MOTORCYCLE FATALITIES AND THE HELMET LAW REPEAL: A POLICY IMPACT STUDY

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PREFACE

This paper analyzes the impact on motorcycle fatalities of the partial repeal in 1977 of Minnesota's mandatory motorcycle helmet law. Several statistical models are developed and estimated using longitudinal data from 1971 through 1980, and an attempt is made to statistically control for the effects of other possible explanatory factors. It is concluded that motorcycle fatalities have increased significantly due to the helmet law revision.

This report has been written for a broad audience of legislators, administrators, and other interested parties. An attempt has been made to present a fairly detailed discussion of the analysis in relatively non-technical language. While a statistical background would be helpful in reading this report, it is not necessary for understanding most of the material presented herein.

Nevertheless, the authors have also prepared a summary report of this analysis for anyone wanting a brief overview of the analytic logic and findings. That report, which may be obtained from this office, is entitled: Minnesota Motorcycle Fatality Rates and the Helmet Law Repeal: A Summary Report (January, 1981).

This policy impact study was performed by James D. Cleary, Research Methodologist, and John M. Williams, Legislative Analyst, of the House Research Department. Mark Reynolds-Rucinski, Research Assistant, contributed significantly to every phase of this research. Carol Thompson, Fran Anthonsen and Marigale McAndrew provided secretarial assistance. Any questions regarding this research should be addressed to either of the authors at 612-296-6753.

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INTRODUCTION

Motorcycle and Non-Motorcycle Traffic Fatality Trends

The 1967 Minnesota Legislature, responding to a Federal mandate under the National Highway Traffic Safety Act of 1966, enacted legislation requiring all motorcycle operators and passengers to wear protective helmets. However, in 1976 Congress removed from the U.S. Department of Transportation the authority to refuse funding to states not having a mandatory helmet law for motorcyclists over the age of 18; many states responded by repealing all or part of their mandatory helmet laws. The 1977 Minnesota Legislature joined this trend by amending Minnesota's helmet law to make it apply only to operators and passengers under the age of 18, a requirement still retained in the federal safety standards (Laws of Minnesota 1977, Chapter 17, Section 5). This partial repeal of the helmet law became effective April 7, 1977.

The Minnesota Department of Public Safety (1979) reported that this action resulted in a decrease in helmet usage of from nearly 100 percent in 1976 to approximately 53 percent in 1979. During the same period, motorcycle traffic fatalities have nearly doubled.[1] Table 1 reveals that the number of motorcycle fatalities increased sharply and suddenly from an average of 56 per year during the 1971-1976 period to an average of 103 per year during the 1977-1980 period.[2] This represents a statistically significant 84 percent increase, compared to a decrease of 9 percent for non-motorcycle traffic fatalities for the same period.[3]

The Research Question

From the data of Table 1 alone, it would be tempting to conclude that the 1977 helmet law revision is responsible for the observed surge in motorcycle fatalities beginning in that year. However, such a conclusion would be premature without a more thorough analysis of the fatality trends, including an analysis of other possible contributing factors. That is the purpose of this paper. Specifically, this analysis will attempt to determine whether and to what extent the increase in motorcycle fatalities may be due to the helmet law revision rather than to other factors.

A premise of this analysis is that the existence of a mandatory helmet law actually influences helmet usage. If this were not true, then any enactment or repeal of a helmet law could not possibly affect motorcycle fatality trends. Fortunately, this premise is well supported by prior research.

As noted above, the Minnesota Department of Public Safety (1979:1) found through actual roadway sampling and observation that compliance with the mandatory helmet law was nearly 100 percent in 1976 and that usage had dropped by nearly half by 1979, just three years after the statutory revision. This experience is consistent with findings in other states. The National Highway Traffic Safety Administration (1980:2) has reported that most states estimated a 95-100 percent compliance with their helmet laws during the time they were in effect, while the use of helmets following a repeal of the legal requirement generally falls to between 40 and 60 percent.

TABLE 1

A. ANNUAL MOTORCYCLE AND NON-MOTORCYCLE TRAFFIC FATALITIES

Year	Motorcycle Fatalities		Non-Motorcycle Traffic Fatalities	
1971	48	56 Average Annual Fatalities	976	864 Average Annual Fatalities
1972	54		977	
1973	63		961	
1974	51		801	
1975	63		714	
1976	57		752	
Helmet law revision[a]				
1977	94	103 Average Annual Fatalities	762	782 Average Annual Fatalities
1978	106		874	
1979	97		784	
1980[b]	116		708	

B. DIFFERENCE IN AVERAGE ANNUAL TRAFFIC FATALITIES: 1971 to 1976 VERSUS 1977 to 1980

	Motorcycle Fatalities	Non-Motorcycle Traffic Fatalities
Fatality Change:	+47 or +84%	-82 or -9%
Statistical Significance:	$F=88.06$ ($p<.0001$)	$F=1.45$ ($p=.26$)
Durbin-Watson Statistic	2.33	.79

[a] The helmet law revision actually became effective on April 6, 1977, and no motorcycle fatalities occurred in 1977 prior to the statutory revision.

[b] The 1980 motorcycle fatality figure is current through October, 1980, and might increas by a few deaths when final data becomes available. The number of 1980 non-motorcycle traffic fatalities includes a projection for November and December, proportionate with the prior year's distribution.

Watson, et al (1980) also observed that helmet usage in states which repealed helmet use laws dropped from nearly 100 percent to approximately 50 percent following repeal.

Overview of This Analysis

There are four principal parts to this analysis. The first part reviews and extends a univariate linear trend analysis of the annual fatality count which was recently published by the Department of Public Safety.[4] The second part involves applying the same analytic logic in developing a univariate seasonal (i.e., monthly) non-linear trend model and using it to assess the impact of the helmet law revision. The third part statistically controls for the possible effects of weather and usage factors which might account for part or all of the increase in motorcycle traffic fatalities. The fourth part statistically controls for the post-revision increase in motorcycle accidents as a proxy measure for an unspecified host of factors which could account for the increase in motorcycle fatalities.

A serious limitation in all of these analyses is the brevity of the pre- and post-statutory change periods. Ideally, one would have longitudinal data for a period at least two or three times longer than that covered by the data available here. Practically speaking, the statistical results are less trustworthy with limited data. However, legislative decision-making can rarely await the compilation of the longitudinal data necessary for statistical certitude. Instead, legislative analysis of important issues must proceed incrementally as the data gradually becomes available.

In such situations where statistical compromises must be made, it is helpful to apply two or more analytic methods if possible. To the extent that similar conclusions are reached, confidence in the results is enhanced. That approach is taken here to help compensate for the limited longitudinal data. In addition, this paper attempts to explicitly describe the analytic methods and logic used herein in order to facilitate extending and updating this analysis a few years hence, should that be desirable.

The DPS Motorcycle Helmet Study

Prior to launching into the analysis, it would be useful to clarify the intended contribution of the present study relative to the motorcycle helmet study recently published by the Minnesota Department of Public Safety (1979). The present study is clearly intended to compliment the DPS study. The DPS study provided a broad analysis of motorcycle accidents, injuries and fatalities. Its primary thrust was to compare the injury and fatality outcomes for helmeted versus non-helmeted motorcycle accident victims. While it used multi-year data, most analyses were cross-sectional rather than longitudinal. Nevertheless, that analysis very clearly and convincingly documented the protective effect of helmet usage.

However, one important shortcoming of the DPS study concerns its method for determining the increased number of motorcycle deaths and related costs resulting from the helmet law revision. The basic problem is that it fails to evaluate

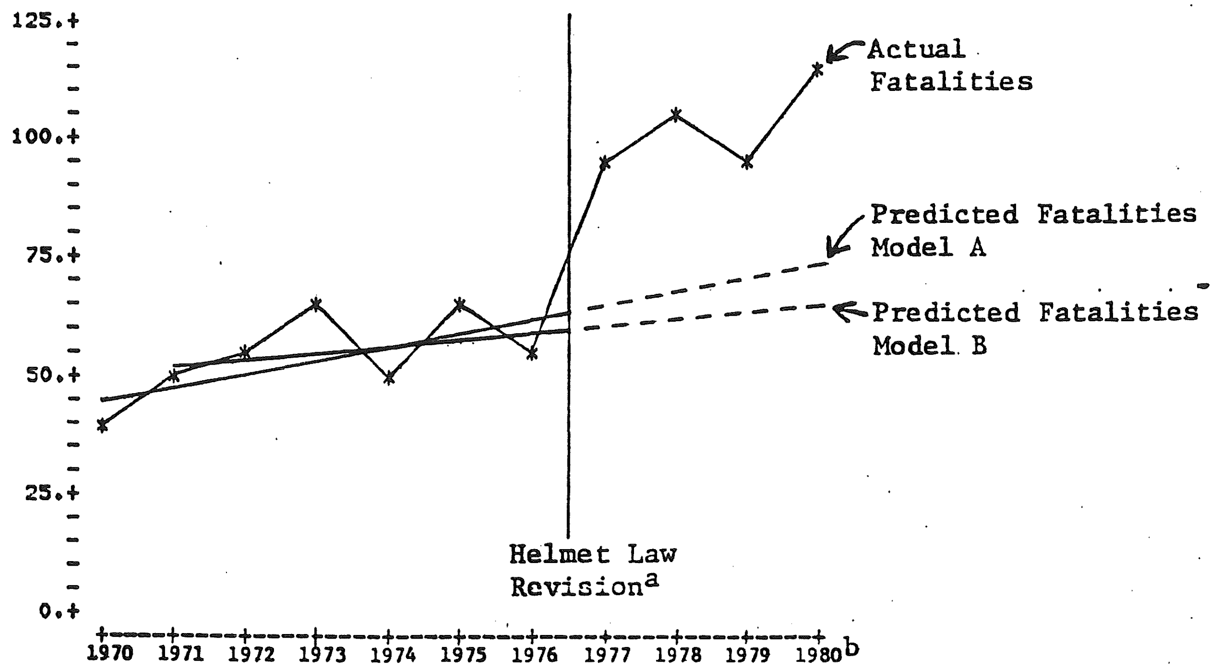
Figure 1 were determined in this manner. Each of these linear trend models is represented by the solid portion of the respective trend line, as well as by the respective equation below the diagram. The fatalities projected from these models for the 1977-1980 period are represented by the dashed portions of the trend lines, which have been computed from the equations.

Model A is the seven-year linear trend model which the Minnesota Department of Public Safety (1979:22-24) fit to the 1970-1976 motorcycle fatality data.[5] On the basis of this model, one would have expected a fatality increase of only 2.8 per year for the 1977-1980 period. Table 2, which summarizes these data, reveals that the actual fatalities exceeded the number predicted by Model A by 132 over the four-year period (i.e., 413 versus 281), an average of 33 per year.

However, prior to concluding anything from these findings it is important to evaluate Model A to determine how well it actually represents the 1970-1976 fatality trend (i.e., how well it "fits" the actual data). The R2 statistic in Figure 1 indicates that the trend line "explains" a considerable portion (53 percent) of the variance among the 1970-1976 fatality figures. Nevertheless, there is still a considerable scattering of the actual observations around the trend line, with the average annual difference (root mean squared error) between the actual and fitted values being 6.3. Thus, Model A "fits" the data only moderately well. Somewhat more disturbing, however, is the fact that the first fatality figure (40 in 1970) is the lowest, since an unusually low or high first or last observation in a time series can unduly influence the slope of the trend

FIGURE 1

ACTUAL AND PREDICTED MOTORCYCLE FATALITIES BASED ON UNIVARIATE LINEAR TREND MODELS



	Data Period	Regression Parameter Estimates[c]	Proportion of Variance Explained	Root Mean Squared Error[d]
MODEL A	1970-76	Y = 42.6 + 2.8(T)	R^2 = .53	6.3
MODEL B	1971-76	Y = 51.7 + 1.7(T)	R^2 = .27	5.9

---

[a] Though the helmet law revision occurred in April 1977, all fatalities for 1977 are regarded as having occurred after the change. Thus, as diagramed here the statutory change is functionally treated as if it occurred effective for all of 1977.

[b] The 1980 motorcycle fatality figure is current through October, 1980, and might increase by a few deaths when final data become available. The number of 1980 non-motorcycle traffic fatalities includes a projection for November and December, proportionate with the prior year's distribution.

[c] The intercept coefficient refers to the expected fatality figure for the first year of data (1970 for Model A, and 1971 for Model B). Also, the variable "T" in the model refers to the year since the starting year of data.

[d] The Durbin-Watson statistic equals 2.35 for Model A and 3.02 for Model B. Neither value appears statistically significant.

TABLE 2

DIFFERENCE BETWEEN ACTUAL MOTORCYCLE FATALITIES
AND FATALITIES PREDICTED USING 3 DIFFERENT TREND MODELS

YEAR	ACTUAL MOTORCYCLE FATALITIES	LINEAR TREND MODELS Model A (1970-1976) Predicted	Model A (1970-1976) Difference	Model B (1971-1976) Predicted	Model B (1971-1976) Difference	SEASONAL TREND MODEL Predicted	SEASONAL TREND MODEL Difference
1977	94	66	+ 28	62	+ 32	53	+ 41
1978	106	69	+ 37	64	+ 42	54	+ 52
1979	97[a]	72	+ 25	65	+ 32	54	+ 43
1980	116[b]	74	+ 42	67	+ 49	54	+ 62
TOTAL	413	281	+132	258	+155	215	+198
AVERAGE	(103.3)	(70.3)	(+33.0)	(64.5)	(+38.8)	(53.8)	(+49.5)

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[a] This figure is one larger than that shown in Table 6 of the Department of Public Safety (1979) report since this is based on completed reporting for that year.

[b] This figure is based on all motorcycle fatality reports available as of October 31, 1980. It is not likely that many additional fatalities will occur this year.

line which is fitted to it using ordinary least-squares regression (McCleary, et.al, 1980: 31-36).

Thus, it seems appropriate to re-fit the trend line to only the 1971-1976 fatality figures in order to determine the extent to which Model A has been influenced by the 1970 fatality figure and to determine whether a better fitting model can be found. This is done below.

Model B. Annual Motorcycle Fatalities 1971-1976

Model B in Figure 1 is the linear trend model fitted to only the 1971-1976 annual motorcycle fatality figures. The smaller average error (5.9 verses 6.3) reveals that Model B fits the fatality data somewhat better than Model A. However, the significant finding is the similarity of the two models. Model B is only slightly flatter and lower than Model A, leading to the conclusion that Model A was not greatly influenced by the rather low 1970 fatality figure. The relatively large discrepancy between the actual 1977-1980 motorcycle fatalities and the predicted number is undiminished and even increased slightly. The four-year total of 413 fatalities exceeds the 258 which would have been predicted from Model B by 155, or by an average of 38.8 per year (Table 2).

The general similarities between Models A and B is reassuring. Had the trend line been greatly influenced by the exclusion of the 1970 fatality figure, one would have greater reason to question its stability. Nevertheless, there is still a considerable scattering of the actual annual fatality figures around each trend line. Perhaps a better fitting non-linear, seasonal trend model could be developed and would provide a basis for more meaningful 1977-1980 predictions. Such a model is developed in the following section.

SEASONAL TREND ANALYSIS

The first step in constructing a seasonal trend model is to compute and interpret the autocorrelations and partial autocorrelations among the lagged monthly fatality figures. The patterns in such correlations generally suggest the type of model to use to represent the data. Figure 2A presents the autocorrelations for lags of 1 to 36 months.[6]

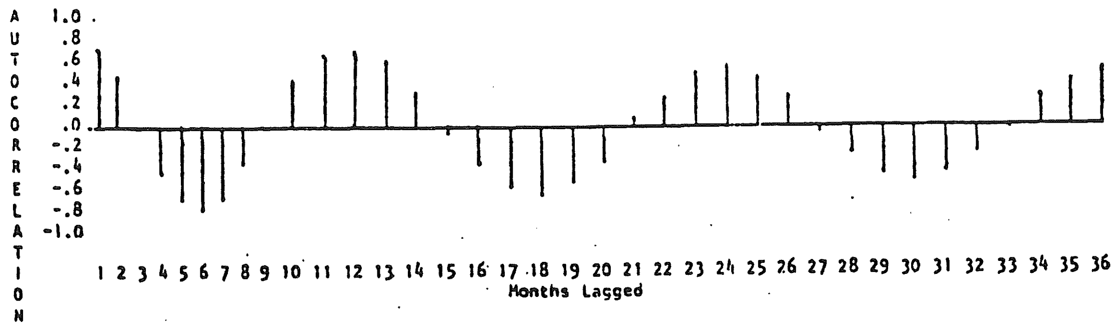
The dominant effect suggested by the autocorrelations in Figure 2A is a twelve-month seasonal effect, since there are strong and significant positive correlations peaking at lags of 12, 24 and 36 months, while there are strong negative correlations centering on lags of 6, 18 and 30 months. This is not surprising for Minnesota, since motorcycling, and thus fatalities, are virtually non-existent during the cold winter months. After experimenting with models suggested by the autocorrelation and partial autocorrelation patterns of both the actual and residual fatality figures, the final parameters of the seasonal model were determined to be ARIMA (0,0,0)(0,1,1)12 (see McCleary, et al. 1980:89-103).[7] The parameter estimates and diagnostic statistics for this ARIMA model are also presented in Figure 2.

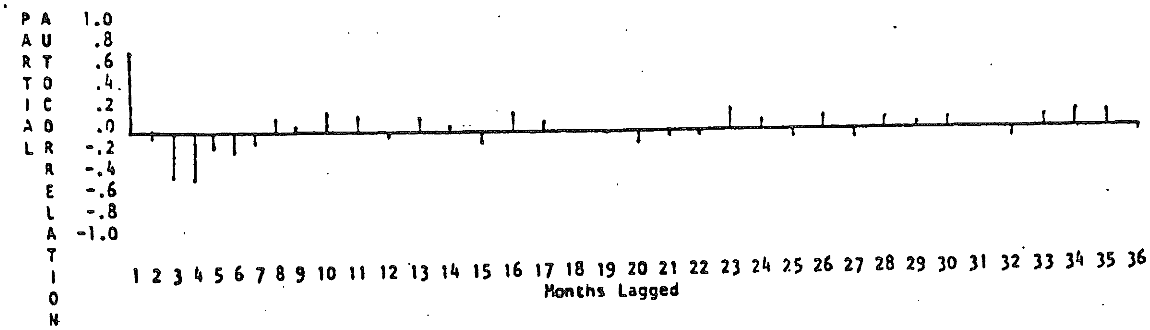
This model apparently fits the actual data very well; the average monthly difference between the actual and fitted values is only 2.13 fatalities and 81 percent of the overall variance

FIGURE 2

UNIVARIATE SEASONAL MODEL

A. AUTOCORRELATIONS OF LAGGED MONTHLY MOTORCYCLE FATALITIES: 1971-1976





B. PARAMETER ESTIMATES: ARIMA (0,0,0)(0,1,1)12

Parameter[a]	Estimate	t-Statistic
12 Month Seasonal Moving Average	.693	6.47

Proportion of Explained Variance (R^2) = .81
Root Mean Squared Error = 2.13

C. DIAGNOSTIC STATISTICS FOR RESIDUALS

	Q Statistic[b]	Degrees of Freedom	Probability
Autocorrelations (25 lags):	7.44	1,24	>.99
Partial Autocorrelations:	4.82	1,24	>.99

Mean	=	0.39
Median	=	0
Mode (N=25)	=	0
St. Dev.	=	2.09
Range	=	-4 to +6

---

[a] The parameter estimate and diagnostic statistics shown here were computed after eliminating the trend constant term which was shown to be insignificant in the initial estimation (i.e., trend constant coefficient = .087, t=1.34).

[b] The Q statistic is computed for the autocorrelations and partial autocorrelations for only the first 25 lags of the residuals as recommended by McCleary, et al (1980:99).

is accounted for by the model. Furthermore, the Q-statistics for the autocorrelations and partial autocorrelations of the residuals (i.e., the unexplained fatalities) indicate that no discernible pattern remains in the data after fitting this model (i.e., the residuals have been reduced to "white noise"). This indicates that no important systematic component has been left out of the model.[8]

It is important to note that univariate time series models per se are atheoretical and uninterpretable in any causal sense. Specifically, the ARIMA model which was identified, estimated and diagnosed above for this time series merely models the seasonal patterns in the motorcycle fatality data; it does not "explain" those patterns. However, this is no different from the linear trend models used earlier in this paper; the purpose of all three models is merely to model the pre-revision fatality trend, to project it to the 1977-1980 period, and to determine whether or not the post-revision fatality figures deviate from this trend.

Figure 3 graphically illustrates the highly seasonal pattern in actual motorcycle fatalities for the 1971-1976 period. Significantly, the fitted model predicts no increase in fatalities for the 1977-1980 period, not even the gradual upward trends predicted by the linear annual models A and B earlier. This seasonal non-linear model predicts only 215 motorcycle fatalities for the 1977-1980 period, which is 198, or 49.5 per year, fewer than actually occurred (Table 2).

Thus, neither the linear trend models fitted to annual data nor the seasonal trend model fitted to monthly data would predict the sharp increase in motorcycle fatalities beginning in 1977. Between 33 and 49.5 fatalities per year, or between 32 and 48 percent of the total, are unaccounted for by the pre-revision trends as modelled here.

The fact that the predictions from the three models differ somewhat is not disconcerting. Each model is fitted to data in such a manner that average annual/monthly error is reduced to a minimum. However, each model is really fit to different data (seven years of annual data, six years of annual data and six years of monthly data) and, thus, some prediction differences are to be expected. However, the models concur on one very important finding: none of them predicts the sharp increase in motorcycle fatalities which occured following the helmet law revision.

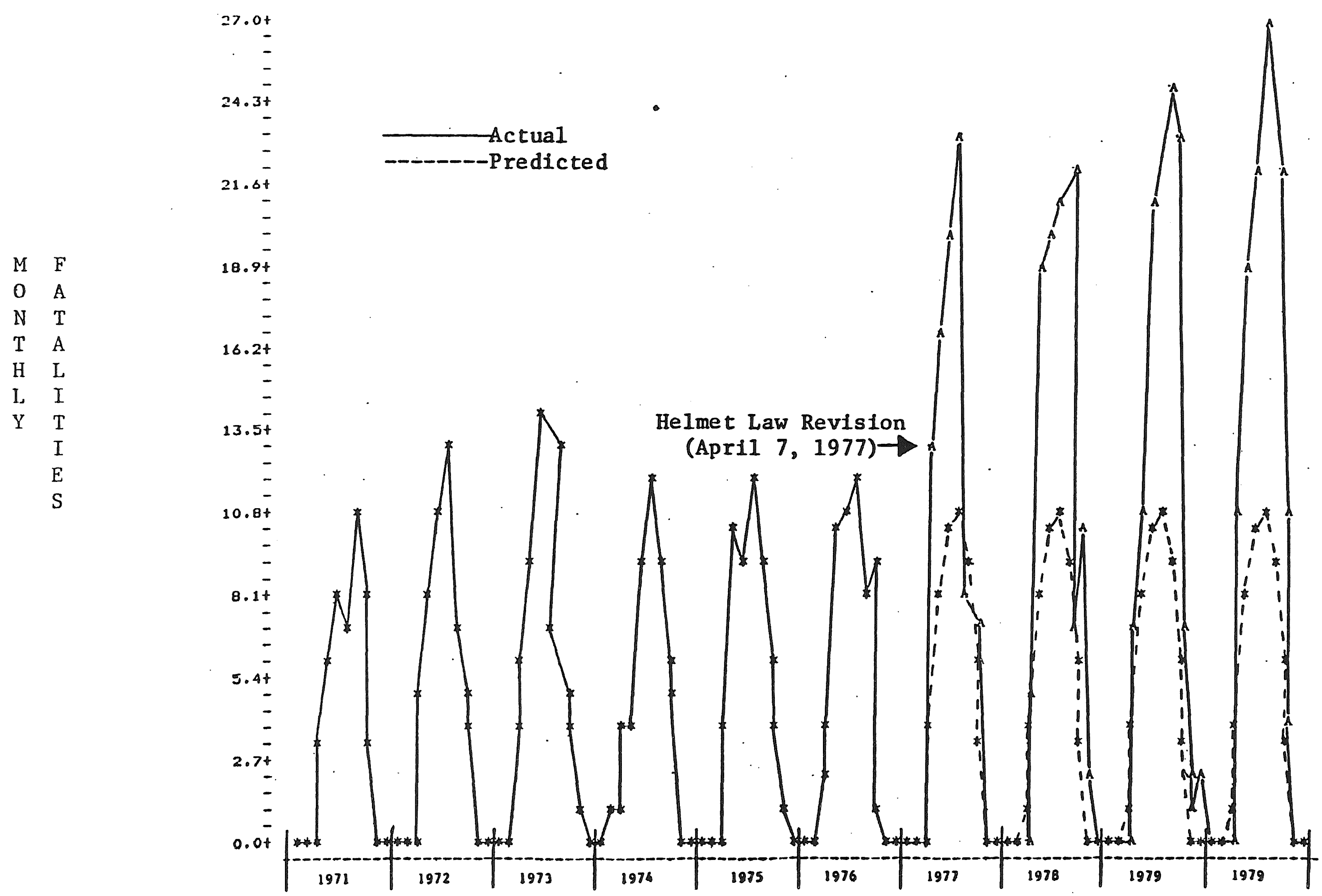
Each of these models suggests that the substantial increase in motorcycle fatalities, beginning in 1977, may be due to the helmet law revision. However, these univariate models cannot determine whether other factors, such as unfavorable weather, motorcycle use, and so on may have also contributed to the fatality increase. A multivariate analysis is required to answer that question.

MULTIVARIATE REGRESSION ANALYSIS

The following multivariate analysis has two purposes. First, it attempts to determine whether the sharp increase in motorcycle fatalities following the helmet law revision may be due to factors other than that revision. Second, it attempts to

FIGURE 3

THE DIFFERENCE BETWEEN ACTUAL AND PREDICTED MOTORCYCLE FATALITIES BASED ON A UNIVARIATE SEASONAL MODEL



statistically control for such other factors while measuring the effects on the fatality level of the revision itself.

However, prior to pursuing these two goals, it is important to note that any such analysis is guided by some implicit or explicit theory about the causal relationships among the variables it uses. It is important to make such assumptions explicit, and this is done below.

The Hypothesized Causal Framework

Figure 4 presents a hypothesized causal model or framework of motorcycle fatalities. According to this framework, increased motorcycle usage will result in an increase in reported accidents; i.e., the former variable is said to have a direct and positive effect on the latter variable. Inclement weather will reduce motorcycle usage but will simultaneously increase the likelihood of an accident for those who nevertheless choose to drive; i.e., inclement weather is viewed as having a direct negative effect on usage, as well as an interactive effect in enhancing the positive relationship between usage and reported accidents. Motorcycle size/power and operator skill/training are regarded as having a direct positive effect and a direct negative effect, repectively, on reported accidents.

The helmet law revision is known to have had a direct negative effect on helmet usage, while helmet usage in turn is hypothesized to reduce accident severity (i.e., an interaction effect). Thus, the motorcycle helmet law revision is hypothesized to increase fatalities primarily through increasing the severity of motorcycle accidents. This, of course, is the key hypothesis of this paper.

However, helmet usage may also have a weak direct and negative effect on the number of reported accidents. That is, it is conceivable that some motorcycle accidents which would have been quite minor given the protection of a helmet and, thus, would have gone unreported are in the absence of a helmet serious enough to be reported. (This is an important consideration for allocating the increased fatalities between the statutory change and other factors, as will be seen somewhat later.)

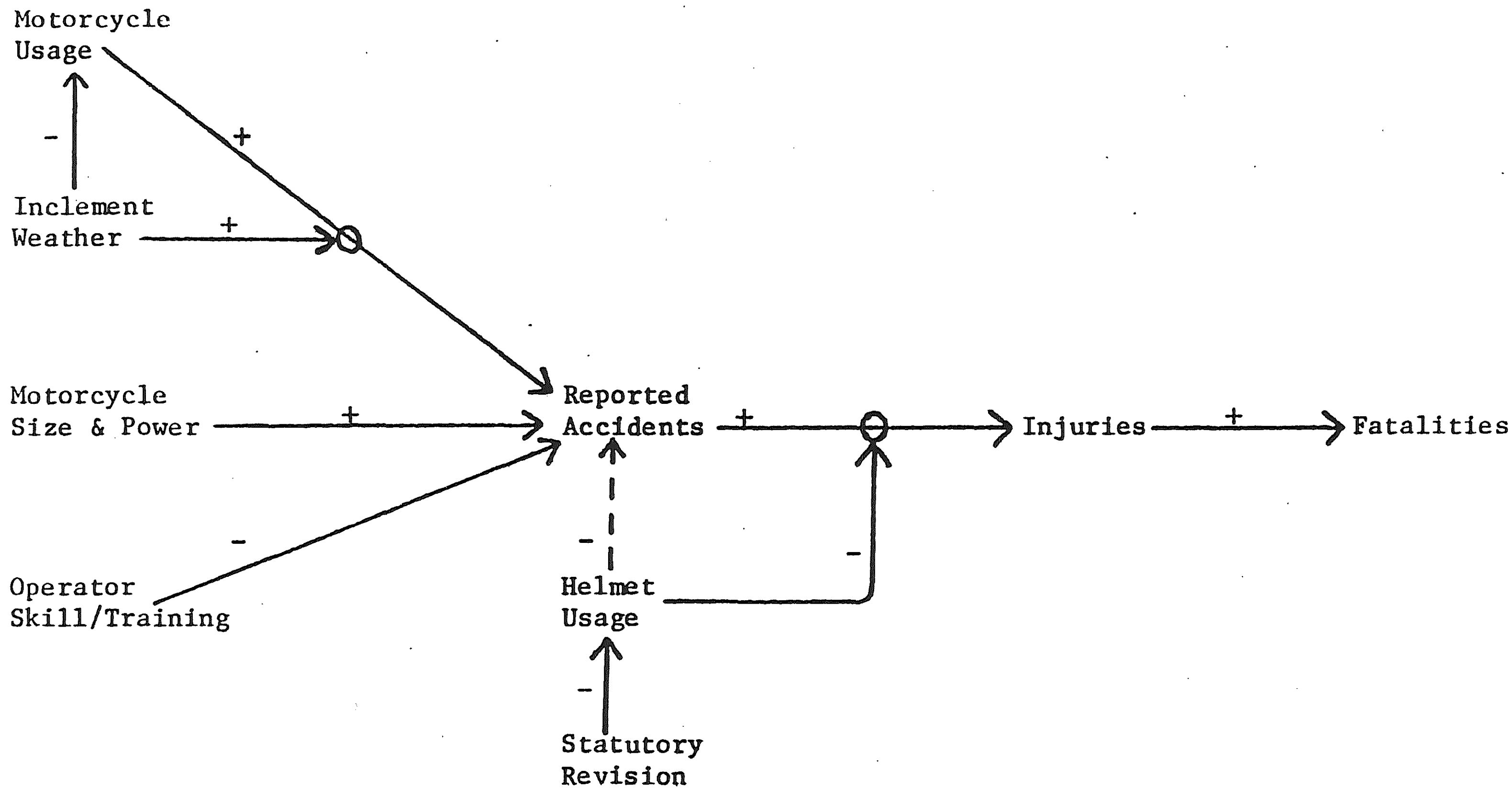
There are undoubtedly other factors which causally affect motorcycle fatality levels, such as the permissible speed limits, consideration shown by other drivers, and so on. However, for two reasons it might not be necessary to explicitly include such other factors. First, they are thought to be less important than the factors which are included. And second, their effects will be indirectly assessed in the final model to be presented which statistically controls for motorcycle accidents as a proxy for all other such factors.

Operationalizing the Variables in the Model

In order to empirically test the hypothesized model of motorcycle fatalities, it is necessary to operationalize its variables; i.e., to select empirical indicators or measures for the variables. For an indicator to be useful, it must reasonably correspond to the conceptual definition of a variable and it must have been reliably measured for each year since

FIGURE 4

A SUGGESTED CAUSAL MODEL OF MOTORCYCLE FATALITIES



<u>Symbols</u>

——→	a direct causal effect
- - - - →	a weak direct causal effect
——→O	an interaction effect
+ -	the positive or negative direction of an effect

1971. As is frequently the case in a retrospective study such as this, the number of empirical indicators and the quality of available data are quite limited. Nevertheless, they appear adequate for the intended analysis.

Three indicators of motorcycle usage were available from Department of Public Safety records. They are:

(1) the total number of registered motorcycles in the state;
(2) the total number of licensed motorcyclists in the state; and
(3) the number of successfully completed motorcycle road tests, which is a requirement for obtaining a motorcycle license (i.e., the number of "new" and less-experienced drivers each year).

However, due to a change in data collection procedures in the Public Safety Department, the number of licensed motorcycle operators was seriously underreported prior to 1975. Thus, that indicator cannot be used in the following regression analyses, though it will be used in a more qualitative manner.[9]

Two weather indicators available from the U.S. Weather Service are (1) average temperature and (2) average monthly precipitation, both measured only over the seven-month (April-October) motorcycle season. In Minnesota, one would expect motorcycle usage to be strongly correlated with temperature since near-freezing temperatures are extremely uncomfortable for motorcyclists. Similarly, precipitation (i.e., rainy weather) could be expected to suppress motorcycle usage. However, one would expect that whatever riding was done in cold, and especially in wet weather, would be more dangerous. Thus, inclement weather is likely to suppress usage while making more hazardous the remaining usage.[10]

The numbers of motorcycle accidents and motorcycle fatalities were obtained directly from records kept by the Department of Public Safety.[11] The helmet law revision was coded as an all-or-none "dummy" variable which, for simplicity, was assumed to have been effective prior to the entire 1977 motorcycle season.

Unfortunately, reliable indicators of motorcycle engine displacement (i.e., size/power) and of operator skill/training were unavailable for this analysis. However, this is less problematic than it might appear on the surface since they are hypothesized to affect fatalities indirectly through impacting the number of accidents, rather than more directly through influencing accident severity; thus, one can assess their effects by statistically controlling for the number of accidents as a catch-all proxy measure. It is conceivable, however, that motorcycle size and operator skill could also influence accident severity directly and apart from their effects on the accident frequency itself. To the extent that this might occur, the statistiscal method suggested above would underestimate their effects.

Bivariate Relationships Among the Variables

For the purpose of testing a multivariate model such as the one proposed in this paper, one would ideally have available a complete set of reliable measurements on all the variables in the model. Then, using an appropriate statistical procedure, one would simultaneously estimate the parameters of the model and draw the appropriate conclusion about the effect of the statutory revision. However, as noted in the previous section, reliable measurements are not available for every variable in this model. Furthermore, the number of time periods (10 years) is relatively short for such multivariate analysis. Thus, one must closely inspect and interpret the bivariate relationships among the variables and, if possible, eliminate some which have little apparent explanatory power.

Table 3 presents the correlations among the various indicators of the variables in the hypothesized causal model. These correlations have been computed using annual data from only the pre-revision period (1971-1976) and, thus, they have not been influenced by the statutory change itself. Figure 5, on the other hand, plots each of these explanatory variables over the full ten-year period (1971-1980).

If a variable actually behaves as hypothesized, then there should be a substantial correlation in Table 3 wherever a strong direct effect has been hypothesized in the causal model of Figure 4, and the sign (+ or -) of the correlation should be as hypothesized. In addition, one would generally, but not necessarily, expect that the factors which account for the sharp increase in fatalities starting in 1977 would also show a marked change in the hypothesized direction at or recently prior to that year (Figure 5).

Since the immediate goal is to pare down the number of hypothesized causal variables in the model, one should begin by inspecting Table 3 and Figure 5 for empirical patterns which do not conform to these expectations. Perhaps the easiest variable to eliminate is precipitation during the cycling season. Though it is negatively correlated as expected and to a fairly substantial degree with the three usage variables (-.70, -.53, and -.78), as well as with motorcycle accidents (-.54), it is virtually unrelated to motorcycle fatalities (-.03) over the 1971-1976 period (Table 3). Figure 5C suggests that these observed relationships may, in fact, be spurious due to the drought in 1976. Furthermore, Figure 5C suggests that the rising and falling precipitation levels during the 1977-1980 period are unlikely to explain the concommitant motorcycle fatality increases. Thus, it appears that little would be lost by eliminating the precipitation variable from the following multivariate analysis. Unfortunately, the limited number of data periods makes it impossible to fully assess the hypothesized interactive effect of this variable; i.e., in making more dangerous whatever cycling it does not suppress. Furthermore, it might be necessary to use a shorter measurement period (e.g., monthly or weekly) to accurately assess the role of precipitation on fatalities.

A second indicator which can probably be eliminated from the analysis with no loss of explanatory power is the number of successful road-tests given each year (i.e., the number of new, less-experienced drivers). This variable has remained virtually constant over the 1971-1980 period (Figure 5A) and, thus, could not possibly explain the increase in fatalities beginning in 1977.

A third indicator which should be eliminated is the total number of licensed motorcycle operators in the state since, as noted earlier, the Public Safety Department's figures seriously

TABLE 3

CORRELATIONS AMONG VARIABLES IN THE CAUSAL MODEL
OF MOTORCYCLE FATALITIES (1971-1976)

	Registered Motorcycles	Successful Road-Tests	Licensed Operators	Average Temperature	Total Precipitation	Motorcycle Accidents	Motorcycle Fatalities
Registered Motorcycles	1.0						
Successful Road-Tests	.09	1.0					
Licensed Operators	.93	.22	1.0				
Average Temperature	.07	.41	.27	1.0			
Total Precipitation	-.70	-.53	-.78	-.40	1.0		
Motorcycle Accidents	.93	-.17	.79	.12	-.54	1.0	
Motorcycle Fatalities	.47	-.56	.45	.32	-.03	.69	1.0

underreport this number prior to 1975. Prior to eliminating this indicator, however, it is interesting (though risky) to note that it correlates (see Table 3) in the expected direction with the other usage variables (.93 and .22) and with accidents and fatalities (.79 and .45). Nevertheless, Figure 5A shows no great discontinuity in the number of motorcycle operators between 1976 and 1977 which would help explain the sudden increase in motorcycle fatalities at that time.

One other variable in the hypothesized model (Figure 4) can be rejected as an insufficient explanatory variable even though no data is available to empirically test it. That is motorcycle size/power. John Wetzch, President of the National Motorcycle Dealers Association, indicated in a phone conversation that there has been no marked change in motorcycle size/power which could explain the sharp increase in motorcycle fatalities that occured after 1976. Instead, typical engine size and power have increased gradually since the 1960's.

After eliminating the above variables, only four of the hypothesized explanatory variables remain for the multivariate analysis. These are: (1) the number of registered motorcyclists as an indicator of usage; (2) average temperature during the April-October motorcycle season; (3) the helmet law revision in 1977; and (4) the number of motorcycle accidents. These variables will be used in the following analysis.

Fatalities As a Function of Usage and Weather

Given the brevity of the pre-revision period (6 years), it is difficult to be statistically rigorous in any multivariate analysis of fatalities over that time period. Nevertheless, it is worth proceeding with such analysis, providing that the results are interpreted in only a very general way. In particular, the probability levels of any parameter estimates should only be viewed as suggestive of statistical significance, while the parameter estimates themselves should only be regarded as suggestive of the relative contribution of a variable, not as an exact estimate of its causal effect.

The first multivariate model involves regressing motorcycle fatalities over the 1971-1976 period on the number of registered motorcycles and average above-freezing temperature during the cycling season. Table 4 presents the parameters and predictions for this model. The regression coefficients in Table 4 indicate that for the 1971-1976 period each increase of approximately 100,000 motorcycles resulted in roughly 13 additional fatalities, while each 1-degree increase in average temperature resulted in an additional 1.57 fatalities. Given the large number of motorcycles and the relatively small annual fluctuations in temperature, usage is much more important than temperature for predicting fatalities (even though it shows less statistical significance as reflected by the F-statistic).

Like the linear trend models A and B presented earlier, this regression model fits the fatality data to only a moderate degree. It accounts for a fairly substantial portion of variation in the 1971-1976 fatality data (R2=.30), though the average difference between the actual and fitted annual fatality figures is 5.78.

This model nevertheless enables one to determine in a gross

TABLE 4

A SIX-YEAR MULTIVARIATE REGRESSION MODEL: 1971-1976

Explanatory Variable	Regression Coefficient[a]	Standard Error	F Statistic(df=2,4) (Probability)
Registered Motorcycles	.00013	.00012	1.15 (p=.34)
Average AF Temperature[b]	1.57	.58	7.46 (p=.05)

Root Mean Squared Error = 5.78

Proportion of Variance Explained (R^2)[c] = .30

Durbin-Watson Statistic = 2.90

Year	Actual Motorcycle Fatalities	Predicted Motorcycle Fatalities	Difference
1977	94	63	+ 31
1978	106	61	+ 45
1979	97[d]	58	+ 39
1980	116[e]	63	+ 53
Total	413	245	+168
Average	(103.3)	(61.3)	(+42.0)

[a] This regression equation was fitted with no intercept coefficient since a prior regression revealed it to be statistically insignificant (i.e., intercept = -8.86, se=81.5, F=.01).

[b] This is the average above-freezing Fahrenheit temperature, where: AF temperature = (Fahrenheit temperature - 32).

[c] R^2 is adjusted for the mean of the dependent variable.

[d] This figure is one larger than that shown in Table 6 of the Department of Public Safety (1979) report since this is based on completed reporting for that year.

[e] This figure is based on all motorcycle fatality reports available as of October 31, 1980; it is not likely that many additional fatalities will occur this year.

way the degree to which the post-revision jump in motorcycle fatalities has been due to changes in these variables. The logic is identical to that underlying the use of the three univariate models earlier; that is, the model can be used to predict the 1977-1980 fatalities, thus permitting the determination of the discrepancies between the predicted and actual fatalities for that period. The only difference is that the predicted fatality number for each post-revision year is computed by applying the parameters of the fitted model to the actual values for the predictor variables for the 1977-1980 period.

This procedure results in a prediction of 245 motorcycle fatalities for the post-revision period, which is 168 or an average of 42 per year fewer than actually occurred (Table 4). Clearly, these indicators of usage and weather are unable to explain the sharp increase in motorcycle fatalities beginning in 1977. This is not surprising in light of the absence of any corresponding sudden changes in the trends of those variables, as can be seen from Figures 5A and 5B.

However, before concluding that the unexplained fatalities in the four models discussed so far are due to the helmet law revision, two other statistical models should be developed.

A Model Including the Helmet Law Revision

The first of these is a model which uses the full 10 years of data and explicitly includes the helmet law revision as a variable whose independent effect can be estimated. The parameter estimates for this model are presented in Table 5. A comparison of the regression coefficients of Tables 4 and 5 reveals little change in the estimated effects of the usage and weather variables, even though the policy variable has been added to the model and the time period has been extended. Such stability enhances one's confidence in those estimates.

The most significant finding in Table 5, however, is the very large and statistically significant regression coefficient associated with the helmet law revision. Since this is an all-or-none (dummy) variable, it can be directly interpreted; that is, an estimated 40 fatalities per year appear to have resulted from the revision of the motorcycle helmet law.

This model fits the fatality data reasonably well, having explained fully 94 percent of the variance in fatalities over the ten-year period. Nevertheless, the validity of any conclusions that one might draw from it depend upon the validity of the causal assumptions underlying it. For example, one might still question whether there is some other factor not included in this regression model which could account for the fatality increase since 1977. This suggests one final regression model which can be tried.

A Model Including Accidents As a Proxy Variable

A variable which intervenes between motorcycle usage, weather and other determinants of motorcycle fatalities is, of course, motorcycle accidents. Thus far, however, we have ignored this intervening variable in fitting the regression models. Alternatively, one could reason that whatever the determinants of motorcycle fatalities happen to be, a fatality

TABLE 5

A TEN-YEAR MULTIVARIATE REGRESSION MODEL: 1971-1980

Explanatory Variable	Regression Coefficient[a]	Standard Error	F Statistic(df=3,7) (Probability)
Helmet Law Revision[b]	40.29	6.71	36.44
Registered Motorcycles	.00019	.00014	1.69 (p=.24)
Average AF Temperature[c]	1.30	.69	3.58 (p=.10)

Root Mean Squared Error = 7.37

Proportion Variance Explained (R^2)[d] = .94

Durbin-Watson Statistic = 2.33

[a] This regression equation was fitted with no intercept coefficient since a prior regression revealed it to be statistically insignificant (i.e., intercept = -1.13, se=63.15, F=.03).

[b] This statutory change was coded as an all-or-none dummy variable.

[c] This is the average above-freezing Fahrenheit temperature, where: AF temperature = (Fahrenheit temperature - 32).

[d] R^2 is adjusted for the mean of the dependent variable.

cannot occur in the absence of an accident. Thus, one could simultaneously regress fatalities on both the number of motorcycle accidents and the dummy variable representing the helmet law revision in order to estimate the effect of this statutory change compared to the combined effects of all other determinants of fatalities (as represented by proxy variable, accidents). This is represented by the model in Table 6.

The regression model in Table 6 fits the fatality data very well; both of the regression coefficients are highly statistically significant, 94 percent of the variance in fatalities is explained by the model (R2=.94), and the average difference between the fitted and actual annual fatality figures is only 6.73.

The regression coefficients for this model indicate that 32 fatalities per year have resulted from the helmet law revision in 1977. However, this should be viewed as the minimum number of fatalities attributable to this factor, since there is some reason to believe that the rather sudden increase of over 10 percent in the number of reported accidents in the year following the statutory change (i.e., in 1977; see Figure 6) may have itself been partly or wholly due to that change. As noted earlier in the discussion of the hypothesized causal model, it is conceivable that some motorcycle accidents which would have been quite minor given the protection of a helmet and, thus would have gone unreported, are in the absence of a helmet serious enough to get reported.

This hypothesis can be tested using partial correlation analysis. Figure 7A illustrates the hypothesized causal connections, while Figure 7B presents the zero-order (uncontrolled) and first-order partial (controlled) correlations. The zero-order correlations indicate strong associations among the helmet law revision, reported accidents and fatalities. However, the correlation between reported accidents and fatalities drops considerably (from .86 to .56, or from .74 to .31 in explained variance) when statistically controlling for the statutory change. Importantly, there is no great decrease in the correlation between the statutory change and fatalities when controlling for the number of accidents (i.e., from .96 to .88, or from .92 to .77 in explained variance). This pattern of relationships supports the interpretation that the statutory revision has directly increased the number of reported motorcycle accidents, in addition to making motorcycle accidents in general more severe.

Additional support for this interpretation comes from an inspection of the partial regression coefficients. Blalock (1964:85) notes that if the type of causal sequence proposed in Figure 7A is correct, then when statistically controlling for first variable the strength of the relationship (i.e., the correlation) between the second and third variables will be reduced even though the form of that relationship (i.e., the regression slope coefficient) will not change substantially. Figure 7B reveals this predicted pattern; the correlation between motorcycle accidents and fatalities dropped from .86 to .56 when controlling for the statutory revision, while the corresponding regression coefficients remained stable (b(32)=b(32.1)=.03).[12]

TABLE 6

A TEN-YEAR MULTIVARIATE REGRESSION MODEL USING ACCIDENTS AS A PROXY MEASURE: 1971-1980

Explanatory Variable	Regression Coefficient[a]	Standard Error	F Statistic(df=2,8) (Probability)
Helmet Law Revision[b]	32.23	4.84	44.38 (p<.00I)
Motorcycle Accidents	.025	.00122	417.85 (p<.001)

Root Mean Squared Error = 6.73

Proportion Variance Explained (R^2)[c] = .94

Durbin-Watson Statistic = 2.28

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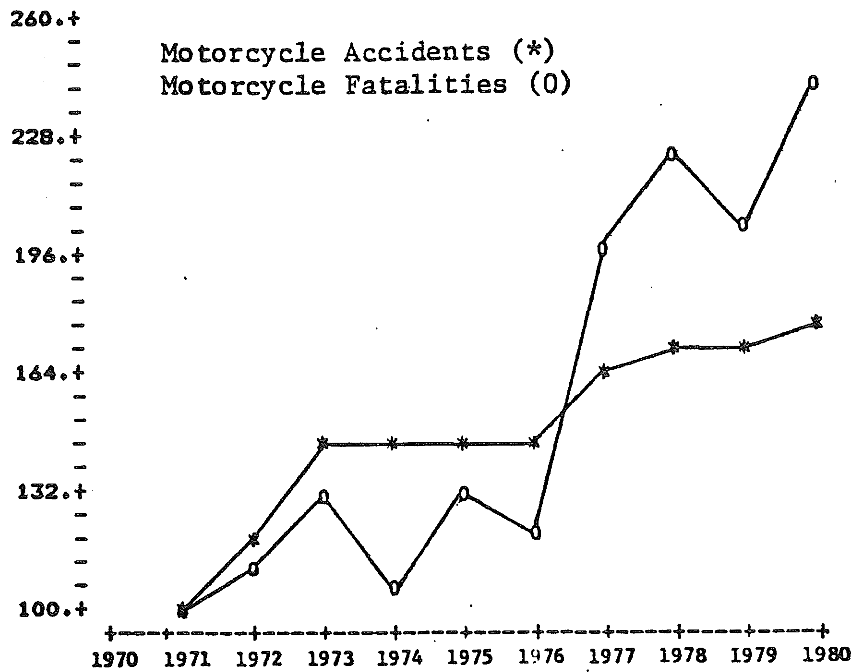
[a] This regression equation was fitted with no intercept coefficient since a prior regression revealed it to be statistically insignificant (i.e., intercept = 17.27, se=21.67, F=.64).

[b] This statutory change was coded as an all-or-none dummy variable.

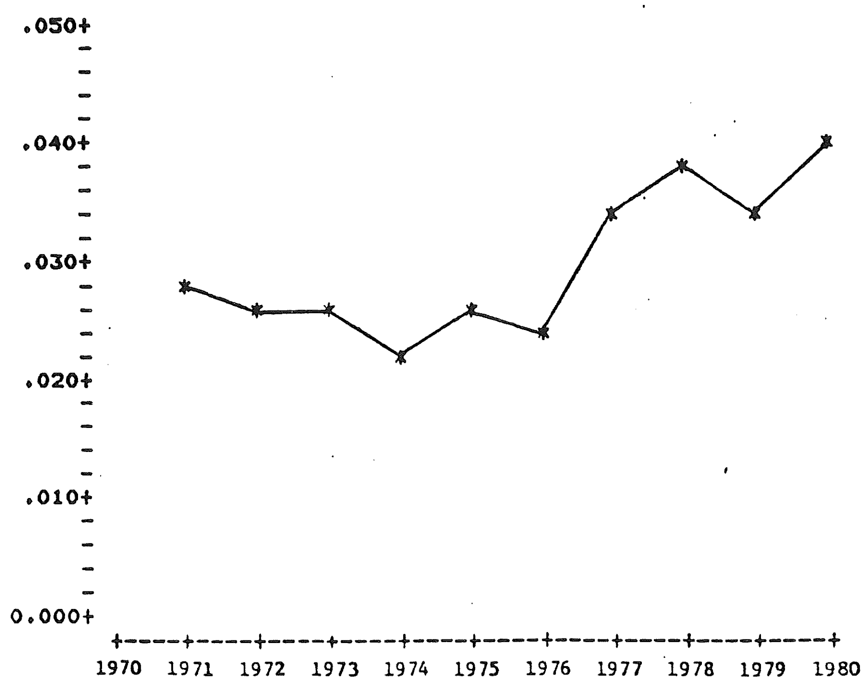
[c] R^2 is adjusted for the mean of the dependent variable.

FIGURE 6

MOTORCYCLE ACCIDENTS, FATALITIES AND ACCIDENT SEVERITY



A. Annual Motorcycle Fatalities and Motorcycle Accidents as Percentages of their 1971 Levels.

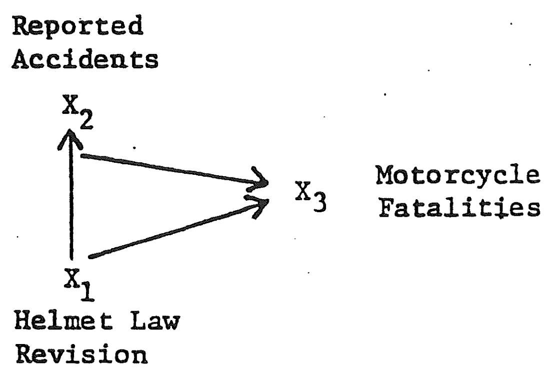


B. Motorcycle Accident Severity Index By Year: Ratio of Fatalities to Accidents.

FIGURE 7

THE HELMET LAW REVISION, MOTORCYCLE ACCIDENTS AND FATALITIES

A. Hypothesized Causal Schema



B. Correlations and Partial-Correlations: 1971-1980

Zero-Order Correlation	Partial Correlation
$r_{12} = .80$	
$r_{23} = .86$	$r_{23.1} = .56$
$r_{13} = .96$	$r_{13.2} = .88$

Squared Zero-Order Correlation	Squared Partial Correlation
$r_{12}^2 = .64$	
$r_{23}^2 = .74$	$r_{23.1}^2 = .31$
$r_{13}^2 = .92$	$r_{13.2}^2 = .77$

[a] The subscripts of the correlations refer to the variables in the causal schema above.

The apparent effect of the helmet law repeal in increasing accident severity and, therefore, in increasing the number of "reported" accidents is also supported by the testimony of reviewing physicians for 233 motorcycle accident victims studied in 1977. The Department of Public Safety (1979:11-12) reported:

> Among those patients wearing helmets the reviewing physician stated that the helmet probably reduced the severity of injury in 59% of the cases, including possibly saving the patient's life in 36% of the cases. Among those patients not wearing helmets, the reviewing physician said that injuries probably would have been avoided or reduced in 75% of all cases.

Thus, it is concluded that the helmet law revision in 1977 has resulted in a minimum of 32 (Table 6) and a maximum of 40 (Table 5) additional motorcycle fatalities per year, depending on the extent to which the corresponding increase in reported motorcycle accidents has itself resulted from the helmet law revision.

SUMMARY AND CONCLUSION

This paper has attempted to determine the impact of the partial repeal of the mandatory motorcycle helmet law on motorcycle fatalities in Minnesota. Two linear trend models and a seasonal trend model were fitted to the pre-revision fatality data, but they were unable to account for the sharp increase in fatalities following that statutory change.

Next, a hypothesized causal schema was used to develop a regression model for determining the effects of motorcycle usage and weather on fatalities. Fatality predictions based on this model were also considerably below the actual level of fatalities which occurred following the helmet law revision. Only when the statutory change was explicitly entered into that regression model as a separate variable, could the model account for the sharp increase in fatalities; an additional 40 fatalities per year were estimated to have resulted from the law revision.

However, that model might have actually overestimated the effect of the statutory change since it did not statistically control for the sudden increase in reported motorcycle accidents corresponding to the helmet law revision. Thus, a final regression model was developed to incorporate the number of accidents as a proxy measure for all other determinants of fatalities; this model estimated that the statutory revision in 1977 has resulted in approximately 32 additional motorcycle fatalities per year.

However, the sudden increase in reported motorcycle accidents may itself have been partially or wholly due to the helmet law revision; that is, the statutory change might have increased the severity of accidents and, thus, may have increased accident reporting. This interpretation was supported by the findings of partial correlation and partial regression analysis. To the extent that the statutory revision has increased accident severity and reporting, then the number of additional annual fatalities resulting from the helmet law change would be closer to the higher estimate of 40 than to the lower estimate of 32.

This study has focused on motorcycle fatalities. No attempt was made to analyze other possible effects of the helmet law revision, such as increased accident severity per se, the costs associated with motorcycle injuries and fatalities, or the distribution of such costs among the motorcycling and non-motorcycling public. Nor has this paper engaged in any debate concerning the appropriateness of limiting individual freedom in order to protect broader societal interests. Such additional analysis and debate is necessary for making any policy recommendations regarding the mandating of helmet usage; thus, this paper makes no policy recommendations. Nevertheless, this research has documented one very important effect of such legislation; i.e., that the partial repeal of the mandatory helmet law has resulted in a significant and demonstrable increase in motorcycle fatalities in Minnesota.

FOOTNOTES

1 The motorcycle fatality figures analyzed in this report include motorcycle operators and passengers, but they exclude pedestrians, bicyclists and drivers of other motor vehicles killed in motorcycle accidents. Non-motorcycle traffic fatalities include all motor vehicle fatalities less the number of motorcycle fatalities as defined above. These data were obtained from the Fatal File data base of the Office of Traffic Safety, Minnesota Department of Public Safety.

Note that this analysis uses a figure of 116 for the number of motorcycle fatalities in 1980 since that was the official count at the time of the analysis. However, the Department of Public Safety subsequently received late reports on 5 fatalities and revised the 1980 figure to 121. Prior to publishing this paper, we reanalyzed the data and determined that the conclusions are still valid; hence, this revised count was not used.

2 We were unable to obtain reliable data on motorcycle fatalities and related factors for any year prior to 1971. Thus, all of the models developed in this paper use data from 1971 and later.

3 The difference in average annual traffic fatalities (Table 1) was computed using ordinary least-squares regression with a dummy coding scheme reflecting the timing of the helmet law revision. Computing the difference in this manner and testing the statistical significance of the difference is equivalent to performing a T-test of the difference between means.

However, the presence of serial correlation in a time series could invalidate a test of the significance of the pre-post level difference, unless the statistical model takes such autocorrelation into account. The fatality level difference does not consider the possibility of serial correlation. Hence, it is advisable (Hibbs, 1974, 257; Malbudakis and Wheelwright, 1978:206,224) to compute the Durbin-Watson statistic to determine whether there is any autocorrelation in the residuals (i.e., actual minus fitted) fatality figures. For the residuals of this OLS model D-W=2.23, which for 10 observations and 1 predictor variable appears to be nonsignificant (Kmenta, 1971:294-297). Thus, this test of the pre-post difference in fatality levels is regarded as valid.

The Durbin-Watson statistic was also computed for the residuals of each other linear model used in this paper (see their respective Tables). For this short time series there are too few degrees of freedom to rigorously apply this test; nevertheless, the moderate size of each D-W statistic suggests that there is no significant autocorrelation pattern remaining in the residuals after fitting any of these models.

4 Trend analysis as used in this paper may be termed "univariate analysis" since no explanatory variables, other than time itself, are involved. The subsequent statistical models which incorporate various possible explanatory variables are

termed "multivariate analysis."

5 In this analysis, the time variable is coded as 0, 1, 2, etc. This differs from the coding which DPS used (i.e., 70, 71, 72). This difference affects only the estimated regression intercept coefficient; the slope coefficient, the MSE, and the R2 are unaffected.

6 The seasonal trend model is fitted to the 75 monthly fatality figures reported by DPS for January, 1971 through March, 1977. For this time series, the actual fatality figure for May, 1975 (i.e., 18) is regarded as an outlier. McCleary (1980:131,200) cautions that the presence of an outlier in a short time series underestimates the low lag autocorrelations and the proportion of explained variance and overestimates the residual mean square. Hence, the fatality figure for May, 1975 was recoded to 9, the average for all the months of May, and the analysis was performed on this modified time series.

7 Actually, identification of the model was fairly straightforward. Level nonstationarity in the time series is indicated by the pattern of linear decay in the seasonal lags as well as by their failure to die out (decrease to zero) rapidly. Hence, the raw time series was seasonally differenced and the correllograms were recomputed. The autocorrelations for the seasonally differenced data revealed a single significant spike at the 12-month lag, while the partial autocorrelations showed a clear exponential decay at the seasonal lags; thus, suggesting a first-order seasonal moving average process. After fitting the seasonal moving average model to the seasonally differenced data, no statistically significant autocorrelations were found in the residuals. Further exploratory attempts were made to regularly difference the time series and/or to add other seasonal or regular terms to the model, but such elaborations were found unnecessary.

The trend parameter estimate was also rejected as statistically insignificant (trend = .087, t= 1.34) and the model was re-estimated. Re-fitting the model after eliminating the constant caused the other parameter estimate and statistics to change somewhat (e.g., from .855 to .693 for the SMA parameter). Note that McCleary, et, al (1980) do not re-estimate the model in this way when the trend is insignificant; instead, they merely drop the trend term without offering any explanation. It seems more appropriate to follow the custom of re-estimating the remaining parameters of a model whenever an insignificant parameter is deleted.

8 Not only is the entire pattern of residual autocorrelations reduced to white noise as indicated by the Q-statistics, but no single one of the autocorrelations or partial autocorrelations are individually significant either.

9 The number of registered motorcycles for 1971-1979 was obtained from reports of the Department of Transportation. The number of registered motorcycles in 1980 was estimated by applying the 1971-1979 average annual percentage change in the

number of registered motorcycles to the 1979 figure.

The number of successfully completed road-tests for 1975-1979 was obtained from reports of the Department of Transportation. The number of successful road-tests for 1971-1974 was estimated by DPS as being 90 percent of the total number of road-tests taken in each of those years. The number of road-tests in 1980 was estimated by applying the 1971-1979 average annual percentage change in the number of successful road-tests to the 1979 figure.

	Registered Motorcycles	Licensed Operators	Total Road-Tests	Successful Road-Tests
1971	90,150	20,000*	23,502	21,151*
1972	103,286	23,795	17,809	16,028*
1973	119,227	55,377	19,385	17,446*
1974	138,193	91,024	22,361	20,125*
1975	136,256	127,081	20,137	17,846
1976	143,237	152,138	22,658	20,243
1977	151,763	172,223	23,303	20,612
1978	151,016	184,545	18,399	16,072
1979	156,552	201,075	23,105	21,390
1980	165,523*	223,743*	N.A.	21,151*

*estimated

10 For this analysis, average temperature is measured in terms of Fahrenheit degrees above-freezing (e.g., 33 degrees = 1). Thus, by excluding the intercept coefficient from a regression equation, one assumes that there will be virtually no motorcycling, and, hence, no fatalities in sub-freezing weather.

Temperature and precipitation data are seven-month (April-October) averages for the east central region of the State of Minnesota. Data are from the U.S. Department of Commerce, National Oceanographic and Atmospheric Administration Monthly and Annual Reports, January 1979-June 1980. The July-October 1980 temperature and precipitation measurements are based on adjusted local observations.

11 The number of motorcycle accidents for 1971-1979 was obtained from reports of the Department of Transportation. The number of accidents in 1980 was estimated by applying the average annual percentage change in the 1971-1979 motorcycle accidents to the 1979 figure. This shorter period of averaging was selected because sources within the Department indicated that motorcycle accidents in 1980 were not expected to exceed 3,000.

The figures reported by the Department are as follows:

	Motorcycle Accidents
1971	1,689
1972	2,013
1973	2,411
1974	2,400
1975	2,400
1976	2,460
1977	2,718
1978	2,827
1979	2,872
1980	2,949*

*estimated

12 These are the unstandardized regression coefficients obtained using regression through the origin. Both the zero-order coefficient and the first-order partial coefficient are highly significant (t=15.03 and t=20.44, respectively).

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