

CRIME, DETERRENCE, AND RATIONAL CHOICE*

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This study examines the deterrent effect of formal sanctions on criminal behavior. While most research on deterrence assumes a rational-choice model of criminal decision-making, few studies consider all of the major elements of the model. In particular, three critical limitations characterize the empirical literature on deterrence: the failure to establish a causal ordering of sanctions and crime consistent with their temporal ordering; the focus on conventional populations and nonserious criminal acts, which are of less interest to the question of how society controls its members; and the inattention to the return or reward component of the decision-making process. To address these issues, we specify, estimate, and test a rational-choice model of crime on data that were collected on individuals, gathered within a longitudinal design, and derived from three distinct populations of persons at high risk of formal sanction. The results support the reward component of the rational-choice model, but fail to support the cost or deterrent component, as measured by perceived risks of formal sanctions.

Traditionally, sociologists have identified two mechanisms by which society elicits conformity in its members: internal control, whereby individuals are inculcated with conventional norms, values, and attitudes; and external control, whereby individuals are coerced, threatened, and sanctioned into conformity. Historically, most research has focused on internal control, leading investigators to examine normative structures, learning processes, subcultural influences, and the like. Recently, however, many social scientists have turned to issues of external control, exploring the process by which illegal behavior is deterred as a significant source of social control. Initially motivated by Wrong's (1961) classic critique of normative sources of control, this work was further stimulated by several theoretical discussions of deterrence (Andenaes, 1974; Zimring and Hawkins, 1973; Gibbs, 1968; 1975), Becker's (1968) seminal paper outlining an economic model of crime

and punishment, and finally a subsequent spate of empirical studies.

Unfortunately, despite numerous calls for a general theory of deterrence, nearly all of the empirical research on the issue takes as its framework "a vague congerie of ideas with no unifying factor other than their being legacies of two major figures in moral philosophy, Cesare Beccaria and Jeremy Bentham" (Gibbs, 1975:5). This is partly due to the practical concerns of criminologists: since much of the American criminal justice system is based directly or indirectly on ideas of Beccaria and Bentham, the testing of those ideas has immediate implications for public policy. As a consequence, deterrence research has been predominantly concerned with the isolated effects of the severity and certainty of sanctions on illegal behavior. A more fruitful approach to the issue of deterrence would examine the relationship between formal sanctions and crime from within an explicit theoretical framework.

This study examines the deterrence hypothesis from within the rational-choice model, a theoretical perspective proposed by economists that not only provides a general explanation of criminal behavior, but also stipulates a specific mechanism by which formal sanctions deter. Consider the following formal statement of an actor's expected utility under conditions of risk:

$$E(U) = (1 - p) U(y) + p U(y - F)$$

where $E(U)$ = the actor's expected utility from a contemplated activity,

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- p = the likelihood of being punished for the activity,
 y = the anticipated returns (material or psychic) from the activity,
 F = the anticipated penalty resulting if the actor is punished for the activity.

According to the statement, if for a given person, the expected utility of an illegal (legal) act is greater than the expected utility of other alternatives, the person will engage in the illegal (legal) act. This behavioral model, which is detailed by Friedman and Savage (1948), Becker (1968), and Block and Heineke (1975) identifies three requisites of a model explaining the decision to engage in crime: it must include the expected rewards from alternative courses of legal or illegal action; it must consider the expected costs of these actions; and it must consider those expectations as subjectively perceived by the actor, not as objectively inhering in the actions.

Empirical research on the importance of deterrence in eliciting conformity has employed one of two strategies. The first, favored by economists, entails macro-level analyses of the relationship between aggregate crime rates and aggregate rates of criminal justice actions such as arrest, conviction, and imprisonment. The second, favored by sociologists and social psychologists, entails micro-level analyses of the relationship between the criminal acts of individuals and their perceptions of the risks of those acts. Such studies, whether micro-level or macro-level, have been hampered by at least four conceptual and methodological shortcomings. First, macro-level analyses have ignored the central role of perceptions in rational-choice models. Second, micro-level analyses have analyzed only restricted populations of conventional persons and nonserious crimes, ignoring more threatening acts that are central to the question of how society controls the behavior of its members. Third, most research has relied on cross-sectional research designs, making causal inferences questionable. Fourth, the statistical models of most studies have omitted important variables—not only control variables, but also variables representing the reward or return component of rational-choice models.

The research reported here seeks to overcome these problems. Based on a two-wave panel study of three independent samples, it examines how persons' perceptions of the costs and rewards of legal and illegal behavior are related to subsequent criminal activity. It considers the impact of perceived returns from crime, as well as perceived opportunities to commit crime—two crucial elements of the rational-choice model of crime ignored in pre-

vious research. The longitudinal design allows us to specify a causal ordering among our variables that coincides with the temporal ordering of their measurement. Our analytical strategy, in addition, allows us to estimate and statistically control for measurement error in our indicators of perceived threat of formal sanctions. Furthermore, the three populations that we sample consist of persons having a high probability of engaging in serious, patterned forms of crime, precisely those persons that previous research concludes will be deterred by threats of sanctions. Finally, the breadth of the dataset allows us to include a variety of exogenous background characteristics in our causal models, thereby reducing the potential bias from specification errors.

The remainder of this article is divided into five sections. In the first, we critically review previous individual-level research on the deterrence hypothesis, highlighting various methodological problems.¹ In the second section we describe our research design, sample, and pertinent variables, and present our structural equation models. Section three presents our analyses of measurement models of perceived risk, while section four specifies, estimates, and tests our model of rational choice and crime. The final section concludes with a discussion of the theoretical implications of our results.

PREVIOUS INDIVIDUAL-LEVEL STUDIES OF DETERRENCE

During the past 15 years, a flood of empirical studies has examined the effects of persons' perceptions of the certainty and severity of formal sanctions on their criminal behavior. From this vast and diverse literature, we can draw three conclusions pertinent to the present research. First, prior research has failed to unearth a consistent deterrent influence of perceived severity of formal sanctions (Waldo and Chiricos, 1972; Silberman, 1976; Bailey and Lott, 1976; Meier and Johnson, 1977). Second, while most studies find a consistent but modest effect of perceived certainty of formal sanctions (Jensen, 1969; Waldo and Chiricos, 1972; Grasmick and Milligan, 1976; Kraut, 1976; Silberman, 1976; Erickson et al., 1977; Jensen et

¹ A critical review of macro-level studies of deterrence is beyond the scope of this article; moreover, our research is oriented to individual-level processes of rational choice and deterrence. We should mention in passing, however, that such macro-level research has recently received stinging criticisms, to the extent that some conclude the approach is virtually bankrupt for assessing deterrence hypotheses (Brier and Fienberg, 1980; Manski, 1978).

al., 1978), others find that this effect is conditional, holding only for persons who are uncommitted to conventional morality (Silberman, 1976) or highly motivated to deviate (Tittle, 1977, 1980). Third, these results may be questionable because of three methodological shortcomings of the studies from which they were generated. We take up these shortcomings in turn.

Inferring Causality From Cross-Sectional Designs

Students of deterrence have long recognized the problem of inferring causality from cross-sectional research designs (Burkett and Jensen, 1975; Logan, 1975; Jensen et al., 1978). This is particularly problematic for individual-level studies of deterrence for two reasons. First, the causal ordering specified among independent and dependent variables contradicts their temporal ordering in the sense that unlawful acts committed prior to an interview are specified as a function of attitudes measured during the interview. This design cannot rule out the possibility that any observed negative relationship is due to the impact of crime on perceived risks. Second, because data on independent and dependent variables are obtained from respondents in the course of one interview, contamination effects also cannot be ruled out. For example, individuals' reports on one set of variables may influence reports on another.

Some researchers have justified their cross-sectional designs by assuming that perceptions of risk remain stable over time, which would make the timing of their measurement inconsequential (Silberman, 1976; Anderson et al., 1977). Others have tried to resolve the problem of changing the time frame to which their measures refer. For example, several have used as their dependent variable, respondents' estimates of their future illegal behavior (Tittle, 1977, 1980; Grasmick and Green, 1980; and Jensen and Stitt, 1982). Teevan (1976), on the other hand, used a different independent variable, asking respondents to recall their perceptions of risk prior to engaging in the deviant acts they report. The approach of the first group must be rejected since they assume away what is in fact an empirical problem. The proposed solutions of the other groups are problematic because at minimum they fail to deal with contamination effects.

A more effective way of attacking this issue—but by no means a panacea for the problem (cf. Kessler and Greenberg, 1981)—draws on a longitudinal research design. Indeed, investigators have recently capitalized on such designs with good success (Paternoster

et al., 1982, 1983; Saltzman et al., 1982; Minor and Harry, 1982). Finding that (1) persons who reported committing crimes between waves had lower subsequent perceptions of risk than those who did not report committing crimes; (2) persons' earlier perceptions of risk were unrelated to these reports of crimes; and (3) persons' earlier and later perceptions of risk were not stable, these researchers concluded that perceived risk is a consequence of crime, not a cause.² Such results underscore the fecundity of longitudinal data for examining issues of deterrence.

Specification Error in the Rational-Choice Model

Many social scientists have correctly noted that to make causal inferences from nonexperimental data, one must have a correctly-specified statistical model. This implies, in particular, that all important nonorthogonal explanatory variables are included in one's model; otherwise, estimates of important parameters may be biased and inconsistent. Viewed in this light, individual-based research on deterrence appears wanting. Most analyses, in fact, are based in large part on bivariate relationships (Waldo and Chiricos, 1972; Teevan, 1976; Kraut, 1976; Erickson et al., 1977; Saltzman et al., 1982; Paternoster et al., 1982; Minor and Harry, 1982). Other analyses include in their models elements of normative controls or informal sanctions, such as deviant associates, moral attachment, criminal motives, and the like (Silberman, 1976; Grasmick and Green, 1980; Meier and Johnson, 1977; Tittle, 1977, 1980; Paternoster et al., 1983). With the possible exception of Tittle (1977, 1980), however, none of these studies include in their models the reward, returns, and opportunity component so crucial to rational-choice models (Heineke, 1978). As we noted earlier, this is the other side of the two-sided rational-choice model, the first being the risks and costs of crime. Because perceptions of risk may be correlated with perceptions of the reward, returns, and opportunity for crime, omitting the reward side may have led to

² These researchers argue that the within-wave negative effect of perceived risk on reported crime reflects what they term "experiential effects". That is, by virtue of their accumulated experience of violating the law and avoiding detection, persons who have engaged in more previous crimes will tend to lower their perceptions of risk in the future. This effect, however, could be confounded with a response effect: in the process of admitting their criminal acts, persons may come to perceive—if only momentarily—and report low risks for these acts.

biased estimates of deterrent effects and consequently to misleading conclusions.³

Sampling Criminal Acts and Actors

Students of deterrence are often less interested in the social psychological process by which any sanctions are related to any form of behavior, and more interested in the implications of their research for the general problem of social order—that is, how society controls the behavior of its members (Silberman, 1976; Meier and Johnson, 1977; Tittle, 1977, 1980). However, most individual-level studies of deterrence have either sampled geographically-defined general populations (Meier and Johnson, 1977; Grasmick and Green, 1980; Tittle, 1977, 1980) or sampled students in high schools or colleges (Chiricos and Waldo, 1970; Kraut, 1976; Teevan, 1976; Bailey and Lott, 1976; Silberman, 1976; Erickson et al., 1977; Jensen et al., 1978; Minor and Harry, 1982; Paternoster et al., 1983). Consequently, because serious crimes are a rare event in such populations, researchers have used as dependent variables nonserious forms of deviance, such as petty theft, drunkenness, and marijuana use. These behaviors pose a threat to the values of some groups, but not others; therefore, the results of these studies may be more relevant to the problem of informal controls by specific groups. Of more importance to the larger issues of social control by society in general is the question, "Why do some people refrain from armed robberies, assaults, and burglaries—behaviors that threaten all groups in society—while others do not?" Stated another way, deterrence may be more relevant to serious forms of *mala en se* offenses (crimes proscribed by both law and public mores) and less relevant to trivial forms of *mala prohibita* offenses (crimes proscribed by law but not by public mores) (Gibbs, 1968, 1975; Silberman, 1976).

Potentially serious offenders are of particular interest to the study of deterrence for another substantive reason. Two previous studies

of relatively conventional populations find an interaction effect, concluding that persons who are morally uncommitted—that is, potentially serious offenders—are more likely to be deterred by formal sanctions (Silberman, 1976; Tittle, 1977, 1980). The null findings of much of the deterrence literature, then, could be due to the focus on essentially morally-committed persons. Finally, the focus on serious offenders and offenses has obvious implications for public policy within our criminal justice system; these are the crimes and criminals our public fears most.

SUPPORTED WORK, SAMPLES, AND MEASURES

The data for the present study pertain to both captured serious criminal acts and actors. Collected between 1975 and 1979 in the course of evaluating the National Supported Work Demonstration—a job-creation program for persons with severe employment problems—the data were obtained from three distinct samples: adult offenders who previously had been incarcerated; adults who were known drug users; and adolescents age 17 to 20 who had dropped out of school. Supported Work was evaluated using data from nine different communities throughout the United States.⁴ In general, to be eligible for Supported Work, persons had to show they were recently and chronically unemployed. Beyond that, Supported Work required that, in the previous six months, participants in the offender sample had spent time in jail or prison, those in the addict sample had been enrolled in a drug treatment program, and members of the youth sample had been out of school. In addition, at least half of the youth sample had to have an arrest record.

These criteria notwithstanding, there was no assurance of systematic recruitment into the program across locales. Rather, in large part, enrollment reflected the diverse and unknown referral practices of workers in local social service agencies. Consequently, we do not know the precise relationship of our three samples to larger populations of substantive interest, such as all ex-offenders, addicts, or dropouts in the United States. Therefore, while our samples appear to capture serious offenders, and thus have a decided advantage over those of previ-

³ Of course, our research is vulnerable to the same criticism from the standpoint of these earlier multivariate studies: perhaps our estimates of deterrent effects are biased because we fail to consider normative controls, moral attachments, deviant associates and so on. The consideration of such additional variables would take us beyond the scope of the present study. We are attempting to consider deterrence from within an explicit and self-contained model that logically specifies a mechanism by which sanctions should deter. We are not attempting to consider an ad hoc model that includes mechanisms largely inconsistent with the underlying assumptions of deterrence theory.

⁴ The offenders participating in the evaluation were recruited in Chicago, Hartford, Jersey City, Newark, Oakland, San Francisco, and Philadelphia. Addicts were drawn from Chicago, Jersey City, Oakland, and Philadelphia. Finally, adolescent dropouts were taken from Atlanta, Hartford, Jersey City, New York, and Philadelphia.

ous research, they have the drawback of being nonprobability samples, which limits generalizations based on statistical inference.⁵

A total of 5,005 participants in the evaluation were randomly assigned to experimental or control conditions, with experimentals provided jobs lasting up to 18 months. Each enrollee was scheduled to receive at least three interviews during the evaluation. The first elicited primarily demographic and background information; the rest, conducted at nine-month intervals after enrollment, procured information about respondents' experiences, circumstances, and contacts with the criminal justice system. The 3,300 offenders, addicts, and youths who completed the first three interviews constitute the samples for the present investigation.⁶

Descriptive statistics for these three samples appear in Table 1. Most members of the three samples have little education, meager employment histories, and extensive contacts with criminal justice agencies. Drug use is prevalent among all groups, not just among addicts. Although direct comparative data are unavailable, these characteristics paint a

plausible picture of serious criminal offenders, drug addicts, and adolescent school dropouts.⁷

During the first-wave interviews, measures were obtained of key theoretical variables, including respondents' perceptions of their opportunities for, returns from, and evaluations of both legal and illegal activities. The specific questions dealt with five concepts: (1) respondents' estimates of the lowest pay they would accept from a "straight" (legal) job (MINIMUM PAY); (2) their belief that they could make more money "on the street" (illegally) than from a straight job (RELATIVE EARNING); (3) their belief that they had frequent opportunities to engage in crime (CRIME OPPORTUNITY); (4) their relative respect for a range of conventional versus illegal jobs (JOB RESPECT); (5) their estimates of the probability they would be sanctioned negatively after engaging in a \$1000 crime. At the second-wave interviews, we also measured whether respondents had become involved in criminal activities, using two dichotomous indicators: self-reports of crime and self-reports of arrest. The self-report measure of crime was constructed from checklists of a variety of serious offenses.⁸ (Descriptions of all observable variables appear in the Appendix.)

Descriptive statistics for these substantive variables parallel the portrayal of sample

⁵ This issue of external validity is substantive: our results cannot be generalized if the cognitive processes of serious offenders in the Supported Work samples differ from their counterparts in the general population. We find this very unlikely. Participants in the Supported Work Program were selected through the sometimes haphazard and idiosyncratic procedures of numerous and widely-dispersed agents within a variety of organizations. That this disparate group of agents selected persons with uniform cognitive styles that differ substantially from other offenders, addicts, and dropouts seems improbable. We should also note that problems of sample selection and external validity are more severe in most previous individual-level research. Such studies typically drew samples from a single school or community.

⁶ In a series of separate analyses conducted for the program evaluation, Brown (1979) attempted to determine whether sample attrition was systematic, and if so, whether it biased estimates of the effects of certain background variables on criminal behavior. In the first analysis, he found that only race influenced attrition: blacks in all three samples were less likely than whites and Hispanics to drop out of the program. In the second analysis, using Heckman's (1976) procedure for estimating and correcting for sample selection bias, he found no biasing effects on selected program outcomes, including self-reported arrest. Furthermore, we initially attempted to use data from all three waves, but could not because of severe and systematic missing data. Most significantly, persons who were incarcerated at the time of their follow-up interviews were not asked several key questions about their perceived risks of sanctions. Therefore, we were forced to restrict the analyses to the first two waves.

⁷ A comparison of members of the Supported Work offender sample and prisoners recently released from federal correctional institutions indicates that the Supported Work offenders are somewhat younger and more extensively involved with criminal justice agencies. Both groups are predominantly black, unmarried, and in the past heavily involved in drug use. See Administrative Office of the United States Courts (1974).

⁸ The specific illegal acts included in our measure of crime ranged from continuous ongoing activities—such as drug dealing, numbers running, and gambling—to discreet activities—such as car theft, mugging, and assault. Rather than using respondents' estimates of the frequency of their crimes, we used, for two reasons, their estimates of whether or not they had engaged in the crimes. First, we are more interested in absolute deterrence—the likelihood that one will not return to a life of crime—and less concerned with restrictive deterrence—the likelihood that one will reduce his or her rate of crime. Second, we have little confidence in respondents' estimates of the frequency of their acts; we found many inadmissible responses to such estimates. Furthermore, conceptually, it may make little sense to speak of the frequency of engaging in continuous illegal activities. For example, exactly how many times in a week has a numbers runner engaged in the act of running numbers? Precisely when the activity begins and ends is unclear. On the other hand, we do feel confident in respondents' reports of whether they have ever engaged in such acts over a nine-month period.

Table 1. Descriptive Statistics on Sample Members' Characteristics at Enrollment in Supported Work

	Offenders		Addicts		Youth	
	Mean	(S.D.)	Mean	(S.D.)	Mean	(S.D.)
Average age	25.3	(6.1)	27.8	(6.7)	18.3	(1.1)
Percent male	94		80		86	
Race and ethnicity						
Percent black	84		78		78	
Percent Hispanic	9		9		16	
Percent white or other	7		14		6	
Education						
Average years of schooling	10.4	(1.8)	10.6	(1.8)	9.7	(1.1)
% with 9 years or less	25		24		39	
Household/family composition						
Percent living with spouse/girlfriend	16		28		6	
Percent with child under 18	12		29		7	
Weeks worked in last year						
Percent with no work	64		51		43	
Percent working 1-9 weeks	16		16		22	
Percent working 10 or more weeks	20		33		35	
Average weeks worked, all respondents	6	(2.6)	10	(3.6)	9	(3.9)
Length of longest job, last 2 years						
Percent with no regular job	49		37		30	
Percent in job 1-6 months	39		40		58	
Percent in job 7 or more months	12		23		12	
Average monthly earnings during "free time" last year	\$94	(\$22)	\$111	(\$19)	\$73	(\$11)
Drug use						
Percent ever used marijuana	81		91		60	
Percent ever used an opiate	53		95		12	
Percent ever used heroin	44		94		8	
Percent with "straight" best friend	77		74		82	
Illegal money-making activity						
Percent ever making money illegally	79		84		41	
Percent making money illegally in last year	41		54		34	
Arrest experience						
Percent with at least one arrest	100		90		54	
Average number, all respondents	9.2	(1.3)	8.3	(1.1)	2.2	(.5)
Conviction experience						
Percent with at least one conviction	95		75		34	
Average number, all respondents	3	(.4)	3	(.4)	.6	(.1)
Incarceration experience						
Percent with any time in jail/prison	96		70		28	
Average weeks incarcerated, ever	195		129		20	
Average weeks incarcerated, last year	31	(1.7)	6	(1.3)	4	(1.0)
Minimum acceptable pay	\$110	(\$36)	\$109	(\$32)	\$97	(\$36)
Greater street relative to straight earning ability	63%		70%		50%	
Frequent criminal opportunities	48%		55%		42%	
Job respect rating	41	(30)	38	(29)	40	(28)
Risks of crime:						
Seen, if committed	3.16	(1.61)	2.99	(1.53)	3.08	(1.55)
Reported, if seen	3.59	(1.54)	3.32	(1.57)	3.31	(1.62)
Arrested, if reported	3.86	(1.48)	3.65	(1.54)	3.59	(1.50)
Job loss, if arrested	4.24	(1.33)	3.72	(1.56)	3.41	(1.60)
Prison, if arrested	4.19	(1.32)	4.00	(1.41)	3.77	(1.50)
Friend loss, if imprisoned	2.35	(1.63)	2.13	(1.53)	1.97	(1.50)
Spouse loss, if imprisoned	2.88	(1.71)	2.60	(1.69)	2.32	(1.65)

Table 1. Continued

	Offenders		Addicts		Youth	
	Mean	(S.D.)	Mean	(S.D.)	Mean	(S.D.)
Anycrime ₁₋₉	30%		27%		21%	
Anyarrest ₁₋₉	33%		20%		18%	
Number in sample	1,497		974		861	

members painted by our background variables: most respondents perceive limited prospects for legal employment, report having been actively involved in crime, and admit having substantial contact with the criminal justice system. The mean minimum wage that respondents would accept from a legitimate job is about what they could receive from Supported Work; for most, however, this estimate is less than the wage they believe could be earned illegally. Half of them perceived frequent (daily or weekly) opportunities for generating such illegal income. Finally, between 20 percent and 30 percent of each sample reported either violating the law or being arrested in the nine months between waves. In all three samples, some respondents who reported being arrested failed to admit to a crime, a pattern strongest in the offender sample, leading to the unusual finding that more offenders reported an arrest than reported a crime.

ANALYSIS OF THE MEASUREMENT MODEL

It is well known that within linear models, random measurement error in explanatory variables can attenuate estimates of substantive coefficients. This problem is particularly acute for individual-level models of deterrence since the critical variable, perceptions of the risk of sanctions—an attitudinal construct—is difficult to measure accurately. Consequently, previous research may have underestimated the impact of perceived formal sanctions on criminal behavior. To overcome this problem, and thereby provide a stronger test of the deterrence hypothesis, we attempted to estimate and statistically control for response errors in our risk construct. Using confirmatory factor analysis (Joreskog, 1969), we specified and estimated measurement models of both perceived risk of sanctions and measures of criminal activity.⁹

⁹ To estimate our models, we used Joreskog and Sorbom's (1981) LISREL V program, which provides maximum likelihood estimates, asymptotic standard errors, and a likelihood ratio test statistic distributed approximately chi-square in large samples. This procedure assumes that the joint distribution of observable variables is approximately multivariate normal, an assumption that could be vio-

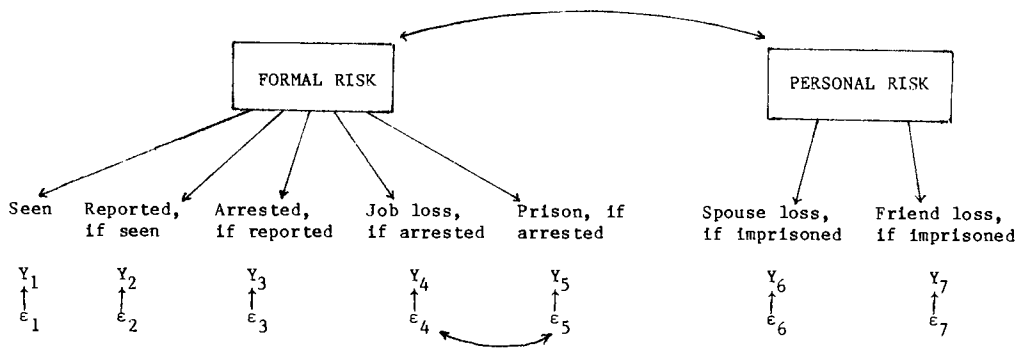
We examined two competing specifications of perceived risk: one in which our seven indicators (see Appendix) are generated by a single risk construct; and another in which the risk of four events culminating in imprisonment (being seen, reported, arrested, and incarcerated), plus the risk of losing a job, reflect a formal risk construct, while fear of losing a spouse or losing friends (if imprisoned) reflect a personal risk construct (see Figure 1). The latter construct resembles, in some respects, the variable, "personal disapproval," which is firmly established in the deterrence literature (cf. Kraut, 1976; Grasmick and Green, 1980; Paternoster et al., 1983). A test of these two nested models, which amounts to whether or not the correlation between the formal and personal risk factors is unity, supports the two-factor model in all three samples.

Standardized parameter estimates of our two-factor measurement model appear in Table 2. Validity coefficients (standardized loadings) suggest that the measures are reasonably reliable indicators of their constructs for all three samples. The risks of being reported and arrested, in particular, are very reliable indicators of persons' perceptions of the risk of formal sanctions. The correlations between factors support the discriminant validity of the two factors: while nontrivial in size, the coefficients are far from unity. We expected the measurement error of "risk of imprisonment (given arrest)" to correlate positively with the measurement error of "risk of job loss (given arrest)."¹⁰ As indicated in Table 2, our expectations were confirmed.

lated here, since some of our measures are ordinal or dichotomous. Recent Monte Carlo results suggest that the LISREL approach is reasonably robust to departures from normality given a large sample size, such as ours (Boomsma, 1983). Furthermore, however, the approach assumes that our indicators are linear functions of their latent constructs, an assumption that could be violated by our dichotomous and ordinal indicators. On the other hand, our attempts to estimate nonlinear logistic models failed to unearth any substantive differences, suggesting that if the functional form is inherently nonlinear the linear form provides a good approximation.

¹⁰ Initially, we expected that the measurement errors of "risk of loss of spouse if imprisoned" and

Figure 1. Measurement Model of Formal and Personal Risk



Although the overall goodness-of-fit test fails to support the model in addict and offender samples, and provides only marginal support for the youth sample, we believe it represents the data adequately. An inductive search for additional significant measurement error correlations found few that could be replicated across samples; moreover, of those that could, each was trivial in size. Even though we could improve the fit of the model by including such correlations, our overall results remain unchanged, and we feel such a model would capitalize on chance by "over-fitting" the data. It appears that we have sufficient statistical power to detect trivial and substantively unimportant measurement error correlations (cf. Saris and Stronkhorst, 1984; Matsueda and Bielby, in Press).¹¹

We also examined the hypothesis that self-reports of crime and arrest reflect a single criminal activity construct, rather than separate phenomena. The results strongly indicate that the two measures tap distinct events: when specified in a factor model, their reliabilities are unacceptably low and when entered as outcomes in regression models, the regressions are far from homogeneous. Therefore, we treat them as separate outcome variables and report both results.¹²

"risk of loss of friend if imprisoned" would also be positively correlated because each risk is conditional on imprisonment. The data did not support this hypothesis, however; in all three samples this correlation was trivial in size and produced inadmissible parameter estimates (negative variances).

¹¹ Note that given the wording of the five measures of formal risk, they do not reflect serially conditional events. For example, respondents were asked their perceptions of risk of being reported if seen, of being arrested if reported, and so on. Thus, they are each serially independent measures of perceptions of formal risk.

¹² Although some economists have recently recommended the use of self-reports of criminal behavior in empirical tests of the rational-choice model

ANALYSIS OF THE SUBSTANTIVE MODEL

Specification and Hypotheses

We incorporated our measurement model of risk into a structural equation model of rational choice and crime. The model, depicted in Figure 2, is a recursive system of seven linear equations, which can be characterized by three blocks of variables—fifteen background variables, six intervening variables, and two outcome variables. The intervening variables, which measure perceived returns and costs of

(see Manski, 1978), some research on the use of the self-report method have questioned its use under certain circumstances. In particular Hindelang et al. (1981), in perhaps the best research on the issue, found that while self-reports appeared reasonably valid and reliable by conventional standards, they may be problematic for use on black males who have had official contact with the criminal justice system. These are not only among the most serious offenders, but they also constitute a major portion of our samples. Hindelang et al. based their conclusions on their reverse record checks: black males with an official record were significantly less likely to report having committed an illegal act. To address this issue a reverse record check for reported arrests was performed on a subsample of the offenders and addicts of the present study (Schore et al., 1979). That check found substantial underreporting of the frequency of arrests (45%) but less of prevalence of arrests (20%). Moreover, the only variable related to underreporting was race: blacks were more likely to underreport. Since blacks tend to perceive lower risks of sanctions than whites, the underreporting could attenuate the effect of risk on crime. To investigate this possibility, we followed the recommendation of Hindelang et al. and ran separate models for blacks and whites. Our results found no differences in the effect of risk on crime. This finding is consistent with Hindelang et al.'s suggestion that because validity coefficients for self-reported delinquency are of similar magnitude for blacks and whites, self reports may be valid for assessing relationships within race, but invalid for assessing differences in behavior across race.

Table 2. Standardized Factor Loadings Factor and Measurement Error Correlations¹ Offender, Addict, and Youth Samples

	Offenders	Addicts	Youths
<i>Factor Loadings</i>			
Formal Risk:			
Seen, if committed	.54	.57	.55
Reported, if seen	.74	.71	.73
Arrested, if reported	.70	.69	.70
Prison, if arrested	.45	.46	.52
Job loss, if arrested	.52	.51	.58
Personal risk:			
Spouse loss, if imprisoned	.69	.70	.79
Friend loss, if imprisoned	.66	.65	.74
<i>Factor Correlation</i>			
Formal Risk and Personal Risk	.36	.27	.39
L ²	74.33	60.07	29.53
df	12	12	12
p	.001	.001	.003

Note: All coefficients significant at the .05 level.

¹ The measurement error correlation between "job loss, if arrested" and "prison, if arrested" is .23 for offenders, .18 for addicts, and .13 for youths.

Figure 2. Path Diagram of the Substantive Model

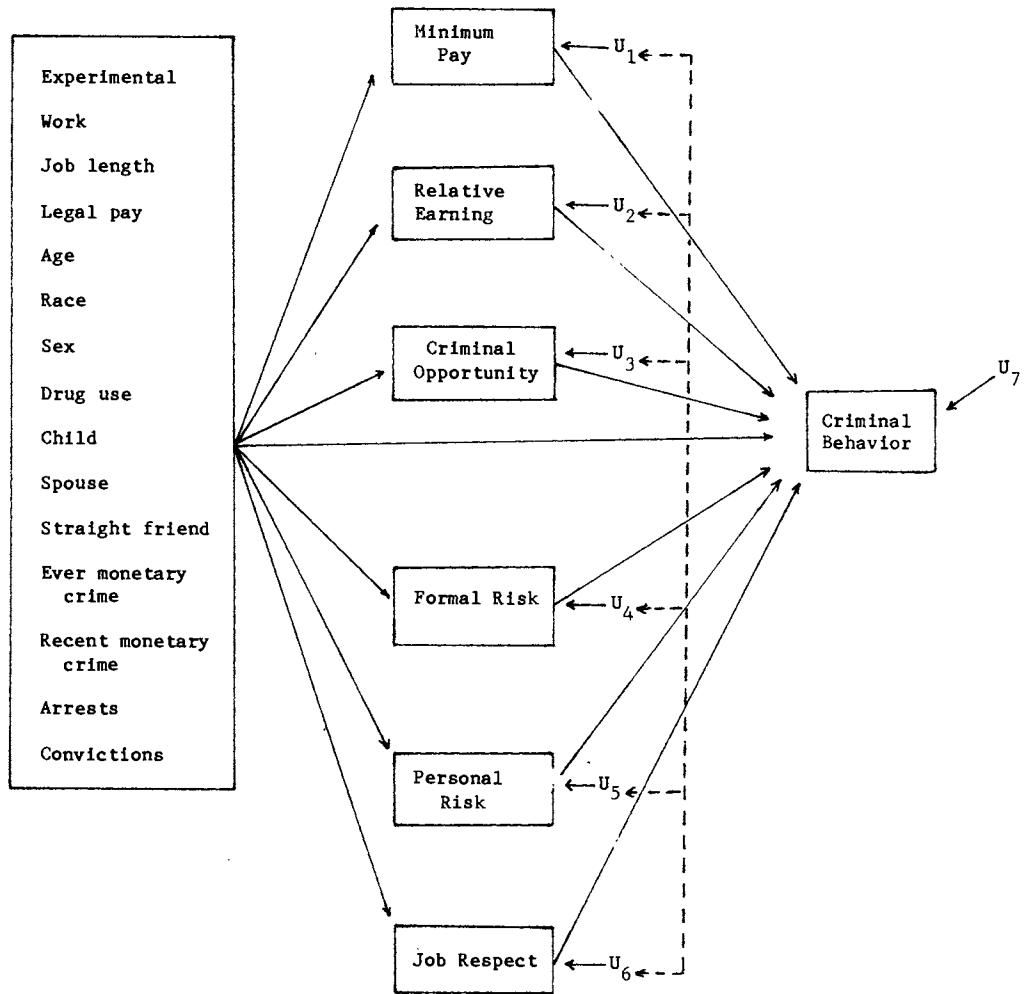


Table 3. Hypothesized Direction of Effects for the Substantive Model

Independent Variables	Dependent Variables						
	Minimum Pay	Relative Earning	Crime Opportunity	Job Respect	Formal Risk	Personal Risk	Criminal Behavior
Experimental status	+	-		+	+		-
Work	+	-		+			-
Job length	+	-		+			-
Legal pay	+	-		+			-
Age ^a	+	-	-	-	+	+	-
Race	-	+	+	-	-	-	-
Sex	+	+	+	-	-	-	+
Drug use		+	+				+
Child	+					+	-
Spouse	+					+	-
Straight friend			-	+		+	-
Ever monetary crime		+	+	-	-	-	+
Recent monetary crime		+	+	-	-	-	+
Arrests		+	+	-	+	-	+
Convictions		+	+	-	+	-	+
Minimum pay							+/-
Relative earning							+
Crime opportunity							+
Job respect							-
Formal risk							-
Personal risk							-

^a Because of the restricted age range of the youth sample, the observed effects of age on the dependent variables may not coincide with the hypothesized effects for this sample.

crime, are each functions of the background variables, and are not causally interrelated among themselves, but instead are left as unanalyzed correlations by allowing their structural disturbances (u_1 - u_6) to correlate. The outcome variables—self-reported crime and arrest—are determined by the background variables plus the intervening variables.

Table 3 presents the direction of effects hypothesized by the model; the important hypotheses, derived from the rational choice model, involve the effect of our endogenous predictors on criminal activity and appear in the last column. Focusing on these effects, we expect that perceptions of higher risks of formal (FORMAL RISK) and personal sanctions (PERSONAL RISK) will reduce the likelihood of crime. Also according to the model, persons who feel they can earn more money illegally than legally (RELATIVE EARNINGS) should be more likely to succumb to criminal temptations. Similarly, persons who perceive more opportunities (CRIME OPPORTUNITY) for crimes should have greater objective opportunities and lower opportunity costs for engaging in crimes—both of which should increase their chances of illegal activity. Finally, persons holding more respect for legal jobs relative to illegal forms of work (JOB RESPECT) should have less taste for crime, greater moral inhibitions, and therefore be less likely to violate the law. Conflicting mechanisms make the direction of the impact of MINIMUM PAY on crime

difficult to predict. Conceivably, persons unwilling to take low-paying conventional jobs are likely to be unemployed and willing to deviate. On the other hand, they could be simply holding out for a high-paying legitimate job, and therefore be less likely to deviate. To determine which of these competing hypotheses holds, we must turn to the data.

Estimation and Results

We estimated both the measurement model of risk and the substantive model of crime simultaneously as a single system. Again, we used the maximum likelihood estimator of Joreskog's LISREL approach (Joreskog and Sorbom, 1981). Given the large sample sizes and the large number of overidentifying restrictions, our models fit the data quite well: for the offenders, the likelihood-ratio statistic (L^2) is 405 with 183 df; for the addicts, $L^2 = 330$ with 163 df, and for the youths, $L^2 = 247$ with 169 df.¹³

Parameter estimates of the substantive model for offenders, addicts, and youths appear in Tables 4, 5, and 6, respectively. By and large these estimates depict a plausible picture of a rational choice model of crime. The last

¹³ The different degrees of freedom for offender, addict, and youth models reflect the different number of sites—a set of exogenous dummy variables not shown in the model—across samples.

Table 4. Unstandardized and Standardized Parameter Estimates of the Structural Model: Offender Sample (N = 1497)

Independent Variables	Dependent Variables									
	Minimum Pay	Relative Earning	Crime Opportunity	Personal Risk	Formal Risk	Job Respect	Any Crime _{t-1,2}	Any Arrest _{t-1,2}		
Experimental	-.005	.001	—	—	-.010	—	-.031	-.016		
Work	.024	-.036*	—	—	—	.220*	-.006	-.008		
Job length	.003	-.039*	—	—	—	.200	-.011	-.038*		
Legal pay	-.002	.011	—	—	—	-.033	-.001	-.000		
Age	.037*	-.056**	-.014	.390***	.210***	.830***	-.021	-.030		
Race	.072**	.009	-.059*	-.360***	-.070	-.460**	-.169***	—		
Sex	.105**	-.037	.155**	.630**	.160	-.290	.098***	-.172**		
Drug use	—	.111***	.063**	—	-.042	.040	-.053	.067**		
Child	.072*	—	—	-.030	—	—	-.038	.048		
Spouse	.023	—	—	-.240*	—	—	-.007	-.067		
Straight friend	—	—	—	-.073	—	—	-.007	-.075**		
Ever monetary crime	-.067**	.144***	-.087**	-.090	.170***	1.07***	0.71***	-.058		
Recent monetary crime	.013	.029	.172***	-.040	-.210***	-.770***	-.124***	-.092		
Arrests	—	.013	.018	-.090	-.021	.044	.018*	.035***		
Convictions	—	.002	.004	.088**	-.021	.044	.005	.004		
Minimum pay	—	—	—	-.013	-.010	-.070***	-.006	-.006		
Relative earning	—	—	—	—	—	—	-.054*	-.017		
Crime opportunity	—	—	—	—	—	—	-.046*	-.083***		
Personal risk	—	—	—	—	—	—	-.022	-.011		
Formal risk	—	—	—	—	—	—	-.020	-.015		
Job respect	—	—	—	—	—	—	-.015***	-.006		
R ²	.05	.07	.07	.08	.07	.09	.15	.10		
								L ² = 405		
								df = 183		

Note: Standardized coefficients appear in parentheses.

* p < .05.

** p < .01.

*** p < .001.

Table 5. Unstandardized and Standardized Parameter Estimates of the Structural Model: Addict Sample (N = 974)

Independent Variables	Dependent Variables									
	Minimum Pay	Relative Earning	Crime Opportunity	Personal Risk	Formal Risk	Job Respect	Any Crime _{t-1,9}	Any Arrest _{t-1,9}		
Experimental	-.008	.013	-.014	-.050	-.050	-.050	.003	-.023		
Work	-.081***	.005	(.010)	-.026	-.026	-.026	(.067)	.005		
Job leav- th	.018	.001	(.002)	-.066	-.066	-.066	(.063)	-.019		
Legal	.038***	.011	(.044)	-.049*	-.049*	-.049*	(.139)	.002		
Age	.042**	-.058**	(-.085)	.120*	.120*	.120*	(.188)	-.042*		
Race	.012	.027	(.024)	-.440***	-.440***	-.440***	(.094)	-.042*		
Sex	.082**	-.135***	(-.117)	-.080	-.080	-.080	(.041)	-.042*		
Drug use	-.001	-.090	(-.043)	.030	.030	.030	(.013)	-.042*		
Child	.030	-.001	(.000)	-.000	-.000	-.000	(.003)	.104**		
Spouse	-.011	-.012	(.011)	-.410***	-.410***	-.410***	(.159)	.090		
Straight friend	.010	.126**	(.099)	.210*	.210*	.210*	(.077)	.090		
Ever monetary crime	-.010	.071*	(.078)	-.090	-.090	-.090	(.028)	-.001		
Recent monetary crime	-.010	.057***	(.136)	-.170	-.170	-.170	(.087)	-.007		
Arrests	-.010	.002	(.014)	.203***	.203***	.203***	(.066)	-.007		
Convictions	-.010	.002	(.014)	.009	.009	.009	(.075)	.091***		
Minimum pay	-.010	.002	(.014)	.010*	.010*	.010*	(.033)	.029*		
Relative earning	-.010	.002	(.014)	.029*	.029*	.029*	(.140)	.004		
Crime opportunity	-.010	.002	(.014)	-.014	-.014	-.014	(.010)	-.013		
Personal risk	-.010	.002	(.014)	.003	.003	.003	(.003)	-.015		
Formal risk	-.010	.002	(.014)	.090***	.090***	.090***	(.102)	.020		
Job respect	-.010	.002	(.014)	.004	.004	.004	(.010)	.007		
R ²	.07	.06	.06	.07	.10	.10	.11	.08		

Note: Standardized coefficients appear in parentheses.

- * p < .05.
- ** p < .01.
- *** p < .001.

L² = 330
df = 163

Table 6. Unstandardized and Standardized Parameter Estimates of the Structural Model: Youth Sample (N = 861)

Independent Variables	Dependent Variables									
	Minimum Pay	Relative Earning	Crime Opportunity	Personal Risk	Formal Risk	Job Respect	Any Crime _{t-9}	Any Arrest _{t-9}		
Experimental	.028 (.039)	.014 (.014)	—	—	-.020 (.009)	—	-.004 (-.005)	-.001 (-.002)		
Work	.006 (.015)	.009 (.017)	—	—	—	-.180 (-.057)	-.043* (.092)	-.040* (.092)		
Job length	-.048* (-.084)	-.036 (-.046)	—	—	—	-.180 (-.039)	-.034 (-.052)	-.048* (-.080)		
Legal pay	.010 (.051)	.007 (.027)	—	—	—	.045	-.020* (-.035)	-.007 (-.035)		
Age	.337*** (.103)	.229 (.050)	-.009 (-.002)	.310 (.028)	-.390 (-.049)	2.35* (.092)	-.211* (-.057)	-.266* (-.077)		
Race	.016 (.019)	.017 (.014)	-.080* (.067)	-.530*** (-.179)	-.350*** (-.163)	-.150 (-.022)	—	—		
Sex	.133*** (.125)	.067 (.045)	-.092* (-.064)	-.090 (-.024)	-.020 (.007)	-.470* (-.081)	.106** (.088)	.089* (.079)		
Drug use	—	.140** (.092)	.136** (.092)	—	—	-.690*** (-.081)	.172*** (.138)	.035 (.031)		
Child	.003 (.002)	—	—	-.180 (-.038)	—	—	-.001 (-.001)	-.018 (-.012)		
Spouse	.004 (.002)	—	—	.030 (.006)	—	—	-.011 (-.006)	-.111* (.067)		
Straight friend	—	—	-.039 (-.031)	.220* (.070)	—	-.760*** (.103)	-.026 (-.024)	.044 (.044)		
Ever monetary crime	-.028 (-.038)	.002 (.002)	.169*** (.169)	-.150 (-.062)	.080 (.044)	-.980*** (.171)	.075 (.089)	-.009 (-.011)		
Recent monetary crime	-.019 (.024)	.115* (.108)	-.006 (-.006)	-.020 (-.009)	-.230* (-.124)	-.220 (-.036)	.068 (.078)	.071 (.088)		
Arrests	—	.061* (.064)	.060* (.064)	.040 (.015)	.140* (.086)	.130 (.025)	.054* (.070)	.064*** (.089)		
Convictions	—	.027* (.076)	.018 (.052)	-.023 (-.027)	-.007 (.011)	-.059 (-.030)	-.030** (.102)	.310*** (.114)		
Minimum pay	—	—	—	—	—	—	-.002 (-.002)	.000 (.000)		
Relative earning	—	—	—	—	—	—	-.031 (-.038)	.036 (.047)		
Crime opportunity	—	—	—	—	—	—	-.095*** (.113)	-.006 (-.008)		
Personal risk	—	—	—	—	—	—	-.012 (-.036)	-.021 (-.067)		
Formal risk	—	—	—	—	—	—	-.018 (-.039)	.029 (.068)		
Job respect	—	—	—	—	—	—	-.004 (-.026)	-.003 (-.020)		
R ²	.08	.08	.09	.05	.07	.10	.22	.11		

Note: Standardized coefficients appear in parentheses.

- * p < .05.
- ** p < .01.
- *** p < .001.

L² = 247
df = 169

two columns describe the equations of substantive interest—self-reported crime and arrest. Across all three samples, the model explains self-reported crime better than arrest: more of the variance is explained and more of the coefficients are consistent with expectations. This suggests that the self-reported crime construct is a more valid indicator of illegal behavior (Mallar and Piliavin, 1984). We therefore emphasize the results for self-reported crime.

Of the background variables, the effects of SEX, PRIOR MONETARY CRIME and PRIOR ARRESTS are substantial and consistent across all three samples. As expected then, males, persons who have committed a monetary crime in the past, and persons who have been arrested more often, are all more likely to violate the law. Also as expected, drug users are more likely to violate the law in the offender and youth samples; but because of insufficient variance, this is not replicated in the addict sample. Finally, LAST YEAR'S MONETARY CRIMES has a significant positive effect on crime in offender and addict samples, but not in youth samples.

Of more importance for the purpose of this article is the impact on crime of the endogenous predictors representing the rational-choice process. Here, the most dramatic finding is that across all samples and for both measures of illegal activity, both formal and personal risks of punishment have virtually no impact on criminal behavior. This finding is all the more compelling since we have attempted to correct for attenuation due to unreliability in our perceptual indicators of risk. Furthermore, our equations predicting FORMAL RISK explain a nontrivial amount of variance (about ten percent), and moreover, contain parameter estimates that coincide with our hypotheses. Our equations for PERSONAL RISK explain less variance (about seven percent), but do contain coefficients consistent with expectations. Therefore, we do not find evidence directly questioning the (criterion) validity of our risk constructs. Instead, we find that in contrast to previous research, which concludes that deterrence should be more effective in less conventional samples, in our samples containing criminally-motivated and morally-uncommitted persons, perceptions of the risk of formal and personal sanctions fail to make a difference in explaining crimes.

What does appear to make a difference is the other side of the rational-choice process—the opportunity or returns component. Specifically, CRIME OPPORTUNITY has a substantial and statistically significant effect on illegal behavior across all three samples. As expected, persons who perceive greater opportu-

nities to earn money illegally are more likely to violate the law. Furthermore, in offender and addict samples, persons who hold more respect for illegitimate occupations relative to legitimate jobs (JOB RESPECT) have more to gain and less to lose by violating the law and therefore are significantly more likely to do so.

The variables representing the returns component of the rational choice process mediate the impact on crime of some of our background variables. For offender and addict samples, persons who are younger and who have committed monetary crimes in the last year commit more crimes in part because they perceive greater opportunities and return to crime. In the youth sample, this mechanism holds for persons who have used drugs and who have been arrested. Overall, however, our block of endogenous variables mediates little of the total effects of our exogenous variables. Furthermore, across all three samples, the increase in explained variance is marginal at best.¹⁴

Could our finding that formal and personal risks fail to deter stem from a methodological artifact? We explored several possibilities. It could be that risks of formal and personal sanctions deter monetary crimes but not other crimes; in fact, this is consistent with some variants of the rational-choice explanation. To test this, we estimated our model using as an outcome variable, self-reports of committing a property crime in the previous nine months. With trivial exceptions, the parameter estimates of this model mirrored those of our earlier models. This result is not surprising since the percentage of persons reporting any crime who also report a monetary crime ranges from 77 percent (addicts) to 86 percent (offenders).

A second possible artifact involves the functional form of the relationship between sanctions and crime. Some researchers have postulated and found nonlinear effects due to diminishing returns (Logan, 1972) or threshold effects (Tittle and Rowe, 1974; Tittle, 1980). According to this hypothesis, sanctions will not deter until the perceived probability of risk reaches a certain threshold; conversely, the deterrent effect of sanctions may diminish when the perceived probability of risk reaches a point of saturation. If true, our linear probability model may have underestimated the

¹⁴ We arrived at these conclusions regarding indirect effects by first locating those reduced-form effects (not shown in our tables) that are substantial and statistically significant. Second, we determined which of these effects was substantially reduced in the structural form. Third, we traced the indirect effect by locating the intervening variable that had a significant effect on crime and was significantly affected by the exogenous variable in question.

slope of the risk variables at moderate levels of risk. To examine this hypothesis, we estimated multivariate logistic regressions on our single-indicator variables plus factor scores of our multiple-indicator risk variables.¹⁵ Again, for all three samples and for both self-reported crime and arrest, the results remain relatively unchanged.

Still, it could be that because the true threshold is so high, extremely high levels of perceived risk are required before sanctions deter, and consequently, the logistic functional form is unable to capture the true nonlinear relationship between sanctions and crime. We therefore estimated an extreme model, postulating that persons are not deterred until they perceive the greatest possible risk on every component of the risk construct. We constructed two dichotomous risk variables, which contrasted persons who scored highest on every indicator of FORMAL and PERSONAL RISK versus all others, and entered them into our full multivariate logit model. Again, however, the parameter estimates retained the sign and statistical significance of those of our LISREL models.

Finally, it could be that the nonlinear deterrent effect can be captured by specifying a conditional effect. That is, because the wording of our indicators of PERSONAL RISK requires that respondents hypothetically consider they have been imprisoned, it follows that PERSONAL RISK might deter only those who consider incarceration a likely result of crime. To test this hypothesis, we estimated the model separately for two groups: those having high scores on FORMAL RISK versus all others. Again, however, PERSONAL RISK failed to affect crime significantly in either group.

SUMMARY AND DISCUSSION

In sum, after estimating a variety of models and examining several hypotheses, our conclu-

sions remain unchanged: we find evidence supporting the opportunity and reward component of the rational-choice model of crime, but no evidence supporting the risk component. The null finding regarding perceived risks is consistent with findings of other individual-level studies of deterrence that have used less rigorous designs and analytical procedures. We have gone beyond previous research not only analytically, but also by extending those results to a different and significant population—namely, the population of serious and high risk offenders. Moreover, our results explicitly refute the hypothesis, proposed by Silberman (1976) and Tittle (1977, 1980), that the threat of legal punishment deters persons who are less committed to conventional morality. Furthermore, taken together with our positive results regarding opportunities and returns, these null findings suggest that the rational-choice model may oversimplify the cognitive process behind criminality. What may be needed is a more complex model that relaxes some of the stringent assumptions of the strict rational-choice approach.

For example, a greater emphasis on the limitations of human beings to acquire and process information, such as the probability of sanctions, may be warranted (cf. Simon, 1957). That is, it may be that people are insensitive to marginal changes in their perceptions of the probabilities of the consequences (sanctions) of their actions, especially when that probability is low (Kunreuther and Slovic, 1978). Instead, they may alter their behavior only after major discontinuous shifts in their perceptions of the risk of sanctions. Moreover, persons may discount the meaning or relevance of certain probabilities: when confronted with a decision, they may discount some outcomes relative to others of equal probability (Kahnemann and Tversky, 1984). For example, persons may slight those consequences or events that are either distant or beyond their direct control, and emphasize those that are immediate and within control (Kogan and Wallach, 1967; Ainslie, 1982). Furthermore, the particular style of discounting—emphasizing some options but not others—probably varies from one person to another.

We are suggesting that persons' evaluations or imputed meanings of sanctions are important in determining their behavior. These evaluations or meanings may be conditioned by elements within the immediate situation confronting the individual. For example, the persons' perceptions of the opportunity, returns, and support for crime within a given situation may influence their perceptions of risks and the extent to which those risks are discounted. This implies that the effective

¹⁵ This strategy has the well-known additional advantages of using logistic regressions over linear probability models in predicting a dichotomous dependent variable. That is, it overcomes the problem of heteroscedastic structural disturbances and the problem of predicting inadmissible values (greater than one and less than zero) of the dependent variable. Furthermore, it relaxes the assumption of multivariate normality found in the LISREL approach. The strategy has the drawback of failing to correct for attenuated regression coefficients due to unreliability—if indeed the appropriate measurement model is a confirmatory factor model and not a weighted linear combination of indicators. The actual factor scores we used were derived from the LISREL program, based on a weighted linear combination of all observables in the model.

assessments of risk are to some extent situationally-induced, transitory, and unstable (Short and Strodtbeck, 1965). If true, this could help explain the ineffectiveness of our risk variables—that is, if persons' perceptions of risk are unstable over time, and the causally-relevant perceptions are those more proximate to crime, our distal measures of perceived risk may be irrelevant to behavior.

We can provide some indirect evidence on the last hypothesis by examining a model of the stability of our risk constructs. This model, depicted in Figure 3, is a two-wave panel version of our multiple-indicator measurement model. The model specifies that the FORMAL (PERSONAL) RISK construct at time two is a linear function of FORMAL (PERSONAL) RISK at time one and self-reported CRIME and ARREST measured at time two. Intertemporal correlations of response errors for each indicator are estimated to disentangle true stability from response effects that remain constant over time. The standardized coefficients, given in Table 7, indicate that both FORMAL and PERSONAL RISK are relatively unstable over time. For offender and youth samples, self-reported crime affects both FORMAL and PERSONAL RISK significantly, a result con-

sistent with the findings by Saltzman et al. (1982) and Minor and Harry (1982) of "experiential" effects. A very small component of the total stability in perceived risk, then, works indirectly through criminal behavior.

These results suggest that persons did change their perceptions of risk substantially over the nine months between waves, and that this change was only modestly affected by crime. Therefore, we cannot rule out the possibility that in our sample, persons' perceptions of risks more proximate to their decision to engage in or refrain from crime do influence the outcome of that decision.

This issue and others raised above can be examined, at least in part, by research capitalizing on multiwave panel data. By collecting waves of data spaced closer in time, a more fine-grained temporal analysis is possible, capturing changes in perceptions of risk and the impact of those changes on criminal behavior. Furthermore, such a strategy would allow one to disentangle within-individual changes in attitudes, perceptions, and behaviors from within-time variation in such variables.

While we recognize the importance of using more sophisticated research designs and

Figure 3. Two-Wave Panel Model of Formal and Personal Risk

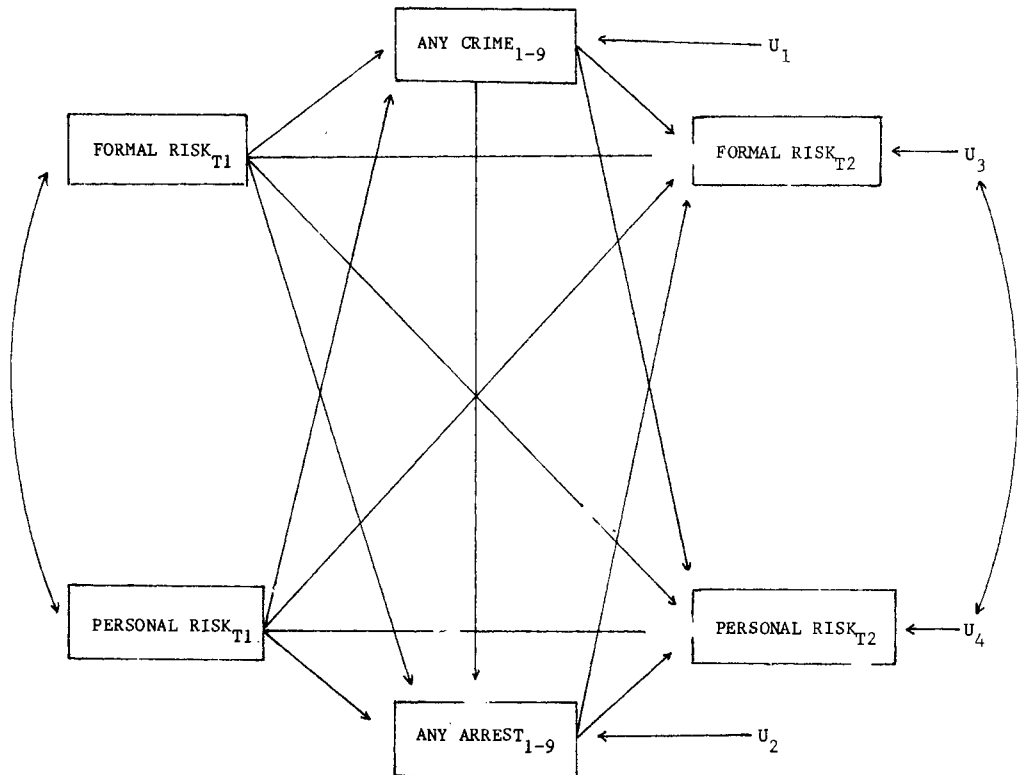


Table 7. Standardized Parameter Estimates for a Two-Wave Panel Model of Formal and Personal Risk

Independent Variables	Offenders		Addicts		Youths	
	FORMAL RISK _{T2}	PERSONAL RISK _{T2}	FORMAL RISK _{T2}	PERSONAL RISK _{T2}	FORMAL RISK _{T2}	PERSONAL RISK _{T2}
FORMAL RISK _{T1}	.243 (6.17)	.052 (1.26)	.294 (6.04)	.093 (1.70)	.221 (4.19)	-.005 (-.10)
PERSONAL RISK _{T1}	-.055 (-1.28)	.443 (8.15)	.087 (1.86)	.427 (6.39)	.081 (1.56)	.286 (5.13)
ANY CRIME ₁₋₉	-.086 (-2.84)	-.060 (-1.81)	-.059 (-1.61)	-.041 (-1.02)	-.190 (4.83)	-.071 (-1.82)
ANY ARREST ₁₋₉	.014 (.48)	.081 (2.46)	.030 (.81)	.017 (.42)	.064 (1.66)	.069 (1.78)

Note: t-values appear in parentheses.

theoretical models, we nevertheless believe the present study provides the best test of the rational-choice model to date. The results of that test find that for persons at high risk of formal sanction, including addicts, and school dropouts, perceptions of the risk of both formal and personal sanctions fail to influence persons' decisions to violate the law. On the other hand, those decisions are influenced by persons' perceptions of their opportunities and respect for criminal activities.

APPENDIX

EXPERIMENTAL	A dummy variable equal to 1 if respondent was in the Supported Work experimental group, and 0 otherwise.	RACE	A dummy variable equal to 1 if respondent was black, 0 otherwise.
WORK	An ordinal variable equal to 0 if respondent reported no employment during the year preceding sample entry; equal to 1 if the individual worked from one to nine weeks; and equal to 2 otherwise.	SEX	A dummy variable equal to 1 if respondent was male, 0 otherwise.
JOB LENGTH	An ordinal variable equal to 0 if respondent reported no job in the two years prior to sample entry; equal to 1 if the individual reported a job lasting 1-6 months; and equal to 2 otherwise.	DRUG USE	A dummy variable equal to 1 if respondent reported ever using opiates prior to sample entry.
LEGAL PAY	A continuous variable measuring respondents' average legal income (in dollars) per month ÷ 100, during the time they were not incarcerated in the last year.	CHILD	A dummy variable equal to 1 if respondent reporting living with a child under 18 years of age, 0 otherwise.
AGE	A continuous variable equal to respondent's age in years.	SPOUSE	A dummy variable equal to 1 if respondent reporting living with a spouse, 0 otherwise.
		STRAIGHT FRIEND	A dummy variable equal to 1 if respondent reported having a straight best friend (i.e., not involved in any "hussles" or crime), 0 otherwise.
		EVER MONETARY CRIME	A dummy variable equal to 1 if respondent reported ever having made money illegally, 0 otherwise.
		RECENT MONETARY CRIME	A dummy variable equal to 1 if respondent reported making money illegally in the year prior to sample entry, 0 otherwise.
		ARRESTS	A continuous variable equal to the total number of arrests respondent reported every having incurred, ÷ 10.
		CONVICTIONS	A continuous variable equal to the total number of convictions respondent reported ever having received.

- MINIMUM PAY** A continuous variable equal to the lowest weekly pre-tax pay respondent would accept for a "straight job," ÷ 100.
- RELATIVE EARNINGS** A dichotomous variable equal to 1 if respondent's expected earnings "on the street" are greater than or equal to expected earnings from a straight job, 0 otherwise.
- CRIME OPPORTUNITY** A dichotomous variable equal to 1 if the respondent perceived frequent (daily or weekly) opportunities for crime, 0 if less frequent opportunities.
- JOB RESPECT** A continuous variable equal to the difference between respondent's mean occupational respect rating (on a 100-point scale) for 6 legitimate occupations and 6 illegitimate occupations, ÷ 10.
- Risks of crime:
- (a) SEEN Estimates of probabilities (1 = "low"; 3 = "50-50"; 5 = "High") associated with seven different events should respondent commit a crime earning \$1,000 including: (a) being seen by police; (b) being reported, if seen; (c) being arrested, if reported; (d) losing one's job, if arrested; (e) going to prison, if arrested; (f) losing one's friends, if sent to prison; (g) losing one's spouse, or girl/boyfriend, if sent to prison.
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