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Highway Accidents in Sweden: Modeling the Process of Drunken Driving Behavior and Control

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There has been considerable interest in the Swedish application of strict law enforcement and severe penalties to the control of drunken driving, and presumably accidents. The Swedes claim that their country has a better record of highway safety than others as a consequence of its enforcement policies. However, various evaluations of the effectiveness of Swedish policy have generated considerable debate. Many people still question whether their enforcement and sanctioning policies succeed in reducing accidents. This paper presents a brief summary of previous approaches to that evaluation and an alternative set of analyses whose methodology avoids pitfalls of some of the earlier work.

Research reported in this paper attempts to distinguish among the effects of alternative sanctions. Sweden was thought to be an ideal country to study because a mix of sanctions is imposed including jail, fines, and driver's license withdrawal. The Swedes themselves were skeptical about the possibilities for positive evidence of sanction effects because they were sure that there was little variance in sanctions either across Swedish jurisdictions or over time. Nonetheless, they have been helpful in providing the data because they too are interested in the possibility of adjusting the mix of sanctions to develop more

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effective control policy. They have no doubts that their policies have the desired impact on accidents.

The results reported here represent a first look at a remarkably broad and detailed data set that provides an unusual opportunity to evaluate the effects of Swedish drunken driving control efforts. We focus on the chain of causality between efforts at control, a number of relevant environmental factors and driving accidents. The scope of the data allows us to reach conclusions that would have been impossible with less detailed data. Even though we regard this study as an important step in reaching a fuller understanding of the role of arrests and sanctions in controlling accidents, the results raise a number of questions that set the stage for additional investigation before sound policy conclusions can be reached.

PREVIOUS STUDIES

Possibly the best known study of the effectiveness of control policies in Scandinavia is that of H. Lawrence Ross (1975) which used interrupted time-series analysis. The technique, as adapted to the Swedish data, was used effectively to measure the impact of the British Road Safety Act of 1967 (Ross, 1973). For Sweden Ross found no evidence for a control effect, or in his words "the widespread belief in the deterrent effect of Swedish and Norwegian laws has little solid support."[1]

Unfortunately, the interrupted time-series technique requires a clean-cut break in policy that can be reflected in the observed data Otherwise, the effects of a policy change can be obscured by other contemporaneous changes. As Klette (1978) points out, the major changes in legislation and their implementation in Sweden occurred before analytical data were collected. Ross' inconclusive results are likely the result of inappropriate data. A second problem with interrupted time-series results is that they have no power to evaluate effects occurring during time periods removed from the date of intervention.[2]

Standard econometric techniques have been used to study the control of drunken driving and consequent accidents.[3] Using these techniques it is possible to evaluate control effects without a specific intervention if there is sufficient variation in controls and sanctions either over time or across jurisdictions. Rather than assume away the variety of other factors that may contribute to accidents, the approach explicitly enters them into the analysis, thus avoiding the two aforementioned problems with the interrupted time-series technique. However, Blumstein, et al. (1978)[4] have pointed out that the conclusions depend upon the validity of the econometric technique of omitted variables to identify control effects.

Alternatively, time-series data can be analyzed using multivariate ARIMA techniques that use leads and lags to identify control effects and direction of causality.[5] In some cases, Phillips et al. (1981) have shown in a study of English data, that ordinary least squares or generalized least squares approximate the results of the multivariate ARIMA techniques with a

considerable cost savings. Our paper applies this latter approach to analyze data on automobile accidents in Sweden.

THE DATA

The data we have used are taken from the record of all fatal and/or serious injury accidents in Sweden from 1976 through 1979. In addition, we have the record of all arrests and convictions for drunken driving over this same time period. The drunken driving data set contains detailed information on the dispositions of all cases. From these data which list all information separately for the three major cities -- Stockholm, Göteborg and Malmö -- and the rest of the country, we are able to compile pooled monthly time-series cross-section data on road accidents. The constructed data set contains information on the number of drunken driving convictions and sanctions imposed; amount of fines, days in jail, months of license withdrawal; as well as environmental data including road quality, rainfall, levels of alcohol consumption, and arrests for drunkenness. In addition, we constructed proxies for distance driven and highway congestion. Definitions of all variables included in the analysis are presented in the Appendix.

A MODEL OF DRUNKEN DRIVING AND ACCIDENTS

In modeling the process of accident generation two forces must be taken into account. The focus of this study is on those that influence drinking and driving-while-intoxicated. We cannot, however, ignore the host of environmental factors that contribute to accidents irrespective of the level of drunken driving. Our statistical analysis combines these two sets of influences into a single relationship.

Driver behavior is captured by application of the rational decision-making model common to the analysis of deterrence effects. An individual will drive following drinking, perhaps planning in advance to do so, simply because the expected benefits of so doing exceed anticipated costs. Thus, given the well-known relationship between alcohol consumption of drivers and accidents,[6] the number of accidents depend on the extent to which the expected benefits of drunken driving outweigh the expected costs. Expected benefits will vary across communities and seasonally with factors that are contemporaneous with accidents. Expected costs will be a function of strings of probabilities and costs associated with alternative sanctions. For example, expected costs associated with jail will be the product of the probability of apprehensions times our variable entitled jail cost (JC), which is defined as the product of the conditional probabilities of conviction $P(C|A)$ and sentencing to jail, $P(J|C)$, times the value to the individual of time lost in jail, i.e.,

$$JC = P(C|A) \cdot P(J|C) \cdot J \quad (1)$$

where J represents the imputed cost of a stay in jail. We use the number of days in jail as a proxy for J. Each of the objective probabilities can be calculated by aggregating over the data

set. Fine costs (FC) and costs of driver's license withdrawal (LWC) are similarly defined. In the analysis that follows we consider separately the probability of arrest (AR) and the expected cost given arrest of each of the sanctions.

Expected costs of sanctions are, of course, subject to an individual's subjective evaluation of the various probabilities involved. These might reasonably be expected to be a function of both current and past objective probabilities and actual sentencing levels. One factor contributing to possibly long information lags that enter the subjective estimates of arrest probabilities is the long lag between arrest and conviction. In our data this lag ranges generally from one to twelve months with a mean of 5 1/2 and a few extreme values of over one year, based on a convicted population of over 50,000 cases.

In addition to personal expectations, accidents are a consequence of the effects of other environmental factors that don't depend upon individual driver characteristics.[7] These include driving distance (KD), vehicle mix (VM), rainfall (RAIN) and the general level of alcohol consumption in the community (ALC). We anticipate that there also may be seasonal effects (S) that are not captured by the environmental variables and effects peculiar to individual communities D.

To account for these effects, it seems appropriate to specify

$$AC_t = f[D,S,E,\sum_i a_{t-i}AR_{t-i},\sum_i b_{t-i}FC_{t-i},\sum_i c_{t-i}JC_{t-i},\sum_i d_{t-i}LWC_{t-i}], \quad (2)$$

where AC_t is the number of accidents at time t; and a series of distributed lag coefficients, indexed by t-i, is postulated for arrests (AR), fine costs (FC), jail costs (JC), and license withdrawal costs (LWC). Contemporaneous effects include dummy variables for city (D), seasonal dummy variables (S) and the environmental influences on accidents (E).

Such a specification incorporates testable behavioral assumptions into a model in which the target is accident rates. On the basis of our behavioral specification, one would expect negative relationships between all control variables and accidents. At the same time, however, there is a contemporaneous relationship that must not be ignored. We know that one source of arrests for drunken driving is accidents in which the driver is not killed or seriously injured.[8] This will yield a positive contemporaneous link between arrests and accidents. These relations are depicted in Figure 1.

The expected effect of including all of these forces in a single relationship can be deduced from Table I in which the target is defined as the accident rate, i.e., the level of accidents per capita in a given district in a given month. The table indicates the linkages already discussed. The point of the table is to emphasize an important potential ambiguity in estimation results. We see that short of applying a multivariate ARIMA technique that relates variables at different lags, we can expect conflicting effects with respect to the sign of the relationship between arrests and accidents if both are measured contemporane-

FIGURE 1 A Schematic Model for Monthly Time-Series Analysis

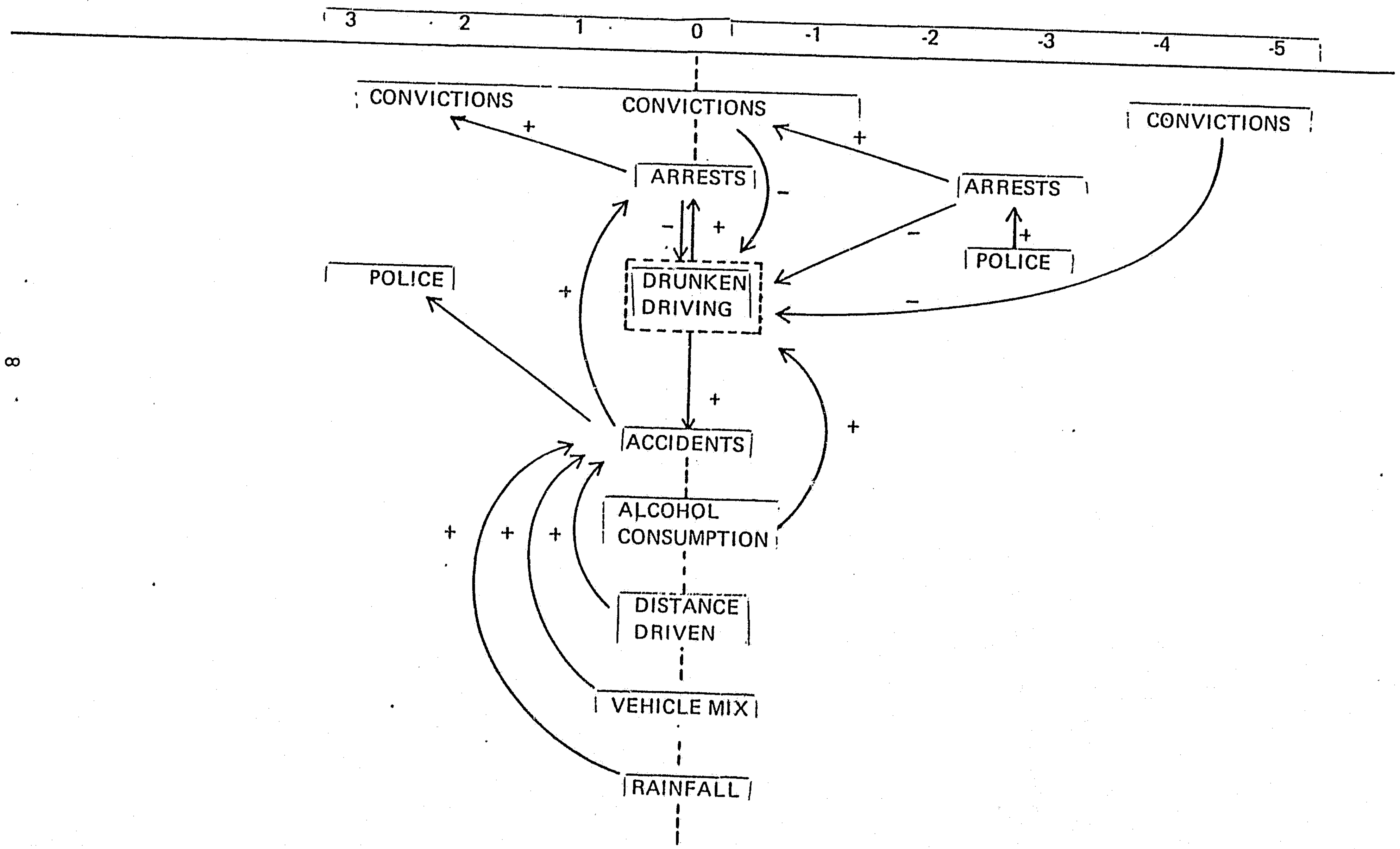

TABLE I: Theoretical Expectations for Relationship between Explanatory Variables and Accidents (Dependent Variable)

Explanatory Variable	Symbol	Contemporaneously Positive	Contemporaneously Negative	Net	Lagged Positive	Lagged Negative
Alcohol Consumption	ALC	+		+		
Distance Driven	KD	+		+		
Vehicle Mix	VM	+		+		
Rainfall	RAIN	+		+		
Arrests for Drunkeness	ARDR	+		+	+	
Arrests(DWI)	AR	+	−	?		−
Fine Costs	FC		−	−		−
Jail Costs	JC		−	−		−
License Withdrawal Costs	LWC		−	−		−

<u>Dependent Variables</u>

Dependent Variable	Symbol
Fatal Accidents	FAC
Serious Injury Accidents	SAC

ously. While it may be that past sanctions have a greater influence on current accidents than current sanction levels, simply because of information lags, still, there should be no ambiguity in the sign of the link between sanctions and accidents. We would expect rainfall[9] and distance driven or traffic density to be positively and contemporaneously related to accidents. Vehicle mix expressed as the ratio of two-wheeled to four-wheeled vehicles should, similarly, be positively linked to accidents.[10] There is the possibility that there will be considerable systematic variation in accidents that is seasonal but not captured by the variables discussed explicitly thus far. To check for this it would be appropriate to deseasonalize the data or to explicitly take account of seasonal variation. The approach used in the evaluation has been one in which all of these factors are considered.

EMPIRICAL EVALUATION

The testing of our hypothesized model as presented in Eq. (2) has taken a number of estimation forms. In every case all control variables, accident rates and environmental variables have been expressed as natural logarithms so that parameter estimates reflect percentage changes. Seasonal effects are captured by monthly dummy variables or have been filtered out by twelve period differencing of the time series. To take account of influences that are unique to specific regions, dummy variables are specified for the three cities. For each test fatal accident

relationships (FAC) and serious injury accident relationships (SAC) were estimated separately.

In all, six estimation forms for time series analysis have been applied to the data. These have included:

(1) Estimating with all variables in levels, i.e., simply expressed as natural logarithms, and all related contemporaneously, i.e., ignoring lagged effects;

(2) Estimating in natural logarithms; taking first differences to eliminate trend and twelve period differences to remove seasonal effects,[11] with all variables related contemporaneously;

(3) Estimating in levels as in 1 (above) except that distributed lag estimates are included for control variables;[12]

(4) Estimating with all variables first differenced to remove trend with distributed lag estimates for control variables;

(5) Estimating with all variables first differenced, then twelve period differenced to filter out both trend and seasonal effects, with distributed lag estimates for control variables;

(6) Estimating with the dependent variable first differenced and seasonally differenced, explanatory variables only first differenced, with distributed lag estimates for control variables.

The estimation sequence was chosen to systematically deal with several difficulties. First, it was found that the set of environmental variables were highly collinear. Second, some of these measures were also strongly seasonal and thus correlated with measures of seasonal effects. Finally, the collinearity among sanction variables made it difficult to reach conclusions regarding significance if one were to include them all as a group in any single estimatimation.

A notable result of all forms used has been that fatal and serious injury accidents vary differently in response to the forces taken into account. Typical of the contrast between serious injury and fatal accidents are the results of estimation in which the data are standardized for trend and seasonal effects so that the impacts of arrest and jail costs can be evaluated. These results are presented in Table II. For both classes of accidents the lag pattern for arrest effects is as our theorizing would predict, with a contemporaneous positive relationship between arrests and accidents and past accidents having a negative influence, presumably as a consequence of deterrence or incapacitation of potential accident drivers. See Figure 2 for a plot of the lag patterns. The results, however, were highly significant for the serious injury accidents that represent close to 80 percent of all serious motoring accidents, but not significant for fatal accidents.

From an examination of the coefficients on sanctions, it is clear that jail costs have a substantial impact on serious injury

TABLE II: Estimation Results
All Variables Differenced and Seasonally Differenced, Arrests and Jail costs

Dep. Var.	CON START	ALCOHOL	Distributed Lag Estimates Indep. Var.	0	-1	-2	-3	-4	-5	-6	-7		R^2	R^2
SAC	-.006 (0.33)	.372 (2.55)											.51	.85
			AR	.203 (2.32)*	-.066 (0.83)	-.193 2.02)*	-.217 (2.21)*	-.174 (1.67)*	-.104 (0.92)	-.043 (0.40)	-.030 (0.26)	-.625 (1.14)		
			JC	-.131 (2.14)*	-.043 (0.83)	-.222 (0.40)	-.046 (0.83)	-.090 (1.64)*	-.132 (2.36)*	-.149 (2.28)*	-.117 (1.79)*	-.729 (2.21)*		
FAC	-.016 (0.34)	1.13 (2.94)*											.38	.80
			AR	.296 (1.26)	.058 (0.27)	-.166 (0.65)	-.346 (1.32)	-.412 (1.6[illegible])	-.454 (1.50)	-.321 (1.15)	-.025 (0.81)	-1.41 (0.96)		
			JC	-.118 (0.74)	.102 (0.73)	.187 (1.24)	.180 (1.22)	.124 (0.84)	.062 (0.42)	.039 (0.27)	.096 (0.56)	.072 (0.76)		

Student's t-statistics (absolute value) are in parentheses.

* Significant at 5% level (1-tailed test).

$R^2\Delta$ = coefficient of determination expressed in changes.

Σw = sum of lag weights (0 to -7).

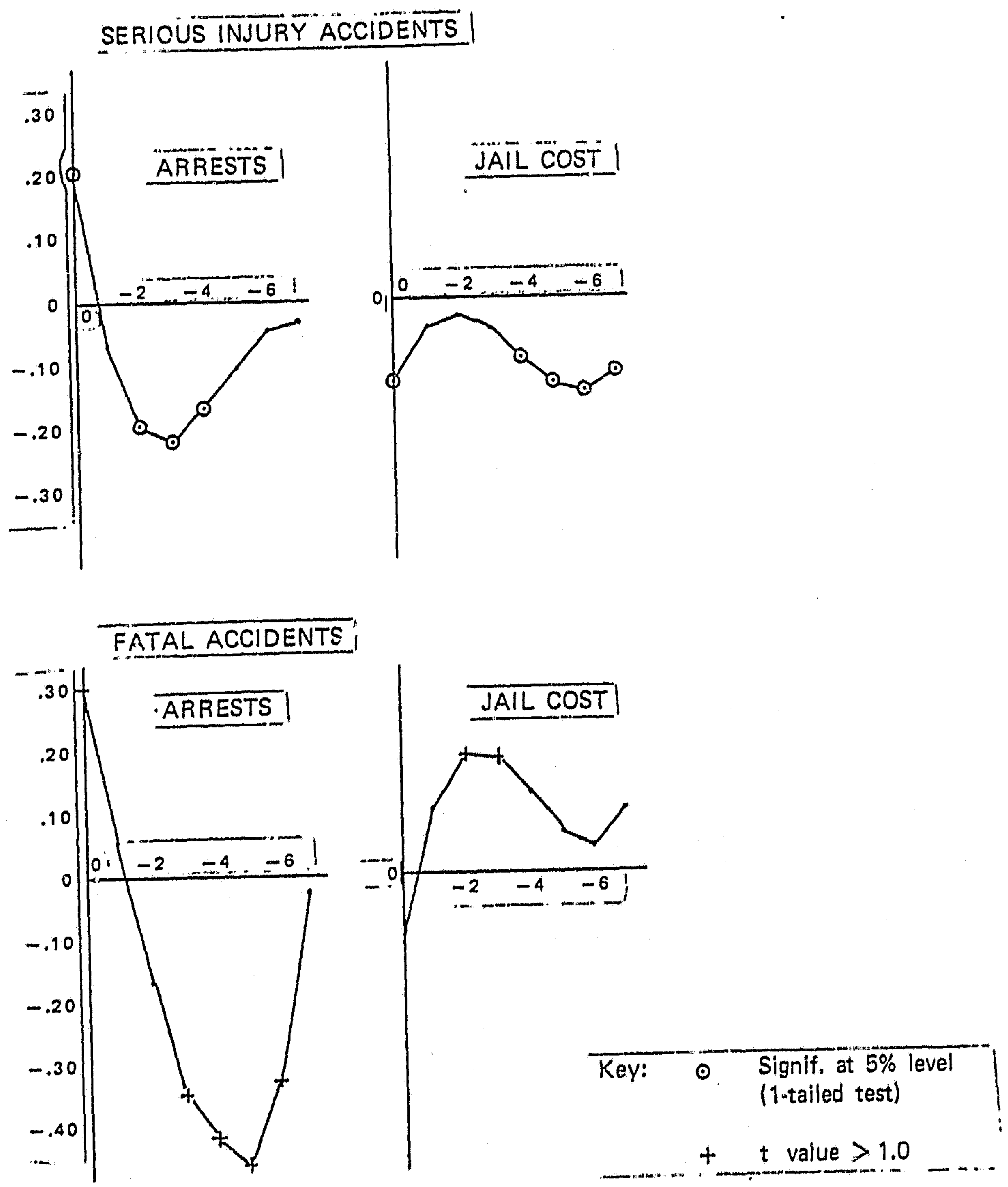

FIGURE 2 Lag Structure for Arrests and Jail Cost for Fatal and Serious Injury Accidents (Data from Table II)

accidents. Lag coefficients that are significant are at four, five, six and seven months prior to the month of arrest. Recall that on the average, convictions lagged arrest by 5 1/2 months. The relevant information on which subjective probabilities are based is obviously spread over an extended period. Surprisingly, jail costs do not seem to influence fatal accident levels, with lag coefficients even being positive although statistically insignificant. This pattern tends to be reinforced with the other estimation techniques tried. When both fine costs and jail costs are included in the various estimation forms, it is jail that invariably is the significant control factor for serious injury accidents and fine costs that in some case are shown to be significantly and negatively related to fatal accidents.

When all three sanctions are included in the estimation along with arrests, significance is weakened, as one might expect with evidence of collinearity among the control variables. Jail and fine costs tend to be insignificant but driver's license withdrawal is generally significant for both classes of accidents. A comparison of estimates is presented in Table III. Even in one test where the lagged weights are not statistically significant for serious injury accidents, the lag pattern shows negative weights at every lag.

Because of the effects of collinearity among the environmental variables, they have been deleted for many of the tests run. When they were included with differenced data, only measures of alcohol consumption were systematically positively related to

TABLE III: Estimation Results:
Statistical Significance of Summed Lag Weights (0 to -7) for Arrests and Sanctions

Form of Estimation Data Expressed in:	Depend. Variable	Exploratory Variables AR	FC	JC	LWC	R^2
1. Levels (ln)	SAC	N.S.	-N.S.	-N.S.	-.597	.49
					(2.33)	.53
	FAC	N.S.	-N.S.		-.893	.49
					(1.93)	.53
2. First Differences (ln)	SAC	N.S.	N.S.	-N.S.	-.953	.49
					(2.30)	.79
	FAC	N.S.	-2.42	N.S.	-1.37	.52
			(1.69)		(1.96)	.85
3. First Differences,	SAC	-N.S.	N.S.	-.973		.52
Seasonal Differences (ln)				(2.90)		.85
	FAC	-N.S.	-N.S.			.44
						.82
4. First Differences,	SAC	N.S.	N.S.	-N.S.	-N.S.	.50
Seasonal Difference (Dep)						.84
First Differences,						
(Expl.) (all in ln.)	FAC	N.S.	-N.S.	-N.S.	-3.57	.43
					(-2.22)	.81

Student's t-statistics (absolute values) are in parentheses.

N.S. indicates insignificant at 5% level (1-tailed test); - indicates a negative sign.

R^2 is the coefficient of determination in terms of changes.

accident levels, and sometimes vehicle mix when seasonal effects were not taken into account in the estimation procedure. It was only when seasonal patterns were ignored and all estimates were contemporaneous were results as theory would predict for distance driven, vehicle mix, and alcohol consumption and the sanction variables. Monthly rainfall levels turned out to be statistically unrelated to accidents in virtually every test attempted.

CONCLUSIONS AND COMMENTS

Our first conclusion is that, for the major share of accidents that lead to serious impairment in Sweden, control efforts have the intended effect. Serious injury accidents are moderated by arrests and jailing of drunken drivers. The estimates we have developed explain, at a minimum, 40 to 50 percent of the variations in both classes of accidents. This is an impressive result when cross-section data are differenced over time.

We find that driver's license withdrawal, an activity not regarded as a "punishment" in Sweden, may have as great or greater effect in alleviating accidental injury, based on the estimates for LWC if we are to put any credence in the sums of lag weights presented in Table III.

As anyone who models the process as a simultaneous system would expect, our results show strong evidence of two-way causality contemporaneously between arrests and accidents. This is borne out by the positive or insignificant contemporaneous values

for the coefficient on arrests even when we find the lag series is essentially negative overall. There is not, of course, any way we can separate that two-way causality with our approach. Such results can only be obtained within a simultaneous estimation framework.[13] What this means is that we cannot recommend policy fine-tuning on the basis of arrest effects because we cannot estimate the full effect.

The level of alcohol consumption, as represented by the proxy variable, number of arrest for drunkenness, generally appears to be a significant predictor of accidents even with deseasonalized data for drunkenness. It is the one proxy for alcohol consumption for which monthly data are published by city and region. While there would generally be concern with using "arrests for drunkenness" as a proxy of this sort, it must be remembered that these are not arrests in the normal sense. In Sweden drunkenness is not a crime and inebriates, while they are dealt with by the police, for lack of an alternative mechanism, are picked up to protect them from harm and either turned over to institutional treatment or released when sober. It is our presumption that patterns of drunkenness will correspond to those for drinking in general. The fact that this variable seems to be positively associated with accidents lends credence to the presumption.

When data are not deseasonalized by including appropriate dummy variables or using a differencing technique for removing seasonal variation, we find that distance driven and occasionally vehicle mix are shown to be positively related to accidents as theory and experience elsewhere would predict. This appears to be more true for serious injury accidents than for fatal accidents.

Rainfall is the one environmental variable that is least collinear with the others. Even so, in most formulations it is not statistically significant. This is particularly surprising in view of the strong role played by rainfall in British monthly time-series results.[14] Of course, much more of Swedish precipitation comes as snow and ice than it does in England. Unfortunately, monthly precipitation figures cannot distinguish between that which results in slippery pavements and that which does not.

We can't say, based on these results, if control effects come from deterrence and/or incapacitation. Both jail and license withdrawal are incapacitating in that they keep some bad drivers off the road if drivers who have driving privileges revoked obey the law. Whether or not the sanction is effective because it deters bad drivers or keeps them from driving may not be very important for some types of policy, nonetheless, it would be worth knowing, should we be able to obtain greater information on the characteristics of drivers likely to recidivate.

IMPLICATIONS OF THESE RESULTS

This research has been motivated by the desire to derive implications for policy. On one level, the implications are

fairly clear. Control efforts in Sweden have a significant impact. Since there can be little doubt that jail and license withdrawal has a moderating effect on accidents, the implication is that we cannot reject those options for control. At the level of developing more finely tuned policy, these results are less than what we might have hoped for. Our regressions imply that fines are an insignificant deterrent for the greater portion of accidents (serious injury). This result could be due to lack of causality but it might be due to little variation in the administration of that sanction as the Swedes have suggested. With small variation we are unable to detect its true impact. For that matter, we could hardly make a strong case for the accuracy of the estimates of accident elasticities with respect to arrests and jail even though we have found them unambiguously negative for the bulk of injury accidents.

We want to point out one of the most curious findings of this analysis. Namely, that sanctions affect fatal accidents differently than they affect serious injury accidents. We can think of little reason why this should be so if fatality is simply a random outcome in a serious injury accident. Our results imply that there are characteristics of fatal accidents that make them clearly distinct from less serious accidents. One might suspect that our observation is merely an anomaly of the particular data set. However, it is interesting to note that the same anomaly was found in a study of road accidents in England.[15] We suspect that the differences might be due to the personal characteristics of the drivers involved in fatal accidents.

We note that the policy in Sweden is to levy fines for cases involving blood alcohol levels between 0.5 and 1.5 pro mille and jail for cases exceeding 1.5 pro mille. We find that fatal accident levels are more sensitive to fines than are serious injury accidents which are more sensitive to jail sentences. Yet it would be difficult to believe that light drinkers are more prone to fatal accidents than heavy drinkers. In the current phase of our research we are trying to establish whether personal characteristics affect the probability of drunken driving as well as the probability of being involved in the two classes of accidents.

Another issue that needs to be investigated, and one that may be possible with the individual data at our disposal is the impact of incapacitation associated with jail, but also with driver's license withdrawal.

A result that merits special note is the effect of driver's license withdrawal. When outsiders observe the control policies imposed in Scandinavia, they are most struck by the emphasis on jail penalities. Yet the most striking effect among our results in terms of consistency across alternative estimation forms is that of driver's license withdrawal in controlling both fatal and serious injury accidents. This is a policy that imposes most costs on the driver rather than on society in general, as is the case with jail, and it is one that imposes a relatively modest cost on society including the driver's family. If our further

investigation confirms the strength of this sanction, this finding could be the greatest contribution to come out of our entire research effort.

FOOTNOTES

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[1]Ross (1975), p. 285.

[2]See Phillips, Ray and Votey (1981) and Votey (1981).

[3]There is by now an extensive literature in criminology and economic journals on such applications. Studies on drunken driving time series and cross-section data from Norway and Sweden are Votey (1978, 1982, and 1983). Typical is a result for cross-section data by county for Sweden which finds the accident rate for fatal and serious injury accidents will be reduced by .515% with a 1% increase in law enforcement manpower, will rise .530% for a 1% increase in per capita alcohol consumption, and .438% and .192% for 1% increases in distance driven and the ratio of two-wheel to four-wheel vehicles, respectively.

[4]The point lies at the core of the debate in regard to whether econometric techniques using simultaneous estimation techniques are appropriate for the analysis of crime control efforts. Because of the nature of identifying restrictions required for analysis of drunken driving control, that issue can be dealt with by assumptions that can stand up to challenge better than in the case of, say, homicide. Multivariate ARIMA analyses are relatively rare because of the costs involved.

[5]Phillips, Ray, and Votey <u>op. cit.</u>, have used the approach in a return to the issues raised by Ross regarding the British case and found a statistically significant decline in serious injury accidents of 16.1% following the passage of the British Road Safety Act of October 1967, a result consistent with Ross. Additionally, it was found that a .908% rise in accidents accompanied a 1% rise in distance driven, a 1.72% rise and .047% rise followed 1% increases in alcohol consumption per capita and precipitation, respectively.

[6]A landmark article in this regard has been Borkenstein (1974).

[7]For environmental variables that have been found to be significant in earlier studies on Sweden and elsewhere, see Votey (1982).

[8]Of our sample of convicted drunken drivers of in excess of 50,000 persons, approximately 1,600 of the arrests have been a

consequence of an accident.

[9]An earlier study in this regard for England is Codling (1972).

[10]For the English experience, see Johnson (1972).

[11]Taking twelve period differences over a monthly time series is one of the least distorting of a number of alternatives to remove unidentified seasonal influences from time series data.

[12]The 3rd order polynomial distributed lag function has been estimated using a technique developed by Almon (1965).

[13]We cannot, of course, estimate the control effect purely associated with arrests without specifying a simultaneous relationship for which there is sufficient information to identify all parameters individually. In general, this will not be possible because we can't observe actual levels of drunken driving.

[14]See Phillips, et al. (1981).

[15]Phillips, et al., ibid.

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APPENDIX

THE VARIABLES DEFINED

Control Variables

Arrests: AR - Total Arrests of drunken driving, per month, per city, per capita.

Fine Costs: FC - persons fined x number of day fines x amount of day fines, per month, per city, per capita.

License Withdrawal Costs: LWC - License withdrawals x months withdrawn, per month, per city, per capita.

Environmental Variables

Alcohol Consumption

Pure Alcohol Consumed per person*:- ALC total of beer, wine, and spirits converted to 100% alcohol sold per month, per city, per capita.
Arrests for Drunkenness: ARDR - arrests (typically overnight detention for public drunkenness, per month, per city, per capita)
Road Quality*: RQ - Miles of Paved Roads relative to total road system, per district - measured annually
Distance Driven: KD - index based on sales of motor fuel, both gasoline and diesel, per month, per city, per capita
Traffic Density*: TD - registered motor vehicles relative to the index of distince driven, per month, per city.
Vehicle Mix: VM - ratio of registered two-wheeled to four-wheeled vehicles per month, for the entire country.

Accidents:

Personal Injury Accidents: SAC - serious injury accidents reported by city, by month, per capita.
Fatal Injury Accidents: FAC - fatal accidents reported by city, by month, per capita.

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*These variables were deleted from the analysis at an early stage because other similarly defined variables were obviously proving to have greater explanatory power with regard to accidents.

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