Criminometrica, 1990/1991, Volume 6/7, pages 19-37

Modeling the Deterrent Effect of Sanctions on Family Violence: Some Variations on the Deterrence Theme*

Leslie A. Hayduk
Department of Sociology
University of Alberta

Edem Frank Avakame
Department of Sociology
University of Alberta

Abstract

The ability of sanctions to block criminal acts has been a mainstay in many criminological debates. We investigate deterrence in the context of interspousal violence by modeling data from the 1987 All Alberta Study. Our model incorporates the perceived probability and severity of a variety of sanctions as determinants of both the respondent's normative acceptance or rejection of wife abuse, and actual violence by the respondent toward their spouse. Our model postulates reciprocal effects between spouse and respondent violence, and it incorporates multiple indicators of violence as well as shared indictors for several other concepts.

The model fits acceptably, with a few measurement modifications, but it presents several surprises in terms of substantive effects. Only two of the sixteen possible effects of sanctions were significant. These effects were small, originated only in the severity of sanctions, and led to normative rejection of wife abuse. Normative rejection of abuse displayed no effect on actual violence, and none of the probability or severity factors displayed any effects on the frequency of respondent violence.

Several variations on the theme of the basic model were examined. The higher frequency of female violence did not seem to be a function of the relatively minor nature of the violence types reported. Models incorporating three violence indicators failed, and conclusively demonstrate the multidimensionality of the several violence reports.

Keywords: Deterrence, Spouse violence, LISREL, reciprocal effects.

Introduction

Since the time of Cesare Beccaria and Jeremy Bentham, the question of rational choice and deterrence has dogged criminologists in their attempts to explain crime and find effective control mechanisms. Beccaria and Bentham believed that human beings are choice making, that they behave rationally with the goal of their rationality being the personal satisfactions attained through the rational pursuit of self-interests, and that humans can be manipulated through threats or appeals (Roshier, 1989). The rational choice model was lost among the deterministic theories of the nineteenth and twentieth centuries, until it was resurrected by Gary Becker, Isaac Ehrlich, and others (c.f. Becker and Landes, 1974). Becker views crime as analytically similar to economic "activity that produces external harm and, when crimes are punishable by fines, the analytical differences virtually vanish" (Becker, 1974:37). Both Becker (1974) and Ehrlich (1974) use US state-level official crime data to confirm that

there is a negative relationship between the probability of apprehension and punishment and the rate of criminal behavior. These results opened the sluice gates for empirical estimation of the effect of deterrence on the probability of criminal activity.

Anderson, Chiricos and Waldo (1977), for example, found that the perceived certainty of punishment deters marijuana users. Tittle (1980:320) agrees with the perceptual nature of deterrence but adds that "any apparent general deterrent effect by the fear of formal sanctions seems a function of the person's perceptions of possible interpersonal respect loss." These views contrast with the field experiment by Sherman and Berk (1984) who found that "swift imposition of a sanction of temporary incarceration may deter male offenders in domestic assault cases," (but see the critique by Straus and Gelles, 1990).

The prospects for reconciling these diverse findings seem to depend on distinguishing the perceived certainty from the perceived severity of potential sanctions. The rational choice model predicts that sanctions (e.g. arrest, physical retaliation, and loss of self-respect) should be more effective at preventing undesired acts if they are perceived as being more likely (probable) consequences of engaging in the undesired act, and that sanctions should be progressively more effective as harsher (more severe) consequences are perceived as accompanying the act.

In the rational choice model, certainty and severity function independently of one another in the sense that increasing the probability of a sanction should progressively deter the undesired act for any given sanction, and increasing the severity of the sanction should progressively deter undesired acts given a constant probability of sanction application. Certainty and severity of sanctions may or may not be linked, however, in that some acts may share high probability and high severity while other acts have low probability of application of minor sanctions, or low probability of strong sanctions, or high probability of weak sanctions. That is, the rationality in the model comes from the actor's variable response to perceived variations in certainty and severity, despite whatever particular levels of certainty and severity of sanctions the actor perceives as applying to a particular sanction in a particular social context. From this, it should be clear that we need an independent assessment of the deterrent effects provided by increasing the perceived certainty of sanctions and the perceived severity of sanctions.

To confound matters, it is possible that the relative effectiveness of certainty and severity may be "sanction specific." Severity might be the primary factor controlling the deterrent value of some types of sanctions while certainty determines the deterrent value of other types of sanctions. It may be the perceived certainty of informal retaliation (e.g. hitting back) that makes this effective, while it is the perceived severity of formal sanctions (e.g. arrest) that provides their deterrent effect.

To sort out whether certainty is generally more or less effective than severity, and whether it is the certainty that controls the effectiveness of some styles of sanctions while severity determines the effectiveness of other sanctions, we developed a model that separately assesses the deterrence capacities of certainty and severity for a variety of styles of sanctions. Since we are considering family violence as the activity to be deterred, the particular sanctions we consider are: whether the abused person would retaliate by hitting back or calling the police, whether the police would arrest the abuser, whether the abuser would lose self-respect (because of self-identification as an aggressor), and whether friends or relatives would disapprove. Our model incorporates peoples' separate assessments of the certainty and severity of each of these styles of sanctions, and models the effectiveness of these

style-certainty and style-severity combinations at deterring family violence (spouse abuse).[1]

Methods and Measurement

The model we develop is based on data from the 1987 All Alberta Study conducted by the Population Research Laboratory of the University of Alberta. The 1987 survey focused on family violence, and reached a total of 1045 respondents using a combination of face to face interviews (in Edmonton) and telephone interviews (for the remainder of the province). The details of the survey are available in Kinzel (1987) and in Population Research Laboratory (1987). Only a brief description of the items and their coding are provided here. A fuller description appears in Appendix A.

The variables we use deal with hitting back (HITBACK), calling the police (POLICE), being arrested (ARRESTED), losing self respect (RESPECT), and friends or relatives disapproving (DISAPPROVE). The respondent's assessment of the *chances* of each of these sanctions happening following an incident in which they were the abuser are given the above labels prefaced by C, for chances, and are taken as indictors of the subjective probabilities of each of these styles of sanctions. These same labels are prefaced with S when they refer to the respondent's assessment of the severity of each of these styles of sanctions. The respondent's reports of the frequency with which they THREW things at, or PUSHED, grabbed, or shoved their spouse are used as the basic indictors of respondent violence, while similar reports of the frequency with which their spouse engaged in such acts constitute the STHREW and SPUSH indicators of spousal violence.

We selected only the respondents who were currently in married or commonlaw relationships, and obtained a weighted pairwise covariance matrix for the preceding items. The maximum pairwise N was 680, and the minimum pairwise N of 588 was used in the LISREL models. The means, variances, correlations and covariances of the observed variables are presented in Table 1.

The means for the probability items indicate that the respondents view loss of self-respect and friends disapproving as much more likely than their spouses hitting back, calling the police, or arrest. The frequency distributions for these variables tend to cluster at 0, 5 and 10 (certain not to happen, 50-50 chance of this happening, and certain to happen, respectively), with the most frequent response being "no chance" (except for the loss of self-respect, where the most frequent response indicated this was "sure to happen").

For the "chances of" items, we have an implicit underlying probability scale to assist us in interpreting the means, but for the "severity" items we have no such benchmark. Hence we place little weight on the fact that the severity scores are generally higher than the probability ("chances of") scores. The respondents judged loss of self-respect as being most severe, followed by arrest, friends' disapproval, and calling the police. "Hitting back" was viewed as least severe but was still, on average, assessed as being near the midpoint of the scale. As with the probability items, the responses tended to cluster at 0, 5 and 10 (not at all, moderate, and extreme) severity. The harm caused to the victim of wife abuse was generally judged to be high and nearly half (48.9%) of the respondents judged it to be extreme.

The means for the frequency of violence items are low, reflecting the fact that the vast majority of respondents (over 90%) reported a frequency of zero for each of these variables. The respondents report that, on average, they threw and pushed slightly more frequently than their spouses did, while their spouses slapped more frequently than the respondents did.

The effects comprising our structural equation model account for the covariances and correlations among the items, so we address these in the context of the basic deterrence model.

TABLE 1. Means, Variances, Covariances for the Observed Variables.[1]

	CHITBACK	CPOLICE	CARRESTED	CSRESPECT	DISAPPROVE	SHITBACK	SPOLICE	SARRESTED	SSRESPECT	SDISAPPROVE	SEX	WIFEABUSE	THREW	PUSH	STHREW	SPUSH	SLAP	SSLAP
CHITBACK	11.219	.30	.25	.14	.19	.14	.07	.07	.09	.11	-.17	.05	.02	.01	.03	.08	-.01	.09
CPOLICE	2.747	7.347	.81	.29	.37	-.04	.16	.12	.15	.19	-.45	-.03	-.05	-.04	-.05	-.06	-.03	-.05
CARRESTED	2.318	5.960	7.441	.27	.37	.02	.21	.16	.16	.17	-.37	.02	-.02	-.01	-.01	-.03	-.04	-.05
CSRESPECT	1.807	2.922	2.789	14.197	.70	.07	.28	.23	.47	.36	-.29	.05	-.05	-.03	-.02	-.07	-.08	-.01
CDISAPPROVE	2.471	3.951	3.938	10.247	15.118	.00	.25	.20	.33	.43	-.42	-.03	-.04	-.03	-.01	-.06	-.06	-.02
SHITBACK	1.670	-0.343	0.162	0.876	0.024	12.794	.36	.34	.26	.25	.30	.13	-.04	-.03	-.06	-.04	-.00	-.06
SPOLICE	0.852	1.491	2.043	3.667	3.395	4.491	12.420	.82	.53	.52	-.17	.15	-.05	-.02	-.01	.02	-.11	.02
SARRESTED	0.870	1.162	1.574	3.116	2.805	4.290	10.233	12.488	.54	.54	-.14	.15	-.06	-.03	-.02	.02	-.10	.02
SSRESPECT	0.908	1.201	1.347	5.313	3.804	2.805	5.569	5.705	9.056	.66	-.10	.18	-.07	-.02	-.07	-.06	-.01	-.02
SDISAPPROVE	1.216	1.688	1.567	4.516	5.510	2.990	5.990	6.233	6.557	10.795	-.18	.09	-.06	-.04	-.05	-.00	-.15	-.01
SEX	-0.285	-0.609	-0.508	-0.554	-0.816	0.531	-0.299	-0.245	-0.145	-0.303	0.250	.12	-.04	.00	-.07	-.03	.06	-.10
WIFEABUSE	0.265	-0.146	0.098	0.276	-0.208	0.777	0.863	0.859	0.856	0.479	0.097	2.830	.03	.05	.02	.03	.03	.06
THREW	0.084	-0.116	-0.052	-0.169	-0.117	-0.131	0.143	-0.169	-0.173	-0.176	-0.016	0.044	0.726	.83	.83	.51	.46	.31
PUSH	0.035	-0.158	-0.038	-0.169	-0.146	-0.166	-0.104	-0.147	-0.078	-0.193	0.001	0.111	0.939	1.780	.81	.58	.50	.34
STHREW	0.079	-0.098	-0.019	-0.057	-0.025	-0.165	-0.023	-0.047	-0.132	-0.127	-0.027	0.031	0.548	0.841	0.607	.60	.35	.50
SPUSH	0.206	-0.139	-0.075	-0.205	-0.181	-0.121	0.061	0.064	-0.032	-0.007	-0.013	0.034	0.351	0.625	0.374	0.645	.11	.28
SLAP	-0.012	-0.027	-0.033	-0.095	-0.069	-0.002	-0.114	-0.105	-0.062	-0.146	0.010	0.017	0.119	0.205	0.084	0.027	0.094	.28
SSLAP	0.143	-0.061	-0.088	-0.025	-0.041	-0.100	0.039	0.044	0.023	-0.014	-0.022	0.045	0.123	0.212	0.181	0.104	0.040	0.215
MEANS	2.400	1.448	1.316	6.568	5.052	4.609	5.427	6.707	7.872	6.419	.490	8.751	.109	.219	.100	.169	.035	.062

1. Correlations appear in the upper-right and covariances in the lower-left of the matrix.

The Basic Deterrence Model

The basic LISREL model is presented in Figure 1, and depicts the *probability* and *severity* of sanction, and sex as background causes of three endogenous variables: rejection of violence, respondent violence, and spouse violence. Some of the most noteworthy aspects of this model include:

a) Effects are allowed from each of the background concepts (probability, severity, and sex) to both rejection of violence and respondent violence. No effects are allowed from the probability and severity items to spousal violence, since the probability and severity items are characteristics of the respondent not their spouse. The normative rejection of violence (as indicated by the respondent's assessment of the harmfulness of wife abuse) is included in an attempt to locate a mechanism for how probability and severity might influence participation in violence. If probability and severity operate by changing one's general view of the harmfulness of wife abuse, the effects of severity and probability will be largely indirect through rejection of violence. If other mechanisms are operative, this will show up as direct effects from probability and severity to respondent violence.

b) The reciprocal effects from respondent violence to spouse violence are constrained to be equal because the random sampling argues against any systematic differences between those who were selected as respondents and those who were designated as spouses.

c) The effect of sex on spouse violence is constrained to be the negative of the effect of sex on respondent violence. If sex influences the frequency of respondent violence, there should be a correspondingly sized but reversed sign effect of sex on spousal violence. If the respondent was male, and hence displays a "male" frequency of violence, the report of spouse's violence must be a frequency of "female" violence.

d) Corresponding multiple indicators are used for both respondent and spouse violence.

e) We postulated that the *chances* and *severity* "of the abused person calling the police" would each be shared between two concepts, the probability of informal and formal sanctions, and the severity of informal and formal sanctions respectively.

f) Each of the concepts in the model is scaled by fixing a 1.0 loading for a particular indicator of that concept (as indicated in Figure 1) and the measurement error variance for that indicator was fixed at a predetermined value. These fixed variances reflect our assessments of the match between our conceptualization of the particular structural concepts and the particular items that provide the 1.0 scalings. We in effect, adjust for a specific degree of measurement error in each of these indicators, and we check on the reasonableness of these assessments wherever we have created multiple or shared indicators, as discussed below. The inevitable difficulties with the precise wordings of the questions prompted us to assess a 10% measurement error for each of the 1.0 scaling items, with the single exception of SEX which was assessed a 1% measurement error (acknowledging a possible data entry error).

g) The two dashed arrows and two dashed error covariances represent data driven modifications to the model. An initial model without these effects failed, with indications that such changes would improve the model. Since we judged each of these to be acceptable as discussed below they were retrospectively included.

h) The final noteworthy feature of this model is that it postulates a "loop within a loop." The reciprocal effects between respondent and spouse violence constitute one loop, which is embedded within a second loop linking normative rejection to respondent violence, to spouse violence, and back to normative rejection.

How well does the overall model work? When this model is estimated using maximum

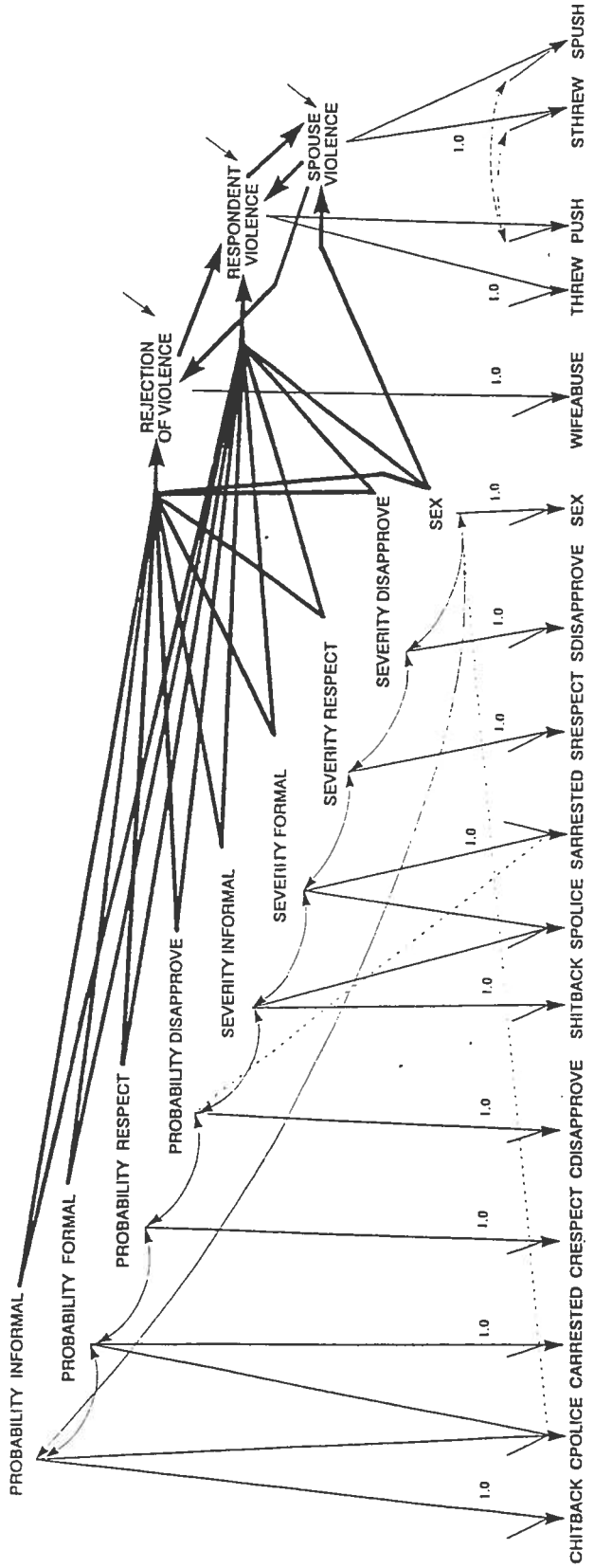

Figure 1. The Basic Deterrence Model

likelihood estimation (Joreskog and Sorbom, 1984), we find a model χ^2=61.88 with 53 d.f. and a probability of .189. The relatively large degrees of freedom indicate the multiple and shared indicators provide a considerable degree of parsimony in accounting for the covariances among the items. The probability indicates that about one in every five or six random samples would provide differences between the observed item covariances and the covariances implied by the estimated model, as large as those we encounter. While it is reassuring that this probability is larger than the customary criterion of ill fit (.05 or less) it is not as high as we might like (in the .5 region). Hence, from this perspective, the model is acceptable but not perfect.[2]

The estimates of the basic model coefficients are provided in Table 2, and we begin our perusal of these by examining the measurement portion of the model, where we find several interesting stories. The loadings of the CPOLICE indicator on the concepts informal and formal sanctions were estimated as .065 and .812 respectively. Both of these values are significant but it is clear that even allowing for the slightly larger variance of informal sanctions, it is the probability of formal sanctions that is accounting for the lion's share of the variance in CPOLICE. That is, the respondents judge the chances that their spouse will call the police as more similar to the chances that they would be arrested (the indicator scaling formal sanctions) than to the chances that their spouse would retaliate informally by hitting back (the indicator scaling informal chances). The conceptual separability of the informal and formal sanctions are further supported by the relatively low correlation between informal and formal sanctions (the 2.317 covariance implies a correlation of only .282). Just over 76% of the variance in the CPOLICE indicator is estimated to arise from non-measurement-error sources (i.e. informal, formal, and sex). The -.719 impact of sex on CPOLICE indicates that females judge there to be less chance that their spouse would respond to a hit by calling the police.

A nearly identical pattern is observed for the connections between the severity of calling the police (SPOLICE) and the severity of informal and formal sanction. The severity of calling the police is judged to be much more similar to the severity associated with being arrested (the variable scaling the severity of formal sanctions concept) than to the severity of informal retaliation by the spouse through hitting back (the variable scaling the severity of informal sanctions). Just over 76% of the variance in SPOLICE is estimated to come from the conceptual variables (severity of informal and formal sanctions).

The effect of the concept "probability of friends disapproving" on the reports of the severity if arrested is -.085 and indicates those who think there is a high probability of friends' disapproval judge arrest to be less severe. When we freed this coefficient, we had anticipated that a high probability of friends' disapproval would produce stronger assessments of the severity of arrest (on the basis of mere correlational thinking). The variances of the cause and effect are reasonably similar (13.6 and 12.5 respectively) so this really is a small effect and not a large effect being obscured by differential measurement scales. This effect appears to arise because the correlation between CDISAPPROVE and SARRESTED (.20) is slightly lower than the correlation between CDISAPPROVE and SPOLICE (.25). In retrospect we realize that these differential correlations could have probably been just as effectively modeled as a positive effect from the concept "probability of friends disapproving" to SPOLICE (with a corresponding minimal reduction in covariance between the two relevant concepts); or as an error covariance between the errors on SDISAPPROVE and either of SPOLICE or SARRESTED. These alternatives introduce a multiplicity of alternative explanations, but we see no particular urge to pursue any of these since

TABLE 2. Maximum Likelihood Estimates for the Deterrence Model.

LAMBDA Y

	PROBABILITY				SEVERITY									R^2	THETA EPSILON
	INFORMAL	FORMAL	RESPECT	DISAPPROVE	INFORMAL	FORMAL	RESPECT	DISAPPROVE	SEX	REJECTION OF VIOLENCE	RESPONDENT VIOLENCE	SPOUSE VIOLENCE	PHANTOM		
CHITBACK	1.000													.900	1.122
CPOLICE	0.065**	.812**							-.719**					.742	1.895
CARRESTED		1.000												.900	.744
CRESPECT			1.000											.900	1.420
CDISAPPROVE				1.000										.900	1.512
SHITBACK					1.000									.900	1.279
SPOLICE					.062**	.872**								.763	2.945
SARRESTED				.085**		1.000								.900	1.249
SRESPECT							1.000							.900	.906
SDISAPPROVE								1.000						.900	1.079
SEX									1.000					.990	.003
WIFEABUSE										1.000				.900	.263
THREW											1.000			.900	.073
PUSH											1.443**			.762	.424'
STHREW												1.000		.900	.061'
SPUSH												.673**		.384	.398'

TABLE 2 (continued)

BETA (non-zero rows only)

	PROBABILITY				SEVERITY										
	INFORMAL	FORMAL	RESPECT	DISAPPROVE	INFORMAL	FORMAL	RESPECT	DISAPPROVE	SEX	REJECTION OF VIOLENCE	RESPONDENT VIOLENCE	SPOUSE VIOLENCE	PHANTOM	R^2	PSI
REJECT VIOLENCE	.029	.023	.024	-.047	.006	.053*	.097*	-.032	.451**					.081	2.177
R-VIOLENCE	.003	-.001	-.010	.008	-.004	-.005	.005	-.007	.063**	.011		.644**		.733	.175
S-VIOLENCE											.644**		.063**	.793	.113
PHANTOM									-1.000						

COVARIANCES AMONG THE EXOGENOUS CONCEPTS

PROBABILITY									
INFORMAL	10.098								
FORMAL	2.317	6.700							
RESPECT	1.812	2.825	12.778						
DISAPPROVE	2.466	3.949	10.251	13.602					
SEVERITY									
INFORMAL	1.660	0.118	0.879	0.024	11.515				
FORMAL	1.013	1.915	4.023	3.940	4.300	11.790			
RESPECT	0.907	1.333	5.313	3.808	2.808	6.066	8.150		
DISAPPROVE	1.219	1.597	4.517	5.509	2.995	6.889	6.557	9.716	
SEX	0.285	-0.508	-0.553	-0.818	0.531	-0.330	-0.145	-0.303	0.248

1 The Theta Epsilon covariances between PUSH and both STHREW and SPUSH are .052** and .104** respectively
* T > 1.6
** T > 2.0

the effect is small and sufficiently removed from the substantive portions of the model to have only a trivial impact.

With the possible exception of this last observation, these estimates for the shared indicators provide no strong evidence to query our predetermined assessments of 10% measurement error variance in the indicators scaling the four concepts that these indicators share.[3]

Moving to the multiple indicators of the respondent and spouse violence concepts, we find strong and significant effects from respondent violence to PUSH and from spouse violence to SPUSH. The larger effect for PUSH than for SPUSH (1.443 versus .673), is partially accounted for by the larger variance in PUSH compared to SPUSH (1.780 versus .645) in contrast to the relatively similar variances for the concepts respondent violence and spouse violence (.655 and .546 respectively), but it also represents there being a larger proportion of explained variance in PUSH as contrasted with SPUSH (76.2% versus 38.4%). This aspect of the model is being driven by the fact that the respondents' reports of their own throwing and pushing activity correlate more highly than do their reports of their spouses' throwing and pushing activity (.83 and .60 respectively). It is therefore not surprising that the violence concepts account for 76% of the variance in PUSH but only about 38% of the variance in SPUSH.

The data prompted insertions of covariances between the errors influencing PUSH and both STHREW and SPUSH imply correlations of .32 and .25 respectively. This implies there must be some source of consistency between the respondents' reports of how often they pushed their spouses and their reports of their spouses' throwing and pushing behavior, over and above the consistency provided by the structural links between the violence concepts. Overall, these pairs of multiple indicators functioned unexpectedly well.[4]

We turn now to the more interesting and substantive upper portion of the model in Figure 1. The picture emerging from the effects of the *probability* and *severity* of the various sorts of sanctions is abundantly clear. None of the probability or severity concepts have effects on either the rejection of violence, or respondent violence, that exceed twice their standard error. Only two of the sixteen effects even exceed 1.6 standard errors and hence reach significance for a one tailed test at the .05 level of significance. These are the effects of the severity of formal sanctions on the rejection of violence (.053, with a T=1.793) and the effects of the severity of loss of self-respect on the rejection of violence (.097, with a T=1.877). These correspond to standardized effects of .119 and .181 respectively, which are too small to provide much solace in comparison to the other 14 essentially null effects of mixed signs originating from the probability and severity conceptual variables. We will return to these disappointing results in our discussion of model Variation-2 below.

The effect of sex on the rejection of violence (.451, standardized .146) is significant at the .05 level, and indicates females reject violence (wife abuse) more strongly than males. The effect of sex on the respondent's own violence (.063) is just barely significant at the .05 level (T=2.002) but it is a bit surprising that it is the females, not males, who are prone to more frequent violence. The violence concepts are most strongly connected to the throwing indicators and this may have slanted the violence concepts toward a stereotypical-female style of violence.

The effect of sex on the frequency of spouse violence was forced to be the reverse of this value by the model's specification—the sex of the spouse is the reverse of the respondent's sex, so the effect of sex on the reported frequency of spousal violence is correspondingly reversed. This -.063 effect shares the T=2.002 and .05 significance with the effect of sex on the respondent's own

reported violence. We examine the implication of adding a more stereotypically-male violence indicator (slapping) when we consider model Variation-1 below.

The effects of respondent violence on spouse violence, and spouse violence on respondent violence, were a bit stronger than anticipated. The unstandardized effect of .644 corresponds to respective standardized effects of .706 and .588.[5] This suggests that there is a considerable tendency for violence to feed on violence. Over half of the violent incidents by one of the partners prompt a violent spouse response, and the cycle repeats two or three times before the looping effect declines to essentially zero.[6]

These reciprocal effects account for the bulk of the explained variance in both respondent violence and spouse violence. Seventy-three percent of the variance in respondents' violence is accounted for, while 79% of the variance in spouses' violence is accounted for. This is in sharp contrast to the mere 8% explained variance in the rejection of violence. The reciprocal effects between spouse and respondent violence are the subject of model Variation-4 below.

The loop we postulated as circulating from rejection of violence, through respondent violence to spouse violence and back to rejection of violence seems to be essentially non-existent. Two of the component paths, the paths from spouse violence to rejection, and from rejection to respondent violence are essentially zero, and hence the loop is convincingly broken. The connection between rejection of violence is further examined in Variation-3 below.

Having scanned the basic model, we summarize it as being acceptable in terms of its overall χ^2 fit, with reasonable measurement structure, but with several unanticipated observations at the conceptual level. Violence (primarily throwing) seems more frequent in females. More importantly, the exogenous causes are largely ineffective. Even the strongest effects in the model (the reciprocal effects between respondent and spouse violence) are intriguing since they seem a bit stronger than we would have anticipated. We pursue these various observations in the model variations that follow.

Variation 1: Three Violence Indicators

We attempted to extend the violence indicators to include more male-like violence — specifically, the reported frequency of slapping by the respondent or spouse. While slapping items also have a slight female slant, we are clearly progressing toward the male stereotype of physical abuse.

The model discussed in this section differs from the model discussed above, only in that it incorporates three rather than two indicators for each of the respondent and spouse violence concepts. SLAP and SSLAP are the new indicators added to the respondent violence and spouse violence concepts respectively.

Unfortunately this model fails. The χ^2 becomes large and significant ($\chi^2 = 261.8$, 84 d.f., p=.000) and the connections between the new indicators and their respective violence concepts are relatively weak. For the respondent and spouse violence, the loadings are .187 and .306 respectively, and these account for only a relatively small proportion of the variance in SLAP and SSLAP (both 24%). The sign of the sex effect does not reverse itself. The current estimate of .070 is close to the .063 in the original model. Hence, even if the model could be made to pass the diagnostics for overall fit, we would still be left with more frequent female violence.[7]

The major implications of this failure is that no single violence dimension underlies the three indicators (THREW, PUSH, SLAP, or STHREW, SPUSH, SSLAP). This conclusion contrasts sharply with the views of TenVergert, Gillespie and Kingma (1988), who used this data set, and concluded that a single dimension is sufficient to

account for the behavior of the full set of violence items. We suspect they would have come to a very different conclusion about the dimensionality of these items had they analyzed even a few correlations between the violence items and substantive non-violence variables.[8]

The failure of the three violence indicators is a failure based on three of the "mild" items, and hence cannot be explained by item difficulty. It remains possible that if we used a pair of the most severe violence items, we might be able to reverse the sex effect, but this demands at least two separate violence dimensions and awaits further analyses.

Variation 2: Violence Effects on Sanctions

Is there any way to salvage the deterrence model, given the 14 null effects and the two merely borderline effects from the severity items to rejection of violence in the basic model? This question gets us into the debates about the stability of perceptions, temporal order, and the need for panel designs (Gottfredson and Hirschi, 1990; Paternoster, Saltzman, Waldo, and Chiricos, 1983; Williams and Hawkins, 1986). Imagine for the moment that people's assessments of the probability and severity of various sanctions are mildly unstable in that they change over time but are not so erratic as to be meaningless. If by this we imagine merely that external sources have been manipulating the respondents' scores on the exogenous concepts, we should be little concerned because the basic model specifically acknowledges the action of such sources by estimating the variances and covariances among the exogenous causes. We could simply assume that it is the current values of these concepts, and not the lagged or historical values, that influence current violence and proceed unperturbed.

If, however, we take this to mean that some of the endogenous variables in our basic model are producing some of the fluctuations in the background causal variables, we have a much more serious problem, for our assumption that the causal flow is generally forward or toward the right of Figure 1 is being questioned. That is, we encounter the potential lability of individual's assessments as a challenge to our assumption of the causal priority of the probability and severity concepts. The usual response to challenges regarding causal sequencing are calls for longitudinal studies (e.g. Williams and Hawkins, 1986), but even this seems unconvincing to some (e.g. Gottfredson and Hirschi, 1990).

Fortunately, there is another tack that can be taken to investigate this possibility, that seems to have escaped criminologists' attention. Hayduk (1987:148,190-212) discusses several of the ways that entering fixed nonzero value into LISREL models can be used to a researcher's advantage. We can gain some leverage on the current problem by imagining that there are real effects from our probability and severity causes to respondent violence and that we had missed these effects because our model does not allow for a two-way causal interplay between our exogenous and endogenous variable. Anyone defending this position must acknowledge that there are some reverse effects from violence to the probability and severity items. *We can investigate this claim directly by entering fixed forward effects from the probability and severity items to the endogenous variables and then estimating the reverse effects that must be present to keep the model's implications consistent with the data.* That is, we can enter a fixed forward effect and estimate a reverse effect conditional on the value of that fixed forward effect.

What kind of forward effect can reasonably be entered? Given the essentially null effects in the current model, it seems reasonable to begin with a small negative value, since opting for larger effects would demand justifications involving excessive hypothecation. We decided to enter forward effects corresponding to a standardized effect of -.100 for each of the probability and

severity items on respondent violence. Since there are eight causal variables we made eight separate LISREL runs, each time fixing the forward effect from one of the causal variables at a value, which when standardized would be -.100, and freeing the corresponding reverse effect.

The results of these runs are rather informative. The standardized estimated reverse effects are from "probability of informal sanctions" to "severity of friends' disapproval" respectively: .177, .084, .032, .046, .101, .078, .048 and .034.[9] Four of these estimates exceeded twice their standard error and all but two are at least 1.5 times their standard errors. This convinces us that if modest negative forward effects are to be found, these must be accompanied by slightly smaller positive reverse effects. (The standardized reverse effects average .075.) This claims that an increase in the frequency of violence produces an increase in the corresponding "forced to be effective" probability or severity cause. This does not seem particularly realistic to us. Nor is it encouraging. If this view is correct, any social program that increases the perceived probability (or severity) of a particular sanction style would be partially self-defeating because a higher probability (or severity) would lead to fewer violent acts, and fewer violent acts would in turn lower the probability (or severity) of perceived sanctions, hence partially undoing precisely what the intervention had accomplished.[10]

The key point, however, is that anyone wishing to persist in claiming that an increase in the perceived probability or severity of sanctions reduces violence, must also accept that more frequent violence increases the actor's assessments of the probability and or severity of sanctions. That is, the more you engage in violence the more likely and the more severe you anticipate sanctions to be. (And, given that perceived sanctions are now effective, this should feedback to reduce violence slightly.) Clearly, one could salvage the "effectiveness of perceived sanctions" if one is willing to also defend the view that increased frequency of engaging in violence leads to one feeling that one can *not* "get away with" the acts that one has engaged in, and/or to the perception that the corresponding sanctions are *more* severe. Equally clearly, this requires moving away from a strictly rational model toward a model that integrates rationality within a context of "perceptual bias" or "learning."

Variation 3: Reciprocal Effects to Normative Rejection

Arguments paralleling those in the preceding section might also be applied to the connection between normative rejection of violence and the respondent's actual violence. Is the essentially null effect here an artifact of our failure to model a reciprocal effect from violence to normative rejection? Here we employed a strategy similar to that in the preceding section, but this time with the effect from respondent violence to normative rejection fixed at values providing standardized effects of -.1, -.2, and -.3. If violence is to influence judgements of the harmfulness of wife abuse, more violence should lead to less assessed harmfulness — assuming cognitive dissonance is to be avoided. The estimated standardized effects of normative rejection on respondent violence for these models are .049, .078 and .106 respectively, with only the last of these exceeding twice its standard error.[11] Hence, one could indeed find a significant effect here if one was willing to demand a rather strong reciprocal effect.

We are unwilling to seize this particular temptation. When we were forced to choose between the "forward" or "reverse" effect during the specification of the original model, we chose the forward effect as likely to be the stronger of the two, and hence we could be charged with being more than a tad inconsistent were now we to gratuitously postulate a triply strong reverse effect to salvage our "desired" forward effect. We will have to live with an essentially zero effect

here, and let others come to their own defense on this point.

Variation 4: Reciprocal Versus Common-Response Violence

The final feature of the basic model that warrants further consideration is the strong reciprocal effects between respondent and spouse violence. The unstandardized effect of .644 indicates that just over half of the violent incidents by either spouse result in repayment in kind by one's partner. This seems a touch strong, but what might be making these reciprocal effects too high?

Given that we have already allowed reciprocal effects, the only other alternative explanations involve omitted common causes. Suppose there was a common cause of both respondent and spouse violence (other than sex which is already modeled), and that by omitting this cause we had inadvertently attributed the covariance arising from this omitted source to the action of the reciprocal causes, and hence we had found overly strong reciprocal effects.

Here again we can play a variant of the modeling strategy used above, by creating a model that incorporates the postulated model components, and then observing how the remaining estimates would have to be modified if this hypothetical model were to remain consistent with the input covariance data. We created a phantom eta variable in LISREL (Hayduk, 1987:Chapter 7) with a fixed 1.0 effect to each of respondent violence and spouse violence. This in itself does not alter the remainder of the model in any way, and it does not even alter the estimates until the variance of that phantom eta is provide some nonzero value. In a series of runs, we provided that eta a variance that was about 10%, 25% and 50% of the value of the covariance in the basic model (.546), hence attributing 10% through 50% of the covariance between the violence concepts to a source other than the operation of the reciprocal causes.

We anticipated that these models would display progressive signs of failure as we increased these values because we would be approaching a model whose implications are at odds with the current model. Suppose all the covariance between the violence concepts was accounted for by the phantom eta (some as yet unidentified cause). In this model the spouse-violence concept could not be influenced by any of the probability or severity causes, and hence the indicators of spouse-violence would have to behave independently of the exogenous sources in the model. This contrasts sharply with the basic model which specifically allows indirect effect of all the probability and severity causes on spouse-violence via respondent-violence, and hence which provides an explanation for non-zero covariances between the indicators of the probability and severity concepts and the indicators of spouse-violence.

This argument is valid, but it certainly gave us pause for reconsideration when the 10%, 25% and 50% models fit about as well as the basic model.[12] Indeed, even a model forcing zero effects between respondent and spouse violence, and estimating the variance of the phantom eta at a value approximating the entire covariance between respondent and spouse violence in the basic model, also worked about as well as the basic model![13]

How is this possible? The mystery is resolved by noting that while the basic model *allows* for indirect effects that could decide between the reciprocal effects from the omitted common cause models, all those indirect effects are essentially zero.[14] None of the probability or severity causes had any significant direct effects on respondent violence, and the indirect effects through normative rejection of violence are eliminated by the essentially null effect from normative rejection to respondent violence. Hence, there actually are no indirect effects to decide between the reciprocal and omitted common cause models despite the depiction of such effects in Figure 1.

This means we must admit that we do not have conclusive proof of the magnitude of the reciprocal effects between respondent and spouse violence. Either a model with the estimated reciprocal effects, or a model attributing some moderate part of the covariance between the violence of respondent and spouse to as yet unspecified causes may still prove to be correct. We must also insert a note of caution with respect to the large explained variances in respondent and spouse violence (73% and 79% respectively). These too must be progressively reduced as an omitted and unspecified cause progressively eats away at the magnitude of the reciprocal effects.

Had we been lucky enough to locate even a single effective predictor of respondent violence, we would have been able to decide between the common cause and reciprocal models, but fate handed us a data set that is precisely as disconcerting in this respect as are the null effects of probability and severity to true believers in deterrence.

Summary and Discussion

We began this paper by investigating a model designed to encapsulate the deterrent effects of the certainty and severity of both formal and informal sanctions on the frequency of interspousal violence. The model was designed to investigate whether sanctions operate directly on violence or whether they operate indirectly though a reduction in the normative acceptance of violence.

The measurement portion of the basic model worked tolerably well (given the numerous multiple and shared indicators), and the model χ^2 was acceptable, but the structural part of the model seemed to confound us at every turn. None of the direct or indirect effects of the perceived probability or severity of any of the styles of sanctions influenced respondent violence.

We were even unable to salvage such effects by attempting to force them into the model. By estimating the effects of violence on perceived sanctions in models with reasonable pre-specified magnitudes of effects of perceived sanctions on violence, we obtained estimates of the kinds of effects violence would be required to display on perceptions of sanctions if one were to believe in "reasonable" forward effects. The estimated reverse effects were positive, and indicated that more frequent violence increases the perceived probability and severity of sanctions. This seems intuitively unreasonable.

Nor can we follow Tittle's (1980) lead and attribute the lack of effectiveness of formal sanctions through the informal sanction of loss of self respect. The free covariances among the probability and severity concepts in our model could possibly be respecified as directed effects converging on the certainty and severity of self-respect loss (as Tittle suggests), but the essentially null effects of both the certainty and severity of self-respect loss on violence eliminates any possibility of these effects being passed along indirectly to the ultimate dependent variable of violence.

We are left with three options: a) admit that there really are no perceived-sanction to violence effects in these data, b) find a way to justify possible positive effects of frequency of violence on probability and certainty of sanctions, or c) find some methodological factor that is producing these observations as artifacts.

The first option questions whether a rationalistic conceptualization applies to spousal violence. Spousal violence may be behavior appearing after the "loading of the last straw" or after the "breaking point" has been passed. That is, this may be behavior that is non-rational because it appears after the breakdown of the supporting rational substructures. Hence, spousal violence may not be coordinated by the self-interested choices which are the foundation of the deterrence conceptualization.

Option three raises three possibilities. As complex as the current model is, it does not control for any demographic or status variables. It is possible that one or more of the excluded variables are suppressing real negative effects from the probability/severity of sanctions to violence by positively influencing both violence and the sanctions, or by negatively influencing both violence and the sanctions. The task would then become to identify and control the interfering variable. Too little is known about the determinants of severity and certainty assessments to speculate about this with any certainty, so this remains an "open" possibility.

A second methodological challenge could be based on the idea that a sanction's severity becomes an issue only after certainty becomes sufficiently high that the sanction becomes salient to the potential offender (c.f. Teevan, 1976:157). This data set is not designed to assess individually varying salience thresholds but one could assume a constant salience threshold across individuals and eliminate all cases below that threshold. That is, the basic model could be re-estimated after pairwise elimination of the certainty and severity ratings for sanctions having "low" certainties (e.g. 1, 2, or 3 on the scale from 1 to 10). This would require multiple recalculations of the input covariance matrix but should be feasible unless the pairwise nature of the matrices posed difficulties.

A third approach could be to argue that some nullifying interaction is obscuring the true effects. Here the primary variable of interest is sex. If males and females are embedded in social contexts that produce differential effects of perceived sanctions on violence (i.e. if the sexes display differential rationalities) the current null effects could arise as the mixing of positive and negative effects. Or, substantial mean sex differences on the implicated variables could be downwardly biasing true negative effects by substantially shifting (vertically or horizontally) essentially parallel "regression" lines for the sexes. The multitude of possibilities here could be addressed by estimating a stacked (multiple-group) LISREL model estimating separate effects for each of the sexes.

Appendix A: Measurement

Items dealing with the probability (chances) of various sanctions

The respondent was read the following statement:

> In family life, sometimes conflict and tension occur. Let us consider the *hypothetical* situation where as a consequence of this conflict you hit your (spouse/partner). I am going to read a list of things which might happen as a result of this action. Please rate the chances of each result, on a "0 to 10" scale. You should give a "0" if you think this has no chance at all of happening, and a "10" if you think this is sure to happen. And you can use any number between "0 and 10". (Population Research Laboratory, 1987: 24)

We developed the following acronyms for the respondent's assessment of the *chances*: that the respondent's spouse "hits you back and hurts you," CHITBACK; that the respondent's spouse "calls the police," CPOLICE; that the respondent would "get arrested for it," CARRESTED; that you, the respondent, would "lose respect for yourself," CRESPECT; and that your "friends or relatives disapprove or lose respect for you", CDISAPPROVE. The probability (chances) of each of these things happening was scored from 0=no chance to 10=sure chance.

Items dealing with the severity of various sanctions

A similar series of items was introduced by:

> Again, suppose you hit your (spouse/partner). How severe would the consequences be for you on a scale of "0" to "10" where "0" is not at all severe and "10" is extremely severe, if..."

We denoted the respondent's assessment of the *severity* of their spouse hitting back and hurting, SHITBACK; of the respondent's spouse calling the police, SPOLICE; of the respondent being arrested, SARRESTED; of the respondent losing self-respect SRESPECT; and of the respondent's friends or relatives disapproving, SDISAPPROVE. Each of these was rated from 0=not at all severe to 10=extremely severe.

Reports of violence by respondent, and respondent's spouse

In another section of the questionnaire the respondent was prompted with:

> No matter how well a couple gets along with one another, there are times when they disagree, and they may use many different ways of trying to settle their disagreements. For instance...a) I would like you to tell me how many times in the last 12 months you 1) threw something at your (spouse/partner)...

We denote this item THREW, and similarly, "pushed, grabbed or shoved your (spouse/partner)" as PUSH, and "slapped your (spouse/partner)" as SLAP.

In a series of parallel questions, the respondent reported on the corresponding frequencies of violent actions engaged in by their spouse during the prior 12 months. We denote these STHREW, SPUSH, SSLAP. The frequencies reported by the respondent constitute the values for the violence items. Four reports of "many times" were initially coded as 97. Three of these four outliers and two of three other reports of extreme numbers, were reduced by recoding these to 1.25 times the highest non-outlying value. The remaining two outliers were on SLAP and were recoded to the third highest value, since this value itself was already a near outlier. Thus the outliers were not removed, but merely reduced to relatively extreme values.

Sex and normative rejection of violence

The respondent's sex was scored 0=male, 1=female. The respondent's assessment of the "severity of the harm caused to the victim" by "wife abuse," was coded from 1 "least severe," to 10 "most severe." We denote this WIFEABUSE and use this as an indicator of the respondent's normative rejection of violence.

Endnotes

* We thank Dr. Robert Silverman for his helpful comments on earlier drafts of this paper.

1. Our model considers "calling the police" as bearing some similarity to both formal and informal sanction styles. The loadings leading to this shared indicator imply that this particular sanction style most closely approximates the informal response of "hitting back," though there is also a significant connection between "calling the police" and the formal action of "being arrested." The sharing of the "calling the police" indicator between formal and informal modes of sanctioning reduces the number of separate estimates of effectiveness in the model, but strengthens the model by providing an ability to assess the measurement properties of the indicators.
2. Readers unfamiliar with LISREL and structural equation modeling more generally, might see Hayduk (1987).
3. One of the largest modification indices for this model corresponds to the effect of sex on CDISAPPROVE (MI=5.1). If this effect had been estimated it probably would have been significant, but we resisted doing this because this would have been yet another data driven change and it would have provided only a marginal improvement in model fit.
4. Though all nine of the standardized residuals that are greater than 1.0 involve THREW, PUSH, STHREW or SPUSH, there seems to be no obvious pattern to these residuals. All the standardized residuals are less than 1.7.
5. The standardized effects differ because the variances of the violence concepts differ slightly. The variance of respondent violence is .655 while the variance of spouse violence is .546.
6. See Hayduk (1987, chapter 8) for a discussion of loops and their interpretation.
7. The relative weakness of the SLAP and SSLAP indicators implies that even in this model the violence concepts are loosely connected to the slapping indicators, so we may still be left with a female-stereotypical slant to the violence concepts. We tried to rectify this by creating two other triple-indicator models.

   One model scaled the violence concepts with 1.0's to the SLAP and SSLAP indicators (and a fixed 10% error variance for each, and free error variance for the THREW and STHREW indicators). This model had a

huge χ^2 (1097.96, d.f. 84, p=.000), with most of the ill fit covariances being associated with the triple indicators. The sex effect was still positive (.045) and a smaller portion of the variance in THREW, PUSH, STHREW, and SPUSH originate in the violence concepts (25%, 39%, 25%, 9% respectively).

The second model used 1.0 values to scale the violence concepts in terms of SLAP and SSLAP but freed the error variances for SLAP and SSLAP and the four other indicators of violence. This scaled the violence concepts in terms of slapping frequency, but leaves the relative importance of these indicators for defining the violence concepts, free to be estimated by the data. This model also had a large χ^2 (225.02, p=.000) and it reverted to explaining only 24.9% and 24.3% of the variance in these indicators — so the SLAP and SSLAP indicators are again weakly connected to the violence concepts, as in the model being discussed in the text. This model also showed signs of unacceptable fit (R^2 greater than 1.0 for the STHREW indicator and for the respondent violence concept). This model also displayed numerous ill-fit covariances for the SLAP and SSLAP indicators.

8. TenVergert, Gillespie and Kingma (1988) used this same basic data set, but selected their cases differently, and forced a 0-1 coding for each of the violence variables (thereby obfuscating the frequency nature of these data). They also used more violence items than the three items we used, including: "kicked bit or hit with a fist," "hit or tried to hit with something," "beat up," "choked," "threatened with a knife or gun," "used a knife or fired a gun."

   They postulate that scales created by summing the "mild" and summing the "severe" violence items would "correlate differently with variables believed to be related to interspousal physical aggression" (1988:13) but they never explicate the precise nature of how item difficulty would produce artifactual differences in anticipated correlations involving the frequency variables. Their prediction is somewhat irrelevant to our current findings because we do no "summing" of items. We focus on the mildest items and treat each item on its own, and we still find that the items do not behave as if they have a common source.
9. The χ^2 for these models ranged from 68.26 to 61.03 and hence differed minimally from the basic model's χ^2 of 61.88. Though there seemed to be no difficulty in obtaining estimates for the coefficients in these models, LISREL's subsequent calculation of the variance for whatever single variable (eta) was being focused on, consistently provided total variances that were slightly less than the error variance for that variable. This resulted in small negative R-squares for whichever concept was being addressed. Overall, these problems seemed minor in comparison to the magnitude of the forced -.100 standardized effect that formed the basis of these models. Seven of the eight models produced exactly a -.100 forward standardized effect, and the eighth model had a standardized effect of -.099.
10. For further discussion of negative feedback loops, see Hayduk (1987:253).
11. These models converged reasonably and the χ^2 for the models were not markedly different from that of the basic model (62.27, 62.67, 63.07). These models displayed the same slight but inexplicable underestimate of the variance for normative violence with a corresponding minor reduction in R^2 (c.f. footnote 9). At least this particular LISREL bug seems to be consistent, even if it isn't large enough to swat!
12. The model χ^2's are 61.89, 61.56 and 60.88 respectively, compared to 61.88 for the base model. The explained variance for respondent violence tended to increase (.750, .771, .798) and the squared multiple correlation for the spouse violence increased even faster (.835, .883, .938). How high an explained variance would anyone accept for spouse violence before claiming that it is too high? If it reaches 1.0 there is no room left for any unique causes of the spouse's actions (each cause explains some variance, so if there is no variance left to explain there are no more causes) and hence the spouse could not respond to any aspect of the social world not also influencing the respondent. Clearly we must call a halt to this some considerable distance away from 1.0.
13. The estimated phantom eta's variance was .547 (compared to a .546 covariance between the violence concepts in the basic model). The model χ^2 was 59.91 with p=.239, but the squared multiple correlation for spouse violence was 1.002, indicating some modeling problem(s).
14. The effect of sex is irrelevant to this discussion as it does not uniquely influence only one or the other of the reciprocally related variables.

References

Anderson, Linda S., Theodore G. Chiricos, and Gordon P. Waldo. 1977. "Formal and informal sanctions: A comparison of deterrent effects." *Social Problems* 25: 91-103.

Becker, Gary S. 1974. "Crime and punishment: An economic approach." Pp. 1-54 in Gary S. Becker and William M. Landes (eds.) *Essays in the Economics of Crime and Punishment*. New York: Columbia University Press.

Becker, Gary S. and William M. Landes (eds.). 1974. *Essays in the Economics of Crime and Punishment.* New York: Columbia University Press.

Ehrlich, Isaac. 1974. "Participation in illegitimate activities: An economic analysis." Pp. 68-134 in G. Becker and W. Landes (eds.) *Essays in the Economics of Crime and Punishment.* New York: Columbia University Press.

Gottfredson, Michael R. and Travis Hirschi. 1990. *A General Theory of Crime.* Stanford. California: Stanford University Press.

Hayduk, Leslie A. 1987. *Structural Equation Modeling with LISREL: Essentials and Advances.* Baltimore: Johns Hopkins University Press.

Joreskog, Karl and Dag Sorbom. 1984. *LISREL VI: Analysis of Linear Structural Relationships by the Method of Maximum Likelihood.* Mooresville, Indiana: Scientific Software Inc.

Kinzel, Clifford. 1987. *1987 All Alberta Study (Including the Edmonton Area Study) Sampling Report.* University of Alberta: Population Research Laboratory.

Paternoster, Raymond. 1989. "Decisions to participate in and desist from four types of common delinquency: Deterrence and the rational choice perspective." *Law and Society Review 23*: 7-40.

Paternoster, Raymond. Linda E. Saltzman, Gordon P. Waldo, and Theodore G. Chiricos. 1983. "Estimating perceptual stability and deterrent effects: The role of perceived legal punishment in the inhibition of criminal involvement." *The Journal of Criminal Law and Criminology 74*: 270-297.

Population Research Laboratory. 1987. *1987 All Alberta Study: Codebook, Frequency and Questionnaire.* University of Alberta: Population Research Laboratory.

Roshier, Bob. 1989. *Controlling Crime: The Classical Perspective in Criminology.* Philadelphia: Open University Press.

Sherman, Lawrence W. and Richard A. Berk. 1984. "The specific deterrent effects of arrest for domestic assault." *American Sociological Review 49*: 261-272.

Straus, Murray A. and Richard J. Gelles. 1990. *Physical Violence in American Families: Risk Factors and Adaptations to Violence in 8,145 Families.* New Brunswick. NJ: Transaction Publishers.

Teevan, James J., Jr. 1976. "Subjective Perception of Deterrence (Continued)." *Journal of Research in Crime and Delinquency 13*: 155-164.

Ten Vergert, Elisabeth, Michael W. Gillespie, and Johannes Kingma. 1988. "The measurement of interspousal physical aggression: using the conflict tactics scale in survey research" Edmonton Area Series Report 62. University of Alberta: Population Research Laboratory.

Tittle, Charles R. 1980. *Sanctions and Social Deviance: The Question of Deterrence.* New York: Praeger.

Williams, Kirk R. and Richard Hawkins. 1986. "Perceptual research on general deterrence: A critical review." *Law and Society Review 20*: 545-572.

Received March 1992; Revised August 1992