ERROR	T VALUE
1	C	CNST	1	0	-.0883	.3881	-.23
2	TH3	FATALS	MA	1	.4312	.1019	4.23
3	TH4	FATALS	MA	1	-.2722	.1000	-2.72
4	TH9	FATALS	MA	1	.3441	.1106	3.11

TOTAL SUM OF SQUARES326632E+04
 TOTAL NUMBER OF OBSERVATIONS 72
 RESIDUAL SUM OF SQUARES..... .242606E+04
 R-SQUARE225
 EFFECTIVE NUMBER OF OBSERVATIONS ... 69
 RESIDUAL VARIANCE ESTIMATE351603E+02
 RESIDUAL STANDARD ERROR..... .592962E+01

Residual Autocorrelation Function

1-12	.10	.10	-.04	.06	.02	-.06	.04	.01	-.06	.12	.09	.04
ST.E.	.12	.12	.12	.12	.12	.12	.12	.12	.12	.12	.13	.13
Q	.7	1.5	1.6	1.9	1.9	2.2	2.3	2.3	2.6	3.8	4.5	4.6
13-24	.01	-.05	.03	-.02	-.02	-.05	-.07	.09	-.06	-.10	.01	.20
ST.E.	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13
Q	4.6	4.9	4.9	5.0	5.0	5.3	5.8	6.5	6.9	7.8	7.9	12.1

MODEL (2) — LAW EFFECT

PARAMETER LABEL	VARIABLE NAME	NUM./DENOM.	FACTOR	ORDER	VALUE	STD ERROR	T VALUE
1	C	CNST	1	0	.1552	.3975	.39
2	W	LAW	NUM.	1	-5.2077	3.2763	-1.59
3	THE3	FATALS	MA	1	.3970	.1009	3.93
4	THE4	FATALS	MA	1	-.2634	.0999	-2.64
5	THE9	FATALS	MA	1	.4282	.1140	3.76

TOTAL SUM OF SQUARES326632E+04
 TOTAL NUMBER OF OBSERVATIONS 72
 RESIDUAL SUM OF SQUARES..... .234888E+04
 R-SQUARE250
 EFFECTIVE NUMBER OF OBSERVATIONS ... 69
 RESIDUAL VARIANCE ESTIMATE340418E+02
 RESIDUAL STANDARD ERROR..... .583453E+01

Residual Autocorrelation Function

1-12	.06	.09	-.08	.06	.01	-.06	.03	-.01	-.06	.08	.08	.05
ST.E.	.12	.12	.12	.12	.12	.12	.12	.12	.12	.12	.12	.13
Q	.3	.8	1.3	1.6	1.6	1.9	2.0	2.0	2.3	2.8	3.3	3.5
13-24	.02	-.03	.07	-.01	-.02	-.06	-.04	.15	-.02	-.06	.01	.22
ST.E.	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13
Q	3.5	3.6	4.1	4.1	4.1	4.5	4.6	7.0	7.0	7.3	7.3	12.6

MODEL (3) — MEDIA EFFECT

PARAMETER LABEL	VARIABLE NAME	NUM./ DENOM.	FACTOR	ORDER	VALUE	STD ERROR	T VALUE
1	C	CNST	1	0	-.0474	.4194	-.11
2	V	MEDIA NUM.	1	0	-0.756	.1350	-.56
3	TH3	FATALS MA	1	3	.4428	.1055	4.20
4	TH4	FATALS MA	1	4	-.2854	.1075	-2.66
5	TH9	FATALS MA	1	9	.3291	.1109	2.97

TOTAL SUM OF SQUARES326632E+04
TOTAL NUMBER OF OBSERVATIONS	72
RESIDUAL SUM OF SQUARES.....	.241449E+04
R-SQUARE229
EFFECTIVE NUMBER OF OBSERVATIONS ...	69
RESIDUAL VARIANCE ESTIMATE349926E+02
RESIDUAL STANDARD ERROR.....	.591546E+01

Residual Autocorrelation Function

1-12	.11	.08	-.04	.04	.02	-.07	.02	.01	-.07	.12	.09	.05
ST.E.	.12	.12	.12	.12	.12	.12	.12	.12	.12	.12	.13	.13
Q	.8	1.3	1.4	1.5	1.5	1.9	1.9	1.9	2.3	3.5	4.2	4.4
13-24	.01	-.04	.04	-.03	-.04	-.06	-.09	.09	-.06	-.11	.03	.20
ST.E.	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13
Q	4.5	4.6	4.8	4.8	5.0	5.3	6.0	6.8	7.2	8.3	8.4	12.6

MODEL (4) — LAW EFFECT AND MEDIA EFFECT

PARAMETER LABEL	VARIABLE NAME	NUM./ DENOM.	FACTOR	ORDER	VALUE	STD ERROR	T VALUE
1	C	CNST	1	0	.2324	.3853	.60
2	W	LAW NUM.	1	0	-5.7363	3.2797	-1.75
3	V	MEDIA NUM.	1	0	-.1100	.1323	-.83
4	TH3	FATALS MA	1	3	.3802	.1013	3.75
5	TH4	FATALS MA	1	4	-.2383	.1022	-2.33
6	TH9	FATALS MA	1	9	.4650	.1137	4.09

TOTAL SUM OF SQUARES326632E+04
TOTAL NUMBER OF OBSERVATIONS	72
RESIDUAL SUM OF SQUARES.....	.232380E+04
R-SQUARE258
EFFECTIVE NUMBER OF OBSERVATIONS ...	69
RESIDUAL VARIANCE ESTIMATE336783E+02
RESIDUAL STANDARD ERROR.....	.580330E+01

Residual Autocorrelation Function

1-12	.06	.05	-.09	.06	.01	-.07	.01	.00	-.05	.08	.09	.06
ST.E.	.12	.12	.12	.12	.12	.12	.12	.12	.12	.12	.12	.13
Q	.3	.4	1.1	1.4	1.4	1.8	1.8	1.8	2.0	2.5	3.1	3.5
13-24	.02	-.03	.09	-.00	-.04	-.07	-.05	.16	-.02	-.06	.01	.22
ST.E.	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13
Q	3.5	3.6	4.3	4.3	4.5	5.0	5.2	7.7	7.8	8.1	8.1	13.4

MODEL (5) — LAW EFFECT, MEDIA EFFECT AND INTERACTION

PARAMETER LABEL	VARIABLE NAME	NUM./DENOM.	FACTOR	ORDER	VALUE	STD ERROR	T VALUE	
1	C	CNST	1	0	.3547	.3592	.99	
2	W	LAW	NUM.	1	0	-9.8843	4.7347	-2.09
3	V	MEDIA	NUM.	1	0	-.3737	.2672	-1.40
4	U	INTER	NUM.	1	0	.3381	.3058	1.11
5	TH3	FATALS	MA	1	3	.3403	.0953	3.57
6	TH4	FATALS	MA	1	4	-.1898	.0916	-2.07
7	TH9	FATALS	MA	1	9	.5355	.1100	4.87

TOTAL SUM OF SQUARES 326632E+04
 TOTAL NUMBER OF OBSERVATIONS 72
 RESIDUAL SUM OF SQUARES..... 228324E+04
 R-SQUARE271
 EFFECTIVE NUMBER OF OBSERVATIONS ... 69
 RESIDUAL VARIANCE ESTIMATE 330904E+02
 RESIDUAL STANDARD ERROR..... 575242E+01

Residual Autocorrelation Function

1-12	.04	.02	-.10	.06	-.01	-.13	-.00	-.01	-.02	.05	.07	.09
ST.E.	.12	.12	.12	.12	.12	.12	.12	.12	.12	.12	.12	.13
Q	.1	.2	1.0	1.2	1.2	2.5	2.5	2.5	2.5	2.7	3.1	3.7
13-24	.06	-.02	.07	-.03	-.02	-.07	-.05	.14	-.01	-.01	.03	.23
ST.E.	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13	.13
Q	4.1	4.1	4.5	4.6	4.7	5.2	5.5	7.4	7.4	7.4	7.5	13.5