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8 Market Incentives for Criminal Behavior

W. Kip Viscusi

8.1 Introduction

Since the seminal article by Becker (1974), the economic analysis of criminal behavior has focused on the economic incentives to engage in illegal actions. These incentives are the integral components of a model of the rational choice to engage in criminal acts. According to this model, if the expected rewards from criminal behavior exceed the net benefits of alternative pursuits, the individual will choose to engage in crime.[1] Crime is not fundamentally different from legitimate occupational pursuits. The expected losses from sanctions against crime enter the criminal choice decision in much the same way as, for example, the nonpecuniary aspects of a job would in the choice of employment.

Consider a theoretical framework in which there are three components to the individual's choice: the expected financial gains from crime, the expected legal sanctions against crime, and the expected job income if the individual chooses not to engage in crime. It should be emphasized that the expected values of these three factors are based on the individual's subjective assessment of the probabilities involved; one need not assume that the individual has full knowledge of the true probabilities affecting the decision. For simplicity let the time allocation decision be a discrete choice between crime and noncriminal employment. Then, the individual will become a criminal if:

$$\begin{bmatrix}\text{expected}\\ \text{gains from}\\ \text{crime}\end{bmatrix} - \begin{bmatrix}\text{expected}\\ \text{legal}\\ \text{sanctions}\end{bmatrix} > \begin{bmatrix}\text{expected}\\ \text{job}\\ \text{income}\end{bmatrix},$$

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which can be rewritten as

$$(1) \quad \begin{bmatrix} \text{expected} \\ \text{gains from} \\ \text{crime} \end{bmatrix} - \begin{bmatrix} \text{expected} \\ \text{job} \\ \text{income} \end{bmatrix} \equiv EXYGAP > \begin{bmatrix} \text{expected} \\ \text{legal} \\ \text{sanctions} \end{bmatrix}.$$

Using data from the NBER Survey of Inner-City Black Youth, I will construct an explicit measure of *EXYGAP* for each of the respondents in the sample based on his assessment of his criminal and legitimate earnings prospects. Also included in the analysis will be information on whether the respondent believed that crime offers greater rewards than employment does. These measures therefore offer the opportunity for a very explicit test of the role of economic incentives in determining criminal behavior. In that regard, this study is distinctive from all previous analyses.

The focus of the existing empirical literature on crime is quite different from that in this study. With one principal exception, recent studies such as Ehrlich (1973, 1975) have focused on aggregative data by location, using either a cross-sectional or a time-series approach. Some studies reviewed in Freeman (1983) take the form of social experiments, thereby raising additional problems of interpretation. For example, was an ex-offender more responsive to the "supported work" experiment than he would have been to market incentives?

The most successful vein of research has been on the deterrent effect of legal sanctions.[2] The legal sanction variables in the aggregative studies include such measures as the average length of prison terms, the arrest rate per crime, the conviction rate per crime, and the imprisonment rate per crime for a particular city or state. In Witte's (1980) study of the criminal behavior of released prisoners, the individual's frequency of arrests and convictions before his prison term was used as the deterrence proxy. Although many of these studies have provoked considerable controversy, the overall thrust of their results is that criminal deterrence clearly decreases criminal activity.

A second line of research has been to examine the linkage between crime and the labor market, principally the relationship between unemployment and crime. In terms of the criminal incentives discussed above, higher unemployment rates should lower the expected-job-income component of *EXYGAP*, thus enhancing the attractiveness of crime. Diminished job prospects also reduce the opportunity costs of incarceration, since the forgone earnings will be less, thereby reinforcing the job-income effect. Although there are many who strongly believe that these relationships hold, there has never been any strong

empirical support for the crime–labor market linkage. The empirical impact of the unemployment variable is typically not as strong as that of the deterrence variables. As Freeman (1983, 106) notes in a recent critical survey of the crime literature, "The bulk of the studies examined here show some connection between unemployment (and other labor market variables) and crime, but they fail to show a well-defined, clearly quantifiable linkage."

The source of the ambiguity varies according to the type of study. Of the ten time-series analyses reviewed by Freeman, nine revealed a crime–labor market linkage, but this effect was not as strong as the deterrence variable in the three studies that examined both effects. In a regression of crime rates on unemployment and a time trend, Freeman (1983, 96–98) found a positive crime-unemployment relationship that fell just short of that required to pass the usual tests of statistical significance. A major inherent drawback of these time-series studies is the extreme collinearity of the aggregative data. Moreover, even if there is an observed effect, one cannot ascertain whether unemployment fluctuations influence the level of crime or simply its timing. Individuals may simply postpone their criminal pursuits until they have more free time during the periods of temporary unemployment.

The implications of the 15 cross-section studies also reviewed by Freeman are more mixed. Unemployment and income measures (such as the percentage of the population in poverty) had no strong effects in the correct direction in more than one-third of the studies, whereas the deterrence variables exhibited predictive strength in two-thirds of the studies.

This impression of the weak performance of the labor market variables persists in studies of individual data and social experiments. In her study of 641 prisoners released in North Carolina, Witte (1980) obtained conflicting results with the wage variable (statistically significant effects with differing signs and some insignificant effects) and an unexpected negative effect of unemployment on crime.

Taking these results at face value, one could conclude that the "stick" is clearly more effective than the "carrot" in discouraging criminal behavior. Indeed, one might question whether there is any solid evidence that improved economic opportunities will exert any substantial effect on crime.

Before dismissing the role of the labor market, however, one should recognize the imbalance in the research questions posed by these studies. In terms of the components of equation (1), the empirical studies have provided a comprehensive analysis of the various legal sanction components that contribute to the losses expected from criminal activities. But no study has provided a comprehensive variable pertaining to *EXYGAP*. The most that has been done is to establish proxies for

expected job income (such as the national unemployment rate),[3] while the expected-crime-income component has been ignored.

If *EXYGAP* is subject to random measurement error, the importance of the labormarket will be underestimated. In practice, it is likely that the error will not be entirely random, since the expected gains from crime will also be correlated with the expected-job-income proxy. And if opportunities for property crime, for example, decline with increases in unemployment, one cannot even be confident that *EXYGAP* is a positive function of the unemployment rate.

Rather than dismiss the importance of the labor market on the basis of fragmentary and potentially biased evidence, we should conduct a fairer test of the economic motivation hypothesis to assess whether a more refined measure of *EXYGAP* also fails to suggest a significant relationship. This test is the primary focus of this paper.

The nature of the NBER sample makes its data base on crime pertinent to many more issues than simply the professional controversy over the significance of *EXYGAP* or, more generally, the role of economic incentives to commit crime. Youths in the age bracket represented in the NBER sample account for well over half of all criminal arrests, and minority youths account for a disproportionate share of that amount. The sample consequently permits the analysis of a major segment of the urban crime problem.

In addition, minority youths comprise a particularly distressed segment of the labor force, to which their attachment has been notoriously low.[4] A fundamental issue of social policy is the nature of the activity of youths who are not employed or in school. Does this group resort to criminal activity as a means of support? If so, we may be less concerned with their welfare in terms of their income status, but we will be more concerned with the adverse impact of their criminal acts on the rest of society.

Section 8.2 provides a description of the sample and the variables employed in the analysis, with particular attention to the overall relationship between criminal activity and the principal variables of interest. The impact of the various substantive variables, such as *EXYGAP*, on individual decisions to engage in crime is the subject of section 8.3. A closely related issue, how the explanatory variables influence the total level of crime income an individual receives, is the focus of section 8.4. These causal influences differ depending on the type of crime. Section 8.5 offers a detailed analysis of the categories of criminal activity, their interrelationships, and the role of the causal variables in each case. In section 8.6 I consider the behavior of three different subsamples of the population: those in school, those who are employed, and those who are neither in school nor at a job. The role of economic incentives and the patterns of criminal behavior will be shown to be quite different across these groups.

The most important implication of this study is that economic incentives do exert a powerful influence on criminal behavior. Both current employment status and the relative economic rewards from crime are instrumental in both the decision to participate in crime and the intensity of criminal activity.

8.2 The Sample and the Variables

The NBER Survey of Inner-City Black Youth provides detailed information on the respondents' personal characteristics and activities, including criminal behavior. The criminal activity questions, which are of primary interest here, provide a comprehensive perspective on the nature of the youths' illegal activities, their earnings from crime, and their perceptions regarding the attractiveness of criminal pursuits. For ease of reference, table 8.1 provides a glossary of the variables used in this study.

The characteristics of the sample are summarized in table 8.2. Means and standard deviations are provided for the entire sample of 2,358 youths and for the two crime subsamples, the 349 individuals (15 percent) who reported having committed a crime in the past month and the 439 youths (19 percent) who reported having committed a crime in the past year.

Through its focus on the criminal behavior of black youths, the NBER sample addresses a substantial part of the overall crime problem, as youths 15 to 24 years of age account for almost 60 percent of all city arrests. Given this concentration of crime among the young, there must necessarily be a drop in criminal activity as one ages. The peak crime years for the types of activities surveyed by NBER occur in the 15–24 age bracket. Specifically, arrests for robbery, drug law violations, and forgery peak between the ages of 17 and 21, while arrests for auto theft, burglary, and larceny peak among youths 15 to 16 years old.[5]

Within the age span covered by the NBER sample, there is a very small relationship between age and crime. The mean age of those who had engaged in crime is somewhat higher than those who had not (see table 8.2). The year-by-year crime breakdowns in table 8.3 suggest that participation in crime appears to increase somewhat with age, with ages 22 and 24 the peak years. The amount of income earned by those who had committed crimes also tends to follow a similar pattern, as monthly crime income peaks for those aged 20 and 22 while yearly crime income is greatest for those aged 22 and 23.

These data do not indicate any age-related decline in crime. The overall trend is relatively flat, with some evidence of an age-related increase. This pattern will be borne out by all of the subsequent statistical analysis, as will most of the other patterns in table 8.2. Age does not play a major role as an independent, statistically significant

Table 8.1 Glossary of Variables

Variable Symbol	Definition
AGE	Age in years
EDUC	Years of schooling
MARRIED	Marital status dummy variable (d.v.); *MARRIED* = 1 if respondent is married, 0 otherwise.
DRINK	Drinking d.v.; *DRINK* = 1 if respondent ever drinks beer, or wine, or hard liquor; 0 otherwise.
DRUGS	Drug use d.v.; *DRUGS* = 1 if respondent ever uses drugs such as cocaine, heroin, barbituates, amphetamines, or LSD; 0 otherwise.
POT	Marijuana use d.v.; *POT* = 1 if respondent currently uses marijuana, 0 otherwise.
RELIGION	Religion d.v.; *RELIGION* = 1 if respondent attends religious services at least once a month, 0 otherwise.
JOB	Employed d.v.; *JOB* = 1 if respondent was working most of the last week, 0 otherwise.
SCHOOL	School d.v.; *SCHOOL* = 1 if respondent was going to school most of the last week, 0 otherwise.
NOJOBSCHOOL	Neither employed nor in school d.v.; *NOJOBSCHOOL* = 1 if respondent was neither employed nor in school most of the last week, 0 otherwise.
PROBT	Number of months on probation during the time-line period.
JAILT	Number of months in jail during the time-line period.
GANG	Gang membership d.v.; *GANG* = 1 if respondent was a member of a gang or had friends in a gang, 0 otherwise.
CNBD	Crime in neighborhood d.v.; *CNBD* = 1 if respondent believed that crime and violence in the neighborhood was a serious problem, 0 otherwise.
CRIME-MO	Crime d.v.; *CRIME-MO* = 1 if respondent committed criminal acts in the past four weeks, 0 otherwise.
CRIME-YR	Crime d.v.; *CRIME-YR* = 1 if respondent committed criminal acts in the past year, 0 otherwise.
EXYGAP	Expected income gap, namely, (adjusted) expected annualized earnings from crime minus expected job income.
YSTREET	Relative rewards d.v.; *YSTREET* = 1 if respondent believes he could make more on the street than on a legitimate job, 0 otherwise.
CHANCE1	Criminal opportunities d.v.; *CHANCE1* = 1 if respondent has opportunities to make money illegally a few times a day.
CHANCE2	Criminal opportunities d.v.; *CHANCE2* = 1 if respondent has no chance at all to make money illegally.
BOS	Region d.v.; *BOS* = 1 if respondent lives in Boston, 0 otherwise.
CHI	Region d.v.; *CHI* = 1 if respondent lives in Chicago, 0 otherwise.
PHILA	Region d.v.; *PHILA* = 1 if respondent lives in Philadelphia, 0 otherwise.

Table 8.2 **NBER Sample Characteristics: Means and Standard Deviations within Sample Groups (standard deviations in parentheses)**

Independent Variable	Full Sample	Committed Crime in Past Month	Committed Crime in Past Year
Personal Characteristics			
AGE	19.1	19.8	19.6
	(2.6)	(2.6)	(2.6)
EDUC	10.8	10.8	10.7
	(1.6)	(1.4)	(1.4)
MARRIED	.036	.043	.043
	—*	—*	—*
DRINK	.59	.82	.82
	—*	—*	—*
DRUGS	.03	.16	.14
	—*	—*	—*
POT	.36	.71	.68
	—*	—*	—*
RELIGION	.33	.22	.23
	—*	—*	—*
Labor Market Status			
JOB	.26	.24	.25
	—*	—*	—*
SCHOOL	.40	.22	.25
	—*	—*	—*
NOJOBSCHOOL	.34	.55	.50
	—*	—*	—*
Earnings (month)	$394.80[a]	$381.66[a]	$379.10[a]
	(339.65)	(342.78)	(337.97)
Expected Earnings (year)	$4,366.44	$3,834.15	$4,013.47
	(4,265.12)	(4,646.74)	(4,837.57)
Total Income	$2,837.31[a]	$3,961.61[a]	$3,832.55[a]
	(3,762.57)	(4,809.74)	(4,612.21)
Crime-Related Background			
Probation Dummy Variable	.07	.20	.21
	—*	—*	—*
Probation Months (Conditional)	6.7	7.1	6.9
	(4.4)	(4.2)	(4.3)
PROBT	.04	1.5	1.4
	(2.0)	(3.4)	(3.4)
Jail Dummy Variable	.03	.09	.08
	—*	—*	—*
Jail Months (Conditional)	4.2	4.2	4.0
	(3.5)	(3.3)	(3.1)
JAILT	.01	.03	.03
	(.84)	(1.5)	(1.4)

Table 8.2 (continued)

Independent Variable	Full Sample	Committed Crime in Past Month	Committed Crime in Past Year
GANG	.08 —*	.17 —*	.16 —*
CNBD	.39 —*	.52 —*	.52 —*
Criminal Activity:			
CRIME-MO (past month)	.15 —*	1.0 —*	.97 —*
Number of Crimes (past month)	.24 (.70)	1.6 (1.1)	1.2 (1.7)
Crime Income (past month)	$35.34[a] (219.93)	$254.98[a] (540.76)	$259.34[a] (546.55)
CRIME-YR (past year)	.19 —*	.77 —*	1.0 —*
Number of Crimes (past year)	.33 (.91)	1.9 (1.4)	1.2 (1.7)
Crime Income (past year)	$252.17 (1,551.04)	$1,568.77 (3,715.61)	$1,354.52 (3,383.69)
Expected Crime Income (week)	$441.25[a] (793.55)	$593.94[a] (939.39)	$601.43[a] (903.41)
EXYGAP	$-1,899.25[b] (5,773.49)	$-601.25[b] (6,992.86)	$-754.69[b] (6,927.00)
YSTREET	.28 —*	.48 —*	.48 —*
CHANCE1	.20 —*	.41 —*	.40 —*
CHANCE2	.43 —*	.09 —*	.11 —*
Location			
BOS	.32 —*	.30 —*	.32 —*
CHI	.34 —*	.29 —*	.28 —*
PHILA	.34 —*	.41 —*	.40 —*
Sample Size	2,358	349	439

[a]Nonresponses were set as missing values and not used in calculating means, that is, the sample size differs from that stated.

[b]Nonresponses to the expected crime income question were set equal to zero.

*Standard deviations of 0-1 dummy variables were omitted because they could be calculated from their fraction m in the sample, where the standard deviation is $(m - m^2)^{0.5}$.

Table 8.3 Criminal Activity in the NBER Sample, by Age

Age, in Years	*CRIME-MO*	Average Monthly Crime Income ($)[a] (Conditional)	*CRIME-YR*	Average Yearly Crime Income ($)[a] (Conditional)
15[b]	0	0	0	0
16	.079	249.38	.124	761.84
17	.128	267.66	.173	1083.74
18	.155	182.96	.185	880.25
19	.152	250.29	.184	2173.76
20	.173	335.24	.206	1601.33
21	.144	190.09	.191	840.03
22	.227	369.51	.271	2285.62
23	.158	181.88	.194	2324.31
24	.223	212.89	.250	1351.20

[a]The crime income figures are conditional on committing crime in the particular time period.

[b]Figures for this age group are not reliable because there was only one 15-year-old in the sample.

influence. This result does not imply that age is not an important determinant of crime, only that age variations within the narrow age range of this sample do not play a pivotal role.

Such a finding is not unprecedented in the crime literature. In a recent study of arrests rates that adjusted for a number of factors, such as differing sizes of offender populations, Blumstein and Cohen (1979) reached a similar conclusion. Although their sample of arrestees was not a random sample from the population, it is not clear a priori how their analysis of the criminal histories of these individuals biased the age-crime results. Blumstein and Cohen found that arrest rates for robbery, aggravated assault, larceny, auto theft, and weapons offenses exhibited no trends, but that arrest rates for burglary, narcotics, and the residual "all other" offense category actually rose with age.

The NBER sample results may provide a more accurate picture of the age-crime linkage than do the aggregative arrest statistics. Moreover, the NBER crime mix is quite different from that in the FBI Uniform Crime Reports. The bulk of the reported crimes in the NBER sample are for numbers and gambling and for drug sales. Since these offenses do not drop off as sharply with age as do, for example, car thefts, it is not surprising that the conventional age effects do not hold.

Two other personal background factors, total years of schooling (*EDUC*) and marital status (*MARRIED*), also display no substantial differences across the entire NBER sample and the crime subsamples in table 8.2. The young men surveyed averaged just under an eleventh grade education, and few of them were married, irrespective of their crime status. The variable *EDUC* shows little variation across the sample, as one would expect in view of legal constraints on schooling.

A series of personal characteristic variables were included as proxies for crime-related personal attributes.[6] There was a higher concentration of crime among those who drank (*DRINK*) or who smoked marijuana (*POT*), but the greatest relative disparity was in the use of more serious drugs (*DRUGS*), which was about five times as frequent among criminals as among the entire sample. Church attendance (*RELIGION*) also was negatively related to illegal activity.

The incentive to engage in crime depends in part on the individual's overall labor market status. An individual can choose to participate in crime as his principal pursuit, to augment schooling or a job with crime, or to engage in no criminal acts. Labor market status is consequently an endogenous part of the crime decision; but given any particular status (such as being in school), that status will also affect the incentives to engage in crime. The results show that a respondent who engaged in crime was almost as likely to be employed (*JOB*) as one who did not. Much more variation is displayed based on whether the respondent was in school (*SCHOOL*) or neither employed nor in school (*NOJOBSCHOOL*).

These general relationships suggest that it is school attendance rather than the level of schooling that is the more salient determinant of criminal behavior among young black men. Employment does not appear to play a key direct role in reducing crime; it does, however, play an indirect role by moving one out of the high crime category of youths who were neither employed nor in school.

It should be emphasized, particularly with respect to the *SCHOOL* variable, that these variables capture not only the influence of educational activities on crime, but also the fact that respondents who chose to attend school had quite distinct personal characteristics that were highly correlated with their decision to self-select into the in-school group. The subsequent empirical results will therefore overstate the likely crime reduction to be obtained by increasing school attendance.

The labor market earnings for all groups in the sample who responded to the wage questions were roughly the same, about $400 per month. Since the group of nonrespondents (those who did not answer the questions) included many individuals without a job, this figure overstates the average earnings across the entire sample. The total expected earnings figure assigns to each respondent his actual wage (calculated on an annual basis using hours data). Those without a job were assigned their expected wage rate.[7] This variable is intended to measure labor market opportunities. In general, there is only a minor difference in criminal status by the level of expected market earnings, which were about 10 percent less among criminals than among the full sample.

The total income figure represents actual income from all sources rather than prospective income. Since many who did not engage in

crime were in school, the total income for the full sample was substantially below their expected earnings. Respondents who engaged in crime had higher overall earnings (crime income plus legitimate earnings) than noncriminals, with a gap of roughly $1,000, or about one-third of the average income of the full sample. The criminals thus were not the most impoverished members of the sample but instead appear to have been among those who displayed the greatest initiative with respect to earning income in a variety of ways.

The next set of variables in table 8.2 measures factors quite closely linked to criminal experience. The probation dummy variable represents the fraction in the sample who had been on probation during the past year. This group constituted roughly one-fifth of those who had committed crime. Conditional on being on probation during the timeline period (13 months), the average probation time was seven months. The probation variable used in the statistical analysis is the actual probation time (*PROBT*), which assumes a value of zero if the respondent was not on probation during the period covered by the time line.

The jail variables are defined similarly and are also designed to measure the degree of recidivism in the sample. Whereas 3 percent of the entire sample had been in jail during the past year, the criminal subsamples contained almost triple this percentage. Among those who had been in jail, the average amount of time spent in jail during the timeline period was about four months. The jail-time (*JAILT*) variable that will be used in the empirical work is the unconditional version of the variable, that is, it assumes a value of zero for respondents who had not been in jail.

The final two crime-related variables may partly reflect the respondent's current decision to engage in crime. First, *GANG* is a dummy variable for whether or not the respondent was a gang member or had friends in a gang. Those who had committed crime were more than twice as likely to meet this criterion as the sample members as a whole. *GANG* is not completely exogenous to the crime decision. An individual may associate with gang members or join a gang to enhance his criminal opportunities. Any prior commitment to engage in criminal acts increased the endogeneity of this variable.

Second, the degree to which the respondent regarded crime in the neighborhood as a serious problem (*CNBD*) was also strongly linked to criminal behavior. This variable may capture greater criminal opportunities and lax criminal enforcement, each of which should boost crime rates. It is also possible that those who committed crime blamed their acts on the alleged prevalence of crime in their neighborhood. To allow for this possibility, the empirical analysis will treat this variable as if it were at least partially endogenous, as in the case of the *GANG* variable.

The variables in the next section of table 8.2 pertain to the degree of criminal activity. They play a central role throughout the analysis. The first issue, which also serves as the basis for the crime subsample stratification, is whether or not the individual engaged in crime. These rates are determined on the basis of self-reported criminal activity. The respondents were asked whether they had participated in any of ten specified criminal activities (such as having "sold or fenced stolen goods"), one of which was "any other illegal activities," The crime participation variables (*CRIME-MO* and *CRIME-YR*) have a value of one if the individual had participated in any of the types of crime during the past month or the past year, respectively. The crime participation rates observed were .15 for *CRIME-MO* and .19 for *CRIME-YR*. The similarity of these figures suggests that crime is a persistent activity of a small group of the sample rather than a random event; if a random event, the past-year crime rate would have been .86.

A different possibility is that the same individuals who were willing to admit to crimes committed in the past month were those willing to discuss their criminal activities over the past year. Suppose, for example, that half of the sample had given a negative response to all of the crime questions, regardless of their actual criminal activities. The observed .15 crime participation in the past month would have been generated if 30 percent of the group willing to discuss crimes had admitted to the crimes, and the .19 annual figure would have been generated if 38 percent of that group had committed crime in the past year. As in the reported data, 79 percent of those who had committed crime in the past month committed crime in the past year. A potential bias arises only if the group unwilling to discuss criminal involvement includes a disproportionately different share of persistent criminals.

Ideally, the use of self-reported data should provide a more refined measure of criminal behavior than is obtained by using, for example, aggregative crime statistics for geographic areas. Particularly in the case of blacks, however, there is a severe underreporting problem. Hindelang, Hirschi, and Weis (1981) estimated that the degree of underreporting by blacks ranges from a factor of 2 to 4. Rather than only one-fifth of the NBER sample participating in crime, the actual amount therefore probably ranges from two-fifths to four-fifths.

This underreporting could potentially create difficulties for the subsequent empirical analysis. The study by Hindelang, Hirschi, and Weis suggests, however, that the behavioral equations for within-group behavior are not distorted; the principal effect of the biased reporting is to shift the constant term rather than the slope in the regressions.[8] Since the NBER sample includes only black youths, the problems that arise in cross-population analyses do not arise here.

When attempting to assess the overall level of criminal behavior, one should nevertheless be cognizant of the likely understatement of criminal activity. Adjustments other than the factor of 2 to 4 yield similar results. Wolfgang, Figlio, and Sellin (1972) estimated that among their sample of nonwhites the rate of juvenile delinquency (namely, police contact at some point in one's youth) was 50 percent. If the true crime rate for the NBER sample is 50 percent rather than the reported annual rate of 19 percent, the reporting bias factor is 2.6, which is just below the midpoint of the earlier range.

A final approach to estimating the actual crime rate of the sample is to use information on the frequency of jail terms for those sampled and to estimate the number of crimes that are likely to be associated with these levels of incarceration. To do this, I employed information for the Washington, D.C. area reported by Blumstein and Cohen (1979), particularly the crime categories in their analysis that were most similar to the NBER groupings. Each crime committed was estimated to have a probability .58 of being reported; if reported, a .15 chance of leading to an arrest; and if arrested, a .16 chance of a conviction that leads to serving time in jail.[9] For every jail term, the number of crimes can be roughly estimated as equalling 74. Three percent of the NBER sample, or 71 respondents, had done time in jail in the past year, implying that the entire sample committed an estimated 5,254 crimes.

If this estimate is valid, the number of crimes was 2.22 times the total sample size. To obtain an estimate of the total number of criminals, one must make some assumptions about the degree of repeated criminal activity. Each respondent who had committed a crime in the past month participated in an average of 1.9 crime categories. If there were no repeated crimes within a crime category during the past month, the underreporting factor would be 6. If crimes in each of these categories were committed twice monthly, an estimate of the total criminal population is three times as large as the reported number. This estimate is consistent with the earlier bias estimates, but is probably more speculative.

The implications of the reporting bias for the relative role of criminal activity is summarized in table 8.4. Based on reported results, crime income accounted for 9 percent of the sample's average income. With an underreporting factor of 3, which I will adopt as the best estimate, crime accounted for 23 percent of the sample's total income. These adjustments take into account only the bias in the frequency of self-reported crime. If the amount of criminal income is also understated, the role of crime income will be even greater.

The number of crime categories in which the respondent had committed crimes tends to be quite low, under two on both the past-month and past-year basis. Since the evidence presented in section 8.5 below

Table 8.4 Reporting Bias and the Role of Crime as an Income Source

Reporting Bias Factor	Average Crime Income ($)	Average Total Income ($)	Crime Income as Fraction of Total Income
1	252.17	2,837.31	.09
2	504.34	3,089.48	.16
3	756.51	3,341.65	.23
4	1,008.68	3,593.82	.28
6	1,513.02	4,350.33	.35

indicates that the respondents were not particularly specialized in their criminal pursuits, a likely explanation for this low number is that the respondents listed only their major criminal activities. One indication that they were selective in their listing is that the list of activities for the past year often did not include all the crime categories in which they individually had participated in the past month.

The two variables governing the relative attractiveness of crime are *YSTREET* and *EXYGAP*. The *YSTREET* variable takes on a value of one if the respondent believed that he could make more on the street than on a legitimate job, and a value of zero otherwise. This variable therefore provides a direct measure of the relative economic attractiveness of crime. Almost half of the criminal subsample had *YSTREET* values of one, whereas just over one-fourth of the full sample had those values. As anticipated, those who expected to make more from crime were more likely to commit crime. These responses may be biased, however, if those who committed crime were seeking to justify their behavior by claiming that crime was a more lucrative activity.[10] As discussed in section 8.4 below these responses are nevertheless broadly consistent with the respondents' crime income expectations.

The variable *EXYGAP* is intended to measure the extent of this discrepancy between actual and expected income. This variable is constructed by subtracting expected earnings in the labor market from expected crime income. The crime income component of the variable represents expectations over a week, which were converted to an annual basis. Criminal pursuits might offer substantial rewards over such a short period, since a person could pursue only his most attractive options. Crime income over the year will be much lower; and as anticipated, the expected crime income amounts exceeded the actual crime income levels for respondents who had recently engaged in crime. For example, those who had committed crime in the past month had an expected weekly crime income of $600 (or yearly income of $31,200), a figure much lower than their observed yearly crime income under $1,600.

For those who had committed crime in the past year, actual crime earnings exceeded expected earnings for a comparable period by a factor of 9.3. To adjust for the likely reduction in actual criminal opportunities over the year as compared with what the respondent might hope to reap in a single week, the expected crime income figures were divided by 9.3 before subtracting expected annual labor market earnings to obtain *EXYGAP*. Even with this adjustment, expected crime income was not too dissimilar from expected labor market earnings. The net effect was that *EXYGAP* had a value of about −1,900 for the entire sample and a much smaller gap for the criminal subsamples, as one might expect. Because of this adjustment, *YSTREET* is a more reliable measure than *EXYGAP* of the relative economic attractiveness of crime.

The economic incentive variables *YSTREET* and *EXYGAP* are strongly correlated with criminal opportunities. The two opportunity variables I will use pertain to whether the respondent had several opportunities a day to make money illegally (*CHANCE1*) and whether he had no such opportunities (*CHANCE2*). The intermediate responses are not included in these two variables.

Over half of all respondents had some criminal opportunities. These opportunities were strongly related to reported criminal activity, as two-fifths of those who had committed crime had several such chances a day. Since the opportunities could be generated by the respondent himself by, for example, cruising the streets or associating with criminals, *CHANCE1* and *CHANCE2* are very endogenous to the criminal participation decision. In the analysis below they will be treated as variants of a crime participation variable. When included as independent variables in the crime equations, *CHANCE1* and *CHANCE2* play a dominant role.

The final set of variables measures the distribution of the sample across the three locations: Boston (*BOS*), Chicago (*CHI*), and Philadelphia (*PHILA*). There were stark differences in reported offense rates across these cities. In 1981 criminal offenses per 100,000 inhabitants were 14,000 in Boston and 6,000 in Chicago and Philadelphia.[11] Boston had a much lower murder rate than the other two cities but a far higher rate of property crime, such as auto theft. These differences are not apparent in the NBER sample, whose Philadelphia respondents exhibited the greatest criminal propensity. If Boston's higher crime rate is due to a greater proportion of crimes committed by whites, the overall crime statistics may be consistent with the pattern in the sample.

8.3 The Crime Participation Decision

An individual will choose to engage in criminal behavior if the expected net rewards from doing so (that is, *EXYGAP*) are positive.

One component of this calculation is the legal penalties likely to be incurred as a result of criminal acts. Although the NBER survey did not ask all respondents what they perceived would be the legal sanctions for their criminal behavior, it did solicit detailed information on the perceptions of those who actually engaged in crime.

The prevailing view of the respondents coincided with the general findings of the crime deterrence literature. Quite simply, there is very little threat of legal sanctions for crime. Only one of the 320 individuals who responded to the enforcement questions viewed the chances of arrest, conviction, and going to prison as being high. In contrast, 60 percent of this subsample viewed each of these possible outcomes as being low.

Table 8.5 summarizes the legal sanction results. Almost three-fourths of the sample viewed the chance of arrest as being low. Conditional on being arrested, an even greater proportion viewed the chance of conviction as being low. And even if convicted, few respondents believed that there was much chance that they would go to prison. The strongest perceived consequence posed by the criminal justice system was the conviction-prison relationship, but even this linkage was quite weak.

The implications of legal sanctions for the respondents' daily lives was also not great. The final three columns in table 8.5 present different possible events following arrest or conviction, such as losing one's job. These questions were worded to generate hypothetical responses from individuals who had no job. The perceived chance of losing one's job was somewhat higher than any of the other risks in the table. The chance of losing one's wife or girlfriend if sent to prison were lower than the chance of losing one's job. By far the lowest risk was that of losing one's friends. Going to prison appears to have a negligible stigmatizing effect among the respondents' peers.

One would expect the law enforcement perceptions of the noncriminal subsample to be somewhat greater, since those who perceive substantial risks will select themselves into the noncriminal group. Nevertheless, unless there is a strong difference in perceptions, it is unlikely that expected legal penalties will be very large. This does not mean that law enforcement is irrelevant to the crime participation decision. Since most values of *EXYGAP* were close to zero, small variations in expected sanctions for crime may alter the decision.

The focus of this section will be on the determinants of respondents' decisions to commit crime during the past month or the past year. In each case, the probability of participating in crime is a function of a set of explanatory variables. Since the dependent variables are dichotomous, the equation to be estimated will not be a linear regression but a logit equation estimated by maximum-likelihood techniques.

Table 8.5 **Perceptions of Law Enforcement**

Perceived Enforcement Level	Chance of Arrest	Conditional Chance of Conviction	Conditional Chance of Prison	Conditional Chance of Losing Job If Arrested	Conditional Chance of Losing Wife or Girlfriend If Sent to Prison	Conditional Chance of Losing Friends If Sent to Prison
Low	.73	.79	.73	.70	.79	.92
Medium	.21	.13	.15	.15	.10	.04
High	.06	.08	.12	.15	.10	.03

Each equation includes a set of personal characteristics, crime-related background variables, and criminal incentive variables. In addition, the equations are run both with and without a set of four variables that are at least partially endogenous to the crime decision: *JOB, SCHOOL, GANG,* and *CNBD*. The semi–reduced form estimates that exclude these intervening linkages consequently capture the full direct and indirect effects of the explanatory variables. Including these variables allows an analysis of the influence of factors such as gang membership; but to the extent that gang membership is simply a mechanism by which the respondent chooses to engage in crime, it will lead to a reduction in the influence of the other variables.

Table 8.6 summarizes the logistic estimates for the *CRIME-MO* equations, and table 8.7 provides the *CRIME-YR* results. What is most striking about these results is the similarity in the two time periods, both in terms of the nature of the effects and their magnitude.

In each case, the three personal characteristic variables (*AGE, EDUC,* and *MARRIED*) had no statistically significant impact. In the case of *EDUC* and *MARRIED*, this pattern seems attributable to the lack of much variability in the sample. Most respondents had the same level of schooling and were unmarried. The age range considered may not be sufficiently broad to capture the dramatic drop in criminal activity during the twenties.

The next four variables, which measure crime-related characteristics, all had the expected effects. The most powerful influence was the use of hard drugs (*DRUGS*).[12] The variable that ranked next in importance was use of marijuana; the *POT* variable had roughly half the impact on crime as did *DRUGS*. Next in importance was the use of alcohol (*DRINK*), which had approximately three-fifths of the impact of *POT*. The least consequential variable was attendance at religious services (*RELIGION*), which had under half the impact of the *DRINK* variable. In each case the variable may be capturing not only the impact of the particular background factor but also omitted factors correlated with it.

The probation time (*PROBT*) and jail time (*JAILT*) variables both have comparable, positive effects on the propensity to commit crime. These effects do not imply that legal sanctions have no deterrent effect, nor do they necessarily suggest that prison experience leads to further crime activity. The high recidivism rate occurs in part because some respondents were, no doubt, hard-core criminals. The *PROBT* and *JAILT* variables may simply serve as a proxy for the crime-related variables that have not been included in the equation.

The two city dummy variables *CHI* and *BOS* are consistently negative, which suggests that the third city, Philadelphia, is more likely to generate crime, holding constant the other variables in the equation.

Table 8.6 **Logistic Parameter Estimates for Past-Month Crime Participation Equations (asymptotic standard errors in parentheses)**

Independent Variable	(1)	(2)	(3)	(4)
Intercept	−2.637	−2.491	−2.148	−2.174
	(.599)	(.608)	(.709)	(.703)
AGE	.012	.014	−.016	−.007
	(.030)	(.030)	(.034)	(.033)
EDUC	−.039	−.035	−.021	−.018
	(.047)	(.047)	(.049)	(.049)
MARRIED	−.079	−.158	.003	−.076
	(.034)	(.336)	(.345)	(.340)
DRINK	.617	.630	.532	.546
	(.170)	(.169)	(.172)	(.172)
DRUGS	1.946	1.976	1.908	1.947
	(.279)	(.276)	(.284)	(.281)
POT	1.178	1.210	1.132	1.167
	(.146)	(.144)	(.148)	(.146)
RELIGION	−.304	−.296	−.279	−.272
	(.155)	(.154)	(.158)	(.157)
PROBT	.013	.013	.012	.012
	(.003)	(.002)	(.003)	(.002)
JAILT	.012	.011	.009	.008
	(.006)	(.006)	(.006)	(.006)
CHI	−.276	−.332	−.288	−.339
	(.160)	(.158)	(.163)	(.161)
BOS	−.623	−.543	−.482	−.404
	(.160)	(.157)	(.167)	(.164)
YSTREET	.620	—	.577	—
	(.138)		(.140)	
EXYGAP	—	25E−6	—	20E−6
		(10E−6)		(11E−6)
JOB	—	—	−.574	−.578
			(.168)	(.167)
SCHOOL	—	—	−.619	−.569
			(.184)	(.186)
GANG	—	—	.904	.968
			(.199)	(.198)
CNBD	—	—	.385	.366
			(.134)	(.133)
−2 Log Likelihood	1,583.01	1,612.02	1,535.15	1,562.75

Table 8.7 **Logistic Parameter Estimates for Past-Year Crime Participation Equations (asymptotic standard errors in parentheses)**

Independent Variable	(1)	(2)	(3)	(4)
Intercept	−1.578	−1.481	−1.212	−1.297
	(.550)	(.557)	(.659)	(.650)
AGE	−.017	−.012	−.038	−.026
	(.028)	(.028)	(.032)	(.031)
EDUC	−.063	−.057	−.053	−.046
	(.044)	(.043)	(.045)	(.044)
MARRIED	.087	−.037	.148	.026
	(.317)	(.312)	(.321)	(.317)
DRINK	.683	.721	.610	.651
	(.153)	(.152)	(.155)	(.154)
DRUGS	2.091	2.100	2.058	2.069
	(.301)	(.298)	(.305)	(.302)
POT	1.094	1.120	1.053	1.080
	(.132)	(.130)	(.134)	(.32)
RELIGION	−.287	−.253	−.269	−.233
	(.140)	(.138)	(.144)	(.141)
PROBT	.016	.016	.015	.015
	(.003)	(.003)	(.003)	(.003)
JAILT	.011	.012	.009	.010
	(.006)	(.006)	(.006)	(.006)
CHI	−.318	−.376	−.336	−.387
	(.150)	(.147)	(.152)	(.150)
BOS	−.545	−.418	−.433	−.303
	(.146)	(.143)	(.152)	(.149)
YSTREET	.743	—	.704	—
	(.128)		(.130)	
EXYGAP	—	26E−6	—	21E−6
		(10E−6)		(10E−6)
JOB	—	—	−.431	−.460
			(.157)	(.155)
SCHOOL	—	—	−.478	−.432
			(.169)	(.170)
GANG	—	—	.939	.999
			(.190)	(.188)
CNBD	—	—	.368	.358
			(.124)	(.122)
−2 Log Likelihood	1,797.22	1,848.01	1,750.19	1,797.71

The size of these effects is relatively modest, roughly on the same order as that of *RELIGION* and *DRINK*.

The key variables of interest are *YSTREET* and *EXYGAP*. Each of these economic incentive variables exerts the expected positive effect on criminal behavior. The size of the influence is roughly the same in all cases, though the past-year effects are a bit larger than the past-month effects. The inclusion of the four partially endogenous variables reduces the role of these economic incentive variables somewhat, in part because the crime decision affects such intervening variables as the decision to work. In terms of the size of the coefficients, the influence of *YSTREET* is comparable to that of *DRINK*, but is much less important than, for example, drug use.

This comparison is somewhat misleading, since *YSTREET* has a mean value of .28, whereas *DRUGS* has a mean value of .03. A better measure of a variable's contribution to the crime probability is its mean value multiplied by the size of the coefficient. By that measure, *YSTREET* is of greater consequence than *DRUGS*. The relative importance of the other variables changes as well, with the ranking of the impact of the personal background factors being, in descending order of importance *POT*, *DRINK*, *RELIGION*, and *DRUGS*. The impact of *YSTREET* lies between that of *DRINK* and *RELIGION*. The *EXYGAP* coefficient multiplied by the mean *EXYGAP* difference between the criminal and noncriminal subsamples reflects a much smaller impact, in part because of the greater measurement error associated with this variable.

The differential earnings from crime and legitimate occupations are a significant, but by no means dominant determinant of criminal behavior. Using the results for *CRIME-YR* in equation (1), one can calculate the overall crime propensity, if the expected crime income was below the expected labor market income, as being .16. Consequently, .03 of the .19 rate of criminal activity is accounted for by *YSTREET*. Although *YSTREET* accounts for only one-sixth of crime participation, the intensity of crime may be affected by *YSTREET* as well. This possibility will be explored in section 8.4 below. Another indicator of the impact of *YSTREET* on crime is that a *YSTREET* value of one instead of zero boosts the probability of engaging in crime from .16 to .29, or almost double. Among respondents who believed the economic rewards from crime were greater than those from a job, economic incentives played a powerful role in their decision to engage in crime.

Although relative earnings from legitimate and illegitimate pursuits did have a significant effect on criminal behavior, the total level of other income sources (such as welfare payments and an allowance from family) never had a statistically significant effect in any of the crime equations and was therefore not included in the final versions of the

equations. In this sample it is not poverty *per se* that drives criminal behavior but the relative economic rewards from crime.

The final set of variables embodies influences that will capture in part the mechanism for the crime participation decision. As one might expect, respondents were less likely to engage in crime if currently employed (*JOB*), thus reinforcing the earnings influence in *YSTREET* and *EXYGAP*. The joint influence of *YSTREET* and *JOB* was roughly one and one-half times the influence of *YSTREET* alone, indicating that relative economic incentives had a more powerful effect than did current employment. Educational level did not affect crime, but being in school did, since *SCHOOL* had roughly the same coefficient as *JOB*. At least in terms of the crime participation decision, what matters most is whether one is gainfully occupied either in school or at a job. A respondent's status in one or the other of these two groups was not of great consequence.

Respondents' peers and criminal environment also played a major role. Gang membership or having friends in a gang was especially important. In terms of their effect on criminal propensity, *GANG* and *POT* had roughly comparable coefficients. The prevalence of a serious problem of crime in the neighborhood (*CNBD*) had a much smaller coefficient than did *GANG*; but since neighborhood crime was much more common than gang membership, the overall contribution of *CNBD* to observed crime levels was greater.

8.4 Crime Income

Since the central economic issue in this discussion is the relationship between the relative rewards from crime and criminal behavior, a fundamental relationship to be investigated here is the nature of the factors that affect both actual and expected rewards from crime. Total crime income is also a fundamental concern, because it reflects both the frequency and intensity of criminal behavior.

The first of these issues I will consider is the determinants of *YSTREET, EXYGAP, CHANCE1,* and *CHANCE2*. What personal characteristics are most likely to lead an individual to believe that criminal pursuits are more lucrative than a legitimate job and thus to seek out criminal opportunities? The equations reported in table 8.8 address this issue.

The two personal characteristics *AGE* and *EDUC* should boost expected market earnings, since these are the principal human capital variables. *AGE* increases expected crime earnings by greater amount than *EDUC* does, since the coefficients in the *EXYGAP* and *YSTREET* equations are always positive. *EDUC* somewhat surprisingly does not have a negative effect on relative criminal and legitimate earnings. The

Table 8.8 **Determinants of the Economic Incentives to Commit Crime (standard errors in parentheses)**

Independent Variable	*YSTREET*	*EXYGAP*	*CHANCE1*	*CHANCE2*	*YSTREET*
Intercept	−3.176	−5468.10	−1.564	−.988	−3.421
	(.512)	(1180.90)	(.562)	(.476)	(.535)
AGE	.053	151.90	.033	.064	.079
	(.025)	(59.00)	(.028)	(.024)	(.027)
EDUC	.060	2.39	.056	−.011	.057
	(.037)	(82.15)	(.04)	(.032)	(.040)
DRINK	.473	669.94	.369	−.631	.330
	(.116)	(248.36)	(.131)	(.100)	(.120)
DRUGS	.701	1781.42	.652	−.459	.493
	(.243)	(496.20)	(.240)	(.255)	(.247)
POT	.535	432.29	.485	−.571	.357
	(.104)	(233.36)	(.117)	(.098)	(.106)
CHI	−.770	−458.17	−.579	.499	−.648
	(.128)	(271.12)	(.139)	(.107)	(.134)
BOS	.340	242.52	.032	−.220	.335
	(.116)	(280.52)	(.130)	(.113)	(.121)
PROBT	.003	7.15	.004	.005	.002
	(.002)	(5.78)	(.002)	(.003)	(.002)
JAILT	.005	13.11	.012	−.012	.001
	(.005)	(13.63)	(.005)	(.008)	(.005)
GANG	.554	570.26	.257	−.551	.471
	(.174)	(426.05)	(.188)	(.183)	(.181)
CNBD	.137	410.78	.360	−.267	.040
	(.100)	(230.80)	(.110)	(.092)	(.105)
JOB	.122	1224.14	−.399	.241	.249
	(.125)	(298.03)	(.138)	(.120)	(.131)
SCHOOL	−.103	−3185.23	−.634	.397	.063
	(.139)	(311.96)	(.151)	(.126)	(.144)
CHANCE1	—	—	—	—	.833
					(.125)
CHANCE2	—	—	—	—	−.938
					(.125)
−2 Log Likelihood	2519.35	NA	2163.30	2933.30	2354.54
$\bar{R}^2$	NA	.17	NA	NA	NA

same types of skills that enhance productivity in a legitimate job may reap criminal rewards as well.

Having been in jail or on probation does not have a consistently strong effect, but the crime-related characteristics *DRINK*, *DRUGS*, and *POT* do. These variables capture in part the role of personal characteristics correlated with the particular activity. Drinking does not

make a better criminal and will usually worsen the drinker's labor market prospects. All three activities were positively related to criminal opportunities. The youths who drank or took drugs were generally more apt to engage in illegitimate pursuits and more likely to be exposed to illegitimate earnings opportunities.

The criminal environment is also of consequence. For *YSTREET*, being a gang member or knowing people in a gang (*GANG*) has a strong effect on the expected criminal earnings. The existence of a severe problem of crime in the neighborhood (*CNBD*) also has a significant influence for both *YSTREET* and *EXYGAP*.

These criminal environment and crime-related background results are to be expected. The more surprising finding is that the same variables (*AGE* and *EDUC*) that are usually linked most strongly to labor market earnings also boost expected criminal earnings by at least the same amount. This is not to say that older and better-educated youths will, on balance, be more prone to crime. Other variables correlated with those human capital variables, such as whether those youths currently hold a job, also enter. The net direct effect in the crime participation equations in section 8.3 was not significant, but these variables exert a significant positive indirect influence on crime through *YSTREET* and *EXYGAP*.

The most striking difference in the *EXYGAP* and *YSTREET* equations is the effect of the *JOB* and *SCHOOL* variables. These status variables have no effect on *YSTREET* but exert a powerful influence on *EXYGAP*. Respondents who were employed were more likely to have positive *EXYGAP* values, whereas those in school were more likely to have strongly negative *EXYGAP* values. Although the schooling result is not unexpected, the positive employment effect may reflect overoptimistic crime income estimates, particularly in light of the weaker *JOB* effect on *YSTREET*.

The determinants of the frequency of criminal opportunities, *CHANCE1* and *CHANCE2*, are not unlike the relative income results. What is most striking is that many of the effects are quite strong. Frequent criminal opportunities (*CHANCE1*) are strongly and positively related to *DRINK*, *DRUGS*, *POT*, and *CNBD*, and negatively related to living in Chicago and being in a job or at school. All of these variables have the opposite effect for the variable measuring no illegal earnings opportunities (*CHANCE2*). The performance of the two opportunity variables gives at least as strong a result in the expected direction as do the crime participation equations. These findings alone suggest that criminal opportunities are at least partially endogenous.

In the final *YSTREET* equation in table 8.8, *CHANCE1* and *CHANCE2* are included to indicate the extent of the interrelationships. The positive effect of *CHANCE1* and the negative effect of *CHANCE2* are dominant

variables in the equation; the coefficients of these variables dwarf those of all other dummy variables included in the equation. The criminal opportunity variables appear to represent a mixture of the crime participation and relative crime income variables. Although the determinants of *CHANCE1* and *CHANCE2* are of interest, these variables do not appear as well suited to serve as explanatory variables in a crime activity equation.

The determinants of actual levels of crime income follow a pattern not unlike the determinants of expected income. For the purposes of this analysis, the dependent variable is the natural logarithm of crime income for all respondents in the sample.[13] This variable thus captures both the decision to commit crime and the extent of criminal activity. This comprehensiveness is a mixed blessing: although it offers a broad perspective on crime income, it raises the potential problem of selectivity bias. Since crime income will be received only by those who choose to engage in crime, the equation can be viewed as suffering from specification error, such that the missing variable pertains to the sample-selection process governing crime participation.

To adjust for these problems, I include a selectivity variable (inverse Mills ratio) based on a probit estimate of the crime participation equation for the past year.[14] The past-month participation equation is not used because of convergence problems. The crime participation equations, analogous to equation (1) in tables 8.6 and 8.7, are estimated using a weighted nonlinear least-squares probit method. These results are then used to construct the distribution function for respondent i's crime participation (CP_i), where $F(CP_i)$ is the cumulative distribution of a standard normal variable and $f(CP_i)$ is the corresponding density function. The selectivity variable is $-f(CP_i)/F(CP_i)$ for those who engaged in crime and $f(CP_i)/[1 - F(CP_i)]$ for those who did not.

Table 8.9 presents the regression results for both time periods. The regressions including the selectivity variables are equations (4) and (8). The other equations differ according to the inclusion of partially endogenous variables and the relative crime versus legitimate earnings variables. Most of the principal effects are consistent across all of the equations.

The series of crime-related personal characteristics has a positive effect on crime income. Respondents who drank, smoked marijuana, or took drugs tended to have more crime income, with the effect of drugs being particularly strong. These variables reflect not only the youth's proclivity toward crime but also his opportunities for crime. Those who took drugs were more likely to have the opportunity to sell drugs and earn criminal income. Similarly, those who attended religious services (*RELIGION*) were less likely to have criminal contacts and may have had the kind of family background that led them to place a

Table 8.9 **Regression Equations for Natural Logarithm of Crime Income (standard errors in parentheses)**

Independent Variable	Past-Month Income Equations				Past-Year Income Equations			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	−5.98	−5.67	−4.91	−5.27	−5.78	−5.33	−4.81	−5.35
	(.64)	(.65)	(.77)	(.67)	(.80)	(.83)	(.96)	(.71)
AGE	.03	.02	−.03	−.01	.02	.01	−.03	−.01
	(.03)	(.03)	(.04)	(.03)	(.04)	(.04)	(.05)	(.04)
EDUC	−.09	−.08	−.05	−.02	−.07	−.05	−.03	.02
	(.05)	(.05)	(.05)	(.04)	(.06)	(.07)	(.06)	(.05)
MARRIED	−.90	−.98	−.77	−.75	−.41	−.51	−.26	−.20
	(.40)	(.40)	(.39)	(.34)	(.50)	(.50)	(.49)	(.36)
DRINK	.32	.37	.22	.13	.61	.67	.47	.28
	(.16)	(.16)	(.16)	(.13)	(.20)	(.20)	(.20)	(.15)
DRUGS	2.45	2.50	2.45	2.61	3.38	3.44	3.37	3.69
	(.32)	(.32)	(.31)	(.27)	(.40)	(.40)	(.39)	(.29)
POT	1.20	1.29	1.14	.77	1.47	1.58	1.39	.77
	(.15)	(.15)	(.15)	(.13)	(.19)	(.19)	(.19)	(.14)
RELIGION	−.46	−.44	−.36	−.43	−.54	−.52	−.44	−.56
	(.15)	(.16)	(.15)	(.13)	(.19)	(.20)	(.19)	(.14)
PROBT	.03	.03	.03	.03	.04	.04	.04	.04
	(4E−3)	(4E−3)	(4E−3)	(3E−3)	(4E−3)	(5E−3)	(5E−3)	(3E−3)
JAILT	.02	.03	.02	.02	.02	.03	.02	.02
	(9E−3)	(9E−3)	(9E−3)	(8E−3)	(.01)	(.01)	(.01)	(8E−3)

CHI	.15	.03	.12	.09	−9E−3	−.16	−.03	−.07
	(.18)	(.17)	(.17)	(.15)	(.22)	(.22)	(.22)	(.16)
BOS	−.43	−.36	−.21	−.22	−.32	−.23	−.06	−.08
	(.18)	(.18)	(.18)	(.16)	(.22)	(.22)	(.22)	(.17)
YSTREET	1.11	—	1.05	1.18	1.45	—	1.34	1.58
	(.17)		(.17)	(.14)	(.21)		(.21)	(.15)
EXYGAP	—	4E−5	—	—	—	5E−5	—	—
		(1E−5)				(2E−5)		
JOB	—	—	−1.00	−1.17	—	—	−1.10	−1.38
			(.19)	(.16)			(.24)	(.18)
SCHOOL	—	—	−.95	−.93	—	—	−1.02	−.99
			(.20)	(.17)			(.25)	(.18)
GANG	—	—	1.20	1.10	—	—	1.89	1.77
			(.27)	(.23)			(.34)	(.25)
CNBD	—	—	.32	.37	—	—	.46	.52
			(.15)	(.13)			(.18)	(.14)
Selectivity Variable	—	—	—	1.32 (.05)	—	—	—	−2.22 (.05)
$\bar{R}^2$	.17	.15	.19	.39	.18	.17	.21	.56

higher value on legal sanctions, thus decreasing their proclivity toward crime.

Past criminal experiences also exert a positive influence on crime income. Both time on probation and time spent in jail over the past year are positively related to current crime income. The stronger of the two effects is probation time, as each month of probation time has roughly double the impact of jail time. These variables are probably serving as proxies for crime-related personal characteristics omitted from the equation.

The two relative crime-versus legitimate income variables have the expected positive effect on criminal behavior. Respondents who expected to make more from crime than from a legal pursuit tended to have more crime income. These economic incentive effects are a fundamental determinant of crime income levels. A youth who viewed crime income as more lucrative than job income had an expected crime income level $78 larger than the expected monthly crime income amount of $26 for the youth who did not believe that crime income was more lucrative. On an annual basis, the perception that crime income was more profitable led to a crime income level of $714, whereas the opposite perception led to a $167 crime income level.[15]

In terms of the mean effects of *YSTREET*, the perception of the economic attractiveness of crime accounted for 26 percent of past-month crime income and 34 percent of past-year crime income. These effects are roughly twice as large as the influences on the crime participation rate. This differential impact suggests that the economic incentives for criminal behavior affect both the crime participation decision and the intensity of criminal behavior, but the larger of these two effects is on intensity.

Since many youths in the sample had minor criminal involvement (such as in numbers and illegal gambling), the crime participation results include many crimes that were not as strongly driven by the income opportunities they offered. Gambling, for example, may provide substantial recreational value. When crime intensity is measured by using a crime income variable, the economic incentives to commit crime play a more dominant role.

The partially endogenous variables also continue to be instrumental in generating crime income. Being employed and being in school have statistically indistinguishable effects. The size of these coefficients is about the same for *YSTREET*. The contribution of *JOB* in explaining crime income is about the same as the contribution of *YSTREET*, whereas the mean effect of *SCHOOL* is much greater. The most important way that any particular youth can reduce his crime income is for him to avoid the crime-prone state of being out of school without a job. Gang associations also have a positive impact on criminal be-

havior, as does the comparatively weaker influence of crime problems in the neighborhood.

Equation (9) reports the results of exploratory regressions including *CHANCE1* and *CHANCE2*. Crime income bears a strongly positive relationship to frequent opportunities to make money illegally and a negative relationship to not having such opportunities. These variables are dominant in part because they not only affect the youths' crime decisions but are also influenced by these decisions. Inclusion of these variables eliminates roughly half of the impact of *YSTREET* because of the substantial overlap between *YSTREET* and the crime opportunity variables. For example, only 5 percent of respondents who believed they could make more money on the street believed that they had no opportunities to make money illegally (*CHANCE2*). Since *CHANCE1* and *CHANCE2* are highly endogenous to the crime decision, they are not included in the subsequent equations.

Equations (4) and (8) report results including the selectivity variable (adjusted for heteroscedasticity). Because of convergence problems with the past-month crime participation equation, the selectivity variable included pertains to past-year crime participation. In each case, the effect of the variable on the other coefficients is very modest. The principal coefficient of interest, *YSTREET*, is unchanged in the past-month income equation and raised by almost 20 percent in the past-year income equation. The latter estimate implies that the mean effect of *YSTREET* accounts for 40 percent of all annual crime income.

8.5 A Profile of Criminal Behavior

The crime participation variables that have been analyzed thus far were based on a series of survey questions that addressed the nature of the respondents' criminal involvement. These self-reported measures are of interest because they indicate to what extent official statistics, such as arrest rates, provide an accurate view of the extent of criminal behavior among black youths. Moreover, in conjunction with the data on earnings from crime, we can construct measures of the levels of income that arise from different types of criminal activity.

Table 8.10 summarizes the pertinent national arrest statistics for 1981. The second column lists the distribution of city arrests of youths 15 to 24 years old. The chief arrest category is larceny-thefts, which accounts for two-fifths of all arrests. Drug violations and burglary each account for almost one-fifth of all arrests, with the remainder spread over the other nine crime categories.

The third column presents a somewhat different perspective on youth crime, indicating the fraction of all city arrests accounted for by arrests of youths aged 15 to 24. For many crime categories—larceny-theft,

Table 8.10 National Data on City Arrests for Selected Offenses, 1981

Offense Category	Fraction of Arrests of 15- to 24-Year-Olds	15- to 24-Year-Olds as Fraction of Arrests In Offense Category
Larceny and Theft	.39	.59
Drug Violations	.18	.62
Burglary	.17	.72
Aggravated Assault	.06	.44
Robbery	.05	.64
Motor Vehicle Theft	.04	.67
Fencing Stolen Property	.04	.59
Fraud	.03	.35
Forgery and Counterfeiting	.02	.47
Gambling	.10	.23
Forcible Rape	.10	.49
Murder	.00	.44
Total	1.0	.58

Source: U.S. Department of Justice, Federal Bureau of Investigation, *Uniform Crime Reports, Crime in the United States* (Washington: GPO, 1982), 186–187, 191.

motor vehicle theft, burglary, robbery, stolen property, and drug violations—arrests of youths in that age range account for well over half of all arrests for that particular crime. Indeed, only in gambling and fraud does this age cohort fail to account for at least 44 percent of all arrests.

The extent of crime in the NBER sample of black youth is quite different, in large part because of differing rates of arrest across crime categories. Tables 8.11 and 8.12 present the distributions for past-month and past-year criminal activity. The second columns in the tables list the fraction of the entire sample in each category. Since crime was a pursuit of under one-fifth of the sample, none of these categories is particularly large. Only numbers (and other illegal gambling) is pursued by at least 10 percent of the sample.

More instructive are the third columns of the tables, which give the crime distribution conditional upon the individual engaging in criminal acts. Once again, numbers and illegal gambling play a dominant role, as about two-thirds of all who committed crimes were guilty of these two offenses. Since gambling accounted for only one percent of all city arrests of all youths aged 15 to 24 in 1981 (see table 8.10), the arrest rate for this activity must be extremely small.

About a third of the criminal subsample admitted to having been a drug dealer (that is, they sold marijuana or other drugs). This activity was about equally important on both a monthly and an annual basis. Many of the more serious crime categories were of greater consequence

Table 8.11 **Past-Month Criminal Activity**

Crime Category	Fraction of Sample Who Committed in in Past Year	Behavior Conditional on Crime Activity		
		Fraction Who Committed Category	Average Total Crime Income ($)	Weighted Average Crime Income ($)
Numbers	.10	.67	187.70	103.01
Fencing Stolen Goods	.03	.17	541.82	191.78
Drug Dealing	.05	.32	412.14	189.42
Burglary	.01	.05	478.24	134.56
Mugging	1E−3	.02	365.71	94.61
Shoplifting	.01	.09	596.67	149.03
Forgery	1E−3	.02	940.00	120.95
Con Games	.02	.16	427.34	133.84
Robbery	.01	.04	737.69	242.78
Other	.01	.05	1,110.00	443.51

Table 8.12 **Past-Year Criminal Activity**

Crime Category	Fraction of Sample Who Committed in in Past Year	Behavior Conditional on Crime Activity		
		Fraction Who Committed Category	Average Total Crime Income ($)	Weighted Average Crime Income ($)
Numbers	.12	.63	1,116.16	565.14
Fencing Stolen Goods	.03	.19	2,670.41	720.85
Drug Dealing	.07	.37	2,181.42	994.54
Burglary	.02	.11	2,708.23	760.78
Mugging	.03	.13	2,989.28	744.36
Shoplifting	.01	.03	1,869.23	373.17
Forgery	.01	.07	3,991.30	891.48
Con Games	.01	.03	1,318.18	337.67
Robbery	.03	.16	1,707.89	624.39
Other	.01	.07	4,900.58	1,928.98

on an annual basis than on a monthly basis. Burglary (burglary and entry, larceny, and auto theft), muggings (and purse snatching), forgery (cashed or forged stolen checks), and robbery (robberies, holdups, and stickups) were much more common on an annual basis, which suggests that participation in these crime categories is a more occasional activity. By a similar token, the share for less-severe crimes was lower on an annual basis. Shoplifting (or stealing from cars and trucks) and con games exhibited the steepest declines.

The final two columns of tables 8.11 and 8.12 report the crime income levels associated with each crime category. The information reported in the NBER survey was for the youth's total crime income rather than his crime income from each type of criminal act. The crime income figures in the fourth columns are unweighted; each crime category is credited with the youth's total crime income. This measure overstates the actual crime income from each form of crime. To adjust for this double-counting problem, the weighted income figures divide all crime income from several activities on a proportional basis (that is, total crime income divided by the number of criminal activities), leading to much lower crime income levels.

By far the most lucrative crime category was "other crimes" (namely, any other illegal activities), which yielded about $2,000 annually. Youths who were reluctant to admit to a specific crime were likely to fall into this category. To the extent that these youths committed more than one type of crime, the figures will overstate the earnings from any specific class of criminal acts. Nevertheless, since even the unweighted crime income figures for the "other crimes" category are higher than those for the specific crime categories, it is likely that many of the high-income criminals refused to admit to their specific crimes, despite the fact that they did disclose their crime income.

For the remaining crime categories the leading sources of income varied depending on the time period considered. For the past-month data, the five principal income-generating crimes were robbery, fencing stolen goods, drug dealing, shoplifting, and burglary. Only three of these categories appeared among the five leading past-year crime income sources: drug dealing, forgery, burglary, muggings, and fencing goods. The most prevalent criminal pursuit, numbers and other illegal gambling, was among the lowest income generators. This activity is popular because it not only is recreational but also poses a very low risk, as there are few gambling arrests among youths. The severity of the punishment for gambling is also low.

Since the respondents generally engaged in several criminal activities, one would expect to observe some interdependencies across activities. Youths who engaged in robbery, for example, might also have fenced stolen goods. For each crime category, tables 8.13 and 8.14 present the linkages between the activities within each time period.

Table 8.13 **Interrelationships Among Past-Month Criminal Activities**

Crime Category	Other Crimes in Which the Respondent Was Involved:									
	Numbers	Fencing	Drug Dealing	Burglary	Mugging	Shoplifting	Forgery	Con Games	Robbery	Other
Numbers	1.0	.09	.20	.03	—*	.05	.02	.11	.02	.01
Fencing	.38	1.0	.40	.22	.05	.31	.09	.36	.14	.09
Drug Dealing	.42	.20	1.0	.08	.03	.11	.03	.22	.07	.05
Burglary	.42	.68	.47	1.0	.16	.37	.05	.42	.21	.10
Mugging	.12	.38	.38	.38	1.0	.38	.12	.50	.62	.25
Shoplifting	.36	.58	.39	.23	.10	1.0	.10	.32	.16	.12
Forgery	.57	.71	.43	.14	.14	.43	1.0	.29	.14	.14
Con Games	.46	.38	.45	.14	.07	.18	.04	1.0	.12	.07
Robbery	.27	.53	.53	.27	.33	.33	.07	.47	1.0	.13
Other	.12	.31	.38	.12	.12	.25	.06	.25	.12	1.0

*Fewer than one percent of respondents.

Table 8.14 **Interrelationships Among Past-Year Criminal Activities**

Crime Category	Other Crimes in Which the Respondent Was Involved:									
	Numbers	Fencing	Drug Dealing	Burglary	Mugging	Shoplifting	Forgery	Con Games	Robbery	Other
Numbers	1.0	.15	.27	.06	.09	.02	.04	.02	.13	.05
Fencing	.51	1.0	.57	.37	.38	.12	.22	.07	.32	.09
Drug Dealing	.46	.28	1.0	.15	.18	.06	.12	.04	.22	.07
Burglary	.34	.60	.50	1.0	.40	.14	.26	.04	.30	.10
Mugging	.45	.53	.52	.34	1.0	.19	.22	.09	.31	.14
Shoplifting	.47	.67	.67	.47	.73	1.0	.73	.13	.67	.13
Forgery	.33	.60	.67	.43	.43	.37	1.0	.20	.47	.13
Con Games	.54	.46	.54	.15	.38	.15	.46	1.0	.31	.23
Robbery	.50	.37	.50	.21	.26	.14	.20	.06	1.0	.14
Other	.45	.26	.38	.17	.28	.07	.14	.10	.34	1.0

Respondents who engaged in numbers and other illegal gambling had little propensity toward any other criminal activity, with the possible exception of dealing in drugs, which was a pursuit of roughly one-fourth of this group. Fencing stolen goods had much stronger linkages to drug dealing and to complementary crimes, such as mugging, shoplifting, burglary, and robbery. Drug dealers exhibited a fairly narrow crime pattern; they engaged in numbers and illegal gambling but otherwise did not exhibit high levels of concentration in the other crime categories.

The majority of burglars also fenced stolen goods. Drug dealing, numbers, muggings, and con games were also frequent pursuits of burglars. Most respondents who reported mugging were also fences and drug dealers. Particularly for the past-year data, shoplifting displays strong interdependencies, as at least two-thirds of all shiplifters were represented in each of five other crime categories. Those who committed forgery were very likely to fence stolen goods or deal in drugs, and the majority of those who were involved in con games or robberies also dealt in drugs and played numbers or engaged in illegal gambling (past-year data).

The picture that emerges is one of widespread criminal involvement among black inner-city youths. The criminal subsample was not engaged in highly specialized forms of crime. Illegal gambling and drug dealing, for example, were highly popular means for augmenting crime income from other sources. Rather than specialize in particular types of crime, the modus operandi appears to be that of taking advantage of a diversity of criminal opportunities as they arise. These opportunities are not random; otherwise the off-diagonal elements in the rows in tables 8.13 and 8.14 would be identical. Many of the discrepancies that appear in these supplementary criminal activities appear to be due to some specialization in terms of the nature of criminal behavior, as, for example, fencing stolen goods was very common among those who engaged in burglary, shoplifting, and mugging.

A comparison of criminal activities during the past month and during the past year is also feasible using the data on the NBER sample. This breakdown, provided in table 8.15, is instructive for two reasons. First, it provides a check on the consistency of individual responses. Second, it indicates which longer-term criminal pursuits are associated with the more continuous pursuits reflected in the past-month data.

The most extreme example of inconsistency in responses is that ten youths who reported having committed particular crimes in the past month did not admit to any criminal activity in the past month. Since seven of these ten aberrant responses were made by youths whose crime was playing numbers, this pattern reflects the more questionable criminality of minor gambling offenses.

Table 8.15 **Relationship Between Past-Month and Past-Year Crime Categories**

Past-Month Crime Categories	Conditional Fractions in Past-Year Crime Categories										
	Numbers	Fencing	Drug Dealing	Burglary	Mugging	Shop-lifting	Forgery	Con Games	Robbery	Other	No Crime
Numbers	.96	.13	.25	.06	.07	.02	.04	.03	.12	.03	—*
Fencing	.48	.97	.52	.34	.38	.10	.24	.09	.36	.09	0
Drug Dealing	.50	.28	.99	.15	.16	.08	.15	.05	.20	.02	0
Burglary	.42	.84	.47	1.0	.47	.16	.37	.05	.42	.10	0
Mugging	.12	.38	.38	.38	.62	.62	.75	.25	.38	.12	0
Shoplifting	.42	.68	.52	.42	.90	.19	.26	.10	.32	.13	—*
Forgery	.71	.71	.57	.14	.43	.14	.57	.86	.14	.14	0
Con Games	.54	.39	.54	.25	.27	.14	.21	.71	.86	.07	—*
Robbery	.27	.53	.53	.47	.47	.53	1.0	.07	.47	.07	0
Other	.31	.38	.38	.12	.31	.12	.12	.06	.25	.81	0
No Crime	.02	—*	.02	—*	—*	—*	—*	—*	—*	.04	.95

*Fewer than one percent of respondents.

The diagonal elements in table 8.15 reflect the fraction of people in each past-month crime category who committed a particular crime in the past year. Since the "last four weeks" is included within the "past 12 months," presumably these figures should all be 1.0. Nevertheless, only one crime category—burglary—meets this test, and only half of the categories have at least 90 percent overlap. In the most extreme case, shoplifting, only 19 percent of those who admitted to this crime during the past month admitted to it in the past year. One possible explanation for these discrepancies is that respondents may have reported only their principal criminal activities during the time period in question.

In some cases, there are no strong interdependencies. Youths who played numbers or gambled illegally in the past month had no strong criminal tendencies during the past year. The majority of fences were also drug dealers, but the reverse was not true; drug dealers mostly engaged in numbers and gambling. Fencing stolen goods was also a dominant pursuit of those who engaged in burglary, shoplifting, or forgery in the past year. Among the strongest cross-crime linkages were that 90 percent of all who shoplifted in the past month participated in muggings in the past year and that 86 percent of all forgers were involved in con games in the past year, while a comparable percentage of those who were involved in con games also committed robberies in the past year.

The last line of table 8.15 lists all the respondents' who reported having committed no crimes in the past month. Ninety-five percent of this group committed no crime in the past year. The three crime categories over which the remaining 5 percent were distributed were numbers, drug dealing, and "other crimes." These categories tend to represent less violent crimes than those of robbery and mugging, for example. In addition, there are not as many strong criminal interdependencies for these categories as there are for the other crime groups.

There consequently appear to be three principal groups. First, a majority of the sample reported no criminal activity. Second, there is a small criminal element involved in comparatively nonviolent crimes, such as drug dealing and numbers, on a periodic basis. Finally, most youth crime stems from a hard-core criminal group for whom crime is a persistent activity.

Because of the small sample size of many of the crime categories, it was not possible to perform a comprehensive statistical analysis of the determinants of all criminal pursuits. It is feasible, however, to present an analysis of the larger crime categories. Table 8.16 displays the logit results for four crime participation equations. Variables that played an insignificant role[16] in the preliminary analysis were omitted to ensure convergence in the maximum-likelihood procedure.

Table 8.16 **Maximum Likelihood Estimates of the Determinants of Participation in Crime Categories (asymptotic standard errors in parentheses)**

Independent Variable	Numbers Games (Year)	Numbers Games (Month)	Fencing (Year)	Drug Dealing (Year)
Intercept	−2.658	−2.904	−1.239	−4.327
	(.176)	(.190)	(1.01)	(.275)
AGE	—	—	−.207	—
			(.055)	
DRINK	.701	.599	.558	—
	(.175)	(.188)	(.361)	
DRUGS	1.063	.800	2.368	2.308
	(.261)	(.272)	(.315)	(.274)
POT	.769	.848	1.645	2.223
	(.150)	(.163)	(.327)	(.254)
RELIGION	−.396	−.341	—	−.318
	(.163)	(.174)		(.230)
PROBT	.010	.001	—	.009
	(.002)	(.002)		(.003)
CHI	−.440	−.325	—	−.333
	(.163)	(.173)		(.237)
BOS	−.958	−.894	—	−.491
	(.174)	(.184)		(.217)
YSTREET	.570	.607	.456	.902
	(.144)	(.153)	(.251)	(.188)
−2 Log Likelihood	1,486.87	1,339.00	565.94	856.69

The estimates for the two equations for participation in numbers games are quite similar. The variables *DRINK*, *DRUGS*, and *POT* all have the expected positive effect on participation in this crime. What is most striking is that the coefficients are all of similar magnitude, unlike the overall crime results. For such a minor and prevalent offense, the nature of one's crime-related personal characteristics does not play a major role. The positive impact of the economic incentives variable *YSTREET* is comparable to that in the aggregative crime equations.

The more serious crimes of fencing stolen goods and dealing in drugs are much more strongly linked to *DRUGS* and *POT*. The role of economic incentives appears to be particularly great in the case of drug dealing, where the *YSTREET* coefficient is about 50 percent larger than in most of the earlier results. This finding is consistent with the crime income results, which suggests that *YSTREET* is a more powerful determinant of intense criminal activity than of crime participation, such as numbers.

8.6 Behavior Conditional on Status

The findings for crime participation and crime income suggested that being in school or holding a job discouraged criminal behavior with roughly similar effects. The primary distinction was whether the respondent was in the relatively idle *NOJOBSCHOOL* state. Although the possibility that respondents' status shifted the intercept of the crime equations was considered, the nature of behavior as reflected in the coefficients may change as well. In this section I will address these differences in behavior in greater detail.

Table 8.17 summarizes the source of income and degree of crime participation conditional on the respondents' status. Those with a job had the highest annual earnings, over $5,000. Nevertheless, about a fifth of those who were employed also committed crime, but the magnitude of criminal earnings had little effect on the average income of those who worked. Because of the very large variance in crime income, however, some employed respondents obviously realized a substantial income supplement from crime. Those in school had very low labor market earnings or illegitimate earnings, and only one-eighth of this group participated in crime during the past year. Among those not then employed or in school, annual income was equally divided between crime income and job earnings. Approximately half of this group participated in crime during the past year.

Tables 8.18 and 8.19 present the *CRIME-MO* and *CRIME-YR* logit equation estimates. Because of the thinness of the subsamples, some variables were omitted from the equations because of convergence problems. For example, since only eight of the in-school population took drugs, the *DRUGS* coefficient could not be estimated for this subsample. The greatest difference from the previous findings is the performance of *YSTREET*. Both in terms of the magnitude of the coef-

Table 8.17 **Means of Income Sources, by Status (standard deviations in parentheses)**

Status	Annual Earnings ($)	Annual Crime Income ($)	*CRIME-YR*
JOB	5,328.89	177.45	.18
	(4,350.33)	(1,163.64)	—*
SCHOOL	609.88	79.49	.12
	(1,497.31)	(563.09)	—*
NOJOBSCHOOL	487.03	505.35	.51
	(2,002.66)	(2,341.14)	—*

*Standard deviations of 0–1 dummy variables were omitted because they could be calculated from their fraction m in the sample, where the standard deviation is $(m - m^2)^{0.5}$.

ficient and its statistical significance, *YSTREET* plays no apparent role for those in the *JOB* or *SCHOOL* states. The behavior of these groups is driven by variables such as *DRINK*, *DRUGS*, *POT*, and *GANG* (for the in-school group).

These variables are of consequence for the *NOJOBSCHOOL* population as well. But here *YSTREET* plays a much more powerful role, with effects up to twice as large as those estimated previously. The thinness of the criminal population among the *JOB* and *SCHOOL* groups should lead one to be cautious in drawing conclusions from these re-

Table 8.18 ***CRIME-MO* Logistic Parameter Estimates, by Status (asymptotic standard errors in parentheses)**

Independent Variable	*JOB*	*SCHOOL*	*NOJOBSCHOOL*
Intercept	−1.006	−4.107	−2.418
	(1.416)	(1.447)	(1.020)
AGE	−.062	.112	−.030
	(.065)	(.088)	(.044)
EDUC	−.037	−.170	.031
	(.105)	(.079)	(.074)
MARRIED	.429	—	−.145
	(.482)		(.491)
DRINK	.550	.808	.366
	(.356)	(.354)	(.246)
DRUGS	1.684	—	1.812
	(.470)		(.401)
POT	.880	1.495	.961
	(.275)	(.325)	(.213)
CHI	−.409	−.163	−.174
	(.332)	(.375)	(.224)
BOS	−.719	−.439	−.343
	(.309)	(.328)	(.260)
RELIGION	−.284	−.145	−.402
	(.292)	(.311)	(.249)
PROBT	.001	.030	.012
	(.006)	(.007)	(.003)
JAILT	.006	.031	.006
	(.014)	(.044)	(.006)
GANG	.381	1.626	.872
	(.381)	(.389)	(.305)
CNBD	.501	.157	.446
	(.259)	(.287)	(.198)
YSTREET	.032	.436	.982
	(.270)	(.308)	(.200)
−2 Log Likelihood	428.42	438.92	693.28

Table 8.19 ***CRIME-YR* Logistic Parameter Estimates, by Status (asymptotic standard errors in parentheses)**

Independent Variable	*JOB*	*SCHOOL*	*NOJOBSCHOOL*
Intercept	−.485	−2.851	−1.359
	(1,271)	(1,230)	(.981)
AGE	−.096	.066	−.04
	(.059)	(.079)	(.043)
EDUC	.005	−.145	−.034
	(.094)	(.073)	(.070)
MARRIED	.415	—	.033
	(.455)		(.463)
DRINK	.549	1.010	.347
	(.315)	(.293)	(.232)
DRUGS	2.408	—	1.644
	(.498)		(.416)
POT	.963	1.067	.959
	(.247)	(.261)	(.203)
CHI	−.338	−.463	−.169
	(.303)	(.330)	(.217)
BOS	−.578	−.190	−.276
	(.279)	(.272)	(.252)
RELIGION	−.120	−.377	−.298
	(.258)	(.262)	(.235)
PROBT	.007	.036	.015
	(.005)	(.008)	(.003)
JAILT	.001	.019	.009
	(.017)	(.040)	(.007)
GANG	.432	1.715	.937
	(.350)	(.354)	(.303)
CNBD	—	—	.362
			(.191)
YSTREET	.116	.292	1.214
	(.244)	(.263)	(.194)
−2 Log Likelihood	569.61	498.66	729.45

sults. Nevertheless, the relative importance of economic incentives for criminal behavior is clearly greater for those in the *NOJOBSCHOOL* state.

This result is what one should expect. Those working at a job have less of an economic motivation to commit crime, since they have their labor market income. Moreover, the decision to accept a job reflects their greater satisfaction with the level of wages offered in the market than that of youths who have no job. Similarly, youths in school tend to be supported by their parents. It is the out-of-school group without

gainful employment who have the greatest economic incentive to engage in criminal acts.

8.7 Conclusion: Crime as an Alternative Income Source

Youth crime has been the subject of a number of analyses because of its central role in the overall crime problem. For the black youths in the NBER sample, crime serves an economic function by providing many with a substantial income source. After adjusting for the likely underreporting of crime, I estimate that roughly one-fourth of all income of those in the sample was from criminal pursuits.

The black youth crime problem is, however, not pervasive among the entire demographic group. Roughly one-fifth of the sample reported criminal activity, but up to three-fifths of the sample may have engaged in crime (after adjusting for self-reporting bias). Those who reported criminal activity were a fairly hard-core criminal group. There was very little entry into and exit from this criminal population, which remained quite stable over the year. Those engaged in crime undertook multiple criminal activities, some of which were related (for example, mugging and burglary), but there also seemed to be an opportunistic effort to take advantage of various criminal activities as they arose.

Those predisposed toward crime also possessed characteristics not conducive to success in legitimate forms of work, and they had greater contact with criminal opportunities. Drinking, drug use, gang membership, past criminal activity (as indicated by probation or jail time), and problems of crime in the neighborhood all were strongly correlated with criminal behavior. In short, the types of youths who commit crime differ quite starkly and systematically from those who do not.

A fundamental influence on criminal behavior is the role of economic factors, such as labor market status. Respondents who were in school or employed were much less likely to engage in crime. What was most important was being out of the *NOJOBSCHOOL* category rather than being in the *JOB* or *SCHOOL* group. Although the respondent's status reflects in part an endogenous decision to allocate time to criminal behavior, the criminal behavior of these groups was quite different. Respondents who were not employed or in school were much more strongly driven by the economic incentives to commit crime.

The role of economic incentives to commit crime received particularly strong support in the *NOJOBSCHOOL* group and was statistically significant overall. If youths can make more money from crime than from labor market earnings, they will be more likely to engage in crime. Although these effects were statistically significant, making the gains from employment larger than the gains from crime would directly eliminate only one-sixth of the crime participation by black youths.

Economic incentives are more consequential for higher-income crimes, such as drug dealing, than for minor crimes, such as numbers. When one takes into account the intensity of criminal behavior, as measured by crime income, the comparative economic rewards for crime account for one-third of all crime. This impact is roughly double the influence on crime participation. Even this amount probably understates the potential long-term effectiveness of economic incentives because of a stabilizing effect of employment. Including the effect of having a job roughly doubles the impact of the labor market; the labor market variables may therefore account for as much as two-thirds of all crime income. Economic incentives are instrumental for a relatively small portion of the criminal population, but this segment accounts for a disproportionate share of all crime income.

In terms of the relative efficacy of the "carrot" of improved economic opportunities and the "stick" of stricter criminal enforcement, there is strong evidence that the "carrot" is more effective. The magnitude of this influence is not so dominant, however, that society should abandon its criminal enforcement efforts. On the other hand, this is not to say that improved economic opportunities are unimportant. The fringe crime group is particularly likely to be affected by more employment opportunities. Although minor changes in the economic environment may not dramatically alter the overall youth crime problem, the criminality among those who are not in school or employed is very sensitive to economic incentives. Since members of this group are responsible for most of youth crime, they comprise a major, economically responsive component of the criminal population.

Notes

1. More rigorously, the expected utility from crime must exceed the expected utility from legitimate activities. In the case of risk-neutral criminals, expected rewards and expected utility are equivalent. In addition to the early articles by Becker (1974), Ehrlich (1973, 1975), and Block and Heineke (1975), see the survey by Heineke (1978) of the theoretical literature on crime. The job-risk counterpart to this problem is the focus of the analysis in Viscusi (1979).

2. Among the many surveys that review this literature are those by Cook (1977, 1980), Taylor (1978), and Wilson and Boland (1976). See Freeman (1983) for a comparison of the deterrence and unemployment literatures.

3. Witte's (1980) study uses the wage received on the first job as the income measure, but even this is not an ideal index of a person's present opportunities.

4. The recent conference volume edited by Freemen and Wise (1982) addresses these issues in great depth.

5. These age-related trends are discussed in Greenberg (forthcoming).

6. Two background variables that might be expected to be linked to crime—being registered to vote and income other than from earnings (such as from food stamps)—

never had a significant influence on criminal behavior and are not included in the results reported here.

7. Respondents who had no job and who had no expected wage were assigned a wage value equal to the mean expected wage of those who were not employed.

8. See, in particular, the authors' discussion on pages 173–76 and 213. They concluded, "We did learn from the data supporting the optimistic conclusion that the self-report method can produce reliable and valid results within the population to which it is generally applied" (p. 213).

9. The crime categories for these calculations were as follows: reporting rate (burglary, larceny, and auto theft); conditional arrest rate (robbery, burglary, larceny, and auto theft); and conditional rates of serving time in jail (robbery, burglary, larceny, auto theft, narcotics, and others). The variation in the crime categories is the result of both differences in data availability and the unreliability of some data. For example, arrest rates for narcotics violations were very high because these violations usually go unreported, except when discovered by the police.

10. I am indebted to James Q. Wilson for this observation.

11. These numbers were derived from population figures and offense reports given in U.S. Department of Justice (1982, 39, 68, 75 and 98).

12. This result is not unexpected. See Moore (1983) for a discussion of the importance of drug use and related "criminogenic" traits.

13. Since some individuals had no crime income, the zero values had to be coded in a manner that would permit the use of natural logarithms. For purposes of estimation the natural logarithm of zero was set equal to -6.9.

14. More specifically, I used the weighted nonlinear least-squares program supplied to me by Gregory M. Duncan. For an excellent discussion of the sample-selectivity correction, see Duncan (1981). A recent application of this correction to labor economics issues appears in Duncan and Leigh (1980).

15. These estimates were obtained using equations (1) and (5) in table 8.9.

16. No variables with t-statistics above 1.0 in the OLS counterpart of these logit equations were dropped. Most excluded variables had t-statistics on the order of 0.1.

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Comment

James W. Thompson and James Cataldo

In this paper, one of the first individual-level employment and crime analyses of a sample *not* drawn from an offender population, Viscusi concludes that "roughly one-fourth of all income of those in the sample was from criminal pursuits." If this finding can be upheld (and we believe that it is generally correct), then it carries far-reaching implications for future research on the inner-city youth labor market. At last we can replace armchair speculations from aggregate data on economic incentives and crime deterrence with close examination of the full spectrum of income generation, ranging from legitimate employment in well-established firms to off-the-books and underground economic activities, and from these to street crimes such as muggings, burglaries, and drug selling. If Viscusi's work succeeds in establishing a commitment to this broader focus, his contribution will remain important.

But accepting that crime occupies an important place in the income-generating activities of inner-city youths does not automatically advance our understanding of the problem. Here, Viscusi's example is instructive, sometimes for its success in addressing thorny issues, and sometimes for its failures. The strength of Viscusi's approach is that he has developed a clear conception of the nature of economic incentives to engage in crime, namely, the difference between illegal and legal earnings expectations (*EXYGAP* and *YSTREET*); and he has adhered to this conception in estimating the determinants of crime participation and of crime income levels. The flaws in his analysis arise in his implementation, and they include the manner in which he specifies *EXYGAP* and his exclusive use of self-reported measures of crime participation.

EXYGAP and Relative Earnings from Work and Crime

In his model of employment and crime, Viscusi claims to adopt a framework of "discrete choice between crime and noncriminal employment." Such a model, though possible, is at variance with Viscusi's own data showing that 24 percent of youths (aged 16 to 24) admitting to crime in the past month *also* held jobs. Indeed, employment and crime studies have typically found a large amount of concurrent crime and employment. For example, the Vera Institute's survey of 902 Brooklyn male arrestees in the summer of 1979 found that roughly 40 percent of the young (aged 16 to 24) blacks in the sample also held

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jobs. This evident mixing of legal with illegal sources of income is also implicitly acknowledged by Viscusi when he develops his logistic regressions. In some of these, *JOB* and *SCHOOL* are introduced as explanatory variables into equations that have crime participation as a dependent variable. This procedure would be meaningless if a "discrete choice between crime and noncriminal employment" were in fact at issue. If crime and work are alternative activities, then the introduction of one as an independent variable would be equivalent to introducing the logical complement of the dependent variable as an explanatory variable.

On the other hand, if the choice framework is not binary, it would appear that the variables *EXYGAP* and *YSTREET* are not entirely appropriate for modeling crime participation, since they measure total potential income rather than differences in earnings at the margin. For example, a working youth is likely to encounter at least a few opportunities for crime that offer marginal returns much higher than those from legitimate work. Nevertheless, such a youth's total potential income from work would likely be much greater than his total income obtainable from crime, since, as Viscusi observes, the quality of his criminal opportunities would deteriorate rapidly as he began to "use up" his best crime options. Even so, some youths would be expected to continue to engage in those occasional criminal activities whose marginal returns equalled or exceeded their wage rates.

Adding to the difficulty associated with the binary choice framework are other perplexities associated with the key variable *EXYGAP* itself. *EXYGAP* is the difference between expected illegal and expected legal earnings. It thus has two distinct components, and problems are inherent in each one.

First consider expected illegal income. A point that Viscusi too little emphasizes is that expected illegal earnings are extraordinarily high; they are $22,945 on an annualized basis for the sample as a whole and they rise to $31,200 for those respondents who reported crime during the past month. Such levels are hardly comparable to either expected legal earnings ($3,800 for the crime-committing subgroup), nor are they at all comparable to *actual* annual crime income ($1,570).

What is going on here? Viscusi suggests, as we already noted, that expectations of illegal income opportunities are overly influenced by a few relatively good opportunities close at hand and by the fact that the youths do not adequately discount their longer-term crime prospects once these short-term options are used up. This phenomenon is familiar to anyone who has tried to sell scout cookies by working outward from his or her immediate family, but one that sharply distinguishes income-producing crimes from legal work.

How expectations about crime income are formed and what factors determine them are at the heart of the crime decision. Viscusi implicitly assumes that everyone exaggerates crime opportunities in the same way, and in fact he develops a single discount factor (9.3) by comparing *offenders'* expected (annualized) crime returns to their actual crime income. But the extent to which youths anchor their crime income potential on their best opportunities implies that the tendency to exaggerate would decline with greater levels of participation in crime. Those who commit no or only a few crimes would be more prone to extrapolate from a few good opportunities not yet explored, while experienced offenders would be more likely to make realistic, long-term appraisals. This hypothesis might be tested by comparing different discount factors, such as between those with low and high actual crime incomes or among different subgroups defined in terms of age and school and job status.

Next consider legal earnings expectations. Just as it is likely that illegal income expectations depend partly on the level of crime experience, it is plausible to suppose that legal income expectations also derive from experience in the labor market. But in a sample of inner-city youths, such experience is very unevenly distributed. In the NBER sample, only about a quarter (26 percent) were at work, two-fifths were in school, and a third (34 percent) were neither at work nor in school. These groups surely form expectations of legal earnings in very different ways. Those in jobs may have realistic (lower?) expectations concerning legal income prospects, since they are already in the market; those in school, by contrast, have less direct knowledge about prevailing legal opportunities, and their expectations may be largely based on their current self-investment activities; and finally, those neither in jobs nor in school may well have the lowest expectations. As with illegal income, Viscusi once again combines actual and expected earnings, using the former for those in jobs and the latter for the remainder.

Such blending of actual with expected legal income, combined with the previously described assumptions underlying the discounting of expected illegal gains, makes *EXYGAP* an odd hybrid with hidden complexity, rather than the seemingly simple motor driving crime decision making that is initially introduced in the paper. There is in fact still one more source of difficulty: The likely dependence of legal and illegal income expectations on the individual's level of participation in these activities raises the same issue of endogeneity that Viscusi notes with respect to several other variables in the study—chances to engage in crime, gang affiliation, and perceived crime in the neighborhood. Clearly, the path of future research should be to develop additional independent measures of legal and illegal opportunities and to employ

longitudinal designs that are capable of unraveling complex causal relationships.

Self-reports and the Prevalence of Criminal Activity

It is from exclusive reliance upon self-reported measures that a second set of difficulties arises. Although Viscusi at various places discusses problems with the self-reported measure of criminal activity (and, indeed, ultimately concludes that there was *three times* as much crime in the sample as sample members themselves reported), he is insufficiently cautious in approaching several pieces of internal evidence that cast doubt on the measure: the surprising lack of an age-crime association, the apparently skewed distribution of types of crimes in the sample, and the nearly complete absence of movement between crime and noncrime states.

In table 8.3 Viscusi finds no association between age and the prevalence of self-reported crime. He correctly observes that the truncated age distribution in his sample (16 to 24 years) would lessen the otherwise very sharp dropout effects that would appear with broader age groupings; but his argument that the failure to see any of this pattern is a result of the virtual absence of muggings, burglaries, and robberies in the self-reports of his sample (crimes that show especially sharp declines with age) begs the question. Only be accepting the veracity of the self-reports can we believe that these street crimes (which are particularly prevalent among younger inhabitants of inner-city minority neighborhoods) are absent from his sample. If they were present, we would expect an age-related decline in crime even in the age range from 16 to 24 years. The absence of this decline suggests that members of one subgroup, active in relatively violent and risky crimes, are not talking about their activities.[1]

Viscusi cites a study by Blumstein and Cohen (1979) to suggest that this lack of an age-crime pattern is "not unprecedented." But the Blumstein-Cohen study addressed an entirely different facet of the age-crime relationship: the intensity of criminal activity among a subgroup of active offenders, rather than rates of participation in crime at different ages.

The Blumstein-Cohen study found that the intensity of illegal activity among people still active in their criminal careers did not vary significantly by age; the well-established sharp decline in aggregate arrests per capita among older age groups in the general population therefore results from the fact that some people "drop out" from crime, rather than from a smooth tapering off of criminal activity as active offenders age.

More concern over the failure to find an expected age-crime association or self-reports of crimes such as muggings and burglaries might have led to more caution concerning a third issue, the near absence of an increase in crime in the sample between the past-month and past-year time periods. The fact that self-reported crime rose only from 15 to 19 percent of the sample between these two periods leads Viscusi to infer, as he puts it in his concluding section, "Those who reported criminal activity were a fairly hard-core criminal group. There was very little entry into and exit from this criminal population, which remained quite stable over the year."

This image of a "hard core" group hardly jibes with Viscusi's own acceptance of an underreporting factor of 3 (which implies that 57 percent of his sample engaged in crime over the past year), or with the close similarity in employment rates between the entire sample (26 percent) and those who admitted to crime (24 to 25 percent). If it is a hard-core criminal group, it is at least an *employed* hard-core group.

Although self-reported measures are valuable in this type of study, they should be complemented with official arrest data. If this is not possible, one should refrain from poorly supported conjectures about a "hard core" of criminal youth. The best current estimate, developed by Blumstein and Grady from a combined study of longitudinal and cross-sectional data, is that black men living in large (not "inner") U.S. cities experience a cumulative lifetime probability of one or more *felony* arrests of 51 percent, whereas white men in the same cities have a cumulative probability of 14 percent.[2] Most of this arrest risk occurs by the middle twenties. This estimate is one of many indications of widespread crime among poor, minority youths, and it further suggests that in some "high risk" inner-city (as against large-city) areas, participation in crime for income gains is a predominant pattern, not at all confined to a hard-core group.

Only if others follow the lead of the NBER study, and only if we overcome the sterile isolation of crime research from the study of broader labor market phenomena, will we be able to come to grips with the full implications of the fact that income-generating street crime is both widespread within poor populations and, within the individual life cycle, relatively short-lived.

Notes

1. Bias might also have arisen in other, indirect ways. For example, perhaps interviewers concerned about their own safety selected meeting times and places or types of respondents that led to the relative exclusion of this group. The study methodology is not described, a serious problem in studies such as this of hard-to-reach populations.

2. Alfred Blumstein and Elizabeth Grady, "Prevalence and Recidivism in Index Arrests: A Feedback Model," *Law and Society Review* 16, no. 2 (1981–82).