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The Choice to Drink and Drive and the Influence of Criminal Sanctions: An Empirical Evaluation of Swedish Data

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This paper reports on research into the effectiveness of efforts to control accidents attributable to drunken driving. Such accidents have become a subject of increasing concern in the U.S. over recent months. Across the U.S. stricter enforcement practices and more severe penalities are being imposed. Almost inevitably Sweden and other Scandinavian countries are held up as examples of what strict enforcement and sentencing can do. Yet, among evaluators the effectiveness of Swedish control policies is still a matter of dispute. Our research takes a new look at the evidence of control effectiveness using an analytical approach designed to avoid alleged shortcomings of previous work.

The primary focus of our research has been on attempting to determine the relative effectiveness of alternative sanctions in controlling accidental injury and death in Sweden. The modeling of the process of accident generation and control follows the general approach applied to evaluation of the effectiveness of law enforcement and sanctions in controlling felony crime. However, the econometric techniques we use to measure the impact of sanctions are a departure from those usually applied to analyzing simultaneous systems to avoid their alleged shortcomings. The scope of the data allow us to reach conclusions that would not have been possible with less complete information.

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EVIDENCE OF CONTROL EFFECTIVENESS

Even though Sweden and other Scandinavian countries have imposed strict penalities on drunken drivers since the 1930s, the application of statistical techniques to the evaluation of their effectiveness was not seriously attempted until the widely noted work of H. Lawrence Ross (1975). The Ross approach was to seek out points at which changes in legal sanctions or rules of evidence could have led to a marked change in accident levels. He used interrupted time-series analysis to measure the impact of legislated change. That same technique had been applied to study the effect of the British Road Safety Act of 1967. In the British case, Ross (1973) found a reduction of approximately 14.6% in serious injury accidents in England and Wales following the change in the law. For Scandinavia, however, he concluded that there was no evidence of a control effect but invited further investigation into the matter.[1]

A response, Votey (1978),(1982) and (1983), modeling the process of drunken driving and accident generation as the joint determination of conviction probabilities, drunken driving levels, and accident levels found a statistically significant effect of law enforcement and sanctions in reducing accidents.[2] Reasons for the difference in results has been attributed to the possibility that the Ross approach failed because of limitations of the investigative technique. For example, Klette (1978) suggested that the major changes in the law were all prior to the period for which adequate data are available for evaluation and the changes that did take place were of such little import as to be unlikely to lead to measurable changes in accident levels. The econometric approach, it was argued, didn't require a major intervention in terms of changes in laws or enforcement practices if one could take account of variations in enforcement efforts and other influences on accidents over an appropriate sample. However, the econometric technique, in general, has been strongly criticized by Blumstein et al. (1978) who argue that there are sufficient difficulties with the assumptions required for evaluation to render the results questionable. In particular, they suggest that the technique of omitting variables for purposes of identification is an unsuitable practice when evaluating impacts of law enforcement and sanctions.

In response to these criticisms, Phillips, Ray and Votey (1983) have used both multivariate ARIMA techniques and regression analysis in a reexamination of the British data. They argued that this was an alternative that didn't suffer from the limitations of either the Ross approach or the econometric modeling of the process as a jointly determined system of equations. Using monthly time-series data, for the British case, they found that serious injury accidents declined 16.1% as a consequence of the new act, a result strikingly similar to that of Ross, but noted also that most of the variation of accidents over time could be explained by variation in distance driven, alcohol consumption, and rainfall.[3] That paper also demonstrated that the results of the costly multivariate ARIMA analysis could be approximated by ordinary least squares or generalized least squares so long as the variables were similarly defined. This

research takes advantage of that latter finding to implement the approach used here. The Swedish data provided a rich resource for this effort.

THE DATA

The dependent variables for this study are the per capita rates for reported fatal and serious injury accidents in Sweden each month for the years 1976 through 1979. These are reported for each of the three major cities -- Stockholm, Göteborg, and Malmö -- as well as the rest of the country so that the data are a pooled cross-section - time-series. In addition, we have obtained detailed data on all individuals arrested and convicted of drunken driving for these same periods and locations. The individual data include complete information on sanctions: time in jail, amounts of fines, and months of driver's license withdrawal. Also, we have monthly environmental information by community that includes per capita consumption of alcoholic beverages, expressed as the equivalent of pure alcohol, arrests for drunkenness, rainfall, vehicle mix, and a proxy for distance driven.

A MODEL OF ACCIDENT GENERATION AND CONTROL

The model underlying our choice of econometric technique can be stated as a refinement of the frequently applied simultaneous system approach to crime generation and control. The illegal behavior in this case is drunken driving (DD) which can be expected to vary in the aggregate because of variations in the general level of alcohol consumption (ALC) and driving (KD).

Drivers will be inhibited by the probability of arrest and subsequent sanctions. We presume that levels of contemporaneous and previous arrests (AR) generate the information on which drivers base their subjective evaluation of the likelihood of arrest. Their expectations as to sanctions (SV) would likewise be a function of both current and past sanctions in the community. Consequently we would expect that drunken driving levels could be approximated by

$$DD_t = d[AR_{t-i}, SV_{t-i}, ALC_t, KD_t]; \quad i = 1,\ldots,n, \tag{1}$$

in which information that contributes to the community's drivers' estimates of subjective probabilities extends over the present and previous months.

Arrests will be a consequence of the load of drunken driving on the law enforcement system combined with the level of resources applied to motoring offenses. It is frequently the case that the first indication to the police that a driver is intoxicated comes from investigation of an accident. In fact, in our population of over 50,000 cases of drunken driving prosecuted, approximately 1,600 arose out of accidents. Thus the production of arrests can be represented by

$$AR_t = a(AC_t, L_t), \tag{2}$$

where AC and L are accident levels and law enforcement for patrol respectively.

Demand for law enforcement inputs for dealing with traffic problems are likely to be endogenous to the system depending upon the values society places on accidents, the costs of resources,

accident levels, and the general level of driving. Thus, we can write

$$L_t = l(r_t, AC_t, w_t, KD_t), \tag{3}$$

in which r_t represents society's perceived average loss rate per accident and w_t, the cost of law enforcement resources used to reduce accidents.

The final relationship for this system is that for accidents.[4] Obviously, accidents are subject to other influences in addition to drunken drivers.[5] These will include such environmental factors as distance driven (KD), vehicle mix (VM), and rainfall levels (RAIN), leading to the relation[6]

$$AC_t = m[DD_t, KD_t, VM_t, RAIN_t]. \tag{4}$$

One difficulty with this system is that we never observe the level of drunken driving, nor do we know the wages of police officers or the values the community places on accident losses. By substitution we can reduce the system to two relations, of which accidents are expressed as

$$AC_t = m^*(AR_{t-i}, SVt\text{-}i, ACL_t, KD_t, VM_t, RAIN_t), \tag{5}$$

and arrests are

$$AR_t = (AC_t, r_t, w_t, KD_t). \tag{6}$$

There is insufficient information available to estimate these two relations as reduced forms, however, there are data on all of the variables in Eq. (5). A problem with estimating (5) is that there is likely to be simultaneous equation bias associated with the estimated coefficient on contemporaneous arrests (AR_t). This is because of accidents (AC) being a factor in the determination of arrests (as well as of law enforcement resources) and arrests being an input into the determination of drivers' subjective probability of arrests and hence a deterrent to accidents. Even if we had sufficient additional variables in the system to estimate the two equation system, avoiding the simultaneity bias and sorting out the causality, we would still have to contend with the Blumstein, et al., criticism. That is, the model has the alleged weakness of all the econometric studies of deterrence: for identification one must rely on an arbitrary choice of excluded variables.

In this case, however, there is no need to meet this issue head on. Our objective is to estimate the relative impacts of sanctions on drunken driving and consequently accidents. While the coefficient on contemporaneous arrests could be consistent with hypothesized behavior and have a net positive or negative value, if the coefficients on sanctions are negative and the sum of the coefficients on lagged arrests overshadow a possible contemporaneous positive value, then the effect of sanctions will be unambiguous. With this point in mind, we have proceeded to estimate Eq. (5) using a number of alternative specifications of the variables in a carefully sequenced strategy to evaluate sanction effects. The variables actually used in estimation are defined in Table I and their hypothesized relationship to accidents summarized.

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

Explanatory Variable	Symbol	Contemporaneously Positive	Contemporaneously Negative	Contemporaneously 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		-	-		-

Dependent Variables	
Fatal Accidents	FAC
Serious Injury Accidents	SAC

EMPIRICAL EVALUATION

The first estimation with our data follows in part, the approach applied to the English data of Phillips, Ray, and Votey (1981), in which ARIMA and GLS regression techniques yielded similar results. First differences were taken in our data to eliminate trend, then twelve period differences were taken to remove seasonal effects. Remaining serial correlation was corrected by using a Cochrane-Orcutt estimation technique. Variables are defined in per-capita terms to minimize the possibilities of heteroscedasticity. Separate estimates are made for fatal accidents and for serious injury accidents. Results of that estimation are displayed in Table II. As can be seen for both fatal and serious injury accidents, most coefficients lack significance with the exception of arrests for drunkenness, and the sanction variables. The latter are not equally significant in both equations, however. Coefficients of determination are large enough to suggest that there is sufficient explanatory power in the set of variables for them to have a significant impact on the target.

Why should apparently plausible explanatory variables appear to be statistically insignificant? First, the environmental variables tend to be highly collinear. Results of running the Farrar and Glauber (1967) test for multicollinearity are displayed in Table III. In addition, several of the variables are strongly seasonal, and much of their influences is likely to be lost with a technique that eliminates systematic seasonal influences. For plots of the time-series pattern of some of

TABLE II: Estimation Results
Data First Differenced, Seasonally Differenced, CORC, Data in natural logarithms

Dep. Var.	Constant	STOCK	GOTE	MALMO	ARDR	RAIN	KD	VM	AR	FC	JC	LWC	R^2/$R^2\Delta$
1. SAC	.001	-.324	-.271	-.188	.224	.003	-.498	.152	-.008	.065	-.052	-.101	.44
	(0.04)	(1.40)	(1.18)	(0.77)	(2.39)*	(0.11)	(1.48)	(0.45)	(0.36)	(1.46)	(1.11)	(1.93)*	.83
2. FAC	.007	-.541	-.368	.305	.529	-.071	.046	.795	-.021	-.210	-.114	-.009	.34
	(0.19)	(0.97)	(0.66)	(0.52)	(2.32)*	(1.29)	(0.06)	(0.97)	(0.39)	(1.98)*	(1.03)	(0.07)	.78
3. SAC	-.003				.207				-.009	.095	-.068	-.011	.41
	(0.23)				(2.41)*				(0.39)	(2.32)*	(1.51)	(1.85)*	.81
4. FAC	-.007				.672				-.017	-.194	-.064	-.016	.32
	(0.18)				(3.27)*				(0.32)	(2.03)*	(0.62)	(0.14)	.78

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

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

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

TABLE III: Test for Collinearity, CORC

Dependent Variable	Explanatory Variables ARDR	RAIN	KD	VM	R^2	$R^2\Delta$
1. ARDR		(2.09)	(1.79)	(0.21)	.938	.642
2. RAIN	(0.45)		(-1.06)	(0.66)	.246	.531
3. KD	(0.55)	(0.55)		(-7.93)	.804	.855
4. VM	(0.04)	(0.13)	(-1.74)		.979	.927

Student's t-statistics are in parentheses.

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

these variables see Appendix Figs. A1 and A2.

As can be seen, distance driven drops drastically in winter months with the severe climate and the vehicle mix changes quite smoothly almost in the shape of a sine wave. The latter can be explained by the fact that vehicle registration costs are assessed on the basis of months in use and two-wheel motor vehicles are taken out of service in inclement weather.

Even among the control variables collinearity appears to be a problem. A similar test to that for the environmental variables shows a lesser degree of group correlation, but suggests a difficulty in running arrests in connection with the sanctions as a group.[7] This should be no surprise, since with per se laws and arrests based on an alkotest, most drivers arrested end up convicted and sanctioned with some combinations of fines, jail, and license withdrawal; probation often is thrown in as well.

The implication is that little will be learned about alternative effects on drunken driving and accidents by including all the environmental variables and all of the sanction variables in a single pair of estimations for serious injury and fatal accidents. Because of the inherent collinearity, an alternative strategy has been worked out. The first step was to test for the impact of a single sanction, jail, in a relationship in which all environmental variables, regional dummies and arrests are included to double check the collinearity effect with the environmental variables. The two control variables were included in a 3rd order, unconstrained, polynomial distributed lag formulation so that the anticipated lag effect can be captured.[8]

The result of that estimation, presented in Table IV and Figure 1, yielded insignificant coefficients for all of the environmental variables except our alcohol consumption proxy which was highly significant. None of the district variables were significant. The coefficients for the arrest variable follow a pattern consistent with the hypotheses of Table I for both fatal and serious injury accidents. For fatal accidents neither the contemporaneous positive value nor the larger lagged negative values quite reach statistical significance, but the net effect is substantially negative overall. For serious injury accidents, the contemporaneous value is positive and significant and the coefficient at lags 2, 3 and 4 are strongly negative and significant for an overall negative impact. All of the weights for jail are insignificant overall for serious injury accidents that comprise roughly eighty percent of accidents involving serious injury or death. A second pair of estimates was made excluding all insignificant variables.

If we run the same relationships replacing jail cost with fine costs, the arrest variable performs similarly for both serious injury and fatal accidents. Fine costs produce a somewhat stronger result for fatal accidents but we find they are actually positively related to serious injury accidents. For license withdrawal costs, arrest again behaves similarly for serious injury accidents still having significant negative values after the contemporaneous positive value. For fatal accidents the result on arrests is weaker, negative but not significant. The sanction itself has a significant contemporaneous coefficient for

TABLE IV: Estimation Results

All Variables Differenced and Seasonally Differenced, Arrests and Jail costs

Dep. Var.	Con- stant	ARDR	Distributed Lag Estimates Indep. Var.	0	-1	-2	-3	-4	-5	-6	-7	Σω	R^2	$R^2\Delta$
SAC	-.006	.372											.51	
	(0.33)	(2.55)												.85
			AR	.203	-.066	-.193	-.217	-.174	-.104	-.043	-.030	-.625		
				(2.32)*	(0.83)	2.02)*	(2.21)*	(1.67)*	(0.92)	(0.40)	(0.26)	(1.14)		
			JC	-.131	-.043	-.222	-.046	-.090	-.132	-.149	-.117	-.729		
				(2.14)*	(0.83)	(0.40)	(0.83)	(1.64)*	(2.36)*	(2.28)*	(1.79)*	(2.21)*		
FAC	-.016	1.13											.38	
	(0.34)	(2.94)*												.80
			AR	.296	.058	-.166	-.346	-.412	-.454	-.321	-.025	-1.41		
				(1.26)	(0.27)	(0.65)	(1.32)	(1.62)	(1.50)	(1.15)	(0.81)	(0.96)		
			JC	-.118	.102	.187	.180	.124	.062	.039	.096	.072		
				(0.74)	(0.73)	(1.24)	(1.22)	(0.84)	(0.42)	(0.27)	(0.56)	(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.

Σω = sum of lag weights (0 to -7).

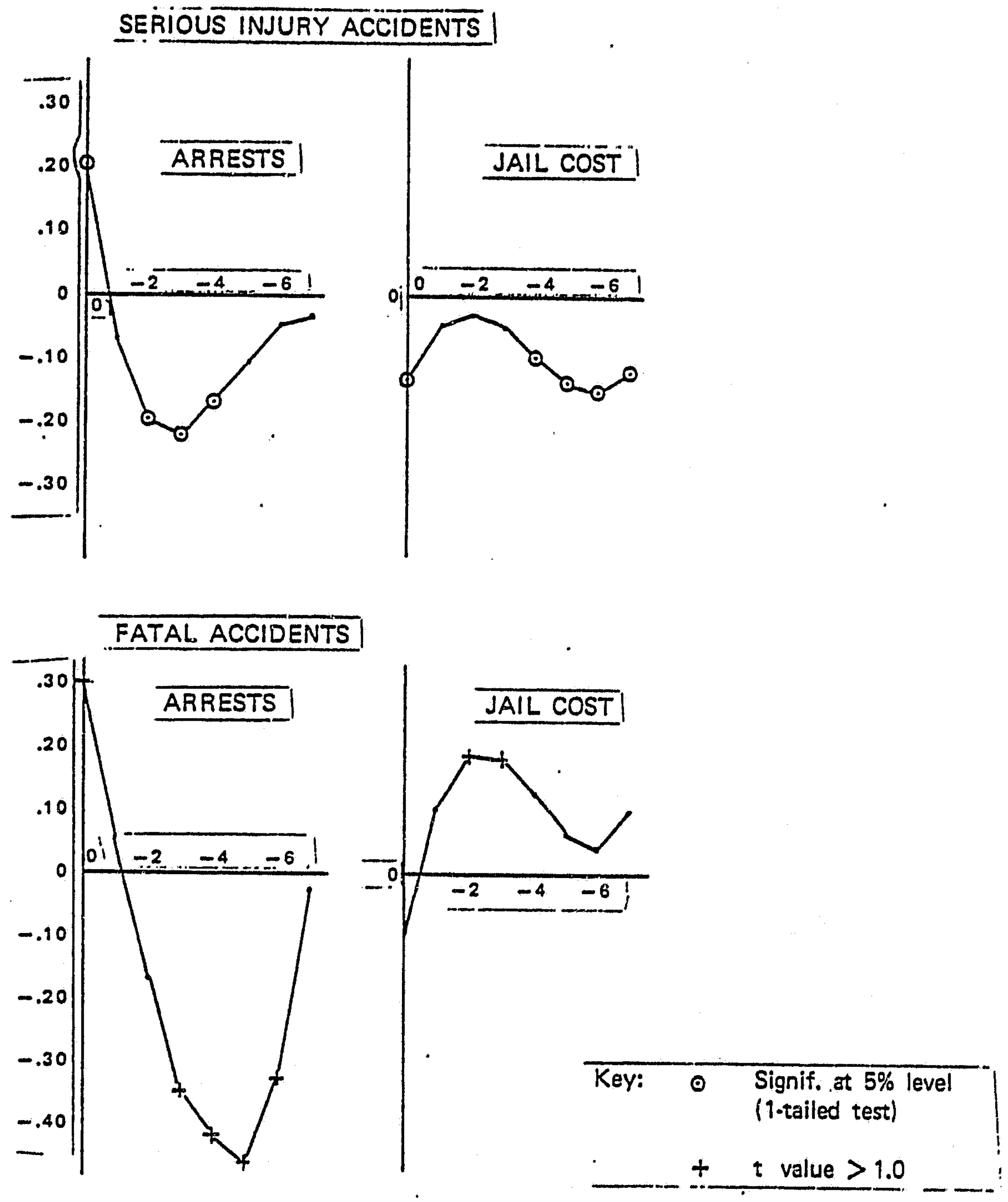

FIGURE 1 Lag Structure for Arrests and Jail Cost for Fatal and Serious Injury Accidents (Data from Table IV).

fatal accidents and is negative on balance, but not significant for serious injury accidents. Thus, from these results we can conclude that control works in Sweden, but the only strong result is in connection with jail sentences and serious injury accidents.

In view of the apparent difficulties in distinguishing effects among sanctions and the strong evidence of collinearity among both environmental variables and control variables, an alternative approach was followed to attempt to distinguish among the various factors contributing to accidents. We reasoned that, if any attempt to correct for seasonality conflicts with identifying effects of the environmental variable that are themselves highly seasonal, then a reasonable approach would be to abandon attempts to filter out seasonal influences and reestimate the relationship including environmental and control variables. This was done with all variables expressed in their natural logarithms. The results are shown in Table IV.

For serious injury accidents, in spite of the collinearity, the coefficient on distance driven is positive and significant, rainfall and vehicle mix have positive but insignificant effects, and alcohol consumption continues to be strongly significant. For fatal accidents none of these variables show any degree of significance and only one district variable, that for Göteborg, has a residual influence on accidents.

The effect with regard to arrests is less dramatic for serious injury accidents than with the double differenced estimates, but the pattern is similar. Again, the estimated coefficient for the arrest variable in the fatal accident equation is insignificant. Patterns with regard to fine costs and jail costs essentially replicate the double differenced result. The striking difference using this estimation form is the result with respect to driver's license withdrawal. Not only are individual lag values significant and negative, the entire string of lag estimates, including the contemporaneous values, are statistically significant as a group for both fatal and serious injury accidents.

The contradiction regarding the effect of driver's license withdrawal between the double differenced results where sanctions were included individually and the results from estimation in levels led us to a further round of estimations.

It is interesting to note that when those regressions are run excluding driver's license withdrawal for fatal accidents, fine costs become highly significant contemporaneous and for lags 1 and 2 with t-values of -2.14, -2.65, and -1.84, respectively and jail costs become more significant for serious injury accidents. Arrests keep the pattern shown in Table V but also become more significant both for initial positive values and negative lagged values. If they are run with all variables being contemporaneous, in the case of fatal accidents fine costs is significant with a t-value of -2.95 and for serious injury accidents jail costs has a t-value of -4.21. Arrests in both cases are insignificant as predicted. In view of our initial objective to be able to separate and compare effects of alternative sanctions, we conducted three additional estimations for

TABLE V: Estimation Results:
Variables in levels (in natural logarithms), Arrests and Sanctions: PDCORC

Dep. Var.	CON	STOCK	GOTE	MALMO	ARDR	RAIN	KD	VM	R^2	$R^2\Delta$
	Independent Variables									
SAC	-2.42	.096	.336	.397	.332	.028	.540	.141	.74	
	(0.47)	(0.21)	(0.91)	(0.70)	(2.37)*	(1.03)	(1.73)*	(0.95)		.62
FAC	1.17	-.975	-1.25	-1.44	-.086	-.036	-.219	.264	.49	
	(0.12)	(1.16)	(1.86)*	(1.38)	(0.33)	(0.62)	(0.32)	(0.97)		.53

Lag Pattern:	Variable Lag:	0	-1	-2	-3	-4	-5	-6	-7	Σω
SAC	AR	.050	.079	.096	.102	.097	.081	.053	.014	.572
		(0.36)	(0.97)	(1.12)	(0.97)	(0.87)	(0.73)	(0.41)	(0.08)	(1.11)
	FC	-.349	-.278	-.222	-.172	-.117	-.046	.052	.186	-.946
		(1.03)	(1.30)	(1.05)	(1.01)	(0.71)	(0.23)	(0.26)	(0.68)	(1.52)
	JC	.006	.001	.003	.012	.027	.048	.076	..111	.283
		(0.05)	(0.02)	(0.04)	(0.12)	(0.29)	(0.57)	(0.85)	(0.84)	(0.55)
	LWC	-.388	-.243	-.130	-.051	-.006	.007	-.014	-.068	-.893
		(2.39)*	(2.65)*	(1.82)*	(0.65)	(0.07)	(0.09)	(0.17)	(0.49)	(1.93)*
FAC	AR	0.86)	0.85	.073	.050	.017	-.027	-.081	-.146	.057
		(1.17)	(1.94)*	(1.56)	(0.87)	(0.28)	(0.44)	(1.18)	(1.46)	(0.20)
	FC	.215	-.124	-.248	-.221	-.112	.014	.090	.049	-.338
		(1.58)	(1.05)	(2.13)*	(2.42)*	(1.20)	(0.12)	(0.81)	(0.37)	(0.91)
	JC	-.138	-.017	0.33	.035	.011	-.014	-.020	-.019	-.092
		(2.01)*	(0.33)	(0.58)	(0.66)	(0.22)	(0.26)	(0.37)	(0.24)	(0.32)
	LWC	-.147	-.090	-.050	-.030	-.027	-.044	-.078	-.131	-.597
		(1.73)*	(1.79)*	(1.25)	(0.67)	(0.60)	(1.05)	(1.77)*	(1.85)*	(2.33)*

Student's t-statistics (absolute value) are in parenthesis.
*Significant at 5% level (1-tailed test).
$R^2\Delta$ = coefficient of determination in terms of changes.
Σω = sum of lag weight (0 to -7).

both accident classes in which all sanction variables were included simultaneously with lag patterns estimated for each. Our approach was to stepwise increase the amount of filtering of the data, progressing from the first case with data expressed in levels to data in first differences, so that trend is presumably filtered out. We then proceeded to double differencing as was done with our initial estimates shown in Table I except that the lag weights are estimated for seven lags prior to the contemporaneous value. Presumably this would filter out both trend and seasonal variations.

Finally, we estimated the results from a data set in which trend and seasonality were removed from the dependent variable by differencing but only first differences were taken of the dependent variable. This seemed to be a reasonable procedure since, whereas it is clear that accidents have a strong seasonal component that reflects environmental influences, inspections of the plots of the time-series for arrests and sanctions indicate that variation over time appears more random than seasonal. Statistics on the sum of the lag weights and their statistical significance as a group are presented. Note that significance for the entire string of lags is a stronger test than testing significance lag by lag, since several lags could be significant but the entire sequence not, if sufficient coefficients in the string have no significance at all. In view of that qualifier, it is instructive to consider Table VI. The results for double differencing (case 3) are essentially the same as in Table IV except that, when all sanctions and arrests are included, arrests become

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

Form of Estimation Data Expressed in:	Depend. Variable	Exploratory Variables Σω AR	FC	JC	LWC	$R^2\Delta$
1. Levels (ln)	SAC	.057 (0.20)	-.338 (0.91)	-.092 0.32)	-.597 (2.33)*	.49 .53
	FAC	.572 (1.11)	-.946 (1.52)	.283 (0.55)	-.893 (1.93)*	.49 .53
2. First Differences (ln)	SAC	.541 (1.05)	.109 (1.21)	-.618 (1.39)	-.953 (2.30)*	.49 .79
	FAC	.869 (1.03)	-2.42 (1.69)*	.399 (0.54)	-1.37 (1.96)*	.52 .85
3. First Differences, Seasonal Differences (ln)	SAC	-.017 (0.17)	.108 (0.53)	-.973 (2.90)*	.403 (1.01)	.52 .85
	FAC	-.120 (0.48)	-.709 (1.40)	.027 0.03)	.624 (0.63)	.44 .82
4. First Differences, Seasonal Difference (Dep) First Differences, (Expl.) (all in ln.)	SAC	.110 (0.16)	.846 (0.66)	-.498 (0.94)	-.520 (0.79)	.50 .84
	FAC	.732 (0.43)	-1.60 (0.51)	-3.13 (0.24)	-3.57 (-2.22)*	.43 .81

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

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

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

Σω = sum of lag weight (0 to -7).

insignificant for serious injury accidents. Recall that in case 3 license withdrawal costs were not significant although the net effect was negative. Note now that for all other cases except serious injury accidents in case 4 license withdrawal costs are negative and significant for the sum of the lag weights. Not clear from the table is the fact that even in case 4 for serious injury accidents, every lag is negative and the lag pattern is smooth. In short, there is substantial evidence that license withdrawal costs have a significant impact on accidents. And, if one considers individual lag values these results do not conflict with the effects observed earlier with respect to jail and serious injury accidents.

CONCLUSIONS AND COMMENTS

(1) Based on the cumulation of tests and particularly on the results in Table IV and Fig. 1, we conclude that arresting and jailing has a substantial impact in Sweden on accidents that would otherwise lead to serious impairment. In particular, the variants of the model in which these are the primary controls can explain 40 to 50 percent of the variation in accident rates. This is an impressive result for time differenced cross-section data.

(2) We find that driver's license withdrawal has as great or greater impact on accident rates, based on the results presented in Table VI. This is a notable finding since the basis for license revocation in Sweden is to protect the public rather than to serve as a sanction against a criminal act.

(3) Not surprisingly, there is strong evidence of two-way causality between arrests and accidents as our discussion of modeling the process as a simultaneous system suggests. This is borne out by the positive or insignificant contemporaneous estimates for arrest coefficients in estimating accident relations, whereas we find overall sums of lag weights tending to be negative, consistent with control theories. While it is not possible to fine tune arrest policy based on such results, the qualitative implications regarding sanctions are unaffected, however.[9]

(4) Alcohol consumption appears to be positively related to accidents, as hypothesized, even when data are deseasonalized. The variable that works most consistently is arrests for drunkenness (ARDR) which is used here as a proxy for for consumption of pure alcohol. This variable has the advantage that it is recorded monthly by community. The usual concern that this variable measures enforcement policy rather than behavior are less likely to be concerns in the case of Sweden since drunkenness is not a crime. Inebriates are dealt with by the police whose role is to pick them up to protect them from harm. They are kept overnight and released when sober or turned over to institutional treatment. We assume that patterns of drunkenness will correspond to general drinking levels.

(5) When no procedure is used to standardize for seasonal influences we find that distance driven (KD) and in some cases

vehicle mix (VM) tend to be positively related to accidents, as hypothesized. These relationships appear to be stronger for serious injury than for fatal accidents.

(6) Rainfall (RAIN), while the least collinear of the environmental variables, appears almost unrelated to accident levels. This contrasts with the strong evidence that accidents and rainfall are related in British monthly time-series results.[10] It is true that in Sweden a greater portion of precipitation comes as snow and ice and our estimation cannot distinguish between that which yields slippery roads and that which does not.

(7) A question that remains unanswered is whether control effects are a result of incapacitation or deterrence. Both jail and driver's license withdrawal could be incapacitating. Yet is is also true that many unlicensed drivers are involved in accidents. Apparently sufficient numbers of drivers who have licenses revoked refrain from driving to create a statistically significant reduction in deaths and injuries. It would be useful for the determination of policy if we knew whether it was the threat of this or revocation itself that reduces accidents.

POLICY IMPLICATIONS

In a qualitative sense policy implications are clear. Legal sanctions and enforcement have an impact on accidents. For the greater share of accidents (SAC), jail appears to be an effective control measure. License withdrawal, however, has a more general

impact. Whether the lack of significance for fines is because they have little effect, or whether we were unable to detect an effect because of collinearity, we cannot say. Further, it is likely to be true, as the Swedes have suggested, that there is relatively little variance among sanctions because of their high degree of uniformity in their applications. Thus, it would be premature to reduce emphasis on fines or other sanctions based on our results. Furthermore, fines play an additional, distributive role which this sort of analysis is not intended to evaluate.

The finding regarding the impact of license withdrawal is of special interest because, in Sweden, this action is used only because it is perceived to have incapacitating effects insofar as accidents are concerned. Yet is is also likely to affect subjective probabilities of costs to potential drinking drivers, as we have hypothesized in our model formulation. Our results cannot distinguish between these two effects but the question is surely one worth pursuing. License withdrawal has the additional advantage that it imposes most costs on the driver and relatively little cost on society for its administration. And, unlike fines, this redistributive effect has small impact on other family members. Given the obvious advantages of license withdrawal, the finding of control effectiveness could be the most important contribution of this research.

The most curious finding of our research is that of the apparent difference in response to sanctions of fatal and serious injury accidents. We can think of little reason why fines, presumably a less severe sentence, seem to have a stronger influence on fatal accidents than jail. It appears that drivers who become involved with fatal accidents respond differently to sanction threats than drivers in general. In response to the suggestion that this may be an anomaly of the Swedish data, we note that a similar result was found in the study of road accidents in England.[11]

Our conjecture is that there may be real differences in the characteristics of fatal accident drivers and drivers in general. We note that in Sweden those drivers who most often receive only fines tend to be in the class whose blood alcohol falls between 0.5 and 1.5 pro mille, while jail is the inevitable consequence of a BAC of greater than 1.5 pro mille. It seems unlikely that moderate drinkers are more involved with fatal crashes, thus the explanation must be more complex. In our current research we are attempting to investigate relationships among personal characteristics of drivers, sanctions, and accidents.

FOOTNOTES

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

[2]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.

[3]That paper finds a statistically significant decline in serious injury accidents of 16.1% following the passage of the British Road Safety Act of October 1967. A .908% rise in accidents accompanies a 1% rise in distance driven, a 1.72% rise and .047% rise follow 1% increases in alcohol consumption per capita and precipitation, respectively.

[4]The system described here is essentially that presented in Votey (1982, 1983).

[5]A landmark article that indicates the effects of alcohol on accidents has been Borkenstein (1974).

[6]For environmental variables that have been found to be significant in earlier studies on Sweden and elsewhere, see Votey (1982). An earlier study that indicates the effects of rainfall on accidents for England is Codling (1972). For the English experience in regard to the effects of vehicle mix on accidents, see Johnson (1972).

[7]When the explanatory variables are regressed on the remaining set of explanatory variables the R^2 with arrests the dependent variable is .81.

[8]The estimation technique is described by Almon (1965).

[9]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.

[10]Phillips, _et al_. (1981).

[11]Phillips, _et al_. (1981).

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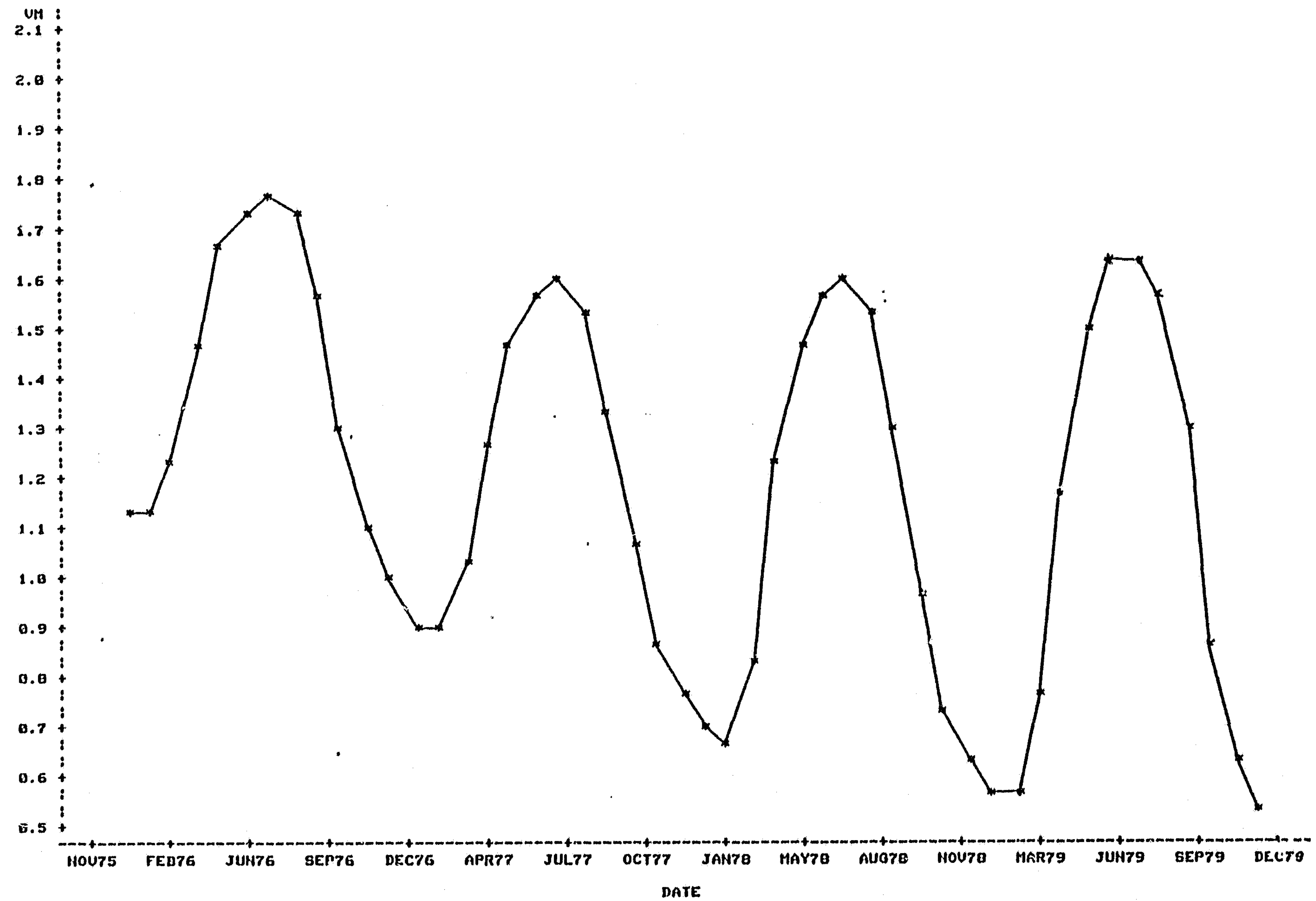
APPENDIX

A1 PLOT OF VEHICLE MIX (MOTORCYCLES/CARS) - SWEDEN

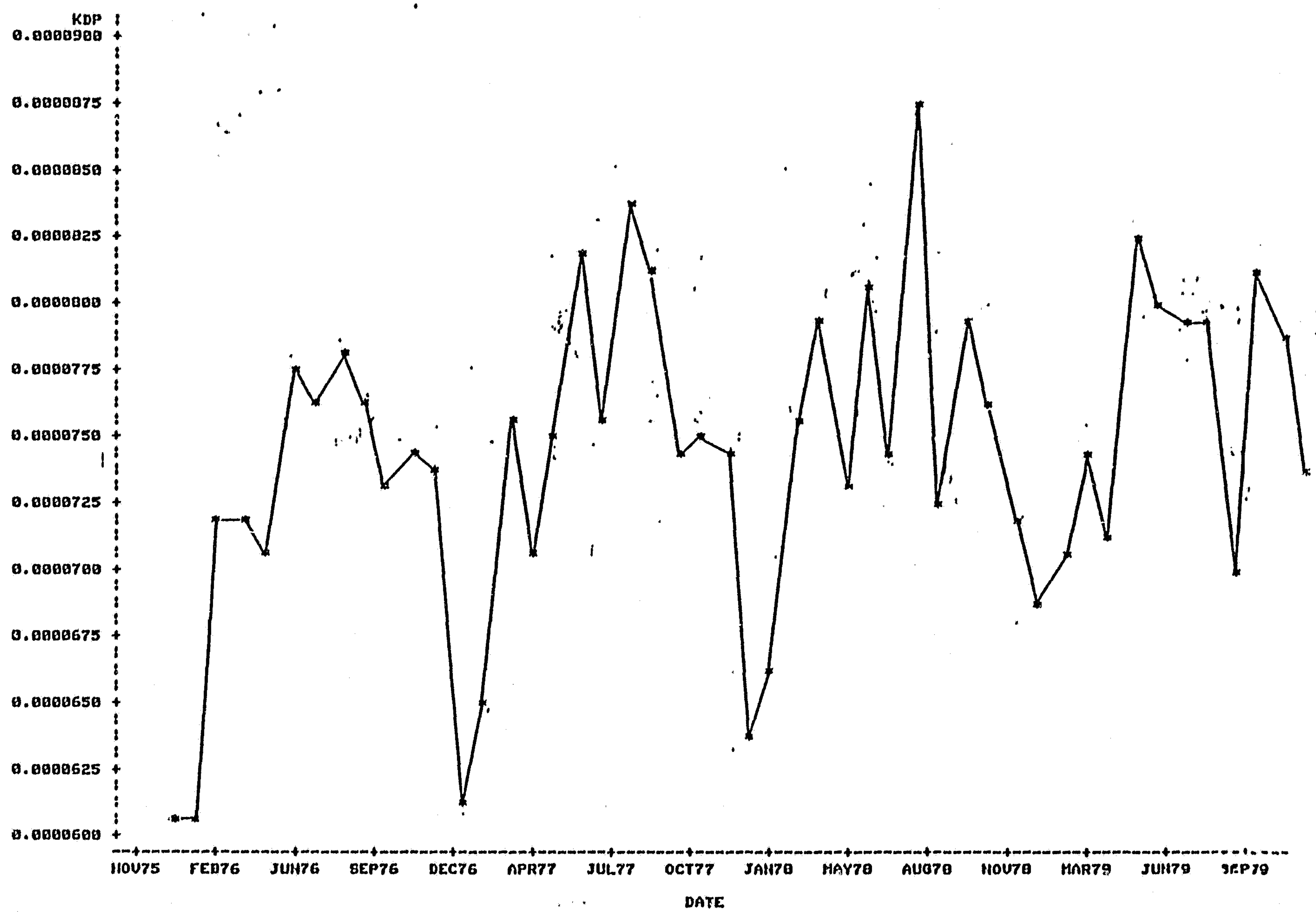
PLOT OF VM*DATE SYMBOL USED IS *



APPENDIX

A2 PLOT OF DISTANCE DRIVEN PER CAPITA-SWEDEN

PLOT OF KDP*DATE SYMBOL USED IS *



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