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LABOR MARKET SEGMENTATION, HUMAN CAPITAL,
AND THE ECONOMICS OF CRIME

by

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ABSTRACT

The research reported in this dissertation analyzes the relationships among human capital, labor market structure, and crime. It tests the relative explanatory power of neo-classical choice theory and labor market segmentation theory on the determinants of labor market outcomes, criminal behavior, and their interaction.

The analysis focuses on separate series of hypotheses for labor market outcomes and for crime. The data used are from a random sample of felony arrestees in Brooklyn, New York, interviewed by the Vera Institute of Justice during the summer of 1979. Labor market and crime experiences for a two year period prior to the arrest provide data to examine the hypotheses.

The empirical results suggest that labor market segmentation theory offers a superior explanation for the labor market experiences of the sample. However, little evidence is found to support simple economic tradeoff models of crime and legal work, whether such a model derives its vision from neoclassical choice theory or segmentation approaches.

It is concluded that the criminal choice decision is a highly complex and multiattribute one, and that the predictions of economic theory do not adequately capture the

multiplicity of factors involved in crime. Factors relating to improved social stability seem to have the strongest dampening effect on crime, but this finding is only tentative.

The final chapter of the dissertation discusses the econometric and theoretical limitations on the study of crime by economists, and locates the results of the dissertation in reference to major policy debates on crime control through employment and deterrence policies. It is concluded that improving job quality is a policy initiative that must be tried to test its effects on crime. Previous employment policies, characterized as "human capital" programs that assume some deficiency on the part of individual workers, are not seen to hold much promise for lowering crime. Deterrence policies, aimed at lowering crime through more certain and severe punishment, are also thought to hold little promise for reducing crime.

PREFACE

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LABOR MARKET SEGMENTATION, HUMAN CAPITAL,
AND THE ECONOMICS OF CRIME

Introduction

Labor market problems such as unemployment, lack of occupational mobility, and low wages have often been seen as directly if not causally related to crime. Empirical studies often claim to locate this relationship. Yet policy and program efforts to reduce crime through labor market interventions have not been very successful. This gap in effective policy may be rooted in inadequate or inappropriate understandings of the labor market.

Numerous studies purport to show a direct link between crime and labor markets. Examination of case records of more than 15,000 persons prosecuted in Washington, D.C. during 1975 showed that 46 percent were unemployed when interviewed concerning bail eligibility. A summary concluded that:

...the level of unemployment reported for persons prosecuted in the District of Columbia (46 percent) is greater than the annual unemployment for the District's general population (7.6 percent). Among those defendants who are employed, a disproportionate number hold unskilled and semi-skilled jobs with half earning less than \$3 an hour. Thus it would appear that not only is unemployment a

major concern for the person entering the criminal justice system, but there also exists a problem of underemployment (U.S. Dept. of Justice, Office of Criminal Justice Plans and Analysis, 1977, n.p.).

Some studies of aggregate crime and economic indicators support these claims. The most well known set of aggregate studies is the work of M. Harvey Brenner (1976a; 1976b). Brenner's work found significant correlations between such diverse phenomena as suicide rates, cardiovascular disease, and prison admissions on the one hand, and economic indicators on the other. Unemployment rates were most significantly correlated with the social problems, especially prison admissions. Brenner's time series analysis concluded that a one percent increase in unemployment sustained over six years would be associated with approximately 3340 additional admissions to prison.

In addition to the link between aggregate indicators of crime and economic conditions, a study prepared for the U.S. Department of Labor claimed that criminal activity is widespread in the labor force, and is not confined to a few career criminals (Miller, 1979). The paper estimated the number of persons in the labor force who have a contact with the police beyond a traffic offense. It was estimated that 36 to 40 million Americans will have such a contact at some point in their lifetime; of these, between 26 and 29 million were estimated to be in the labor force, and another 3.5 million were expected to reenter in the future.

Finally, special attention has been given to labor market and crime problems among disadvantaged youth. A Vice-Presidential Task Force on Youth Employment made "special needs" youth a priority, especially those involved with the criminal justice system. A central aspect of proposed policy involved direct employment or manpower training (Vice-Presidential Task Force on Youth Employment, 1979).

But there is a paradox that stems from attempts to reduce or eliminate crime through the use of employment programs. For instance, the supported work project of the Manpower Demonstration Research Project (MDRC) hoped for lowered crime as a result of program participation. But the project evaluation showed no differences between experimentals and controls in terms of lowered crime for high school dropouts or ex-offenders (MDRC, 1980). Many other employment programs aimed at crime also have shown mixed results.

Then Secretary of Labor Ray Marshall, in hearings before a subcommittee of the House Committee on the Judiciary (1978, pp. 536-7), summarized the state of knowledge on employment and crime relationships. He said that "Although it can be said that employment rates are associated with crime rates, the exact nature of the relationship of crime and unemployment is currently unknown." He endorsed the continued use of manpower programs to reduce crime, while admitting that studies of the effects of such programs were "not conclusive."

Thus there is some consensus that crime and labor market problems are intimately related, but the exact nature of the relationships is still debated. Over the past decade, economists have participated in this debate, on both a theoretical and empirical level. The neoclassical choice-theoretic perspective has provided the major, if not exclusive, theoretical underpinning for economists' work on crime. Policy conclusions based on this model have taken two forms: manpower training, to improve individuals' positions in the legal labor market through increased levels of human capital, and a revival of deterrence theory, to raise the costs of participation in illegal activities.

The ambiguous results of both policy options should cause reconsideration of the underlying theoretical model. Until now, no systematic use has been made of an important alternative viewpoint in labor economics--the segmented labor market approach. This approach provides an alternative orientation for the study of crime and economic conditions. It also contains important policy implications for these problems. And a serious use of the segmentation perspective for the study of crime can help illuminate the strengths and weaknesses of the neoclassical approach.

The two theoretical approaches rest on different visions of how the economy operates. The application of neoclassical choice theory to labor market dynamics emphasizes market payoffs to variations in individual worker productivity, variations that are critically determined by the

education and training of individual workers. In contrast, labor market segmentation approaches envision an economy with sharp structural boundaries to job rewards, boundaries that are rooted in the historical development of the American economy and are largely independent of individual variations in worker productivity. Segmentation theory argues that these differences in labor market structure establish different absolute levels of occupational rewards and limits on the variation of those rewards; in the lower segments of the labor market, occupational rewards are seen as very low in absolute terms and susceptible to very limited variation.

It seems critical for the advance of theory and policy to test the relevance of the segmentation approach to crime, and to compare its explanatory power with neoclassical choice theory. The research and policy failures of the past decade may conceivably be based on problems with the dominant theoretical perspective, not on poor research and program execution. Comparison of the two alternative views should help clarify the theoretical, empirical, and policy issues.

This study provides such a comparison and evaluation. There are three major aspects to the study: an exploration of the relevance of segmentation theory for the study of crime; an empirical comparison of neoclassical and segmentation approaches to crime, using a unique micro-level data set; and an evaluation of theory and policy implications.

CHAPTER ONE: LABOR ECONOMICS AND CRIME

A. Neoclassical Economic Theory and Criminal Behavior

The most natural and frequent question people ask about crime is "Why?" They ask it about individual crimes and about crime as a whole. In either case it is almost an impossible question to answer. Each single crime is a response to a specific situation by a person with an infinitely complicated psychological and emotional makeup who is subject to infinitely complicated external pressures...The causes of crime, then, are numerous, mysterious, and intertwined (Task Force of the President's Commission on Law Enforcement and Administration of Justice, 1967, p. 45).

The approach taken here follows the economists' usual analysis of choice and assumes that a person commits an offense if the expected utility to him exceeds the utility he could get by using his time and other resources at other activities. Some persons become "criminals," therefore, not because their basic motivation differs from that of other persons, but because their benefits and costs differ (Becker, 1968, p. 176).

Social Theory and Criminal Justice Policy

Prior to the mid-1960s, crime was not often studied by economists. Sociological theory, in particular criminology, analyzed criminal activity as a type of "deviant" behavior, while sociology of law and some branches of political thought analyzed how laws were made and enforced. As crime statistics dramatically rose during the 1960s, sociologists

and criminologists tried to apply their theories to understanding and controlling crime, often with government backing. But the variety of theories of criminal behavior which they employed did not result in very effective policy application--crime indicators continued to rise, and the issue of street crime and demands for "law and order" became one of the principal issues of the 1960s and 70s.

In the mid-1960s, economists began to enter the debate about crime, drawing on the different intellectual and policy legacies provided by their discipline. The major theoretical approach employed was derived from neoclassical labor economics developed by economists associated with the Chicago school. Economists claiming allegiance to this perspective began to generate a large number of empirical studies trying to relate crime and diverse economic phenomena such as unemployment, labor force participation, inflation, and income dispersion, as well as demographic factors such as race and age. The policy recommendations of such studies have contributed to the increasingly shrill calls for using harsher penalties to deter crime. Unlike most sociologists, who had largely abandoned deterrence, many neoclassical economists claimed to find measurable, quantitative effects from harsher enforcement of criminal law, effects which were predicted and explained by their theoretical models of labor supply and returns to human capital.

In his landmark article on crime, Becker included a remarkable statement. After outlining the "economists'

usual analysis of choice" and its application to the study of crime, he stated: "I cannot pause to discuss the many general implications of this approach, except to remark that criminal behavior becomes part of a much more general theory, and does not require ad hoc concepts of differential association, anomie, and the like (Becker, 1968, p. 209." This offhand dismissal of several of the central concepts in criminological and sociological theory reflected an impatience with the variety of sometimes conflicting explanations offered by sociologists to explain crime.

Sociological theory has traditionally been split on how to explain the nature of social reality, variously emphasizing social order, conflict, structure, psychological factors, and the like. Important approaches to crime include structure of opportunity theory, subcultural and culture of poverty perspectives, labelling theory, differential association, control and conflict perspectives, and atheoretical empirical studies (Rodriguez and McGahey, 1979).

Many of these perspectives overlap, and many of them contain significant subdivisions; the critical point is that sociological theory provides no unified approach to criminal behavior, and thus no unambiguous set of policy implications. Policy makers in the 1960s, confronted with rising crime statistics, became impatient with the variety of messages they were getting from sociology. James Q. Wilson summed up the situation:

Sociological theories of crime, widely known and discussed in the 1960s, have certain features in common. All seek to explain the "causes" of crime and delinquency, or at least their persistence. All make attitude formation a key variable. All stressed that these attitudes are shaped and supported by intimate groups--the family and close friends. All were serious, intelligent efforts at constructing social theories, and while no theory was proven empirically, all were consistent with at least some important observations about crime. But none could supply a plausible basis for the advocacy of public policy (Wilson, 1975, p. 53; emphasis in text).

This vacuum created space for economists' analysis of the crime problem, which drew heavily on work in labor economics. During the 1960s, much of labor economics was designed to influence government policy decisions on such issues as training, educational policy, poverty programs, anti-discrimination efforts, and the like. Although there were a variety of approaches to these problems, only the Chicago economists, led by Becker, systematically applied their perspective to crime.

Human Capital, Rational Choice and Crime

Becker's initial work on criminal behavior stemmed from his work on human capital. Human capital theory can be seen as an important attempt to salvage the traditional theory of marginal productivity, by grounding it in a theory of individual choice, capacity, and endowment, subject to constraints. Becker has been the leading figure in this work; his approach to crime grows out of his work in labor economics.

Becker's work was significant in its attempt to join a theory of income distribution to a theory of labor markets, something the institutionalists were unable to do systematically. For all of the modifications introduced by the institutionalists into labor market theory, they never completely rejected marginal productivity approaches, although their work increasingly focused on the structure of labor markets--unions, monopoly and oligopoly industries, job clusters, wage contours. This implicitly undercut the sovereign individual of marginal analysis (McGahey, 1978). Becker reasserted many of the tenets of marginal productivity theory, grounding them in a somewhat different theoretical structure. In Human Capital, he wrote "The renewed interest in investment in human capital may provide a means of bringing the theory of personal income distribution back into economics (Becker, 1964, p. 66)."

By combining personal distributions of the supplies of factors with derived demand payments for those factors, Becker could discuss changes in occupational rewards without moving to an institutional perspective. In particular, he grounded wage rates and employment levels in this fashion. Through an extensive discussion of the effects of on-the-job training and general education, Becker called attention to the "effect of human capital on earning, employment, and other economic variables (1964, p. 8)."

This perspective also provides an explanation for the overall dynamic of the economic system in terms of income

distribution. As investment in human capital is rewarded, "influence" in the form of income is weighted towards those who have invested more. (No real explanation is given for why some "choose" not to invest.)

When education and training can be viewed as investments, it is a short step to thinking about all human activity as investments of a scarce resource--time--in order to maximize personal utilities.* Thus an analysis of criminal behavior could be included in Becker's general framework: "Some people become 'criminals,' therefore, not because their basic motivation differs from that of any other persons, but because their benefits and costs differ (1968, p. 176)."

The analysis of crime had an explicit policy purpose: "This essay uses economic analysis to develop optimal public and private policies to combat illegal behavior (Becker, 1968, p. 207)." Given a "supply" of crimes, Becker focused on social policies to prevent crime, or at least contain it--the stress was on finding an optimum level of law enforcement. Police and court activity was viewed as a production process, which incurs costs as the activity increases.

In defining the "supply of offenses," Becker offered his now familiar ground for activity: "...a person commits

*Indeed, Becker has recently claimed that "...all human behavior can be viewed as involving participants who maximize their utility...the economic approach provides a valuable unified framework for understanding all human behavior (1976, p. 14; emphasis in text).

an offense if the expected utility to him exceeds the utility he could get by using his time and resources at other activities (1968, p. 176)." The "costs" of committing offenses relied on the probability of arrest and punishment; if these occurred, earnings would be foregone, especially during imprisonment. Income opportunities would be reduced over a lifetime, due to the stigma of a criminal record and failure to accumulate human capital. The person's choice was also made by considering legal opportunities, although Becker seemed to take these as fixed. (This should not be surprising, since a person's opportunities are held to be determined by their personal supply of human capital.)

Becker concluded that an increase in the probability of punishment would deter more crime than an increase in severity of sentences, although the conclusion is reached entirely at a formal level. Becker thus proposed that fines be substituted for most other forms of punishment, since incarceration involves a double cost. The prisoner loses earnings, while society must pay the costs of imprisonment.

Note that Becker's emphasis is not primarily an explanation of individual behavior so much as an argument about appropriate policies and resource allocation to contain that behavior, which is taken as more or less fixed. Further, all policy conclusions were about enforcement, since the structure of legal opportunities was implicitly determined by personal factors. Optimal policy boiled down

to increased probability of punishment, and the use of fines as the most cost effective form of punishment. But the focus on fines as cost optimal punishment was rapidly lost by other economists, and studies of crime focused on increasing both the severity and the probability of punishment, usually in the form of prison sentences.

Theoretical Development and Stalemate

The key figure in this change was Issac Ehrlich, a student of Becker's. In a series of papers, Ehrlich (1973; 1975; 1979) developed an individualized mathematical model, tested it with aggregate data, and concluded that both increased certainty of sentences and increased sentence lengths lowered crime rates.

Ehrlich expanded on Becker's model in three ways. Opportunities came to include rewards and punishments, not just the costs of punishment; this tied the level of property crime to "income inequality as well as law enforcement activity." Criminal activity was linked to "the general theory of occupational choices," by placing the criminal decision in a context of legal and illegal income opportunities. Using empirical information, the interrelationship of crime and criminal justice activity was analyzed, to "provide some tentative estimates of the effectiveness of law enforcement in deterring crime and reducing the social loss from crime (1973, pp. 522-23)." Although the model seemed

better specified, Ehrlich ended focusing on deterrence again; his recent work claimed to find measurable deterrent effects from the death penalty (McGahey, 1980a).

The major policy discovery from this sort of work has been labelled the "incarceration effect." Simply put, if people are in jail, they cannot commit crimes. (Or they can only victimize other prisoners or penal workers.) If it is assumed that some important degree of crime is committed by "career criminals," then putting those people in prison will "save" the crimes they would have otherwise committed while on the street. It follows that the longer someone is kept in jail, the greater the reduction in the "supply of offenses."*

The models proposed by Becker, Ehrlich and others have been severely criticized by other neoclassical economists. While not going outside the framework of individual utility maximization, these economists have characterized the work of Becker, Ehrlich and others as portfolio balance models, a term taken from investment analysis (Heineke, 1978). In these models, all benefits and costs associated with crime

*In the rare case where dollar costs have actually been projected for incarceration policies, estimates range from eight hundred to four thousand dollars expenditure per offense "saved." These figures do not include police and court time, but only added prison time. Projected reductions in crime rates are marginal except at absurdly high levels of imprisonment--for instance, a five year prison term for every person convicted of a felony (Drug Law Evaluation Project, 1976).

are assumed to have dollar equivalents, and the individual selects an optimal portfolio--much the same as choosing between bonds and stocks. An optimal amount of risk and return is selected, sacrificing potential gain for less risk (if the investor is risk averse) in accordance with some preference ranking. The optimal portfolio is thought to consist of some mix of high-yield, high-risk activities such as crime, and low-yield, low-risk activities such as legal employment. If the variance of criminal income is larger than that of legal income, then the amount of risk embodied in a portfolio varies directly with the amount of crime.

Block and Heineke (1975) provided the most sophisticated neoclassical critique of portfolio models of crime. They attacked the portfolio approach because those models included wealth as the only argument in the utility function. Block and Heineke also argued that even time allocation models were flawed, as the crime choice decision required multi-attribute analysis and that the unambiguous comparative static conclusions about the effects of severity and certainty of punishment and the effects of changes in legal earnings stemmed from improper model construction. To account for other non-monetary aspects of work and crime, Block and Heineke included amounts of time devoted to legal and illegal activities explicitly as arguments in the utility function, as well as attempts to model risk preference and moral "noxiousness."

This approach drew on neoclassical literature on labor supply, which in turn owed a great deal to Becker's (1965) original work on time allocation. Much recent economic work on time allocation and labor supply has been aimed at deriving theoretically optimal policies for taxation (e.g., Abbott and Ashenfelter, 1976) or experimentally investigating the effects of policies such as the negative income-tax experiments (e.g. Cain and Watts; eds., 1973; Burtless and Hausman, 1978).

Block and Heineke (1973) had previously demonstrated that, within the confines of neoclassical labor supply theory, no unambiguous behavioral predictions could be generated about responses to parameter changes. Using an expected utility framework, they focused on labor supply changes in response to shifts in subjective probabilities of wage rate changes, and found that increased uncertainty from rising dispersion in wage rates did not unambiguously predict more work effort, as earlier literature assumed. The earlier results were only obtainable by assuming a particular attitude towards risk on the part of the individual, the decreasing absolute risk aversion found in the Arrow-Pratt hypothesis. Without this assumed constraint, no clear effects could be theoretically derived about disincentive effects, and empirical research was required to estimate the relative magnitudes and directions of changes in labor supply in response to changes in wage rates. (Rosen (1980)

reached a similar conclusion about the theory's inability to predict the effects of taxation on labor supply.)

The critique of portfolio crime models was based on this critique of theory. Block and Lind (1975) had criticized the portfolio approach for assuming strict monetary equivalents for all crime rewards and punishments; within a neoclassical framework, they demonstrated that Becker's policy conclusions about the effects of increased certainty and severity of punishment were not warranted. Block and Heineke (1975) extended this insight to the analysis of participation in crime. They went beyond their earlier critique of labor supply and found that even knowing the individual's attitude towards risk did not allow predictions about responses to changes in rewards and punishments.

The critique concluded that changes in relative returns of an activity such as crime induced not only potential substitution but wealth effects, especially wealth effects in non-pecuniary forms. To derive the comparative static results about the certainty and severity of punishments claimed by Becker, Ehrlich and others, no psychic or moral "costs" could be associated with particular activities like crime--what Block and Heineke termed the "noxiousness" of certain activity. They concluded that "in the area of law enforcement as in taxation policy, policy recommendations do not follow from theory but rather require empirical determination of relative magnitudes (Block and Heineke, 1975, p. 323)."

We have come full circle. From Becker's promise to develop "optimal" crime control policies (1968, p. 209), it turns out that no determinate policy recommendations follow from the theory when it is rigorously extended. As the next section will discuss, empirical studies based on these models also have failed to provide clear confirmation of the theory, or conclusively answer the policy questions that it posed.

B. Aggregate Econometric Studies of Crime

The empirical literature on crime and economic conditions is a large and confusing one. Crime has been studied from economic, sociological, psychological, environmental, biological, legal, systems operations and almost any other aspect imaginable. Similarly, discussions about relationships with economic conditions, especially employment, range from theoretical arguments about the nature of labor markets to specific reports of the impact of various employment programs. Gillespie's 1975 review of empirical evidence on the relationship between crime or delinquency and economic factors lists 123 items in the bibliography. Leiberg (1978) presented a bibliography on crime and employment consisting of approximately 540 items. The Employment and Crime Project at the Vera Institute of Justice has currently assembled a library of over 800 items.

Not all of this material is on the direct relationship of crime and economic factors. Much of it is either about

crime per se, or economic factors (especially employment and manpower literature). But much of the empirical economic literature is very similar, involving more or less complex regression models with some aggregate measure of crime as the dependent variable, and some combination of economic and other variables as independent variables. Some models attempt to use simultaneous equation systems to model deterrence effects, and resource allocation of police and courts.

This section focuses on econometric studies which use aggregate measures of crime and economic conditions, principally some measure of unemployment. Almost all of the studies have been done in the last decade, when research of this sort took off.

Most of this review focuses on selected econometric studies that employ aggregate data, outlining the lack of consensus produced. My principal critique of these studies is their failure to test alternative theories of the labor market. There are other critiques to be made: the use of aggregate data to test a theory of individual behavior and the difficulty of testing such a theory through exclusive use of positivist approaches. These issues will also be discussed below.

Imprisonment and Unemployment

Probably the most well known set of aggregate studies is the work of M. Harvey Brenner, a specialist in the "epidemiology" of diverse phenomena such as cardiovascular

disease, prison and mental institution admissions, and suicide rates. Starting in 1971, Brenner analyzed these phenomena in conjunction with a variety of economic indicators. He found that unemployment rates were most significantly correlated with the social problems. In his study for the Congressional Budget Office, Brenner's object was "to translate the research findings on the pathological effects of unemployment and other forms of social distress into a form that could be useful for national economic policy decisions (1976a, p. 9)." For an index of crime, he used prison admissions. When analyzed in conjunction with various series of economic indicators, he found unemployment rates to have the most significant association with prison admissions. His time series analysis concluded that a one percent increase in unemployment sustained over six years would be associated with approximately 3340 additional admissions to prison (Brenner, 1976a, p. 6).

Brenner's work rested on such eclectic theoretical supports that it is hard to say what his results show. In work published by the United Nations, Brenner listed seven different theoretical viewpoints about crime, including "economic loss" as a cause, along with relative deprivation, subcultures, and urbanization (1976b). He claimed that his work supported the "economic loss" and relative deprivation aspects, although he found some support for the other views. The discussion of why he felt some of these approaches were supported was short and unclear.

Other studies have also found correlations between prison admissions and unemployment rates. However, it is problematic whether prison admissions are reliable indicators of changes in criminal activity.* In addition to serious data measurement problems, there are problems in interpreting any empirical relation.

The prison studies also present technical difficulties, some of which are shared by other analyses of crime and economic conditions. The prison studies are time series analyses, relating changes in prison admissions to changes in unemployment rates. In a period of rising unemployment and rising prison admissions, a positive correlation between the two variables might be spurious. (This is especially true when the observation points are close together; one of the studies uses monthly data.) This is the familiar "time trend" problem, whose usual solution is the introduction of dummy trend variables (Rao and Miller, 1971, pp. 99 ff.).

*For example, Jankovic (1977) argued that unemployment does influence prison rates, but not because of an increase in crime. The unemployment-prison relationship was seen to be relatively independent of the amount of crime which occurs. Imprisonment, for Jankovic, plays an important but largely symbolic role in social control, since rises in imprisonment neither dampen crime rates nor significantly remove people from the labor force.

But use of subpopulation rates gives a slightly different picture on this latter point. If the white male prisoners were added to the white male unemployed in 1976, the unemployment rate would only have moved from 6.4 percent to 6.7 percent. But if black male prisoners were added to the black unemployed, their unemployment rate would have risen from 12.8 percent to 15.0 percent.

Time series data is also subject to autocorrelation, or serial correlation of the disturbance terms, where the error terms from one or more preceeding periods affect the values in the current period (Johnston, 1972, pp. 243 ff.).

Finally, since several of these studies used data aggregated over the United States, both the crime and employment data potentially suffer from aggregation bias. For instance, to the extent that different jurisdictions define different acts as crimes, make different enforcement decisions, and use different policies for imprisonment and these differences are determined independently from differences in unemployment, aggregation of the data can create problems for analysis (Grunfeld and Griliches, 1960).*

Most of the studies hypothesized a lagged relationship between unemployment and changes in prison admissions. Cox's study for the Georgia system used monthly data for a seven year period; he used a trend dummy, adjusted the data seasonally, and found that the "effects of the unemployment data series on the prison population data series" are characterized by "zero lag" (1975, p. 7). No test for autocorrelation is reported. There was some speculation on what individual behavior patterns could account for this statistical relationship.

*For instance, on December 31, 1976, the estimated prisoner population in North Carolina was 214 per 100,000 population, third highest in the nation. New Hampshire had the second lowest rate in the nation--30 per 100,000. North Carolina's unemployment rate for 1976 was 5.9 percent; for New Hampshire, it was 5.1 percent.

A U.S. Bureau of Prisons (1975) study used admissions to the Federal system and the unemployment rates for adult males. They provided no documentation concerning time dummies, aggregation adjustments, or tests for autocorrelation. But in sharp contrast to Cox's finding of "zero lag," they found that a 15 month lag best fit their data series. Again, some speculation was included about why this lag might exist, focusing on the amount of time that the criminal justice system takes to process a case.

Brenner's work was more careful although there is no real theoretical focus. Brenner (1976a) posited a two year lag relationship for the crime and unemployment data, using the Almon distributed lag procedure to adjust the data. He also tested a variety of time trend dummies. He found a statistically significant relationship between unemployment rates and all of his measures of "social trauma"--mortality rates, cardiovascular and renal disease mortality rates, suicides, homicides, mental hospital and prison admission rates. But like the other analysts, Brenner could only speculate on why these indicators are related, or why his lag had the shape it did.

Brenner's econometric work has been subjected to close scrutiny on three grounds: sensitivity to the lag structures used, computation of the model on subperiods of time series data to test for stability, and failure to include potentially relevant independent variables. Brenner's work

was not sensitive to the shape of the lag structures, but was found to be unstable and to have omitted important independent variables (Center for Econometric Studies of the Justice System, 1979).

The Limits to Atheoretical Studies

This lack of theoretical orientation, and the attendant inability to interpret empirical results, plagues a great deal of the empirical work on crime and economic conditions. The aggregate studies are fraught with technical difficulties, both in the data available and the techniques used. But a more central problem is the lack of theory; bracketing the technical problems, none of the prison studies could interpret their findings on anything more than an ad hoc, speculative basis. Cox constructed a short explanation for his finding of no lag; the U.S. Bureau of Prisons included a paragraph or two to account for their 15 month lag. Brenner computed a two year wave effect, to which he devoted a page or two of speculation.

In many of the econometric studies of crime and economic indicators, the researchers seem to have simply taken some data, dumped it into a computer, and then attempted to make sense of the results. There is often little or no theoretical justification offered for the inclusion or exclusion of variables, the choice of data sets or the levels of aggregation used, the use of particular econometric techniques, or the sorts of analyses offered.

Most cross sectional work studying crime rates in cities has been without much theoretical precision. Most of the discussion in these works is about formal regression models and data adjustment, with only passing reference to what the researcher hopes to prove, disprove, or shed light on. Allison (1972), Fleisher (1966), Sjoquist (1973), and Swimmer (1974) all did this sort of cross sectional work for cities. The overall results are ambiguous.

As Gillespie noted, "two studies find a significantly positive relationship [between crime and unemployment rates] and two find no statistically significant relationship (1975, p. 32)." Allison and Sjoquist found a significant relationship; Swimmer and Fleisher found none. Based on technical considerations concerning equation specification and possible aggregation biases in the data, Gillespie placed more confidence in Sjoquist's work than in Swimmer's.

But when looking at all four studies, both the least sophisticated and specified model (Allison's) and the most complex econometric formulation (Sjoquist's) agreed in finding a significant relationship. The other two relatively sophisticated studies found none. Thus it seems hard to evaluate these studies on technical grounds alone. But these researchers, and Gillespie's review, have no other method of evaluation.

Although the econometric studies discussed here like to claim the mantle of "scientific" work, problems arise in their positivist approach. When contradictory results are

obtained, technical solutions are the only way presented to resolve the contradictions. Although such considerations are necessary, they are not sufficient, as the above example of the four cross sectional studies of cities illustrates. Successful study of complex social phenomena such as crime and economic conditions may not be amenable to an exclusively positivist approach. Although the positive economist claims to be testing hypotheses, the approach in practice often becomes immune from refutation:

When his model repeatedly fails to predict correctly, the physicist blames his model. The positive economist, on the other hand, is tempted to blame the ceteris paribus conditions, the data, or the specific testing procedure itself. Consequently, for the positive economist, hypotheses are seldom disconfirmed and general theory rarely or never disproved. Positive economics thus becomes perfectly insulated from refutation: it cannot be harmed by demonstrating its assumptions to be unrealistic, and it is not rejected when its predictions fail to fit the facts (Wilber and Wisman, 1976, p. 406).

Samuelson's comment on utility theory is also germane:

Often nothing more is stated than the conclusion that people behave as they behave, a theorem which has no empirical implications, since it contains no hypotheses and is consistent with all conceivable behavior, while refutable by none (1974, p. 107).

Rather than continue a recitation of the variety of studies that approach the crime and economic problem as a problem of pure technique, we will turn to detailed discussion of three important studies that attempted to rigorously define the crime problem in a neoclassical context,

paid careful attention to limitations on data and econometric methodology, and attempted to arrive at policy oriented conclusions. As we shall see, even these three careful studies end up in substantial disagreement about the importance and strength of the crime and employment relationship.

Three Aggregate Studies

Isaac Ehrlich has conducted a number of empirical analyses on crime (e.g. 1973; 1975). Consideration of penalties has become increasingly important for Ehrlich and others; his work has been extremely important in the rebirth of deterrence theory. Basically, deterrence is claimed to be discoverable through aggregate regression analyses tying crime rates in various jurisdictions and over time to measures of enforcement. (These conclusions have been roundly criticized in Blumstein, Cohen and Nagin (1978) and elsewhere.) Ehrlich's work is considered here for the findings on crime and economic indicators, not deterrence.

To connect the theoretical model of individual wealth maximization with empirical study, Ehrlich needed some measures of individual opportunities, legal and illegal. His only data were aggregate information, which does not fit well with his highly individualized model. (Ehrlich admitted that "alternative income opportunities cannot be estimated unless one is able to identify a control group representing potential offenders and study its alternative income prospects (1973, p. 538).")

Ehrlich introduced a proxy for income prospects, showing income dispersion in states. He defended this proxy on these grounds:

We postulate that payoffs on such crimes [property crimes] depend, primarily, on the level of transferrable assets in the community, that is, on opportunities provided by potential victims of crime, and to a much lesser extent on the offender's education and legitimate training (Ehrlich, 1973, p. 538).

There are three problems with this approach. First, the idea that income inequality in a community is the major determinant of crime rates is at variance with most statistics which show crime rates to be consistently higher in poor communities. Wealthy people also have the ability to purchase private protection, demand and get more public protection, and some geographic isolation from poor communities, where more economically motivated criminals would be expected to live.

Second, Ehrlich assumed that legitimate opportunities are solely determined by an individual's education and legitimate training, the familiar human capital theory. This excluded other analyses of the urban labor market which stress the structure of jobs, discrimination, and other structural variables as prime determinants of legitimate opportunity.

Finally, Ehrlich used data for states, but a state is probably too large a unit to produce crime because of income dispersion. Ehrlich did not identify what mechanism is at

work in the states with greater income dispersion; it might be the "relative deprivation" of sociology, but no interpretation was offered.

Ehrlich thus wanted to provide an individual level analysis using aggregated data for states. The connections between his individual theory and his data are extremely tenuous. Manski's comment on Ehrlich's capital punishment studies is equally appropriate here: "The empirical analysis performed in Ehrlich is at the macro level and is only marginally related to the theoretical individual-choice model he develops in the same paper (1978, p. 403)."

In his 1973 paper, Ehrlich found a consistently significant relationship between income inequality and property crime rates. However, as could be the case with unemployment rates, income inequality may be serving as a proxy for a whole range of economic difficulties. Ehrlich seemed to interpret the income measure as "more to steal" (in his words, "the level of transferrable assets"). But highly unequal income distribution more likely reflects high concentrations of poverty, with greater economic need among the poor. Ehrlich also found that the percentage of non-whites in the population is significantly related to property crime rates. Since income in the United States is unequally distributed by racial groups, this could also be interpreted as a proxy for economic need. (Not all poor are minorities; an absolute majority are white. But non-whites are disproportionately represented in the poor population.)

In examining other economic indicators, Ehrlich found a "generally disappointing" relationship between unemployment and crime rates for 14 to 24 year old males. Labor force participation rates for this group also showed no significant relationship to crime. When family size is held constant, some effect emerged, leading Ehrlich to conclude that "an increase in legitimate wages available to young workers is expected to reduce their incentive to participate in all crimes (1973, p. 556)."

Theoretical vagueness aside, another reason that Ehrlich got confusing results might be the equation he was testing. He used the unemployment, labor force participation, and proportion of males for ages 14-24, and related it to the arrests for that group. These variables may be heavily collinear; specifically, the numerator of the labor force participation rate is the denominator of the unemployment rate, and both age specific rates are clearly affected by the proportion of males in those groups. Ehrlich did not report any tests for multicollinearity.

The only effect Ehrlich found from labor force participation was on crimes against the person, not property crimes. His explanation for this, as Gillespie noted, "is theoretically consistent but strains credulity (1975, p. 36)." Ehrlich claimed that crimes against the person are time intensive consumption activities; thus, as labor force participation falls, more time is available for such consumption activities. Ehrlich labelled this as a "pure scale effect (1973, p. 556)."

Consideration of labor force participation versus unemployment rates as an explanation of youth crime was taken up by Phillips, Votey and Maxwell, who in a paper drawn from an earlier larger study found that "properly weighted, participation rates may be a better measure of economic opportunity than simple unemployment rates (1972, p. 491)." Their work provided alternate subdivisions of the aggregate population, in order to allow more precise measurement. Use of data in the study was relatively careful, with specific attention devoted to partitioning the data into race and age subdivisions.

They tested two alternative divisions of the population: taking those with employment and those not in the labor force as different, they added the active unemployed (in the labor force, but not working) to both groups alternately. They concluded that the division between those in the labor force and those out of it provided a better explanation for crime than a working/not working split. Thus the labor force participation rate was found to have a strong negative relationship with the crime rate.

Phillips, Votey and Maxwell also concluded, somewhat boldly, that "changing labor market opportunities are sufficient to explain increasing crime rates for youth (1972, p. 502)." The changes in these opportunities were best reflected in the labor force participation rate: a decline in this rate reflected individuals dropping out of the labor force, with a presumed entry into criminal activity. Their

work could not ascertain whether rises in youth crime were due to increases in existing criminal activity, or whether individuals who formerly did not commit crimes began to do so. (A different explanation would be needed for women, whose labor force participation and crime rates have both been rising.)

But the result seemed very strong. In their original study, Votey and Phillips claimed that "approximately 98 percent of rising crime is explained by the worsening of economic conditions (1969, p. 41)." In view of Ehrlich's inability to find any consistent relationship between crime and unemployment, or crime and labor force participation rates, this is doubly surprising. Although working during the same period, neither study references the other. We are thus deprived of any attempt on their part to reconcile these opposed findings.

The impact of labor force participation on crime was called into question in Leveson's (1976) study for the Hudson Institute. In a book length report, Leveson examined the effects of youth unemployment on crime rates. Although finding that youth unemployment has some impact, Leveson only counted it as one among many factors. His summary of factors that affect crime included age distribution, cohort factors, urbanization, race, sex, age, poverty, addiction, and deterrence, along with youth unemployment. (Leveson claimed that adult unemployment shows no significant relation to crime.)

In Leveson's work, the use of many diverse factors greatly increased the difficulty of finding distinct effects for any one specific factor such as drug addiction or youth unemployment. Theoretically, these specifics do not appear easily separable from such general phenomena as urbanization, racial discrimination, and poverty. Leveson's factors were highly collinear, which he recognized, but he still concluded that "the magnitude of the influences can often be determined within reasonable bounds (1976, p. VII-2)."

Specifically, Leveson estimated that youth unemployment accounted for 25-30 percent of the change in crime rates from 1952 through 1963, and 30-40 percent of the changes from 1963-1973 (1976, p. VII-3, table 1). This estimate is quite different from Votey and Phillips' 98 percent, and Ehrlich's failure to find any significant relationship between crime and unemployment.

Leveson's explanation of the determinants of youth unemployment was also striking. Since he claimed that the adult unemployment rate has no effect on the crime rate, he looked for some causes of youth unemployment which were independent of adult unemployment. Leveson attributed changes in youth unemployment, and therefore changes in crime, to "increase in the number of youth as a percentage of the labor force and to increases in the level and coverage of the minimum wage (1976, p. III-36)."

The arguments about the effects of youth in the population are associated in studies of crime with the "demographic optimists." If the crime problem is primarily due to large numbers of young people, then when they get older, crime will decline, cet. par., along with unemployment. Although Leveson did wish to be labelled a "demographic optimist," his argument took him in that direction.

Other labor economists have suggested that age dispersion in the labor force, and not age levels per se, is responsible for volatility in unemployment (Ulman, 1972). And it is not clear that mere aging of the work force will solve the unemployment problems of minorities. Adams and Mangum (1978) provided sobering figures on this. Based on a 4.8 percent overall unemployment rate, they projected an unemployment rate for 16-19 year old nonwhites of 28.5 percent in 1985. Although this might seem an advance over the 1976 rate of 37.1 percent, the rate for 20-24 year old nonwhites in 1985 is projected as 40.9 percent, up from the 1976 level of 21.2 percent (Adams and Mangum, 1978, p. 61, tables 4 and 5).

Leveson's minimum wage argument has also been challenged by many economists. Fisher's study of youth unemployment problems had some instructive comments on the economic perspective undergirding the argument that minimum wages cause youth unemployment:

Of the 16 authors for the 14 studies (on minimum wages and youth unemployment), I would classify 11

as primarily associated with the Chicago school, and a major Chicago position for years has been that minimum wages cause unemployment, especially for groups like teenagers. Moreover, several of their studies are loaded towards finding a significant minimum wage impact. In that case, one is not surprised that all 11 find a significant impact of minimum wages (would any Chicagoan believe his results if he found otherwise?); perhaps more interesting is that four or five non-Chicagoans find no significant impact (1973, p. 174).

Fisher himself found no separate teenage labor market: "Employers do not view teenagers and adults as separate, identifiable factors of production (1973, p. 121)."

A further problem with the minimum wage argument, especially as it is applied to crime by Leveson, is the implication that it is the condition of having a job, rather than the level of income derived from that job, which is critical for preventing crime. Although such a position may be theoretically consistent with a pure time allocation argument, it has questionable relevance to the world of underemployment. If crime is seen as at least partially motivated by inadequate income, then a job which pays subpoverty wages will not solve the problem. (It is not clear to what extent Leveson accepts the time allocation argument.) It might also be possible to draw on sociological literature, and relate nonpecuniary aspects of legal work (e.g. stability, work attitudes) to diminished criminal activity, but Leveson did not do so. The policy implications that follow from

*Earlier debate on unemployment effects of the minimum wage can be found in Adie and Chapin (1970), Gavett (1970), and Kruege (1970).

this line of argument are clear: to help reduce youth crime, cut the minimum wage, or eliminate it all together. (Similar arguments are advanced as "solutions" for the underemployment of groups such as minorities and women.) The implied promise that such a reduction in coverage would lead to more jobs for youth, and that this in turn would lead to reduced crime, is not the simple causal sequence it seems to be.

In fact, there is some disagreement among different Chicago economists on the relation of wage levels, job openings, and crime. Remember that Ehrlich concluded that "an increase in legitimate wages available to young workers is expected to reduce their incentive to participate in all crimes (1973, p. 556)." Block and Nold (1978) tested employment and deterrence effects on crime for 121 SMSA's. Their preliminary findings indicated that "property crime rates are moderately responsive to permanent changes in employment opportunities, with crime commission decreasing when there is a perceived permanent increase in employment rates (Block and Nold, 1978, n.p.)." Since most arguments about the minimum wage see it as a way to create temporary jobs for teenagers, and there is some fear that cuts in the minimum wage might cause permanent job loss for older workers replaced by cheaper, younger workers, the minimum wage "solution" to youth crime is highly questionable.

These three aggregate studies of crime and economic conditions are some of the most rigorous ones available.

Ehrlich, Votey et al., and Leveson all shared the basic thrust of neoclassical methodology--that socioeconomic conditions are essentially aggregates of individual behavior, and can be explained by a combination of simple behavioral assumptions about individuals which can be tested through the use of aggregate data. All three employed sophisticated and careful econometric techniques. All three made claims about the relationship of crime and economic conditions, at least in so far as that relationship was captured in aggregate data. All came up with some numerical estimates of the relationships between crime and various economic indicators such as unemployment, labor force participation, and income measures. And all disagreed on the relationship. Ehrlich found little significant relation between unemployment rates, labor force participation rates, and crime. Phillips et al. found extremely significant relationships between labor force participation and crime. Leveson found crime to be tied to youth unemployment, which he saw as determined by minimum wage coverage and cohort composition.

Empirical and Theoretical Limits on Aggregate Data

This lack of consensus stems in part from the misuse of aggregate data as variables in econometric analyses. Empirical work on crime and economic conditions done by economists uses data generated by governmental bodies, in particular the Bureau of Labor Statistics (BLS) and the Federal

Bureau of Investigation (FBI). The construction of these data series variables can play an important, but usually hidden, role in the outcome of the studies. And sensitivity to the limitations on official data is usually not shown by economists.

On the crime side, available data include prison admissions and populations, arrests and crime reported to the police, and victimization surveys. On the economic side, researchers use unemployment rates, labor force participation, inflation, and income dispersion. Both sets of data are varied by units of aggregation (e.g. state, city, nation, region) and by subpopulations (e.g. race, sex, age) where the data exists in that form.

Measures of crime cannot ever accurately count the amount of crime that occurs. Criminal activity is one of those things that people are very interested in concealing. The use of other indicators reflects a search for a useful proxy for crime.

The seemingly "hardest" data available are characteristics of inmates, or admissions to prisons and jails. Several studies have used such information as their dependent variable. Gillespie noted that "Prison admissions and unemployment rates show the most consistently significant relationship" in empirical studies (1975, p. 4). But there are several problems with prisoner statistics.

People who are sentenced to prison terms represent an extremely small proportion of people brought into the

criminal justice system. A 1975 Vera Institute of Justice study of criminal court dispositions illustrated this. Table 1.1 shows dispositions of estimated felonies* occurring in New York City during 1971.

The table shows the problem: 25.4 percent of criminal prosecutions result in jail or prison sentences,** but only 5.2 percent in state prison sentences, which are over one year. Since all cases are not prosecuted, the study estimates that these sentences represent only 2.8 percent of all felonies reported to the police, with only 0.6 percent of those going to a state facility. And, since many crimes are not reported to the police in the first place, the Vera study estimated that only 1.2 percent of all felonies committed result in a jail sentence, and only 0.3 percent in a sentence of over one year.

Using prisoner statistics for New York City would thus have to assume that either the 1.5 percent of sentenced prisoners were an accurate cross-section of the crime committing population, or that the 1.5 percent had committed an extremely large number of offenses. But using these statistics is further confounded by their relative imprecision

*These felonies are only the seven FBI Index Crimes. They do not include narcotics offenses, white collar crimes, and the like. Problems with the Index are discussed below. Narcotics arrests are about 20 percent of all arrests in a year in New York City.

**The convention is to use "jails" for local institutions, with sentences under one year for either misdemeanors or felonies. "Prison" refers to state or Federal institutions, with sentences for felonies over one year.

TABLE 1.1

CRIMES, ARRESTS AND DISPOSITIONS IN NEW YORK CITY, 1971

Steps in Criminal Justice Process	1971 Figures	% of Committed Felonies	% of Fel- onies Re- ported to Police	% of Criminal Prosecution
Felonies Committed	847,800	100.0	--	--
Reported to Police	459,800	54.2	100.0	--
(Arrested)	(57,700)	6.8	12.5	--
Removed to Family Court	7,300	.9	1.6	--
(Criminal Prosecution)	(50,400)	(5.9)	(11.0)	(100.0)
Jumped Bail	3,100	.3	.7	6.2
(Adjudicated)	(47,300)	(5.6)	(10.3)	(93.8)
Dismissed & Acquitted	21,100	2.5	4.6	41.8
(Convicted)	(26,200)	(3.1)	(5.7)	(52.0)
"Walk"	13,400	1.6	2.9	26.6
(Prison)	(12,800)	(1.5)	(2.8)	(25.4)
Prison up to 1 year	10,200	1.2	2.2	20.2
Prison over 1 year	2,600	0.3	0.6	5.2

Note: Figures in parentheses are those which continue through the process. Table adapted from data in Vera Institute of Justice (1975, p. 77).

even for the sentenced group. Jails contain many individuals awaiting arraignment, disposition, sentencing, or movement to another jail or prison (Goldfarb, 1975). Many researchers thus only use populations and admissions to state and/or Federal prisons. But even here, where an accurate count should be available, information is highly qualified, especially concerning subpopulations. Prisoners are overwhelmingly male, and disproportionately minorities. However, the National Prisoner Statistics program of the United States Justice Department has been unable to provide racial breakdowns for state and Federal prison populations for most years in the 1970s.

Prisoner statistics thus cannot be easily justified by asserting that prisoners are some rough approximation of crime committers, or that admissions to prison serve in some useful way as a proxy for criminal activity. Such assertions are not easily supported. Other problems arise when using these figures in conjunction with economic indicators. Here, I only want to show that even the most seemingly "hard" data available--that concerning a captive prisoner population--must be approached cautiously.

The next level of specificity in the data is information on arrests. (Information on defendants in court is not considered, since none of the empirical studies uses such data.) Characteristics of arrestees--the crimes they are arrested for, and their demographic traits--are the most widely used source of data in economists' studies. This

information is usually drawn from the Uniform Crime Reports (UCR) of the FBI.

The UCR's are a compilation of all offenses known to the police and reported to the FBI, which serves as both the repository for and interpreter of this information. Over 13,000 law enforcement agencies (local, county, and state police for the most part) participate in the program. These various agencies cover approximately 96 percent of the population of the United States, with slightly higher coverage for urban areas.

As mentioned above, not all crime known to the police is reported to the FBI, and not all crime reported to the FBI goes into the UCR Crime Index. The Index is made up of seven offenses--murder and nonnegligent manslaughter, forcible rape, robbery, aggravated assault, burglary, larceny theft, and auto theft.. Other offenses are sent to the FBI, but not counted in the Index. The FBI takes a total of these seven offenses, divides it by the population of the relevant unit, and produces a "crime rate" per 100,000 population. This figure is the rate cited by politicians, the media, and most researchers.

The Index suffers from several difficulties. Different jurisdictions may define crimes in different ways, so an act in one area might not be legally a crime, where in another area it might be. More importantly, different police jurisdictions might treat acts differently, either as a formal policy or informally (Wilson, 1968). Police departments

have been suspected of over or under reporting crime statistics to meet particular ends. Changes in reporting techniques may cause seemingly sharp changes in crime patterns which are in reality artifacts of the technical change. An oft-cited example is when New York City went to centralized reporting in 1950. Reported robberies rose 400 percent; reported burglaries rose 1300 percent (Silberman, 1978, p. 609).

The problems are compounded when using arrest figures. Arrest records provide the major source of demographic information on alleged criminals; without an "alleged perpetrator" there can be no such data. If researchers were confident that police were catching a representative sample of crime committers, this would not be a problem.

But there is considerable doubt whether police in the United States provide such a representative pool. Differing police practices, official and unofficial, make arrestees a potentially skewed population. Strictly speaking, UCR's tell about police arrest and reporting practices. It is difficult to ascertain with any precision the direction and nature of the biases.

One attempt at going behind the arrest statistics has been made by the victimization surveys conducted by the Law Enforcement Assistance Administration (LEAA). These survey panels ask representative samples of a population whether they have been victimized in a certain time period. Economists do not use these data very much, as the studies are carried out on an inconsistent time and location basis,

making them problematic for time series and cross sectional work.

Some researchers feel that the victimization panels have been useful in supporting the information reported by the UCR's. Silberman, for instance, felt that "What data are available from the victimization surveys support the view that current arrest statistics are a reasonably reliable, if rough, approximation of actual criminal behavior (1978, p. 454)." Others took a different view. Hood and Sparks claimed that

...the evidence reviewed so far throws some doubt on the assumption that the proportion and 'quality' of serious crimes which are known to the police will remain relatively constant over time...it is necessary to make the assumption of stability in the proportion of crime known if an index is to be constructed from official statistics to compare crime rates at different periods of time and between different parts of a country (1970, p. 37).

Not all of the researchers would go as far as Silberman; nonetheless, many feel that the rise in crime reported by the Index is more than just a statistical artifact (Biderman and Reiss, 1967; Hindelang, 1974; Skogan, 1975).

Economic researchers interested in using this data as dependent variables in time series analyses are best off at the highest level of aggregation possible, at least to try and wash out any significant reporting differences. Use of such data in cross sectional work should examine jurisdictions to see if reporting changes are occurring; this is usually not done.

Nevertheless, researchers who want to do empirical studies of crime are often stuck with these flawed data sets, unless they construct a set of their own. Little or no use has been made of self reported crime studies, even though these show different trends than the official data. (No doubt this is partially due to economists' distrust of self reported data. Thurow cited a colleague who employs "Hall's Law": "All survey questions about economic motivations elicit stupid answers (1975, p. 73).")

A recent review of 35 studies which examined the relationship between social class and criminality found that "contemporary data do not support the conclusion of a negative relationship between social class and crime/delinquency (Tittle, Villemez and Smith, 1978, p. 643)." But there is little argument that arrest and imprisonment statistics show a correlation with low income and minority status (themselves highly related). It is in the self reported crime studies where the relationship between measures of social class (often some variation on income levels) and crime is not apparent.

It may be that arrest statistics serve as a good proxy for certain types of criminal behavior, since self report studies cover a wide range of potentially criminal or delinquent activities (Elliott and Ageton, 1980). The use of arrest figures must draw our attention to those acts which are prosecuted in practice, as well as discretion exercised by all parts of the criminal justice system. The ideology

of what constitutes a crime, and the accompanying allocation of resources in the criminal justice system, is heavily geared towards the Index crimes, and not towards such acts as price fixing, industrial negligence, environmental destruction, and the like. On-the-job theft is also not covered by the Index, yet the U.S. Department of Commerce (1976) estimated that "ordinary" crimes against business in 1975 would cost over 23.6 billion dollars.

So we must recognize that what is usually meant by "crime" is only a small subset of formally illegal activities, captured in the FBI Index, often known as "street crime." Street crimes are committed most often by the poor; the poor are also most often victimized by it. Although special concern is directed towards violent crimes, such offenses make up less than 10 percent of the Index.* Thus it is not clear exactly what aggregate crime statistics measure, or in what directions they may be biased. At the very least, researchers would be expected to treat such data cautiously. By and large, the economists who conduct empirical studies of crime do not treat the data with any care at all.

There are also difficulties with the data used by economists to represent economic conditions in these studies.

*The FBI statistics obscure this by reporting robbery as a violent crime. Robbery is forcible property taking, accompanied by violence or the threat of violence. Adding robbery to violent crimes raises the percentage of violent crime in the Index from around 10 percent to around 30 percent.

Usually these data are taken from government series: unemployment and labor force participation rates, income dispersion figures, and occasionally the consumer price index. There is widespread dissatisfaction in the economics profession and among policy makers with these figures, especially the unemployment data. Concern has grown in the last several years to the point where a new Presidential commission was appointed to review and revise unemployment statistics. The concern has been furthered by the use of local unemployment figures to allocate funds for such programs as CETA. Local and regional unemployment figures are projected from national BLS data; thus more importance has become attached to this information (National Commission on Employment and Unemployment Statistics, 1979).

Of particular concern to the critics of unemployment statistics has been the failure of such figures to adequately represent the experiences of the urban poor. Measures of labor force participation are vague; "discouraged" workers are not counted as unemployed; insufficient attention is paid to spells of unemployment, often tied to unstable jobs. Additionally, there has been no merger of poverty and unemployment data. Even if a person is employed, they can earn below poverty wages, especially if the job is not full time, full year.

A variety of alternative schemes has been offered to supplement or replace the current unemployment figures. Recomputation of labor force and unemployment figures that

include the above concerns produces dramatic changes in the figures.

Smith and Vanski (1975) devised a "jobless rate" for the Urban Institute, which attempted to measure the "potential labor force" as the denominator of the rate, rather than the actual force as defined by the BLS. This rate generally ran about 1.0 percent to 1.5 percent higher than the official unemployment rate.

Levitan and Taggart (1974) have devised an "Earnings and Employment Inadequacy Index" (EEI), which includes discouraged workers, involuntary part time workers, household heads earning below poverty level incomes, and excludes students, people over 65, and unemployed persons from households with above average income. Computing the EEI for 1972, based on the data provided by BLS, they got a figure of 11.5 percent, compared with an official unemployment rate of 6.1 percent.

Gordon (1977) computed "underemployment" rates, including discouraged workers, involuntary part-timers, the working poor, members of the armed forces, inmates held in institutions, and an allowance for the failure of the Census to accurately count inner city residents. For 1975, with an official unemployment rate of 8.5 percent, Gordon's estimates of underemployment ranged from a low of 17.0 percent to a high of 33.8 percent. Underemployment was found to be disproportionately concentrated among women and minorities.

All of the above recomputations use official figures. Gordon's adjustment for the failure of Census enumeration techniques and his findings about women and minorities were born out by Leggett and Gioglio (1978), who conducted their own survey of unemployment in New Brunswick, New Jersey. Completing 309 out of 310 interviews, they computed several measures of unemployment. When using strict BLS criteria, they found a 15 percent unemployment rate, versus the 9 percent reported by the BLS. Forty-three percent of the labor force participants experienced some unemployment during the year. Again, problems were most acute for women, minorities, and blue collar workers. They cited similar studies in California which found BLS methods to be underreporting local unemployment problems.

It is difficult to predict exactly what effect more refined and accurate indicators would have on the empirical studies done by economists. In so far as crime is associated with low income and a truncated set of legal opportunities, measures such as the EEI or Gordon's underemployment figures ought to show better fits with crime rates. Both of these alternate formulations focus more on poor and disadvantaged persons, who are assumed to be the most sensitive to a direct relation between underemployment, inadequate income, and crime. Global unemployment rates represent many groups with relatively low crime rates, such as women and older people. They also include some higher income individuals who may have some type of cushion against unemployment, such as unemployment insurance, savings, or a second

earner in the household. Economic researchers would be better off to try and match specific subpopulation unemployment indicators (including the alternate formulations) with specific subpopulations of arrestees, or victimization data from specific geographic areas.

A similar gain in precision might also obtain from using industry specific unemployment rates. If the poor are structurally limited in the types of jobs available to them, then changes in the conditions for those jobs might be expected to have more of a direct impact on street crime. Interesting results might also be obtained by using self reported crime data, and matching it where possible to labor force information.

But not too much reliance should be placed on improved data sets. Although using different sources in a more careful way might give better mathematical results, interpretation of those results still must rest on some theoretical foundation. Most of the empirical work done by economists rests on problematic theoretical grounds. As a result, explanations of why unemployment rates and crime rates are (or aren't) related have an ad hoc quality to them. Discussions of how such aggregate relations might translate into behavior at an individual or group level are likewise unconvincing. Even more rigorous analyses, like those discussed above, encounter many of the same problems.

Consider, for example, aggregate tests of the relationship between crime levels and income. Many econometric

studies try and test the hypothesis that crime levels vary directly with illegal income options and indirectly with legal options. These studies often employ a measure of per capita income, but use it in a variety of ways. Some analysts use it as a measure of legitimate income; others use it as an index of the demand for illegal commodities; still others use it as a proxy for levels of illegitimate income. Witte and Orsagh chronicled this shifting use of just one variable and concluded that "One's interpretation of the measure becomes largely a matter of belief (1980, p. 7)." Orsagh, in detailing the statistical problems inherent in the use of aggregate data and the particular uses made of those data by economists studying crime, concluded that "Statistical results based on aggregate data are, in part, artifacts of discretionary research activity (1979, p. 294)."

C. Studies Using Individual Level Data

The lack of a clear link between micro theory and macro data, and the variety of problems involved with the aggregate data sources, led Manski to conclude in a study for the National Academy of Sciences that "The macro approach... suffers from deep logical problems that make clear interpretation of empirical findings difficult to achieve (1978, p. 400)." These statistical and theoretical limits on the use of aggregate data, using a theory which models individual behavior, have not led to many individual level studies

of crime and employment relations. What evidence exists comes from studying participants in labor market programs or groups of prison releasees; this evidence is discussed below.

Findings from Employment Programs

Generalization about findings from employment programs must be tempered by the lack of rigorous evaluation research. Dixon and Wright found that most vocational programs aimed at delinquents had been "so under-evaluated as to make it difficult to judge program effectiveness (1974, p. 68)." Isolated successes and failures were reported, but both the quality and small numbers of evaluations made any broad conclusions difficult. (Only 350 out of 6600 program abstracts reported empirical findings about project results. Even where data were reported, the quality of the evaluation varied widely.)

There have been a few studies which used rigorous evaluation methods, and have the advantage of individually matched data on program experiences, labor market behavior, and crime. These have generally reported discouraging findings. For instance, Robin (1969) evaluated Neighborhood Youth Corps programs which attempted to reduce youth crime by generating more socially acceptable attitudes and values in youths and by filling up leisure time with employment. But in his controlled design research, he found no evidence that program participation reduced police contacts. Since

experimentals spent a substantial amount of what would have been leisure time in the program, this time allocation alone might have been expected to reduce their police contacts. Robin suggested that the importance of such programs for reducing youth crime may have been "more illusory than real (1969, p. 331)."

Discouraging findings about reducing crime have been reported in evaluation studies of two recent employment oriented programs: the Court Employment Project (CEP), a diversion program in the New York City criminal courts, and the supported work experiment conducted by the Manpower Demonstration Research Corporation (MDRC). Both of these studies used a rigorous controlled design format, with careful attention to methodology. The findings from this research raise major concerns about the efficacy of these programs in reducing crime.

CEP began as a demonstration project of the Vera Institute of Justice, funded by the U.S. Department of Labor. The original CEP proposal set fairly extensive goals. Through early diversion from the criminal justice system, and a four month exposure to social services and job development efforts, CEP hoped to:

...combat recidivism; reduce court backlogs; reduce expenditures for prosecution, trial, and incarceration; increase the supply of skilled labor; and provide a tax-paying, trained asset to the community in the form of a law-abiding employee, rather than a liability in the form of a prisoner (Baker and Sadd, 1979, note 1 on p. 64).

CEP had fairly broad eligibility criteria, and accepted most cases that the courts were willing to divert. This meant that they received many young, out of school, and unemployed people, since these people make up a substantial part of the intake in New York City courts.

Baker and Sadd made note of the economic rationales which underpinned CEP: "The conceptual links between employment services and rehabilitation were the same as those underlying all the DOL diversion programs--employment programs would give defendants an economic stake in avoiding crime and thus abort the all too casual process by which they were thought to develop criminal careers (1979, p. 63)." The program shifted its emphasis over time from providing manpower services to more generalized social service counseling, but the basic thrust remained focused on curing individual deficiencies which were seen as the major cause of poor labor market experiences.

The evaluation research employed a controlled design with an N of 410 experimentals and 256 controls. It covered a variety of topics, including the history of diversion programs, effects of CEP on the court system, and effects on employment, "life stability," and recidivism of participants. My focus here is on the disappointing results for recidivism and employment.

Recidivism rates showed no significant differences between experimentals and controls, even while experimentals were in the program. Seven measures of employment likewise

showed no differences between experimentals and controls in the rates of change, although Baker and Sadd cautioned against interpreting the strength of this finding. Controls and experimentals increased labor market entry over time, although the overall employment pattern was erratic. This change was attributed to the increasing age of the entire study population. On the whole, "CEP did not seem to improve employment to a greater extent than did whatever outside factors affected the control group (Baker and Sadd, 1979, p. 225-26)."

Baker and Sadd also noted that participation in school had little impact on employment and recidivism. At one point they mention that the supposed link between more education and better jobs might not have been "realistic" for the CEP population, but they provide no extensive discussion of alternative explanations. Indeed, elsewhere in the evaluation report, extensive underemployment for this group was attributed in substantial part to the age, educational level, and attitudes of the participants. CEP clients "were neither educationally nor socially prepared to make the transition into the world of regular employment"; "As predictable from their age and education, very few were employed at intake (Baker and Sadd, 1979, p. 76; p. 80)."

Results from the MDRC's (1980) supported work experiment were also very limited. Rather than services or job placements, supported work attempted to provide a work atmosphere where "participants can acquire the skills, habits,

and credentials necessary to find and hold permanent, unsubsidized employment (MDRC, 1980, p. 13)." The imagery about program participants concentrated almost exclusively on individual deficiencies: "...by participating in the program, a significant number of people who are severely handicapped for employment may be able to join the labor force and do productive work"; "given the severe handicaps of the groups..."; supported work would provide a bridge for these people "who live at the margin of society (MDRC, 1980, p. 1; p. 2; p. 13)."

Although some positive program effects were reported for some participants, notably women receiving Aid for Families with Dependent Children, effects on criminal behavior through employment changes were not very substantial. MDRC evaluated three different target populations in terms of their criminal activity: ex-addicts, youth, and ex-offenders. The three groups were almost all males. The ex-addict participants had been in a drug treatment program within the six months prior to participation in MDRC. Youth were between 17 and 20 years old, school dropouts, and unemployed; 50 percent of them had an official record of delinquency or criminal arrest. The ex-offender group had been incarcerated sometime in the six months prior to enrollment.

The ex-addicts had slightly better labor market experiences than the control group, and markedly lower crime rates. For ex-offenders and the youth group, no differences

were found on employment or crime indicators. The reductions in crime for the "ex" addicts were attributed to a lessened need to steal to support a drug habit; although arrests were significantly lower for experimentals, drug use was not (MDRC, 1980, pp. 91-95).

For the youth sample, the researchers reported that "Overall, supported work appears to have little impact on the employment, drug use, or criminal activities of the youth target groups," although there was some "modest evidence that supported work works better for those who are most disadvantaged." Like CEP, some improvement over time was found for both controls and experimentals. The MDRC researchers attributed it not only to aging effects but also to general improvements in the economy and increased levels of government programs (1980, pp. 99-101).

The research showed no statistically significant differences between controls and experimentals on hours worked or earnings, once the hours and earnings provided by the program were taken into account. Length of time spent in the program did not significantly influence hours or earnings for experimentals. The program had no effect on post-program schooling. Drug use and criminal justice involvements showed no significant differences, even while experimentals were in the program (MDRC, 1980, pp. 103-11).

MDRC cautioned against strong interpretations of the data from the final study period, due to a small sample size that was concentrated at one program site. They also found

some statistically insignificant evidence that the most disadvantaged groups in the experiment showed some improvement in employment and reductions in crime. But these hints of marginal positive findings do not obscure the overall lack of program effects on employment, schooling, drug abuse, and criminal justice involvement for the youth target sample.

For the ex-offender group, MDRC reported that "While participants were in the program they worked more and were less dependent on welfare programs, but overall there was no reduction in crime or drug use. Moreover, in the post-program period the employment impacts declined sharply to the point where they became statistically insignificant." The participants in this group only stayed in the program an average of 5.9 months, and the reduction in welfare dependency was probably largely related to the experimentals' ineligibility for welfare while they were receiving program payments (MDRC, 1980, pp. 117-20).

A recent program which provided unemployment insurance to prison releasees gives other individual level information on income and crime relationships. In 1976, the U.S. Department of Labor sponsored an experiment which offered unemployment benefits to prison releasees in Georgia and Texas. The program, Transitional Aid to Released Prisoners (TARP), was conducted as a random assignment research and demonstration effort. At first glance, the program appears to have been a failure. Experimentals and controls did not differ significantly in property or nonproperty rearrest

rates; experimentals worked less than controls; and the wage rates for the two groups did not vary significantly.

The evaluation, however, made the surprising claim that the program, contrary to appearances, did reduce recidivism. Lowered arrests due to income payments were offset by higher arrests due to subsidized unemployment. Thus net arrests did not differ between controls and experimentals, but this surface lack of difference hid real differences between the two groups. This "counterbalancing" effect was only discovered through complex econometric modeling (Rossi, Berk and Lenihan, 1980).

Even given repeated methodological cautions, the concrete findings about the counterbalancing and "masking" effects were problematic. Rossi, Berk and Lenihan properly warned that "the counterbalancing model was fitted to the TARP data with a considerable amount of custom tailoring and creative stitching (1980, p. 113)." But this warning tended to be overwhelmed by the rest of the text, where the findings were presented in a much less ambiguous manner. "We will show that the payments actually did lower arrests, but that this effect was counteracted by the fact that payments also indirectly increased arrests by fostering unemployment." "First, payments are useful in lowering recidivism." "TARP payments reduced the number of property and nonproperty arrests in a substantively and statistically significant manner (Rossi, Berk and Lenihan, 1980, p. 91; p. 19; p. 263; emphasis in text)."

This counterbalancing effect--reduced arrests through increased income, but increased arrests through subsidized leisure time--relied on a complex five-equation model, and the use of three-stage least squares, a relatively esoteric analytic procedure. Although parts of the model seem to have been confirmed with different equations and procedures, the overall combination of equations and technique was critical in locating the alleged effect. As the authors were fully aware, there is no way to choose a technique at this level of complexity which does not involve some risk of statistical distortion. Hypothesized relations involving complex single and multiple equation systems can be analyzed by a variety of techniques, and reviews of these techniques provide no simple criteria for selection. When findings depends on the use of one technique, as these do, less confidence can be placed in them; the findings are less robust.

In addition to the statistical problems, the reported results seem implausible when compared to other parallel research. First, there is the effect of program participation on arrests. The researchers reported that the payments of 63 to 70 dollars per week "can decrease the arrests experience by ex-felons in the year following release" by 25 to 50 percent (Rossi, Berk and Lenihan, 1980, p. 7)." Programs such as MDRC which provided work to ex-convicts and other disadvantaged groups, as well as studies of prison releasees generally, have not found such strong effects of low changes in income on arrest rates.

Second, and perhaps more critically, is the alleged work disincentive effect, which supposedly led to more crime. The TARP payments were found to lower work effort by two-thirds for every dollar paid out; that is, for each 100 dollars in payments, experimentals reduced their work effort by about two-thirds of a week (Rossi, Berk and Lenihan, 1980, p. 243; p. 25a). There have been many studies by economists of the effects of taxation and changes in income on work effort, using both aggregate and experimental data, and the estimated effects are much smaller than the TARP researchers report (e.g. Cain and Watts, eds., 1973; Abbott and Ashenfelter, 1976; Burtless and Hausman, 1978). Given the low level of TARP participants' income, and the lack of similar findings in similar literature, these high levels of work disincentive are very problematic.

The authors' claims about the complex "masked" effect of TARP are undercut by the statistical fragility and the seeming uniqueness of the results. The authors were aware of these problems, but did not always incorporate their own cautions in the presentation of the results.

The limited and ambiguous results from these controlled design experiments echo the ambiguous findings of earlier studies. Rovner-Pieczenik's (1974) review of pretrial intervention programs found that several programs made claims about increased employment and reduced recidivism from diverting young arrestees from the court process. However,

the research on these programs was fraught with methodological difficulties, leading Rovner-Pieczenik to accept reductions of recidivism during program participation, but to discount claims of longer term impacts on recidivism or employment.

Taggart (1972) reviewed a broad range of vocational efforts aimed at offenders, including vocational training and education in prison, pretrial intervention, work release programs, and a variety of post-release services. Taggart noted that the characteristics of arrestees and prison inmates did not differ substantially from other disadvantaged groups, saying that for many first offenders and prison inmates, "the only characteristics that distinguish them from other disadvantaged groups in the population is that they got caught (1972, p. 3)."

Taggart advocated an employment strategy aimed at offenders on its own merits, not because it might reduce recidivism:

...proving there is a correlation between unemployment and crime does not prove there is a cause and effect relationship, nor does it prove that employment can be improved to a degree or at a cost which will make it an effective means of reducing illicit activity (1972, p. 16).

Prefiguring the disappointing results of controlled design interventions, Taggart found "no proof that any single manpower service has had more than a marginal impact on its recipients, and no proof that any combination of services can make a substantial contribution (1972, p. 157)."

Non-Experimental Studies of Released Prisoners

Recent work studying the labor market and crime experiences of individuals has focused on populations of prison releasees. This is a group which is relatively easy to survey, at least at the time when they are being released. However, as indicated above, so few arrestees actually go to prison that it is an open question whether or not their employment and crime experiences are representative of broader relationships in society, or whether they can be used to test the neoclassical choice model of crime. These studies do, however, have the advantage of using careful multivariate techniques, and do have individual level data, however truncated the samples might be.

Cook (1975) analyzed a sample of Massachusetts parole releasees to test the relationships between job stability and satisfaction, parole success, and recidivism. Testing a somewhat intuitive version of the economic choice model of crime, Cook found that "the empirical results are at least compatible with the prediction that improved job opportunities for parolees will reduce the recidivism rate (1975, p. 47)." He found that high unemployment rates of prison releasees are tied to high turnover on jobs, and that high turnover is related to low job quality: "Many releasees do not work steadily because they cannot find a job attractive enough to hold them for more than a few months (Cook, 1975, p. 26)."

Witte, alone and in conjunction with others, has performed several analyses of a sample of North Carolina prison releasees. The work generally "provides consistent but weak support for the expected inverse relationship between higher wages and crime, but weak, if any support, for the relationship between unemployment and crime (Orsagh and Witte, 1980, p. 10)."

Of particular interest was Witte's (1980) analysis of a version of the formal economic choice model, using arrests and post-release imprisonment as dependent variables, and including measures of criminal justice severity, wealth, and a host of so-called "taste" factors as independent variables. Witte analyzed the entire sample, and then subpopulations of offender types, under the reasonable speculation that different specializations in terms of criminal offense may indicate different underlying choice frameworks.

She found supportive evidence for deterrence among the pooled sample, but unexpected outcomes occurred when the sample was partitioned. Expected prison sentences appeared to be significant deterrents for non-income offenders, the probability of imprisonment seemed to affect misdemeanor income offenders, but felony income offenders showed no responsiveness to deterrence, a finding which seemed strange: "A priori, one would have expected income offenders to behave more rationally than other types of offenders (Witte, 1980, p. 79)." Legitimate opportunities were found to have only weak and marginal effects on recidivism; for

felony income offenders, higher wages were associated with increased recidivism measures, a result that Witte correctly characterized as "perverse (1980, p. 80)" in terms of the expectations derived from economic choice models.

The evidence on the relationship between employment status and crime from these individually based data gives, at best, weak support to the economic choice model. The studies avoid the problem of using macro level data to test the individual model, but program studies still suffer from problems of aggregation. MDRC, for instance, pooled data from 14 different sites across variable time periods, although they attempted to introduce controlling variables for some of these effects. Witte's interesting but complex results use a sample of prison releasees, and even among that group different subpopulation relationships are found, often behaving in ways that the economic choice model would not predict.

Individual level data is clearly more relevant in testing the economic choice model, both in terms of its econometric properties as well as in theoretical terms. But many problems still remain; individual level data, by itself, is unlikely to resolve many of the problems associated with the economic choice model. As with the aggregate analyses, one problem rests in the failure of these analyses to test alternative theoretical conceptions of labor market and crime relationships. With the admitted failure of the neoclassical model to provide determinate predictions about behavioral responses to changes in sanctions or rewards, the

analyses become strictly empirical, and theoretical interpretations are offered post hoc.

D. Segmented Labor Markets and Street Crime

Crime, we feel, is just like any other business... there's setbacks and there's deficits in crime, just like you run a business and there's a chance you might burn down...of course, the penalty for going bankrupt in crime is much stiffer, but at the same time your material gain is much more than it is in a regular business (Two career robbers quoted in Jackson, 1972, p. 30).

Interviewer: ...so you have a life that satisfies you?

S: No, I don't have a life that satisfies me. To me my life is not the way I want it, because I don't want to be dealing for the rest of my life, I want to have a job. If I don't have one, I got to do something.

A drug dealer, interviewed by the Vera Institute of Justice (1981, n.p.).

The details of occupational choice in illegal activity are not different from those encountered in the legitimate occupations.

George Stigler (1974, p. 59).

Analyses of crime and social policy are extensions of what Becker calls a "much more general theory," linked explicitly to labor market dynamics. The approach has become heavily laden with policy implications, focusing mainly on increasing the probability and severity of punishment. The theory and empirical studies do not usually address the relevance of structural economic factors for understanding crime and its causes.

The influence of this particular economic approach to crime can be seen in a 1972 conference held by the American Enterprise Institute for Public Policy Research, a conservative think tank. In the preface to the papers, the organizers noted the heavy predominance of economic work at a conference which was intended to provide interchange between economists and sociologists. The organizers, economists with a Chicago bent, explained this by claiming that the "work of the economists...has been of such a high quality that it is very hard to find a spokesman for the prevailing orthodoxy...our efforts to find criminologists to discuss these diverging views of the crime problems with economists were to no avail (Rottenberg, ed., 1973, n.p.)."

I might question whether the dominance of these economists is due to the superior "quality" of the analysis; even neoclassical theory notes that resources can sometimes flow to inferior goods. But the recent dominance of the neoclassical choice perspective in the study of crime cannot be doubted. Recall Becker's dismissal of around one hundred years of social theory in a single phrase; similar self-confident assertions by economists, usually tied to simplistic ideas about science, can be found in most writing on crime and economics. And sociologists have been placed on the defensive. Daniel Glaser (1977), a leading sociologist who has long considered economic conditions as important for the study of crime, titled a recent article "Sociology--Not Just Economics--As Key to Youth Unemployment and Crime Problems

(1978a)." It is impossible to imagine such a defensive title appearing even five years ago.

Part of the problem in interpreting these economic studies is their lack of alternative frameworks for understanding labor market dynamics. Tied to a single framework, the empirical discrepancies described above are not easily reconciled. In this section, segmented labor market theory is examined to help locate the problem of street crime in the context of labor market structure.

Structural Economic Issues and Crime

The choice-theoretic approach to crime is part of a broader theory which attempts to explain economic activity through a rigorous use of simple individual behavioral assumptions, with an unspoken deemphasising of the importance of structural factors in determining economic outcomes. Stigler's comment above reflects these assumptions, as does the empirical work reviewed in the preceeding section. The analyses reviewed share the basic neoclassical emphases: that individuals respond to socioeconomic conditions in simple, maximizing ways; that such responses, although not observed (and perhaps not observable) can be inferred from econometric uses of aggregate data; and that the choices confronting individuals in the labor market are basically determined by individual characteristics, especially training and education.

But, as also noted above, the empirical work comes to a variety of seemingly irreconcilable conclusions. Such problems are not unique to the studies reviewed. A recent review of empirical research on crime and economic factors by Long and Witte provided "only weak support" for simple models of crime (1980, p. 20)." A similar conclusion was drawn by Gillespie in his review of "theoretically informed" work on economic factors and crime (1975, p. 53).

It is easy to sympathize with Tropp's complaints about "this inconclusive and chaotic mass of data (1978, p. 53)." In his review, Tropp lamented the failure of econometric work to give any meaningful policy guidance for government about crime--the exact complaint that Becker levelled at sociologists. Tropp noted, on the issue of crime and unemployment, that "It is also possible...that all offender unemployment is explained by their being disproportionately young, unskilled, minority, and poorly located. It seems intuitively obvious that a great deal of offender unemployment is explainable by these shared characteristics (1978, p. 28)."

The "shared characteristics" included industry relocation to the suburbs, changes in the job structure towards service and "white collar" jobs, the "demographic bulge" due to the baby boom, student and part time employment, effects of business cycles on minority unemployment, changes in aggregate demand, military enlistment, school attendance rates, fertility changes, minimum wage effects, "frictional"

unemployment and unstable work habits, criminal opportunities, racial discrimination and discrimination against offenders by employers.

It seems like an exhaustive list. Confronted with this encyclopedia of problems, Tropp adopted a "pragmatic" approach, and focused on a narrow range of problems concerning ex-convicts, such as licensing restrictions, prison industries, minimum wage exemptions, and so forth. Tropp presented no attempt to explain the variety of factors he introduced--indeed, he brought in the factors mainly to treat most of them as unsolvable. No consistent theory of the economy or labor market was applied to try and make sense of these problems.

But without such an orientation, policy makers may replicate the failures of programs in the late 1960s. The Employment and Crime Project of the Vera Institute of Justice has concluded that employment programs aimed at averting crime offered types of employment which were restricted to low-level and relatively unrewarding work. Along with confusing evaluation techniques, the project staff (of which I was a member) found that employment per se does not avert crime for every subpopulation, since employment programs often appear to ignore the nature of the unsubsidized labor markets into which participants move (McElroy, et al., 1980).

Tropp's failure to try and resolve the economic issues he raised raises the possibility of more ill-conceived programs that could easily fail from their lack of systematic

linkage between program goals and broader economic problems. Conventional economic theory attempts to deal with such broad problems by reference to imperfections in labor markets that should be resolved over time by the pressures of competition. But as Thurow noted:

An observer of the economic game should be extremely reluctant to label anything that has existed for long periods of time a "market imperfection." If the phenomenon has survived, the chances are high that it is an integral part of the game and not a market imperfection. Or at least, this possibility should be seriously investigated and each of the deviant observations should be examined to see if they can be explained in some consistent manner that does not rely on ex post ad hoc market imperfection (1975, p. 55).

Labor Market Segmentation and Street Crime

The operation of the labor market has been hotly debated in recent economic work. The neoclassical human capital approach has been attacked with alternative theories of the labor market, in particular those gathered under the heading of segmented labor market theory.

Segmented labor market theory stresses the institutional and structural boundaries that determine how individual factors are played out in the labor market. There is a considerable range of disagreement in this approach as to how much individual factors matter, what the exact structure of jobs is and how it is determined, how much mobility occurs, and so on. But the unifying thrust to this approach comes from a variety of real world economic problems that

are not well accounted for by neoclassical economic theory. These problems include persistent racial and sexual discrimination, unequal returns to education by race and sex, persistent unemployment and underemployment, long term poverty, the operation of oligopoly and monopoly firms, the effects of labor unions, and the marginal impacts of job training programs.

The segmentation approach is unified on the rejection of human capital approaches as a sufficiently broad enough theory of labor market dynamics. For segmentation theory, the historical development of labor markets has led to sharp differences in occupational rewards in different markets, differences that are mainly due to structural factors and not to variations in individual worker productivity. In the lower segments of the labor market, occupational rewards are very low and of extremely limited variation.

Segmentation theory draws on critiques first developed in the postwar period by what can be termed a "new institutionalist" school of thought, which focused on large corporations and labor unions in the operation of the American postwar economy. John Dunlop, Clark Kerr, Richard Lester, Lloyd Reynolds, and others helped to formulate this perspective, which has been extremely important for policy makers even while it has failed to fully penetrate the textbooks (McGahey, 1978).

From this institutionalist work, economists who began to study the dynamics of the urban ghetto in the 1960s also

found a limited applicability of marginal utility theory. Labor economists, notably Doeringer and Piore (1969), refined work on "internal labor markets" within firms, and found that their results were "indicative of a major divergence between neoclassical theory and the complications of the world it seeks to describe." Piore (1977) further extended the analysis to the concept of a "dual labor market," where the dynamics of the entire labor market were seen to be different than the neoclassical description. The labor market was seen as dichotomized into "primary" and "secondary" markets, where different types of firms condition different labor demands:

The primary market offers jobs which possess several of the following traits: high wages, good working conditions, employment stability and job security, equity and due process in the administration of work rules, and changes for advancement. The other, or secondary sector, has jobs which, relative to those in the primary sector, are decidedly less attractive. They tend to involve low wages, poor working conditions, considerable variability in employment, harsh and arbitrary discipline, and little opportunity to advance. The poor are confined to the secondary labor market (Piore, 1977, pp. 93-4). ✓

The idea of segmented labor markets has produced theoretical refinements and empirical studies, as well as criticism from neoclassical labor economists.*

*"Segmented" labor markets is used to allow consideration of a number of ideas about the labor market, including dual theory, "job competition" models, and the like. The conceptions are united by their view of the labor market as structurally determined and discontinuous, and by their rejection

Although there is no developed theory of crime in the segmented labor market perspective, these theorists would agree with the neoclassical idea that people who engage in property crime are acting in a rational fashion; where the two schools differ is in their relative emphases on the structure of income opportunities. Work on the "irregular" or "underground" economy has documented the large amount of economic activity that goes on outside of officially measured and regulated channels (Ferman, et al., 1978). In terms of individual activity, Harrison (1972b) found that behavior of those in the secondary labor market involves moving between secondary jobs, welfare, training programs, and the "irregular" economy. Rather than seeing these as distinct areas of involvement, Harrison claimed that these represent "four kinds of labor-time-consuming activities in urban economies which display remarkably similar characteristics (1972b, p. 7)."

Neoclassical analysis treats the decision to engage in legal or illegal activity as determined by the relative costs and benefits from each activity. As mentioned earlier, returns from legal activities are held to be determined mostly, if not entirely, by an individual's human capital.

of a marginal utility and human capital approach to labor markets. Good bibliographies and interesting neoclassical critiques have been provided in Wachter (1974) and Cain (1976).

Wages are seen as returns to education and training; over a lifetime of earnings, more education and training is expected to result in more income. Those with more to spend (families with more money to invest in children) will produce workers who have more education, and they thus will get higher incomes over their lifetime. In turn, these families will have more to invest, locking workers from poorer families into an intergenerational pattern of low earnings.

Studies that examine long term intergenerational inequality have reached a variety of conclusions; a recent study for the Brookings Institution shows that socioeconomic background and educational attainment "account for as much as two-thirds of the variation of measures of success (Brittain, 1977, p. 3)." One root of a segmented labor market might be the long term intergenerational effects of unequal income distribution.*

Both economic approaches accept the view that property crime is a rational economic choice among income opportunities. The choice-theoretic approach tends to stress the time allocation aspects of these choices, while the segmented labor market approach stresses the income producing aspects, and explicitly denies the relevance of the time allocation perspective. Harrison, after noting the anecdotal evidence which shows that many people mix crime, legal

*Other important discussants in the debate on intergenerational success include Blau and Duncan (1967); Bowles (1972); Jencks, et al. (1972); and Jencks, et al. (1979).

work, welfare, and hustling, said that "conventional labor-leisure constructs cannot serve as useful models of the real time-allocation process in the inner city (1972b, p. 28)."

Harrison and other segmented labor market theorists have not done much direct work on crime, although some have begun to relate their labor market conception to crime. In work on the heroin market, Helmer and Vietorisz described the conditions confronting participants in that market as "labor market dualism (1974, p. 34)." Helmer (1975) has since written a book which attempted to tie changes in the enactment and enforcement of narcotics laws to swings in the business cycle, split labor market dynamics, and deterioration in expectations of economic welfare.

Gordon (1973, p. 175) has drawn explicit links between street crime and capitalist social relations, although his work on crime is not integrated with his work on dual labor markets. Gordon saw crimes as perfectly rational responses to the structure of institutions upon which capitalist societies are based (1973, p. 175), but focused more on differential enforcement by the state, and the ideological payoffs to the system from characterizing certain activities as crimes. Crime was seen as a byproduct of the system: "If we managed somehow to eliminate ghetto crime, for instance, the competitiveness, inequalities, and racism of our institutions would tend to reproduce it (Gordon, 1973, p. 184)."

Although the position of the segmented labor market theorists bears some resemblance to the neoclassical

approach with the stress on property crime as a rational activity, the segmented labor market theorists see street crime as one of a set of socially determined income options that is available to some part of the population. The two economic perspectives agree on rejecting much of the "deviance" approach to crime. Both also share the idea that crime is a response to economic conditions, whether those conditions are seen as determined by individual capacities and preferences (as in neoclassical theory) or by institutional and structural pressures (as in segmented theory).

A Segmentation View of Crime and Employment

The segmented labor market approach can help to make some sense out of the confusions in the econometric literature on crime. The strongest findings in aggregate econometric studies relate crime to the prevalence of low incomes in a population. A segmented labor market approach, focusing more on inadequate income and subemployment, and less on time allocation, is compatible with this finding. The relation might be strengthened if specific subpopulation rates and data were used, and if only property crime rates were examined. Conversely, the human capital approach is driven back onto its time allocation foundations, and has recently fostered a debate in the literature on whether or not crimes are best thought of as "timeless" or "time intensive" activities.

There are many unresolved problems in the segmented labor market literature. A variety of different studies stress different factors as crucial for describing and accounting for labor market patterns (e.g. job stability, racial discrimination, industry conditions). Different schemes have been proposed to describe segments, ranging from a strict duality to Freedman's (1976) empirical estimation of fifteen distinct segments.* Much of the empirical work has been necessarily done with aggregate data, leading to some confusion about how to tie individual level behavior to this model. (As noted earlier, this is critical for studying crime.)

Segmented labor market theory is not an exhaustive explanation of street crime. For instance, by any measure women are heavily concentrated in the secondary labor market, yet their street crime behavior is negligible when compared to that of men, except for gender-specific crimes like prostitution. It may be that women are segregated in crime just as in the legal labor market, but there is little evidence either way on this (Bartel, 1979). Crime is also importantly determined by the state and the structure of laws, and the allocation of public resources.

But some tentative notions about the relationship of property street crime and labor market segments can be advanced. Most young workers begin their careers in secondary

*Empirical work relating to segmentation can be found in Gordon (1971); Harrison (1972a); Rosenberg (1975); and Buchele (1976).

jobs. Recent studies show that whites move out into primary work much more than blacks do (Buchele, 1976; Osterman, 1980). Blacks also have higher arrest rates at older ages than whites. For a study of crime, it would be useful to see if blocked mobility for blacks leads to more street crime. This is sort of a variation on the "structure of opportunity" approach of some sociologists (Cloward and Ohlin, 1960). In urban settings, crime may appear as a relatively attractive income option to young people. Bullock's (1973) studies of inner city minority youth in Los Angeles found that these youth were well informed about relative wage rates for legal work, even though they had little knowledge of concrete work tasks. They also knew a great deal about the illegal opportunities that were available to them (e.g. drugs, theft, numbers) although here they had much more concrete knowledge of the tasks involved.

Confronted with a well known low paying job market in the legal sector, returns from criminal activity may be overvalued. Street crime often requires a "front" in the form of visible displays of money. The possibility of making a "big score" always seems present, especially at the start of criminal activity. Probability of arrest and punishment increases over time with increases in criminal activity (Glaser, 1978b). But as a person ages, the probability of the score lessens; difficulties with the criminal justice system increase; and the money obtained from crime turns out to be minimal. Only with ties into protected

criminal networks is a real criminal "career" possible (Ianni, 1974). Street crime may thus be serving as a means of resigning people to a lifetime of secondary employment; this may shed some light on the oft-noted, but little explained, "aging out" of crime reflected in arrest and crime statistics.

Segmented labor market theory, by providing a richer approach to the structure of the urban labor market, should be better able to guide research on crime and economic conditions. Becker's promise to provide "optimal policies" for controlling crime has not been realized. And the empirical work produced under the neoclassical banner has been contradictory and confusing.

This failure may stem from the failure to construct a meaningful theoretical explanation of labor market behavior. The adaptation of this individualistic choice framework to crime has come into serious question--not because street crime cannot be understood in the context of economics, but because the neoclassical approach may give a distorted picture of the labor market. The major advance resulting from its application has been an emphasis on the rational character of much property crime, deemphasizing the "deviance" approach that characterizes many sociological theories of crime. But the failure to consider the importance of structural and institutional events in determining the "choice sets" that are available to individuals weakens neoclassical choice theory for study of such issues as

crime. The use of segmented labor market theory may provide a superior understanding of how these "choice sets" are generated and maintained; it may also be able to provide a more meaningful framework for understanding crime.

CHAPTER TWO: HYPOTHESES, DATA AND SAMPLE CHARACTERISTICS

A. Developing Comparative Hypotheses

Studying the economics of criminal behavior should be advanced by an analysis of the two different labor market approaches to crime outlined in chapter 1. However, the problems with empirically testing these approaches should not be minimized. The neoclassical individual choice model is in theoretical disarray, and the segmentation approach was not designed to examine marginal variations in individual behavior. These considerations present problems in developing comparative determinate hypotheses. Empirical work can still be done on the questions, but interpretation of results will require caution and attention to the limits of econometric analyses.

As discussed above, the formal extension of the neoclassical framework finds the model unable to make testable predictions. Adapted from labor supply models, this model requires a highly restrictive set of assumptions to make determinate predictions about behavioral changes in response to changes in legal and illegal costs and benefits. This problem has been exacerbated by the use of aggregate data. Witte (1980) noted that use of aggregate data has led to

unrealistic assumptions about individual crime functions.* So the neoclassical models of criminal behavior have produced very complex theoretical results, results that do not translate easily into testable hypotheses.

It is possible to examine a set of simpler propositions--that, all else being equal, improved employment opportunities are associated with less severe and less frequent crime. This idea is compatible with both neoclassical and labor market segmentation approaches. The comparative analysis presented here focuses first on explaining labor market outcomes, and then seeing if labor market position is "traded off" against criminal behavior in any meaningful or consistent fashion. Both labor market theories, focusing on individual tradeoffs, must deal with the problems of monetary equivalents for gains and losses, income and substitution effects, risk preference, and "noxiousness."

The research reported here does not try to develop a new theory of individual labor supply and adapt it to

*Curiously, Witte went on to develop a formal time allocation model that suffered most if not all of the same problems she criticized, and did empirical work using a sample of prison releasees. After careful econometric analysis, she could only state that "the broadest conclusion we can make based on the evidence presented in this paper is that releasees from North Carolina's prison system during the 1970s behaved in a manner that was weakly supportive of the economic model of crime (Witte, 1980, p. 81)," by which she meant that weak effects were found for both deterrence and economic opportunity variables. However, these effects varied sharply between subpopulations of offenders, in ways which sometimes were the reverse of earlier neoclassical predictions.

crime. In fact, the attempt to develop a model of individual labor supply in a labor market segmentation framework may be problematic. For although the segmentation approach does not deny autonomous aspects to individual behavior such as labor supply, it does suggest that the principal determinants of labor market outcomes are the structural conditions of job opportunities. Further, behavior is seen to vary sharply between the segments, due to different job structures and potential "feedback" effects of job characteristics on the attitudes and skills of workers. Thus there is not, and probably cannot be, a continuous uniform labor supply function across segments. Hours and wages are seen as determined largely outside the individual's choices, especially in the secondary sector.

This view highlights another contrast with neoclassical labor supply. Neoclassical choice models of labor supply are designed to look at marginal changes of labor supply in response to changes in relevant parameters. Attempts to model these changes are in disarray, both theoretically and empirically. One result has been increasingly complex econometric modelling, leading to Byzantine theoretical and econometric approaches. Thus the neoclassical approach is quite far away from its goal of unambiguously predicting and measuring marginal changes.

Segmentation theory, on the other hand, is more concerned to account for group similarities and differences between groups, not marginal differences between individuals

on some uniform labor supply function. Differences ought to be pronounced between sectors, while marginal changes within sectors have not been extensively studied. This difference in accounting for occupational rewards remains at the heart of the debate between the two approaches. The questions of trading off labor market returns against crime must follow an examination of these differences. In the hypotheses developed below, the first set examines the effects of traditional human capital variables and crime variables on different labor market outcomes, with particular attention to segmentation issues. After that examination, crime variables will be treated as dependent, testing the effects of independent labor market variables. Since more research exists on the determinants of labor market performance, clarification of the two explanations for labor market outcomes will aid in focusing a more exploratory examination of dependent crime variables.

Hypotheses on Labor Market Outcomes

The first set of hypotheses focuses on the determinants of labor market success. As discussed above, neoclassical approaches contrast sharply with labor market segmentation approaches to this question. Neoclassical theory argues that human capital variables such as schooling and job experience should be positively associated with labor market success, notably with earnings as a dependent variable. Segmentation theories agree that this may hold in primary markets, but the associations between human capital

variables and earnings in the secondary market are seen as indeterminate.

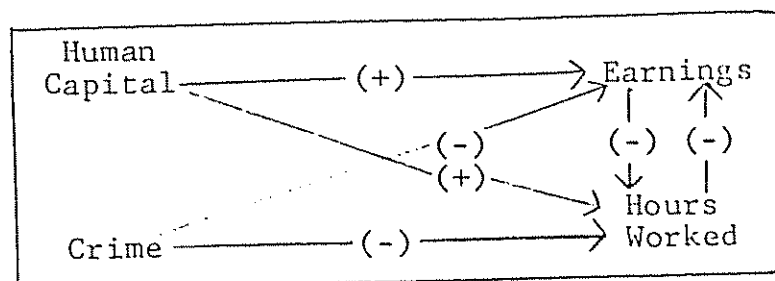
The role of crime in affecting labor market outcomes has a clear prediction in neoclassical choice theory, at least at the level of simple wealth considerations. To the extent that crime is a market activity, then legal and illegal activity are two parts of a whole which is traded off against non-market time. To the extent that crime is a non-market activity, there is a tradeoff between legal work and crime. In the second case, there ought to be a determinate negative relationship between higher returns to legal work and hours spent in legal work, and criminal activity. Crime and incarceration can also be seen as harming legal opportunities, due to withdrawal of time from legal work and thus from accumulation of human capital, and from the stigma of a criminal record. The relationship between legal returns and legal hours worked is subject, of course, to the problems associated with the "backward bending" supply curve of labor. Since work is disutility, higher wages at some point ought to be traded off against hours worked.

The segmentation approach sees no determinate opposition between legal and illegal earnings in the secondary sector. To the extent that crime offers another source of earnings, and provides an alternative category for time allocation, these tradeoffs are most likely to be found in the primary sector. For secondary workers, legal work and crime are not seen as opposites, but supplements. The issue of a

"backward bending" supply curve in the secondary market has not been addressed in the literature, because segmentation theorists do not see hours and wages as principally determined by individual preferences or returns to human capital stocks. Secondary employers require a highly plastic and variable supply of labor; for these reasons, the alleged drawbacks to criminal records are not seen as harming chances in the secondary market.

The hypothesized effects of human capital and crime variables for the two approaches are schematized below. For a neoclassical earning model, human capital raises attractiveness to employers, leading to higher earnings offered by employers. Higher paying jobs are usually more likely to be stable, full-time ones. Crime is seen as negatively associated with earnings and hours. Earnings and hours are themselves seen as negatively related at some level--the point where the labor supply curve bends backwards. For a labor market segmentation model such effects might only be found in the primary labor market. In the secondary market, there should be no determinate relationship between human capital, crime and earnings.

The neoclassical model is schematized below:



These relationships will be tested using human capital, demographic, and crime variables as independent variables, and earnings and work hours as dependent variables. The issue of the relationship between earnings and hours will also be examined. The analyses will be carried out for workers in different labor market sectors, to see if the shape and directions of the relationships change between the two labor markets, or whether there is a uniform labor supply and crime function that describes behavior in both sectors.

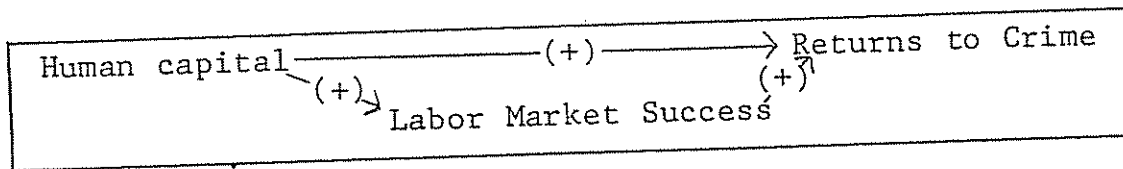
Hypotheses on Criminal Behavior

Following the analysis of labor market dependent variables, I will carry out several analyses on dependent variables measuring crime. Again, the two labor market theories will be compared on their abilities to explain criminal behavior.

A principal analysis will involve the determinants of crime payoffs. The neoclassical approach holds that the structure of legal opportunities available to an individual is determined by stocks of human capital. Since the individual is also presumed to choose between legal and illegal activities, levels of individual human capital indirectly affect the valuation of illegal opportunities as well. For this approach to hold, there should be a determinate and statistically significant relationship between levels of

human capital and labor market success. Because those with higher legal opportunities would require higher illegal net payoffs to compensate for the opportunity costs of foregone legal options, higher levels of human capital should be associated with higher labor market success, and both with higher returns to particular crimes, cet. par.

Schematically, the prediction looks like this:



Segmented labor market theory claims that the relationship between levels of human capital and labor market success does not hold for workers in the secondary sector; the presumed link to crime should also be attenuated or absent. Given secondary employment, there should be little or no statistically significant relationship between levels of human capital and labor market success, and between either variable and crime severity.

Examination of prior criminal history will also be conducted, to see what impact labor market experiences have over time in relation to prior arrest rates. Again, the neoclassical choice model would predict a negative relationship between criminal activity and work time and a positive relationship between criminal activity and legal earnings rates. Segmentation theory, again, would only expect to find such relationships in the primary sector.

B. Data Requirements--Sampling Individuals

As noted in chapter 1, almost all empirical analysis on crime and labor market issues has been performed with aggregate data. This follows the conventional practice of much economic analysis, where the problems of generalizing individual behavior functions to aggregate data are often not confronted or subsumed under assumptions about positivist methodology.

However, in the case of analysis of crime, the methodological problems have become critical. Partially because of the extreme sensitivity of the relevant policy issues, the data and method problems in the economics of crime have been subjected to intense scrutiny, far beyond the norm of most economic analyses. The most pressing example was the U.S. Solicitor General's use of Ehrlich's capital punishment work before the Supreme Court to help buttress an argument on the constitutionality of capital punishment.*

During this methodological examination, the use of aggregate data for individual analyses has been sharply criticized on empirical and theoretical grounds.** For the study of the hypotheses posed in this study, individual

*The court decided that the evidence was not conclusive enough to admit, although Ehrlich's work continues to be cited as "scientific" proof of the deterrent power of executions (McGahey, 1980a).

**For a fuller review of these issues, see the papers in Blumstein, Cohen and Nagin, eds., 1978, and the discussion in chapter 1 above.

level data is preferable for labor market and crime information.

The first issue to be confronted involves the population from which to draw a sample. Manski concluded that the "relevant decision making population for a study of criminal behavior should be the entire population of an area (1978, p. 408)."

While theoretically desirable, such a sample is all but impossible to obtain. Criminal behavior is something that people have an interest in concealing. Getting reliable data on criminal behavior from a random sample of an area's population, especially when desiring to couple such information with economic data, would be extremely time consuming and costly, with little probability of success or trustworthiness.

Those few analyses which have used individual data draw from two major sources: data from social program experiments, or cohorts of released convicts, often those on parole.* There are problems with both types of data. Program data is designed primarily to yield information comparing controls and experimentals on program participation effects. Programs use specific selection and eligibility criteria, as the experiments have been aimed at rather tightly targeted groups. Although it might be possible to

*See the discussion above in chapter 1.

mine program data in a different way (for instance, recombining those with criminal behavior as one subpopulation, and those without as another, without regard to experimental status), the truncated variation and different objectives of social programs limit the use of such data.

The second group studied, prison releasees, have the appeal of relative tractability. But prisoners are a small selective subpopulation whose labor market and other behavior after release on parole is importantly affected by the possibility of being returned to prison, a pressure which can be expected to affect labor market behavior.

Sample selection is therefore caught between the theoretically desirable but practically unobtainable random survey of an entire population, and the available but limited populations filtered through social programs or the criminal justice process. The sampling strategy adopted in this study, providing variation on crime and employment variables along with a practical although limited population source, is a random survey of felony arrestees. This population represents the best compromise between the two poles of theoretical rigor and practical feasibility. Witte suggested just such a sample as the "most realistic sampling frame" for individual analysis of criminal behavior (1980, p. 82).

Accordingly, the empirical research presented here makes use of a random survey of felony arrestees for the borough of Brooklyn, New York. Conducted during the summer

of 1979 by the Employment and Crime Project of the Vera Institute of Justice, this survey obtained detailed individual labor market data that was then coupled with official criminal histories. While such an arrest-intake population is not representative either of the unknown population of all offenders or the population of an urban area, it is superior to other less inclusive subpopulations that are sorted out by the workings of the criminal justice system.

Preliminary research done by the Employment and Crime Project revealed that in New York City during the first quarter of 1978, 38 percent of all criminal court cases were disposed of at the first court appearance, arrangement on the arrest charge. This very high rate of disposition at arraignment suggested that a representative sample of arrestees would have to be interviewed prior to arraignment. It was decided to explore sampling immediately following arrest during the police booking process.*

*The population available turned out to be slightly more restricted than all arrestees. Those arrested on violations and most misdemeanor charges are not routed through the arrest-to-arraignment process in the same manner as persons arrested on felony charges. Many are released shortly after arrest and before arraignment on a Desk Appearance Ticket (DAT). The study population is thus composed of males arrested on felony and serious misdemeanor charges in Brooklyn during July and August of 1979. This still provides the broadest feasible sample with individually matched employment and crime data.

Limitations of an Arrested Sample

An arrested population, though including a broader population than most other criminal justice research samples, is not without its limitations. The fact that the population is defined by a police arrest remains an important constraint. In effect, the police "selected" a population from the overall population of Brooklyn. In addition, of course, some substantial portion of the population is "self-selected" by engaging in criminal behavior. But no other data sources are available that fully remedy the limitations of studying an arrested population.

At first glance, cohorts of young, disadvantaged and minority males identified by the Census or other labor market surveys would appear to offer valuable comparisons to the labor market experiences of a defendant sample. For example, it might be possible to construct labor market profiles for selected Brooklyn areas, and then to compare unemployment, labor force participation and wage rates for these areas and cohorts with like indicators from the defendant sample. Although this aggregate level approach would be suggestive, it suffers from the drawbacks common to aggregate level research. For example, the comparison cohorts would probably contain high levels of offenders, but the actual proportions would be unknown. Any comparison data so drawn would contain a mix of offenders and nonoffenders, and no indicator is available for distinguishing between the two groups, indicating their relative sizes, or knowing the extent of their criminal behavior.

In drawing contrasts with other labor market cohorts, additional difficulties would arise because of the strong association between arrest probabilities and age. Nationally based data show a rapid decrease in annual per capita arrest rates with increasing age (Greenberg, 1977). This rapid drop in arrests with age, presumably reflecting actual declines in offending behavior, has been variously attributed to physical maturation, the onset of adult responsibilities, the cumulation of experiences with negative sanctions and other factors.

However it is to be explained theoretically, the phenomenon suggests that comparisons between arrestees and other labor market cohorts will have different meanings for different age groups. For example, among teenagers, national data suggests a relative peak in arrests. Sampling of young defendants might be more representative of the entire stratum within that age cohort. Among those aged 35 and older, however, the incidence of arrests in the overall population is at least an order of magnitude lower. A sampling of older defendants would yield data on a much narrower segment of that older cohort.

Selection of an appropriate study site was based on a mix of conceptual and logistical considerations. A site was required that enabled access to a large number of defendants who could be interviewed prior to arraignment. A broad population of defendants was desired so that generalizations to other jurisdictions could be facilitated. Thus, choice

of a research site required review of demographic and charge characteristics of defendant populations as well as processing procedures governing case flow from arrest to arraignment.

Administrative practices, environmental and architectural constraints, and differences in case volumes contribute to making police, prosecution and court procedures somewhat different in each of New York City's jurisdictions. A review of these procedures--including site visits and talks with experienced personnel on the scene--convinced the Vera researchers that implementation of the research in just one borough would greatly reduce the overhead and logistical difficulties associated with the study.

Once the decision to sample from a single borough was made, it was necessary to choose among the three large boroughs (Manhattan, the Bronx, and Brooklyn). Manhattan, the largest of the jurisdictions in terms of arrest volume, was found least desirable because many of the arrests in that borough result from the presence of centers of employment, entertainment and recreation that attract populations from outside of the borough and even outside of the city. The high incidence of commuters and transients in Manhattan would have posed both practical and conceptual problems related to the generalization of the findings.

The Bronx, which contributes the third largest arrest volume of the city's boroughs, was rejected because of administrative problems involved in defendant security during

the pre-arraignment period. It would have been more difficult to gain access to Bronx arrestees on a seven-day-a-week, 24-hour-a-day basis in that borough than in either Manhattan or Brooklyn. A second reason for rejecting the Bronx was concern about the borough's recent massive loss of population from its southern section. The rapid rise in crimes like arson associated with housing abandonment and depopulation would make a Bronx sample less comparable to other urban areas.

Brooklyn remained as the final borough that offered a sufficiently large arrest volume for drawing on arrestees. Several positive features also recommended the selection of Brooklyn. Traditionally, Brooklyn has maintained an independent identity within New York City. It has a large downtown shopping area, an extensive--though diminishing--industrial employment base, and many residential neighborhoods that span the socioeconomic ladder. With a population of over 2.5 million, Brooklyn on its own would be one of the ten largest cities in the United States. Brooklyn represents a relatively distinct and varied urban area, offering a range of legitimate and illegitimate opportunities without drawing in large transient groups from outside of the borough either for legal work or crime.

Labor Market Data Requirements

To test the hypotheses of this study, different pieces of information were needed on arrestees labor market experiences. For modeling the returns from legal activity,

information is needed on the time spent and wages received from employment, the ability to obtain alternate income from government transfers or personal sources, and other factors. Human capital analyses require information on schooling and special training programs, the existence of degrees and diplomas, on-the-job training, and durations of particular jobs, along with individual demographic data.

Many of these items are usually tapped by some standard labor market questionnaires. For instance, the household surveys of the Bureau of Labor Statistics were adapted so that standard labor market variables about employment and labor force participation could be computed for the survey population. The other information is collected by labor market surveys, and a variety of different surveys were examined, including the National Longitudinal Survey of disadvantaged youth.

But study of the labor market segmentation issues outlined above presents issues not generally dealt with in conventional labor market research. Most labor market surveys do not gather industry or occupational information in sufficient detail to allow implementation or development of labor market segmentation schemes. Labor market histories and characteristics of jobs such as unionization, on-the-job supervision and benefits are needed, along with precise descriptions of tasks performed on jobs and the type of production engaged in by employers.*

*Harrison and Sum (1978) discussed these issues for aggregate, establishment, and household surveys. Review of their work helped focus the data needs in my survey.

Most empirical schemes of labor market segmentation rely on coding jobs via the industry-occupation codes of the Census. In the design of the labor market survey for arrestees, the capacity for such coding was built in, along with direct information on wages, benefits, the existence of promotional ladders, unionization, on-the-job supervision.

(A copy of the labor market questionnaire is appended.)

Although a complete labor market history would be desirable, practical constraints in the Brooklyn police setting and the likely inability of respondents to remember distant events led to a decision to obtain information on all jobs and non-working periods for the two years prior to the instant arrest. Half of the sample was given a supplement which obtained information on the first and longest held jobs to examine mobility issues, and the other half was given a sample on barriers to and attitudes towards work.

Crime Data Requirements

Unlike the labor market data, there are substantial difficulties in deciding on and obtaining appropriate data to represent criminal behavior. The central problem involves the use of official records versus self-reported crime.

Both sources have problems. Official data only record arrests; thus, even assuming that all arrests actually represent crime (that is, no one is falsely arrested or inaccurately charged), the frequency of arrests is lower than

the frequency of crimes. Arrests represent a fraction of complaints to police, and this fraction varies by the type of crime. For some crimes, such as homicide, there are very high reporting and arrest rates. For other crimes, like burglary, reporting rates and arrest rates are very low. Thus arrests as a proxy for crime vary between types of crime.

Additionally, arrests for a given type of crime are unlikely to be distributed evenly among offenders. Some less skilled offenders, or those with characteristics targeted by the police for arrest through patrol deployment in certain neighborhoods or prejudice, will be arrested more frequently than other offenders arrested for the same type of crime. The above considerations lead all analysts to question the usefulness of official records, and have led some like Manski (1978) to favor the use of self-reported criminal activity.

Having decided to sample arrestees prior to their arraignment, self-reports were ruled out as a practical matter. Accused persons are unlikely to admit criminal behavior to a researcher while they wait in a police facility to appear before a judge. But in addition to this practical consideration, there are many problems with using self-report data, problems that other analysts too easily discount.*

*See the discussion above in chapter 1.

The importance of the arrest versus self-report problem depends to some extent on what the proposed analysis wants to measure. In some of the hypotheses outlined here, the type of crime will be used to model the prospective gains and losses from a single criminal event; thus the issue of the frequency of crimes represented by an arrest is less important. The number of arrests will also be examined as an indicator of the frequency of crimes over a time period, and here the question of arrest-to-crime frequency becomes more important, but unresolvable.

These difficulties must be admitted as limiting but not vitiating the analysis. At a minimum, such use of arrest data is the norm in most analyses, so the current study fits into the body of existing literature on the subject. The Vera research project is gathering self-report data on a follow-up subsample of the arrested population, and will try to shed some light on the arrest-crime ratio problem.

The source used for official crime data is the official history, or "rap sheet," kept by New York State. These histories were obtained through the generous cooperation of the New York City Police Department. Data on the type, severity, and frequency of all adult arrests, along with data on length of incarceration sentences and time actually served on such sentences is coded for each sample member. (A sample rap sheet, purged of individual identifiers, is appended.)

C. Sample Selection, Interviewing and Coding

Arrest-Intake Processing in Brooklyn

Arrest-intake in Brooklyn provided access to a large, representative sample of persons arrested in that borough. Prior to arraignment, virtually all defendants arrested on felony charges are brought to the Police Department's central booking facility for processing and fingerprinting.

Persons arrested for misdemeanors or violations are frequently released on a Desk Appearance Ticket (DAT) immediately following arrest, and are not processed through the borough's central booking facility. The New York State Criminal Procedure Law allows for DAT issuance in any non-felony arrest, but the Police Department imposes additional restrictions.*

While awaiting transfer to arraignment court, non-DAT defendants are interviewed by the New York City Criminal Justice Agency (CJA) to assess community ties for a recommendation of release without bail. Adult male defendants

*A defendant may not be considered for a DAT if wanted by other law enforcement officials; if arrested on a charge that would be raised to a felony given a prior conviction; or if arrested on "photographable" offenses (commonly weapons charges). Most DAT's are issued for non-property oriented offenses that are not of primary interest to the research. For example, during a sample week in 1977, a count by the New York City Criminal Justice Agency disclosed that 26 percent of a total of 215 DATs were for traffic offenses; 14 percent for conduct; five percent for assaults; 12 percent for narcotics, often marijuana possession; and 14 percent "other" offenses. Twenty-seven percent of the 215 DATs issued that week were for relatively minor property offenses such as "theft of services" (e.g., subway turnstile jumping).

are interviewed through the mesh screen of a holding pen located between police and CJA staff offices. After review of the available interview opportunities, it was determined that an interview administered immediately following the CJA community ties interview and conducted while the defendant remained in the police holding pen awaiting transportation to arraignment offered the best access.

Interviewing persons in a holding pen was far from ideal. The pen was often noisy and when filled to its capacity of 20 defendants offered little privacy. Although benches were available on the far wall of the pen, defendants had to stand during interviews. Pilot testing of the research interview did indicate, however, that interviewing in the pen was feasible and that the setting did not seem to impair the quality of the interview.

Drawing and Interviewing the Sample

During the eight weeks between July 5 and August 31, 1979, 912 men arrested in Brooklyn on non-DAT offenses were interviewed by Vera staff. The interviews, conducted 24 hours a day, 7 days a week at Brooklyn Central Booking, were administered within a few hours of arrest while the defendants awaited transfer to arraignment court.

Table 2.1 on the following page tabulates frequencies of interviews lost to the study for various reasons. The data are grouped into two parts: the first part describes cases lost before any attempt to start an interview, while

TABLE 2.1
BREAKDOWN OF PARTICIPATION IN THE WORK EXPERIENCE SURVEY

Fall-Off Between Sample Selection and Agreement to be Interviewed		
	<u>N</u>	<u>Percent</u>
Total selected for interview	<u>1511</u>	<u>100%</u>
Language problems*	43	2.8
Illness or intoxication	93	6.2
Refused interview	203	13.4
Transported to court prior to interview	<u>260</u>	<u>17.2</u>
Total excluded	599	39.6
Fall-Off Between Agreement and Completed Interviews		
Total agreeing to interview	<u>912</u>	<u>100%</u>
Interview stopped because:		
Defendant deemed unable to continue	11	1.2
Defendant refused to complete interview	19	2.1
Interrupted for transportation to court	<u>89</u>	<u>9.8</u>
Total interrupted	119	13.1
Completed interviews	793	--
As percent of total approached (N=1511)		52.5
As percent of total agreeing (N=912)		87.0

*These refer to defendants who could not speak English or Spanish; project staff conducted interviews in these two languages.

the second part shows cases where an agreement to the interview had been obtained but the interview was subsequently broken off. By far the largest cause of failure to interview defendants had to do with the location of the interviewing within the arrest-intake process. As can be seen from table 2.1, 260 arrestees out of a total 1511 initially selected (17.2 percent) could not be interviewed because they were transported to court. Since transportation to court was arranged by the police at nonsystematic intervals (when the holding pen was filled, or when a transport bus was available), it is likely that these lost cases do not at all affect the representativeness of the remaining sample.

Table 2.1 also presents a breakdown of losses from the sample that occurred after initial agreements to participate had been obtained. In all, 912 of 1511 defendants initially sampled agreed to participate (60.4 percent). About 13 percent of this group were ultimately lost because of subsequent events. One and two-tenths percent were deemed by the project's interviewer to be unable to continue; 2.1 percent stopped sometime after beginning the interview. Finally, transportation to court interrupted 9.8 percent of the interviews after they were begun but before meaningful progress had been made.

As shown at the bottom of table 2.1, the combined result of all of these factors is that only about half of the sample initially selected (52.5 percent) were included in the count of successfully completed interviews. However, 349 of the noncompleted interviews were due to involuntary

factors. Twenty-three percent of those approached either could not or would not participate, making the project successful with over three-fourths of those approached.

Since the survey interviewing was administered immediately following completion of the CJA community ties interview, sample selection was based on the final digit of that interview's pre-printed identification number. After the community ties interview, CJA interviewers recorded interview numbers and other identifiers in a log book adjacent to the holding pen. Vera interviewers then checked the log for interview numbers ending with "1," "3," "6" and "9," which provided a 40 percent sample of arrested defendants. Identifiers such as precinct arrest numbers and CJA interview numbers were not drawn sequentially, nor entered sequentially in the log, pushing the sample closer to a true random sample.

Vera interviewers, using the sampling procedure described above, identified prospective respondents and solicited their cooperation. Interviewers informed defendants of the purpose of the interview, and described confidentiality procedures. When the defendant's informed consent was obtained, the interview was administered.

Upon completion of the interview, interviewers solicited the respondents' continued participation in a follow-up study. Defendants were informed that they would be paid for future interviews and that their participation was voluntary. If they agreed to participate, names and addresses of contacts were recorded for the follow-up.

Interviewer training was aimed at minimizing sources of bias. Items and response categories were discussed and practiced at length. Mock interviews permitted comparison of interview responses. While there has been considerable discussion of the relative effectiveness of gender and race matches between interviewers and respondents, the three white (one male), two black (one male) and one Hispanic interviewer who administered the survey reported relatively little difficulty in obtaining the confidence of the respondents interviewed.

Data Management, Supervision and Coding

The logistical problems created by 24-hour, 7-day-a-week interviews conducted in the CJA office were offset by the quality of the interviewing, editing and coding staff, as well as by meticulous logging and management of data. Although interviewers were sometimes supervised at the site by Vera research staff, heavy reliance was placed on self-supervision. At the Vera office, interview logs were checked for accuracy and interviews were carefully reviewed. When errors or inconsistencies were detected, the interviewer was notified at the site by telephone or memo so that they could be corrected or eliminated.

The interview log data was recorded by interviewers at the site in a manner that permitted easy computer entry of the data. Once received in the office, the log data were entered in the computer, yielding daily tabulations of the

numbers of interviews completed or missed by each interviewer. Both the quality and flow of data were tracked throughout the course of data collection.

The bulk of the questionnaire contained open-ended questions with pre-coded response categories. Thus, few verbal responses had to be coded at the Vera office. However, data pertaining to employment activities and employer types were coded at the Vera office using standard 1970 Census occupation codes. This coding scheme facilitated comparability of the Vera data set with nationally based surveys.

Criminal histories were divided into three time frames oriented to items in the questionnaire. Arrests were summarized as occurring in the "Interview period" (the two year period covered by the interview); the "Prior period" (the period preceeding the "Interview period"); and the "Subsequent period" (the year following the interview), all taken from official records.

The number of arrests, convictions, dispositions and time sentenced in jail and prison was recorded for each of the three time periods specified. In addition, arrests occurring in the "Interview" and "Subsequent" periods were summarized in terms of the felony-misdemeanor weights of top charge types contained in each arrest. Top charge type reflected an interest in income producing crimes. The coding scheme employed allowed for recording of the top felony weights and charge types of the top income-producing crime, as well as the top non-income-producing charges, prevented

the "masking" of certain property charges (e.g., larceny) that are frequently outweighed by dangerous non-property charges (e.g., weapons or assaults).

In addition to arrests and dispositions, "time at risk" could be computed from incarceration time coded from the arrest report. Incarceration lengths reported on the arrest report were coded, in addition to actual or project devised estimates of time spent in incarceration.*

D. Labor Market and Crime Characteristics of the Sample

Labor Market Experiences of the Sample

Tabular examination of the labor market experiences of the arrestees sample indicates important relationships among age, race and labor market experiences. Age is a critical variable in examining changes in labor market position. Work status is related to age, although labor force participation is less so. Involvement in school helps explain some of the labor force data, but not all of it. The jobs held by the defendants reflect the lower paying jobs to be found in urban labor markets. The sample's unemployment rates and job durations compare unfavorably with national labor market

*Since prison admission and release dates appear on the arrest reports, specific months incarcerated were determined directly from the record. Local jail sentences, with a maximum of one year, were calculated at 70 percent of the maximum. This reflects a standard "good behavior" allowance awarded routinely in New York City jails. Hardly any prisoner serves the full maximum on a local jail sentence.

cohorts, but not always as unfavorably as traditional literature on employment and crime might suggest. In general, the survey respondents have very poor labor market experiences, but they are in the labor market. They do not represent the stereotype of people solely devoted to crime or an "underclass" totally removed from legal markets (McGahey, 1981).

Table 2.2 shows that, as expected, the work status of the sample improved with age. In the table, three interrelated measures of labor market status are utilized. First, the table compares the four age groupings in terms of employment-to-population ratio (given by the ratio of those currently employed at the time of arrest to the total sample). This ratio increases steadily with age from .466 for 16-19 year olds to .633 for those aged 35 and older. Matching the sharp drop-off in employment to population ratios is an even sharper decline in unemployment with increasing age. Slightly less than half (42.0 percent) of the youngest defendants report themselves as unemployed in comparison to 12.7 percent of the oldest. In contrast to the national labor market cohorts, table 2.2 presents the puzzling picture of a relatively constant labor force participation rate across age groupings. It would be expected that labor force participation would increase with age, a pattern not always supported by the defendant sample.

One factor in the young cohort's high labor force participation rate is its relatively low level of school enrollment. Only about a quarter (24.6 percent) of sampled

TABLE 2.2
AGE AND LABOR FORCE STATUS AT
TIME OF ARREST

<u>Age</u>	<u>Labor Force Status at Time of Instant Arrest</u>		
	<u>Employment to Population Ratio</u>	<u>Labor Force Par- ticipation Rate</u>	<u>Unemploy- ment Rate</u>
16-19	.446	.769	.420
20-24	.535	.843	.365
25-34	.619	.801	.227
35+	.633	.725	.127
All ages	.526	.787	.332

defendants aged 16 to 24 are enrolled in school, compared with 45.5 percent of all U.S. blacks and 46.5 percent of all U.S. males in this age range.

The pooled sample data obscure the sharp differences among ethnic and racial groups within the sample.. Table 2.3 shows labor market indicators by race and age.

Employment to population ratios are lower for young blacks and Hispanics than for whites. All racial groups are roughly equivalent in the 25-34 and in the 35 and older brackets. Labor force participation rates (LFPR) reflect this pattern, although less sharply. For teenagers, whites have a higher LFPR than blacks or Hispanics. For 20-24 year olds, blacks and whites have roughly equal labor force participation, while Hispanics remain lower. Black and white rates decline with increasing age, although the white rate drops much more sharply. The rate for Hispanics continues to increase for the 25-34 year old group, dropping off sharply for the 35+ group.

These two patterns of employment to population and labor force participation help illuminate the unemployment rates for the sample. As table 2.3 shows, for all racial groups, unemployment declines with increasing age. But for blacks and Hispanics, the unemployment rates are consistently higher than the unemployment rate for whites. The sharp fall in white unemployment rates between the 16-19 year old group and the 20-24 year old group is largely due to increased employment, as indicated by the sharp rise in employment to population along with a rise in labor force

TABLE 2.3

AGE, RACE/ETHNICITY AND LABOR MARKET
STATUS AT TIME OF ARREST

Age	Employment to Population Ratio		Labor Force Participation Rate			Unemployment Rate			
	Black	Hispanic	White & Other	Black	Hispanic	White & Other	Black	Hispanic	White & Other
16-19	.415	.432	.591	.755	.779	.833	.450	.446	.291
20-24	.487	.510	.758	.867	.784	.879	.438	.349	.113
25-34	.606	.667	.600	.798	.881	.700	.241	.243	.143
35+	.633	.600	.579	.769	.680	.632	.140	.118	.082
All ages	.505	.516	.628	.792	.789	.790	.363	.369	.218

participation. Employment gains outstrip the rise in labor force participation for whites in these different age groups.

But for blacks, unemployment rates do not fall significantly until the 25-34 year old group. Like the whites, black labor force participation rises with this group, but unlike the whites, employment gains are not sufficient to reduce unemployment rates. For whites, the employment to population ratio jumps from .591 for 16-19 year olds to .758 for 20-24 year olds. For blacks, the gain is only from .415 to .487, making a marginal improvement in employment, but one that is almost entirely offset by the rise in labor force participation; thus the unemployment rate for 20-24 year old blacks barely changes.

Hispanics seem to stand between these two patterns, showing sharper rises in employment to population than young blacks, although less sharp than for whites. Since Hispanic labor force participation hardly changes at all between the two youngest groups, this gain in employment lowers their unemployment rate from 44.6 percent for 16-19 year olds to 34.9 percent for 20-24 year olds. For the older groups, especially the 25-34 year olds, black and Hispanic experiences are roughly equivalent. Employment to population ratios improve; Hispanic labor force participation rises while the black rate falls somewhat, leading to similar unemployment rates. For white 25-34 year olds, employment to population and labor force participation both fall off, leading to a deceptively stable unemployment rate.

These data suggest that 25-34 year old white arrestees are different than the younger white arrestees, and also unlike the black and Hispanic 25-34 year olds. The 25-34 year old whites show declining attachments to the labor market, while the 25-34 year old black and Hispanic groups continue to show relatively high rates of employment and labor force participation. This pattern of declining labor market attachment seems to hold for the 35+ year old whites and Hispanics, while the blacks in this oldest age group have more stable labor force indicators.

Again, though, lower unemployment rates for older blacks and Hispanics result from different movements. The 35+ Hispanic group goes through a change similar to the 25-34 year old whites--employment to population and labor force participation drop, leading to a lower unemployment rate, indicating less attachment to the legal labor market. For 35+ year old blacks, employment to population rises and labor force participation falls slightly, leading to an improvement in the unemployment rate.

The Hispanic arrestees have similar patterns to those of the whites, but they occur at older ages. For whites, the rise in employment and labor force participation occurs for the 20-24 year old group; for Hispanics, these rises are most pronounced for the 25-34 year olds. White employment and labor force participation drop off for the 25-34 year olds, while for Hispanics this drop occurs for the 35+ year olds.

The black pattern is somewhat less comparable--sharp rises in labor force participation for 20-24 year olds, like the whites, but without the white or Hispanic gains in employment. Employment improves for the black 25-34 and the 35+ year olds, with a fall in labor force participation. This is unlike Hispanics, whose rises in employment are coupled with stable or rising levels of labor force participation.

These patterns suggest more labor market hardship for black arrestees. Their employment to population ratios do not improve until the 25-34 year old group, a group which also shows declining labor force participation. These two contradictory tendencies suggest that the older black arrestees may be composed of two relatively distinct groups, one more attached to the labor market via employment, and a second group more outside of the labor force. In contrast, the older white arrestees may be more homogeneous--the white 25-34 year olds have lower employment and labor force participation. The Hispanic pattern resembles the white pattern, but with a lag in age.

The comparisons to national data shown in tables 2.4, 2.5 and 2.6 indicate that white and black arrestees are in worse condition than their respective national labor force cohorts. For August 1979, the black arrestees' unemployment rates were higher than those for all black males; this finding holds up in all age categories. The same is true for whites. Black and white employment to population ratios are

TABLE 2.4

EMPLOYMENT TO POPULATION RATIO FOR SAMPLE ARRESTEES AND ALL U.S. MALES, AUGUST 1979, BY AGE AND RACE/ETHNICITY

Age	Employment to Population Ratio*					
	Blacks in Sample	All U.S. Black Males	Whites & others** in Sample	All U.S. Whites	All Sample	All U.S. Males
16-19	.415	.423	.591	.649	.446	.620
20-24	.487	.675	.758	.863	.535	.841
25-34	.606	.827	.600	.941	.619	.919
35+	.633	.650	.579	n.a.	.633	.705
All ages	.505	.643	.628	.750	.526	.737

TABLE 2.5

LABOR FORCE PARTICIPATION RATES FOR SAMPLE ARRESTEES AND ALL U.S. MALES, AUGUST 1979, BY AGE AND RACE/ETHNICITY

Age	Labor Force Participation Rates*					
	Blacks in Sample	All U.S. Black Males	Whites & others** in Sample	All U.S. Whites	All Sample	All U.S. Males
16-19	.755	.563	.833	.737	.769	.714
20-24	.867	.817	.879	.925	.843	.912
25-34	.798	.914	.700	.963	.801	.957
35+	.769	.683	.631	n.a.	.725	.724
All ages	.792	.753	.790	.805	.787	.799

*Source for national data: U.S. Department of Labor, Bureau of Labor Statistics, Employment and Earnings, 26:9 (September 1979), Table A-3, p. 23.

**Does not include Hispanics.

TABLE 2.6

UNEMPLOYMENT RATES FOR SAMPLE ARRESTEES AND ALL U.S.
MALES, AUGUST 1979, BY AGE AND RACE/ETHNICITY

Age	Unemployment Rates*					
	Blacks in Sample	All U.S. Black Males	Whites & others** in Sample	All U.S. Whites	All Sample	All U.S. Males
16-19	.450	.247	.291	.119	.418	.132
20-24	.438	.173	.113	.067	.365	.078
25-34	.241	.095	.143	.023	.227	.040
35+	.140	.048	.082	n.a.	.127	.026
All ages	.363	.092	.218	.042	.332	.047

*Source for national data: U.S. Department of Labor, Bureau of Labor Statistics, Employment and Earnings, 26:9 (September 1979), Table A-3, p. 23.

**Does not include Hispanics.

also worse for the arrestees than for national age cohorts, with the notable exception of the 16-19 year old group.

This difference for teenagers can be clarified by looking at labor force participation rates. The arrestees have higher rates of labor force participation and similar employment to population rates, leading to higher unemployment rates. Some of these higher labor force participation rates might be explained by school status, although the fact that the sample was drawn in the summer lessens the power of that explanation. But school attendance is likely related to lower family and household income for the arrestees than for national cohorts, since the latter include families from across the income and class spectrum of American society. The teenagers in the arrestees group seek work more than the national cohort, as shown in their higher labor force participation, but their ability to find work is about the same as for all teenagers in the respective racial cohorts. (The sharply better employment rates for whites relative to blacks should not be overlooked.) Thus for the teenage arrestees, higher unemployment rates may be more a reflection of their more pressing income needs than an indicator of their relative failure to locate employment.

For the older arrestee groups, labor force participation rates resemble national data for 20-24 year olds; for arrestees 25 and older, these rates lag behind. Employment to population ratios for the arrestees drop behind those of national cohorts starting with the 20-24 year olds, with the notable exception of the 35 and older black group. This

group also has higher labor force participation rates than the national cohort, partially because more of the arrestees are close to prime working ages. (The national cohort includes men up to the age of 65.) But in general, more severe unemployment rates for the older cohorts reflect sharply worsened employment conditions, as their lower labor force participation rates would lower their unemployment rates if their level of employment stayed constant.

So for teenage arrestees, the problems associated with underemployment are reflected primarily in their high labor force participation relative to employment, reflecting possibly higher income need and less involvement in school than other American teenagers. For older arrestees, the pattern is different, with lower employment tied to lower labor force participation, indicating less attachment to good labor markets.

Other labor market data can be compared to national cohorts to clarify the position of the arrestees. For instance, job durations for sample members roughly correspond with the data from national cohorts. Currently employed arrestees had been in their current job for a median interval of 6.5 months, compared with 8.4 months among all U.S. males aged 16 to 24. But the occupational experiences of the defendant sample are less similar in other respects. Table 2.7 shows the distribution of sample members by Census two-digit occupational classifications compared to selected other groups.

TABLE 2.7

CENSUS OCCUPATIONAL CLASSIFICATIONS OF CURRENT OR MOST
RECENT JOB OF ARRESTEES AT TIME OF ARREST, COMPARED
TO ALL U.S. BLACK MALES AND ALL U.S. MALES,
AUGUST 1979

Occupational Classification	Arrestees Sample	All U.S. Black and Other Males	All U.S. Males
Professional and Technical	.048	.107	.143
Managers and Administrators	.046	.064	.141
Sales Workers	.048	.027	.058
Clerical and Kindred	.095	.076	.061
Craft and Kindred	.166	.163	.215
Operatives, except Transport	.137	.149	.116
Transport operatives	.068	.084	.058
Non-farm laborers	.185	.128	.080
Service workers, except private household	.201	.159	.085

Note: columns do not add to 1.00; farm workers and managers excluded. Source for national data: U.S. Department of Labor, Bureau of Labor Statistics, Employment and Earnings, 26:9 (September 1979), table A-22, p. 38.

The data show important differences by occupational type when the sample is compared to the labor force as a whole. The most noticeable characteristic of the sample's occupational distribution is its concentration in lower-tier blue collar jobs. Two categories--nonfarm laborers and service workers not in private households--account for 38.6 percent of the jobs currently held compared to 16.9 percent for males and 29.5 percent for all black males in the labor force.

There are two likely reasons for this. First, the sort of jobs available in the New York City labor market, especially for young males, are concentrated in the unskilled labor and service sector--helpers on construction sites, warehousemen, food service workers. But in addition to this effect of the sample's age, the concentration of sample members in laborer and service jobs may also likely reflect the secular transformation of the New York City labor market. Manufacturing jobs have been lost over the last two decades, principally from outside Manhattan. Service jobs, especially those in food service industries, generally offer significantly fewer long term job ladders than older manufacturing entry-level jobs. To the extent that this is true, there are fewer long run incentives to stick with such jobs, or to develop a set of work attitudes and behaviors based on the expectation of mobility. This may in turn increase turnover and instability, and have deleterious long run effects on younger individuals' future job paths. The instability in labor force experience seen earlier for young

blacks may in part be tied to these secular job changes. With fewer stable jobs, entry and attachment to the labor force may be delayed, possibly leading to a longer period of criminal explorations.

The relationship between disappointments in the legal labor market and extended criminal explorations has been suggested by Bullock (1972) and Osterman (1980). With lower payoffs to legal work, there may be more incentive for males in their late teens to continue or even increase their involvement in more serious organized criminal activity, in an attempt to create some sort of "shelter" not available to them in the legal market.*

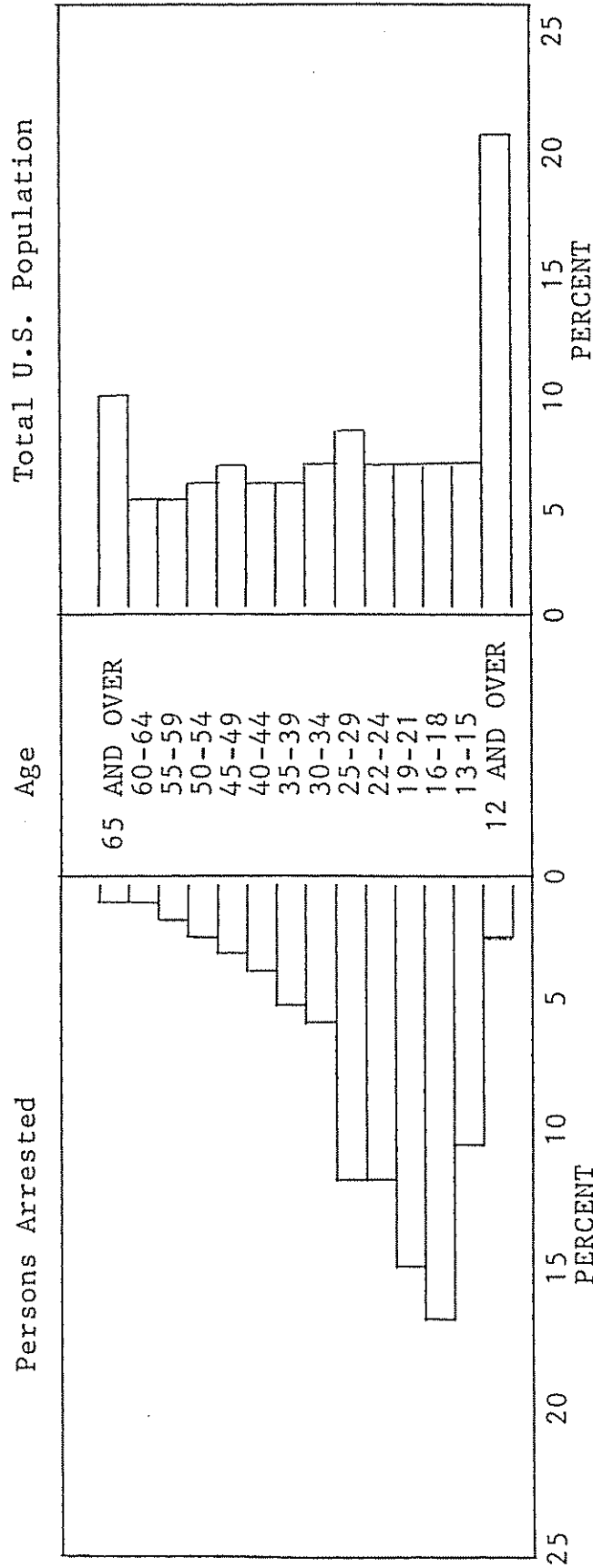
Arrest Histories of the Sample

As with labor force experience, age and race are also key factors in arrest histories. Figure 2.1 shows that the arrested population in the United States is younger than the general population; the sample members show a similarly youthful age profile. The age groupings of arrestees can also be compared to labor force data for the population as a whole.

Table 2.8 presents the age distribution of the defendant sample separately for three race/ethnicity groupings and for the total sample. The table shows that, in keeping with national arrest data, the Brooklyn sampling yielded a predominantly young and heavily minority sample. Looking

*For a discussion of legal "shelters," see Freedman (1976).

FIGURE 2.1
AGE DISTRIBUTION OF PERSON ARRESTED FOR ALL OFFENSES AND
OF TOTAL U.S. POPULATION, 1977*



*Figure adopted from Flanagan, T.J., Hindelang, M.J., and Gottfredson, M.R., eds., Source-Book of Criminal Justice Statistics-1979. U.S. Law Enforcement Assistance Administration, National Criminal Justice Information and Statistics Service (Washington: U.S. GPO, 1980), p. 460. Original data from U.S. Department of Justice Federal Bureau of Investigation. Uniform Crime Reports for the United States, 1977 (Washington: U.S. GPO, 1978), p. 171.

first at the age distribution, the right-hand column of table 2.8 shows that two out of five sample members are aged 16 to 19, and that almost another quarter of the sample is between the ages of 20 and 24.

The sample is also heavily populated by minority group members. Looking across the bottom row of table 2.8, it is seen that about three out of five defendants in the sample are black and about a quarter of the sample is Hispanic; 17 percent of the sample is white.*

While the sampling of Brooklyn criminal court defendants yielded a very young and largely minority sample, the percentages inside table 2.8 do not indicate further within-sample relationships between ethnicity and age. Black, Hispanic and white defendants are found in equal measure across the four age categories in the table.

Though the jail-based interview did not collect detailed information on the social and personal backgrounds of the defendants, it was discovered that relatively few sample members were married. About a quarter of the sample reported either a current legal marriage (16.0 percent) or else a common-law relationship (9.4 percent), not a surprising finding for such a young group.

The immediate cause for intake into the sample was an arrest on felony or serious misdemeanor charges. Table 2.9

*The tables present data for "white and other"; almost all of these are white, with four individuals classified as "other."

TABLE 2.8
AGE AND RACE/ETHNICITY OF ARRESTEES SAMPLE

Age	Race/Ethnicity			Total Sample
	Black	Hispanic	White & Other	
16-10	.575	.250	.175	.430
20-24	.582	.252	.166	.231
25-34	.599	.229	.172	.215
35+	.595	.234	.171	.124
All ages	.584	.244	.171	1.000

TABLE 2.9
AGE AND FIRST ADULT ARREST STATUS

Age	Instant Arrest is First Adult Arrest	Previous Adult Arrests
16-19	.395	.605
20-24	.269	.731
25-34	.207	.793
35+	.392	.608

shows whether this encounter with the criminal justice system was the first in the defendant's adult experience.* Contrary to the researchers' expectations, the incidence of first arrests in the sample did not decline smoothly with increasing age. Though the youngest age grouping did show the greatest incidence of first arrests (39.5 percent), those thirty-five years and older had a similar level (38.8 percent).

Two major factors may account for this. The first is that some of the 35-and-older group may be drawn from a distinctive population that did not have young crime involvements, and only now appears in the sample due to some non-employment related event. Examples would include older, relatively stable workers who get involved in fights and are brought in on assault or other personal conduct charges. The second explanation would involve a cohort effect. (Sample members who are over 34 years old were 16 years old in 1961.) Crime rates in the United States, especially among the young, began to rise rapidly in the 1960s, for reasons that are still unclear (Silberman, 1978). The absence of early arrest experiences of older sample members may thus represent the norm for their cohort, and the differences between them and the younger sample members may be tied to such a cohort effect.

*As is true in most jurisdictions, data concerning the juvenile involvements of the sample were unavailable from official sources.

The data presented in table 2.10 lend some support to the notion that the older sample members are drawn from a distinctive population. As can be seen, there is a rather sharp decline in the proportion of income oriented arrests* occurring with increasing age. About 70 percent of the 16-19 year olds, compared to 32 percent of the over-35 year olds, were arrested for income producing charges. This reinforces the idea that the older group may include persons without extensive earlier criminal involvements, whose current arrests are related to largely noneconomic factors.

Further examination of specific types of property crimes committed by the various age groups shows that among income offenders, charge types vary with age. Table 2.11 shows two possibly interdependent trends. First, there is a continuous and relatively sharp falling off of robbery charges with advancing age. About a third of 16-19 year olds are arrested on a robbery charge but only 11.1 percent of the 35 and older group are so charged. Table 2.9 suggests that robbery is a "young man's game." This suggestion is reinforced by the Vera project's ethnographic work and conversations with police officials. In New York City, comparatively few robbery arrests result from so-called "cowboy robberies," where an armed gunman enters a business establishment to obtain money. Rather, the majority of robberies

*Income oriented arrests are defined here as robbery, arson, grand or petit larceny, burglary, criminal impersonation, possession of gambling records, sale of marijuana or other controlled substances, and promoting prostitution.

TABLE 2.10
AGE AND INCOME ORIENTED ARREST CHARGES

Age	Number of Income Oriented Arrest Charges in Instant Arrest	
	One or more	None
16-19	.695	.305
20-24	.575	.425
25-34	.563	.437
35+	.324	.676
All ages	.619	.382

TABLE 2.11
AGE AND TYPE OF MOST SEVERE INCOME
ORIENTED ARREST CHARGE
(Severity According to New York State Penal Law Weight)

Age	Type of Most Severe Income Oriented Arrest Charge				
	Robbery	Burglary	Grand Larceny	Petty Larceny	Other
16-19	.330	.341	.216	.092	.021
20-24	.233	.358	.158	.142	.109
25-34	.204	.296	.148	.222	.130
35+	.111	.389	.167	.056	.277
All ages	.269	.339	.186	.127	.079

represent the use of force or threat of force in the course of street mugging. Ethnographic notes from the Vera research suggest that this type of offense is particularly inviting to young males without recourse to more structured crime opportunities and possibly without the skill or experience to engage in less risky, more rewarding offenses.

The other clear-cut trend in table 2.11 shows a continuous, sharp increase in "other" income oriented offenses (the last row in the table). These offenses appear to diverge from a narrow definition of income-oriented offenses and in some cases are victimless crimes, such as possession of gambling records, which often indicates employment in the numbers business. The relatively high incidence of such mixed cases among the oldest grouping (27.7 percent) and their infrequency among the youngest defendants (2.1 percent) again suggests that with increasing age, defendants are drawn into the sample from distinct criminal populations. The data suggest not only that the probability of arrest declines with age, but that the types of criminal involvement leading to arrest differ for different ages and that these differences represent a trend away from risky income oriented offenses.

CHAPTER THREE: THE DETERMINANTS OF LABOR MARKET OUTCOMES

This chapter analyzes the labor market experiences of the arrestees sample. Multivariate analysis is conducted on the determinants of wages and hours for the sample, to test the hypotheses on labor market outcomes discussed in chapter 2. Human capital, demographic, labor market segmentation and crime impacts on labor market outcomes are examined.

The issue of evaluating labor market segmentation requires operationalizing a scheme of segmentation and applying it to the arrestees sample. (Appendix B describes my adaptation of Gordon's segmentation scheme.) After assignment of segments, the characteristics of the sample are discussed in light of their primary or secondary worker status. Following that discussion, analyses of earnings and hours worked are presented in single equation, two stage least squares, and recursive formats, with descriptions of the variables used, analytic techniques employed, and interpretation of the empirical findings.

A. Empirical Use of Labor Market Segmentation

Labor market segmentation theory is a relative newcomer to labor economics, having its first important mention in a chapter appended to Doeringer and Piore's work on internal labor markets (1971). Harrison (1972) discussed dual labor market theory, but the empirical work he conducted was more

focused on issues such as credentialing, unequal returns to human capital by race, and suburbanization of jobs.

The first major attempt to quantify and test segmentation theory was in a dissertation by Gordon (1971). Gordon used factor analysis to distinguish jobs by primary and secondary markets, and tested labor market outcomes in different cities from a dual labor market perspective. Later empirical work on segmentation (Rosenberg, 1975; Buchele 1976) made use of variations on Gordon's work, coupled with data on occupational characteristics drawn from the Dictionary of Occupational Titles.

Gordon's work is the main empirical basis in economics for segmentation approaches, but two others deserve mention. Andriasani (1973) tested a dual scheme based on dividing a median wage distribution with some industry controls. He seemed unaware of Gordon's work, writing in 1973 that no dual labor market theorist "has developed an operational definition so that specific jobs might be classified (p. 35)." Andriasani tested an extreme version of dual theory--the existence of "impenetrable boundaries separating two broadly defined market sectors (p. 86)." He also divided his markets largely on the basis of individual median earnings, which led to the unsurprising result that a fair amount of movement was found from one "market" to the other.

Any scheme that divides a continuous distribution like earnings may fail to reflect possible underlying discontinuities masked by the continuity of the earnings distribution. For these reasons, some of Andriasani's rejections of

dual labor market theory are not convincing. He did find, even with his problematic measure of dual markets, that "dual market theory contributes substantially to an explanation of intercolor differences in labor market success", as his research showed pervasive racial discrimination in primary jobs and "inconsistent and ineffectual" payoffs to human capital in the labor market (Andriasani, 1973, p. 90).

Freedman (1976) conducted a more sophisticated study of the determinants of earnings distributions to identify segments. Using the computer technique of Automatic Interaction Detector (AID), which is essentially a sequential one-way analysis of variance that partitions data into mutually exclusive subgroups, Freedman identified 15 distinct segments based on explaining variance in mean annual earnings for 270 industry-occupation groups. (To my knowledge, no one has compared Freedman's empirical work with the various works stemming from Gordon studies.)

Freedman's 15 categories cover the entire range of the industry-occupation distribution, as do Gordon's. I have used an adaptation of Gordon's most recent work on segmentation in the research that follows. This scheme is chosen for several reasons. First, it can easily be adapted to a primary/secondary distinction, whereas Freedman's 15 sectors do not offer any clear means of grouping; in addition, the truncated nature of my sample limits the usefulness of a fine-grained 15 segment scheme. Second, Gordon's scheme is based more on variables that are addressed by economic

theory, rather than using a computer algorithm for identification of segments. Finally, Gordon's work avoids relying too extensively on subdividing a continuous earnings distribution, in contrast to Andriasani or Freedman. Later work which modifies Gordon's did not seem significantly different for empirical purposes.

The only other alternative would have been to develop some original scheme of segmentation based on the job information provided in the sample. This was rejected due to the truncated nature of the data set. An arrestees sample, while useful for studying crime and labor markets, is not a sample of the entire range of industries and occupations found in the U.S. economy. It is likely to be disproportionately filled with secondary workers, if for no other reason that most arrestees are young, male and disproportionately minority. For this reason, development of an original segmentation scheme based on the sample data was ruled out.*

Once jobs had been assigned a segment score, there remained the problem of classifying workers in terms of primary or secondary market status. The major data on work experience came from self-reported labor market experience for the two years prior to the arrest that brought the individual into the sample. Many sample members (36.1 percent) held more than one job during that period, and many

*Gordon's scheme did require some adaptation for use in this analysis; the transformation is described in appendix B.

experienced varying non-working spells between jobs. Most (78.1 percent) did not have more than two jobs during the period. Nineteen percent of the sample reported working at the same job continuously for the two years preceding arrest.

I used an assignment criteria for workers which cumulated their time at work, and examined primary and secondary jobs as a percentage of time employed. If a worker spent 67 percent or more of his work time in primary jobs, he was classified as primary; if 67 percent or more of his working time was in secondary jobs, he was classified as secondary. Those who did not meet the 67 percent criterion were assigned according to the segment of their most recent job. Those who had not worked in the last two years were counted as secondary workers.

This assignment procedure produced the results shown in table 3.1. Eighty-one percent of the workers were assigned on the basis of the 67 percent criterion; only 5.1 percent were assigned on the basis of their most recent job. About 14 percent had not worked in the two years prior to arrest. (Some workers had to be excluded from the sample due to a lack of data on either industry or occupation, or time spent at a particular job. These missing value cases were about 10 percent of the sample. Comparison of mean values of other variables for the pre and post-excluded groups indicated no significant biases due to this exclusion.) The assignment technique led to a split of 37.6 percent primary workers, and 62.4 percent secondary.

TABLE 3.1
CLASSIFICATION OF SAMPLE MEMBERS BY LABOR MARKET SECTOR

Classification Criteria Based on 2 Years Prior to Arrest	Labor Market Sector	
	Primary	Secondary
67 percent or more of total work time in sector	.356	.454
Less than 67 percent of total work time either sector; most recent job in sector	.020	.031
No reported work in 2 years prior to arrest	--	.139
Total classified by sector	.376	.624

TABLE 3.2
MEAN VALUES OF SELECTED VARIABLES BY LABOR MARKET SECTOR
(Standard Deviations in Parentheses)

Variable*	Labor Market Sector		
	Primary	Secondary	Total Sample
Age in years	25.5 (8.56)	24.0 (8.81)	24.6 (8.75)
Black	.58 (.495)	.58 (.494)	.58 (.494)
Hispanic	.22 (.417)	.27 (.443)	.25 (.434)
Married at time of arrest	.20 (.397)	.14 (.350)	.16 (.369)
Highest school grade completed	10.9 (2.25)	10.2 (1.73)	10.5 (1.97)
Average hours per week worked during 2 years prior to arrest*	24.3 (20.6)	14.2 (18.6)	17.9 (19.9)
Average gross hourly earnings when working	4.52 (2.96)	2.81 (2.46)	3.45 (2.78)

*Includes out of work periods.

This may seem like a large number of primary workers in a sample arrested for predominantly street crimes. Yet it contains many more secondary workers than broader surveys of the work force. Gordon (1979, p. 37, table 1) has made a rough estimate that the economy as a whole contains about 63 percent primary jobs and 37 percent secondary ones.

This finding is roughly similar to other empirical schemes, even those based on earnings distributions. For instance, Andriasani (1973, p. 126, table C-8) computed a probability of first job being primary for young men, and found that 42 percent of whites and 23 percent of blacks had a primary first job, using a nationally representative sample of young men drawn in the mid-1960s. Freedman (1976, p. 35, table 1.7) divided her data into thirds based on the mean earnings of a segment in relation to the grand mean of earnings. For 1960 she found 36 percent of the civilian labor force in the bottom third of this distribution; for 1970 the bottom third contained 46 percent of the labor force.

B. Labor Market Segment and Characteristics of the Sample

The relative sizes of primary and secondary groups in the arrestees sample are not drastically out of line with findings based on broader samples. Additionally, factors often associated with primary and secondary jobs and the workers in those segments are found in the arrestees sample. As table 3.2 shows, average ages of primary and secondary workers are virtually the same; blacks are found

equally in both markets, with marginally more Hispanics in the secondary market. Primary workers are slightly more likely to be married at the time of arrest, and to have slightly more schooling.

The major differences between markets are average earnings and hours worked. Primary workers averaged \$4.52 per hour in gross earnings when working, while secondary workers averaged \$2.81 per hour. Primary workers averaged 24.3 hours per week of paid work over the two year period, while secondary workers averaged 14.2 hours.* Some of the low amounts of hours are due to the fact that this is a young sample, and many members are still in school. But the average age is roughly the same for both worker groups, indicating that there is not a differential age effect which accounts for lower secondary worker hours. Secondary workers work less, and earn less when they work.

This link between earnings, hours and primary jobs is also shown in table 3.3. A continuous variable measuring percent of work time in primary jobs, PCTPRIM, is shown with zero-order correlations on selected variables. PCTPRIM is significantly associated with age, although the effects are not very strong. It has about the same strength with marital status, being positively associated if the sample member was married at the time of the arrest. School grade is positive and significantly associated with PCTPRIM, as are wages and hours. Being black or Hispanic is not strongly

*This figure includes out of work periods.

TABLE 3.3

ZERO-ORDER CORRELATIONS BETWEEN SELECTED VARIABLES AND
PERCENT WORK TIME SPENT IN PRIMARY JOBS (PCTPRIM)

Variable	PCTPRIM
Age	.078 ^b (.030)
Black	-.013 (.715)
Hispanic	-.045 (.201)
Married at time of arrest	.073 ^b (.040)
Highest school grade completed	.174 ^d (.0001)
Total hours worked in 2 years preceeding arrest	.248 ^d (.0001)
Average gross hourly earn- ings when working	.315 ^d (.0001)
Prior arrests per months not incarcerated in preceeding 2 years	-.089 ^c (.013)
Months incarcerated in pre- ceeding 2 years	-.077 ^b (.032)

^aSignificant at .10 level

^bSignificant at .05 level

^cSignificant at .01 level

^dSignificant at .001 level

associated with PCTPRIM. PCTPRIM is negatively associated with arrests per month free and time incarcerated.

These associations are in turn illuminated by examining the zero-order correlations among the independent variables, shown in table 3.4. Correlations are reported separately for primary and secondary workers. Age and marital status are strongly correlated with hours and earnings variables for primary workers. For secondary workers, age is not related to average earnings, but is tied to hours. Marital status is tied to hours and earnings for secondary workers, although with less strength. (Marital status and age are themselves strongly correlated; examination of their net effects on labor market outcomes will be discussed below in the presentation of multivariate analyses.)

Highest grade completed is tied to age, hourly earnings, and hours worked for primary workers; for secondary workers, grade has some influence on earnings, although much less than for primary workers, and has no significant tie to hours worked.

There is also evidence of discrimination in both markets, but especially in the primary market. Being black or Hispanic has virtually identical negative effects on earnings in the primary market. These effects cannot be explained by reference to lower schooling for blacks, although Hispanics have significantly lower schooling in the primary market. Likewise, age as a human capital proxy is not significantly associated with being black or Hispanic.

TABLE 3.4
ZERO-ORDER CORRELATIONS AMONG SELECTED VARIABLES BY LABOR MARKET SECTORS
(Primary Workers Above Diagonal, Secondary Workers Below)

	Age	Black	Hispanic	Married at time of arrest	Highest school grade completed	Total hours worked	Average gross hourly earnings	Prior arrests	Incarcera- tion Time
Age	---	-.038 (.521)	-.037 (.536)	.457 ^d (.0001)	.166 ^c (.005)	.380 ^d (.0001)	.408 ^d (.0001)	-.089 (.135)	-.008 (.893)
Black	.009 (.845)	---	-.626 ^d (.0001)	-.070 (.225)	.005 (.935)	-.080 (.182)	-.123 ^b (.044)	.084 (.149)	.045 (.446)
Hispanic	.030 (.510)	-.711 ^d (.0001)	---	.039 (.537)	-.237 ^d (.0001)	-.070 (.242)	-.124 ^b (.043)	-.023 (.697)	.040 (.492)
Married at time of arrest	.360 ^d (.0001)	-.049 (.279)	.078 ^a (.084)	---	.095 ^a (.101)	.386 ^d (.0001)	.274 ^d (.0001)	-.133 ^b (.053)	-.081 (.168)
Highest school grade completed	-.151 ^d (.001)	.196 ^d (.0001)	-.249 ^d (.0001)	-.041 (.360)	---	.154 ^c (.010)	.362 ^d (.0001)	-.025 (.670)	.008 (.301)
Total hours worked	.268 ^d (.0001)	-.097 ^b (.035)	.032 (.492)	.273 ^d (.0001)	-.010 (.827)	---	.235 ^d (.0001)	-.188 ^c (.002)	-.063 (.892)
Average gross hourly earnings	.015 (.749)	-.059 (.216)	-.001 (.981)	.136 ^c (.004)	.091 ^b (.053)	.325 ^d (.0001)	---	-.031 (.618)	-.045 (.477)
Prior arrests	-.052 (.266)	.079 ^a (.085)	-.025 (.579)	-.068 (.137)	.035 (.442)	-.118 ^c (.012)	-.044 (.360)	---	.298 ^d (.0001)
Incarceration time	.007 (.878)	.102 ^b (.026)	-.062 (.176)	.016 (.733)	.033 (.469)	-.107 ^b (.022)	-.024 (.618)	.473 ^d (.0001)	---

^aSignificant at .10 level; ^bSignificant at .05 level; ^cSignificant at .01 level; ^dSignificant at .001 level.

In the secondary market, there are no significant correlations between being black or Hispanic and earnings, but blacks work significantly fewer hours. There are clear differences in schooling between blacks and Hispanics in the secondary market, which may account for lower working hours for blacks. Differences in highest school grade are not generally associated with hours worked in the secondary market. Grade differences are associated with higher earnings for secondary workers, although much less strongly than for primary workers. Hours worked and earnings are positively associated in both markets.

The crime variables show weak patterns of association in the two markets. For primary workers, marital status and hours worked are significantly associated with fewer prior arrests, while no factors are significantly associated with incarceration time for primary workers.* For secondary workers, hours worked are also significantly related to fewer prior arrests and less incarceration time; marital status is not related, but being black is. Blacks in the secondary market have a higher arrest rate and more imprisonment time, along with fewer hours worked. (Whether this is an independent effect of race, or an indirect effect from hours worked will be examined in chapter 4.)

*The strong association between arrests and incarceration time may be tied to computing arrests per month free. Months free is simply total months (24) minus any incarceration time, so incarceration time is implicitly in the denominator of the arrest variable.

A series of multivariate analyses was also conducted on PCTPRIM, using both ordinary least square and other types of maximum likelihood estimation. These analyses supported the patterns found in the simple correlations; highest school grade, hours, and earnings were all significantly and positively associated with PCTPRIM. Prior arrests are significantly and negatively associated with PCTPRIM. Age is marginally significant in accounting for PCTPRIM, while race and marital status did not appear to be significant.

Table 3.5 shows the difference in labor force indicators for the arrestees in primary and secondary markets, and compares them to all U.S. males. Labor force participation for primary workers is consistently close to the national cohort, surpassing it for the youngest and oldest group. For secondary workers, labor force participation is lower, except for teenagers. The employment to population ratio for the two groups shows differences between the two markets, but even sharper differences between the arrestees and national cohorts.

This suggests that immediate lack of employment may be associated with arrest, as attachment to the labor market via labor force participation does not seem excessively low, at least for primary workers. This lack of immediate employment is reflected in high unemployment rates for both groups of workers; teenagers in both markets suffer equally high rates of unemployment, although primary workers do better by comparison in the older groups. But the primary workers in the arrestees sample are much worse off in terms

TABLE 3.5

LABOR FORCE INDICATORS BY LABOR
MARKET SEGMENT AND AGE

Age	Employment to Population Ratio		Labor Force Participation Rate		Unemployment Rate				
	Primary	Secondary	All U.S. Males	Primary	Secondary	All U.S. Males			
16-19	.456	.438	.620	.825	.759	.714	.447	.423	.132
20-24	.603	.500	.841	.890	.851	.912	.322	.412	.078
25-34	.693	.584	.919	.853	.820	.957	.188	.288	.040
35+	.809	.536	.705	.857	.679	.724	.056	.211	.026
All ages	.599	.489	.737	.852	.781	.799	.297	.374	.047

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Source for national data: U.S. Department of Labor, Bureau of Labor Statistics, Employment and Earnings, 26:9 (September 1979), table A-3, p. 23.

of unemployment rates than the national cohort, again suggesting that even for primary workers, loss of employment is tied to arrest. Whether the type of arrest (e.g. income oriented or not) differs between labor market segment will be discussed in chapter 4 below.

Table 3.6 shows the age distribution of labor market segment by the three race/ethnicity categories. For teenagers, the striking finding is the similar nature of distribution across racial groups. For blacks and Hispanics, this distribution does not change markedly over time, although there is some improvement, principally in the 25-34 year old group. But for whites, there are sharp improvements in each age class, ending with a 2:1 ratio of primary to secondary workers in the oldest group. This indicates the confounding nature of race and labor market segment. Older white arrestees are much more likely to be primary workers than blacks or Hispanics. This finding of sharp improvements for whites with age resembles the findings of Osterman (1980) in his study of Boston youth. There may also be sharp differences in the sorts of crimes that the older white arrestees are accused of.

C. Empirical Analyses of Labor Market Experiences

This section presents the single equation analyses of determinants of earnings rates. Following that, single equation analyses of hours worked are presented. The chapter concludes with simultaneous and recursive analyses of earnings and hours.

TABLE 3.6
PERCENT OF SAMPLE IN PRIMARY SECTOR
BY RACE/ETHNICITY AND AGE

Age	Black	Hispanic	White and Other
16-19	.325	.303	.302
20-24	.387	.340	.483
25-34	.448	.405	.577
35+	.404	.304	.677
All ages	.375	.333	.441

Wages, Labor Market Segments and Crime

This section presents the results of a multivariate analysis of hourly earnings for the arrestee's sample. As discussed in chapter 2, neoclassical and labor market segmentation models differ on the predicted determinants of earnings, and on the expected relationships between crime and earnings.

Neoclassical theory predicts a positive and statistically significant relationship between measures of human capital, principally schooling and wages. These payoffs should not vary sharply by market segment, or by demographic factors unrelated to human capital stocks, such as race. If the labor market functions according to the neoclassical vision, then human capital factors ought to dominate the explanation of wage levels, reflecting nondiscriminating equilibrium labor market processes.

Labor market segmentation predicts, at its strongest, no significant relation between human capital measures and earnings for the secondary market. To the extent that these relations between human capital and earnings are found, they ought to appear in the primary market. Payoffs to ascriptive factors like race ought to distinguish the two markets. As already noted in table 3.6 above, blacks and Hispanics are more likely to be in the secondary market, especially at older ages. Whites are in the secondary market during their teenage years, but older white arrestees are

more often found in the primary sector in their early twenties. Some segmentation theorists seem to suggest that all labor is substitutable in the secondary market, which would imply little or no discrimination based on race or ethnic status in that market. Others argue that discrimination works both to concentrate minorities within the secondary market and to lower earnings for minorities in both markets, as neither market really fits the neoclassical vision of nondiscriminating employers seeking only to maximize their returns.

Crime, in both neoclassical and segmentation visions, is seen as traded off against higher returns for legal work. Thus both approaches would expect more criminal involvement by secondary workers. Insofar as crime represents lower legal opportunities that are in turn the result of lower levels of human capital, then neoclassical choice theory predicts a significant negative relationship between human capital and legal earnings on the one hand, and crime on the other. This negative relationship is also predicted on the simple opportunity cost notion that higher legal earnings would lead to less time spent in illegal activity, cet. par.

Segmentation theory, again, sees little or no payoff to human capital factors in the secondary market. Crime may be traded off against legal earnings in the primary market, but since earnings are unaffected by human capital stocks in the secondary market, crime may not be a tradeoff so much as a

supplement. If earnings are low, and cannot be increased in the secondary market, then there is little opportunity cost to criminal participation. Secondary employers are also thought to be relatively indifferent to a criminal record, so the negative credentials associated with crime are likely to have effects only in the primary market.

Testing the Determinants of Earning Rates

To test the determinants of earnings for the arrestees, I conducted several multivariate analyses of hourly earnings rates. The definitions of variables used in the analysis are listed here.

The dependent variable is LOGHRGW, the natural log of the average hourly gross earnings (XHRGW) received during work periods in the two years prior to the instant arrest.* The earnings rates for each job in the two year period are discounted by the amount of time at each job, where total work time equals 1.00. For example, where job A had a rate of \$3.00 per hour, and was 80 percent of working time, while job B had a rate of \$4.00 per hour, and was 20 percent of working time, LOGHRGW is the natural log of $(.80)(3.00) + (.20)(4.00)$, $\log(3.20)$, or 1.163. (Those who reported no paid work during the two year period were excluded from the analysis, as the log of zero is not defined.)

*Using the log of the earnings rate has become a norm in most labor economics research, often justified on the statistical properties of the residuals of log wage equations (e.g. Flanagan, 1978, p. 195).

The independent variables in the analysis are:

AGEYRS--the age in years at the time of arrest;

BLACK--a dummy variable equal to 1 if the sample member is black, and 0 otherwise;

HISPANIC--a dummy variable equal to 1 if the sample member was classified as of Hispanic origin by the police, and 0 otherwise;

MARRY--a dummy variable equal to 1 if the sample member was married at the time of arrest, and 0 otherwise;

GRADE--the highest grade in school completed by the sample member;

EXP--the duration in days of the longest continuous job held by the sample member;

TOTHRSWK--the total hours worked during the two years preceeding the arrest;

GOVTRANS--a dummy variable equal to 1 if government transfer payments were received by the sample member during nonworking periods, and 0 if otherwise (transfer payments are defined as access to welfare, unemployment compensation, or any other transfer, such as disability);

PERSSUP--a dummy variable equal to 1 if the sample member had access to personal monetary supports during nonworking periods, and 0 if otherwise (personal supports are defined as personal savings, or support from family or friends);

PRIARR--the number of prior arrests in the two year period, divided by the number of months not incarcerated, to control for time at risk;

GSCONV--the ratio of convictions to arrests during the two year period;

INCARTMN--the number of months incarcerated during the two year period.

The demographic variables, AGEYRS, BLACK, HISPANIC, and MARRY are self-explanatory. AGEYRS may also be construed as a human capital proxy, representing longer labor market

experience for older persons. However, the variables EXP and TOTHRSWK are intended to capture these effects, leaving AGEYRS aimed at capturing more pure age effects. EXP can be seen as representing job specific human capital and worker stability, while TOTHRSWK measures hours spent when working. GRADE is a schooling measure aimed at testing general human capital effects. GOVTRANS and PERSSUP are intended to test the effects of alternative income sources on earnings.

The three crime variables are designed to capture potentially different impacts of crime. PRIARR is a tentative, but best available measure, of participation in crime during the two year period. (The difficulties with using arrests for this measure were considered in chapter 1 above.) GSCONV attempts to look at separable effects of convictions against arrests; following other econometric work, this was to test the effects of possible deterrence impacts from convictions. However, indications of substantial collinearity with PRIARR led to excluding GSCONV from the analyses reported below. Finally, INCARTMN can be viewed as measuring the impacts of incarceration, either as a negative stigma, or as a loss of time and thus human capital from legal work. Bearing in mind the limits to determine predictions of effects discussed in chapter 2, the expectations for the effects of each variable on LOGHRGW are presented in table 3.7 for neoclassical and segmentation approaches.

TABLE 3.7

EXPECTED RELATIONS BETWEEN LOGHRGW
AND INDEPENDENT VARIABLES

Variable	Theoretical Framework	
	Neoclassical Choice Model	Labor Market Segmentation
AGEYRS	Positive	Positive for primary workers; not significant for secondary
BLACK HISPANIC	Not significant	Negative, especially for primary workers
MARRY	Indeterminate; positive if marriage indicates higher income needs, negative if it indicates the presence of other earners in the household.	
EXP	Positive	Positive for primary workers; not significant for secondary
TOTHRSWK	Negative	Negative for primary workers because of "backward bending" labor supply; also negative for secondary workers because of sporadic and low-paying character of secondary jobs
GOVTRANS, PERSSUP	Negative	Negative for primary workers, due to tie between work and wage rates--negative direction indicates out of work periods. Indeterminate for secondary workers, due to simultaneous or sequential mixture of income sources in the secondary market.
PRIARR, GSCONV, INCARTMN	Negative	Negative for primary, indeterminate for secondary

Table 3.8 presents the results of ordinary least squares (OLS) analyses with LOGHRGW as the dependent variable, for the entire sample, primary workers only, and secondary workers only.

The equations significantly explain some portion of the variation in earnings levels for all three groups--the pooled sample, primary workers only, and less so for secondary workers only. Although R^2 's below .50 might seem low, they are well within the norm for individual level data. (For instance, Mincer's classic work on human capital and earnings functions finds R^2 's ranging from .067 to .557 for a sample of white nonfarm men in 1959 with annual earnings as the dependent variable (1974, p. 92).)

The results do not present a clear victory for either neoclassical or labor market segmentation theories of earnings. Supporting human capital approaches are the positive effects of grade and continuous job experience, a finding that holds for the pooled sample and for both sectors independently.

The neoclassical choice model predicts a negative relationship between hours worked and earnings per hour, due to the backward bending labor supply hypothesis, and this negative relation is found in all three equations. However, it cannot be determined to what extent this is a function of types of employment represented in the sample. Some primary sample members have jobs in construction, a job which pays high hourly wages while having seasonal employment. There

TABLE 3.8
OLS HOURLY EARNINGS EQUATIONS - LOGHRGW DEPENDENT
(Standardized Beta Coefficients, absolute value of t-Statistic in Parentheses)

Data Set	R ²	F Statistic degrees of freedom	Independent Variables						
			Int.*	BLACK	HISPANIC	AGEYRS	MARRY	GRADE	EXP
Total Sample	.258	15.42 ^d (487)	d (5.22)	-.270 ^d (4.78)	-.146 ^c (2.54)	.108 ^b (2.02)	.076 ^a (1.67)	.223 ^d (5.42)	.290 ^d (5.55)
Primary workers only	.372	11.35 ^d (211)	d (3.36)	-.265 ^d (3.57)	-.132 ^a (1.73)	.208 ^c (2.67)	.049 (0.76)	.242 ^d (4.09)	.247 ^d (3.21)
Secondary workers only	.165	4.65 ^d (258)	d (4.31)	-.234 ^c (2.59)	-.147 ^a (1.65)	.017 (0.22)	.105 (1.51)	.136 ^b (2.20)	.293 ^d (3.83)

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Data Set	Independent Variables				
	TOTHRSWK	GOVTRANS	PERSSUP	PRIARR	INCARTMN
Total Sample	-.229 ^d (4.52)	-.038 (0.91)	-.159 ^d (3.44)	.013 (0.29)	-.010 (0.23)
Primary workers only	-.155 ^b (2.25)	-.042 (0.71)	-.156 ^b (2.31)	.034 (0.56)	-.037 (0.64)
Secondary workers only	-.257 ^d (3.60)	-.042 (0.67)	-.190 ^c (2.89)	.012 (0.18)	.014 (0.21)

*The Intercept does not have a standardized beta coefficient; only its level of statistical significance is indicated.

^aSignificant at .10 level; ^bSignificant at .05 level; ^cSignificant at .01 level; ^dSignificant at .001 level.

are also a number of self-employed workers in the sample, who report exceptionally long hours and low returns, often running their own small businesses such as neighborhood bodegas, auto repair shops, or selling merchandise on the street. Often these businesses are nothing more than marginal store fronts.

PERSSUP is also negative for all groups, showing a negative relationship between personal supports and earnings rates. As with many of the independent variables, the interpretation of PERSSUP is confounded by multicollinearity. PERSSUP is strongly and negatively correlated with AGEYRS, and many sample members are young and still living at home or at least in contact with their parents. A neoclassical interpretation would be that the opportunity cost of working is higher for the sample to the extent that they have access to personal supports during nonworking periods--thus their reservation wage is higher.

But the strong correlation of PERSSUP with AGEYRS is in a negative direction, so that older workers are less likely to score "yes" on PERSSUP. As many of the sample members are young and still dependent on their parents, this is not surprising. It does suggest that PERSSUP may be capturing some of the effects of age rather than a raised reservation wage. This interpretation is strengthened by a strong negative correlation between EXP and PERSSUP. Younger workers are less likely to have held a single job for an extensive length of time; the simple r between EXP and AGEYRS is .553.

The opposite directions of EXP and TOTHRSWK deserve some comment. EXP, a measure of longest single job duration, is closely and positively correlated with TOTHRSWK. Yet EXP has a positive relation to LOGHRGW, while TOTHRSWK's relation to LOGHRGW is negative.

As discussed above, the negative relation between earnings levels and hours worked might be taken as evidence of the hypothesized neoclassical backward bending labor supply curve. Alternatively, a segmentation approach might argue that earnings and hours are positively related--more stable work generally pays more.

EXP's positive relation to LOGHRGW can similarly be interpreted by both theories. If EXP captures job specific human capital, then its positive relation to LOGHRGW offers supports for neoclassical theories of earnings determination. Segmentation theory would again argue that stable jobs also pay better, as shown by the higher coefficient size and significance level of EXP versus GRADE for secondary workers. For primary workers, EXP and GRADE have similar positive impacts on LOGHRGW, with GRADE marginally more significant.

So GRADE and EXP both work in positive ways on LOGHRGW, and TOTWKHRS and PERSSUP are both negative for all three equations. The other variable which is significantly negative in all three cases is BLACK, indicating racial discrimination on earnings across both labor markets. There is

a marginally significant negative effect of being Hispanic in both markets.

TOTHRSWK is much more negative in the secondary than the primary market, lending strength to a segmentation interpretation of earnings and hours relationships. Earnings and hours are lower in the secondary market. (For primary workers, the mean values when working of gross hourly earnings and weekly hours are \$4.52 and 41.28 hours respectively; for secondary workers, they are \$2.81 and 30.73 hours.) Rather than seeing the negative coefficient on TOTWKHRS as an indicator of a backward bending individual supply curve of labor, the lower mean earnings in the secondary market suggest that lower earnings are associated with fewer hours as a structural characteristic of secondary jobs.

Several variables differ in their impacts for different markets. For instance, AGEYRS is positive for primary workers, but has no impact on earnings rates for secondary workers. This is in line with the segmentation prediction.

GOVTRANS and MARRY are insignificant in both markets, lending support to the interpretation of PERSSUP offered above. If the existence of personal income supports served to raise the reservation wage, as a neoclassical interpretation would argue, then the receipt of government transfer payments ought to have the same effect, and GOVTRANS and MARRY ought to have a significant negative effect on earnings. The lack of this effect from GOVTRANS and MARRY

suggests that PERSSUP is more associated with age and family structure of the sample members, younger ones having access to familial income supports.

In summary, a human capital interpretation of earnings determination is supported by the positive significant effects of GRADE and EXP for both groups of workers, although secondary workers receive a lower payoff to years of school. Blacks are paid less in both markets, suggesting discrimination in both; the effects are larger in the primary market. The segmentation approach to earnings determination is supported by the failure of age to be significant in the secondary market, and by the larger negative relation of TOTWKHRS to LOGHRGW in a market with significantly lower mean hourly earnings. Again, this latter finding suggests that lower hours in the secondary market are a structural characteristic coupled with lower earnings rates, not a reflection of individual labor supply preferences as in the neoclassical choice model.

When the impacts of crime variables on LOGHRGW are examined, the somewhat surprising finding is that none of the variables has any statistically significant impacts in any of the three equations. Although PRIARR, as reported above, was found to be significantly negative in association with percent time worked in primary jobs as a percentage of all working time, within the primary market it exercises no impact on LOGHRGW. The same is true for incarceration time (INCARTMN).

Due to the problematic nature of what the variables capture, this lack of significant impacts must be analyzed cautiously. PRIARR is a dubious measure for time spent in crime. The lack of impact from INCARTMN weakens a case for deterrent or negative credentialing from criminal justice involvement, as well as for arguing that time spent out of the labor market while incarcerated negatively affects human capital and thus wages. Neither of the two crime variables showed significant simple correlations with variables affecting earnings, such as GRADE or AGEYRS, not to mention the lack of simple correlation with XHRGW or LOGHRGW themselves.

This failure of crime variables to be significantly associated with hourly earnings weakens the case for assuming simple monetary tradeoffs or associations between legal and illegal activity. A similar weakening occurs when the determinants of hours worked are examined below.

A note on the robustness of the ordinary least squares findings on LOGHRGW is in order. Several authors have criticized segmentation research for use of "truncated" samples. When examining a low-wage population, the critics argued that ordinary least squares biases the results in favor of segmentation findings by implicitly limiting the variation found between earnings and schooling, both of which are artificially truncated in samples drawn from urban poverty populations. Later econometric work like Hausman and Wise (1977) or Heckman (1979), analyzing policy experiments aimed at altering the labor supply of low-wage workers,

reached the same conclusion on the potentially distorting effects of ordinary least squares, and recommended use of a variety of maximum likelihood approaches which can allow for truncation on dependent variables.

The most appropriate of these techniques for the analyses presented here is tobit analysis, first developed by Tobin (1958), generalized by Amemiya (1973), and increasingly used in econometric work in recent years. Tobit analysis allows for a variable which clusters at some limit value and then takes on a range of values away from the limit. In this regard, it is superior for some analyses to other limited dependent variable techniques, such as logit or probit, which can only assess binary dependent variables. To illustrate, probit and logit analyses might be appropriate in studying the decision to purchase or not to purchase some commodity like an automobile--a binary yes/no decision. Tobit analyses can be used to look at the yes/no decision (i.e. the cluster at the limit), and then to assess how much was spent on the purchase (i.e. the non-limit observations).

When LOGHRGW is the dependent variable, zero values of XHRGW (the underlying average hourly earnings when working) were excluded. This presented another reason for concern with truncation. In addition to potential constrictions on variation due to the sample issues discussed in chapter 2, the exclusion of those with no work in the two years prior to the arrest might further bias the ordinary least squares

analysis of LOGHRGW. Therefore, a tobit analysis was conducted on XHRGW, the raw values of gross hourly earnings for the entire sample, and including PCTPRIM (percent work time in primary jobs) as an independent variable. The procedure was adapted from Fair (1977) and programmed through the SAS MATRIX procedure as developed by Gust and Klemm (1980). By and large, the tobit findings on XHRGW resemble those discussed above for LOGHRGW, lending strength to the analysis of LOGHRGW.

Work Hours, Labor Market Segment and Crime

This section presents the results of analyzing the total hours worked in the two years prior to the arrest, examining the relationships between human capital factors, labor market segment and crime. As discussed in chapter 2, neoclassical models and labor market segmentation differ on the determinants of hours worked, and by extension on the relationship of criminal activity to legal work.

To recapitulate, neoclassical theory predicts a positive and statistically significant relationship between work hours and human capital factors. These relationships should not vary sharply by market segment, nor by demographic factors. If the neoclassical interpretation holds, then human capital factors ought to dominate the explanation of hours worked. These predictions are complicated by the hypothesized backward bending supply curve of labor, which predicts that the marginal effect of higher earnings is reduction of

hours worked. Thus earnings and hours worked are predicted to have a negative relationship. As hypothesized, negative relationship between other wealth and income factors and hours worked is advanced for the same reasons. Work being disutility, the existence of other income sources should reduce willingness to work, cet. par. (This finding has already been observed in the analysis of earnings levels presented above, but its interpretation is open to question.)

In segmentation theory, the determinants of hours ought to vary between segments. Unlike the neoclassical explanation of a negative relationship between hours worked and earnings, segmentation theory is more compatible with the institutionalist observation that higher paying jobs often have more consistent work hours. Rather than seeing a negative relationship between work hours and earnings as reflecting individual labor preferences, hours and wages are seen as conditions of jobs that are significantly determined outside of human capital and wealth endowment factors. Wealth tradeoffs are unlikely in the secondary market, where subsistence earnings and virtually zero wealth holdings are the norm. Human capital factors are seen by segmentation approaches as having their main impact in the primary market.

Crime is again seen by both schools as traded off against legal work. But since the segmentation theorists see no determinate impact from human capital factors in the

secondary market and mostly variations in hours in that market, there should be no consistent impact of crime on hours worked. As hours are seen as linked to inadequate earnings levels in the secondary market, crime again is hypothesized to be a supplement, not a substitute for legal work. Thus in segmentation theory, there is no determinate relationship between criminal activity and hours worked in the secondary market. To the extent that these relations are found, they are expected only for primary workers. Neoclassical choice theory, again, would expect a determinate negative relationship between legal hours worked and criminal activity, subject to the theoretical constraints specified in more complex versions of the labor supply model, discussed above.

The analysis of hours worked for the arrestees sample closely resembles the analysis of earnings rates. In the analysis that follows, the total number of hours worked in the two years preceeding the arrest (TOTHRSWK) is the dependent variable, and the variable definitions are the same as in the above analysis of LOGHRGW. Table 3.9 presents the predictions of the impacts of independent variables from the two labor market perspectives.

Table 3.10 shows the ordinary least squares (OLS) results of three regressions with TOTHRSWK dependent. The regressions were run for the pooled sample, and for primary and secondary worker groups separately. The OLS results are very similar in all three cases; all are highly significant,

TABLE 3.9
 EXPECTED RELATIONS BETWEEN TOTHSWK
 AND INDEPENDENT VARIABLES

Variable	Theoretical Framework	
	Neoclassical Choice Model	Labor Market Segmentation
AGEYRS	Positive	Positive for primary; no relation for secondary
BLACK, HISPANIC	Not significant	Negative, especially for primary workers
MARRY	Indeterminate; positive if marriage indi- cates higher income needs, negative if it indicates the presence of other income sources in the household.	
EXP	Positive	Positive for primary; not significant for secondary
LOGHRGW	Negative	Positive
GOVTRANS, PERSSUP	Negative	Negative for primary workers, due to the tie between work hours and benefits--these variables do not get a positive value if the person was employed the entire two years. Indeterminate for secondary, due to in- adequate income as a whole and institutional constraints on hours worked.
PRIARR, GSCONV, INCARTIN	Negative	Negative for primary, not significant for secondary

and all have high R^2 values. The independent variables that appear to be significant are the same for all three equations, and they are related in the same directions, with the exceptions of HISPANIC and AGEYRS for primary workers. HISPANIC is negatively associated and AGEYRS is positively associated with hours worked for secondary workers.

There are some variations in size and significance of coefficients between the two markets. BLACK is marginally significant for primary workers, having a much stronger negative impact in the secondary market. This suggests substantial discrimination on hours in the secondary market, especially with the negative effect of HISPANIC in that market. Whites work more hours than nonwhites in the secondary market. The other negatively associated variables are LOGHRGW, GOVTRANS and PERSSUP, all three of which have slightly higher negative associations in the secondary market.

Variables which are positively associated with TOTHRSWK in both markets are MARRY, GRADE, and EXP. MARRY and EXP have higher associations in the primary market, while GRADE is more significant in the secondary market.

At first glance, these results are fairly unfavorable to the segmentation perspective. The same variables are associated with variations in hours worked for both markets, in similar directions and strengths. Further, the directions of association are generally in line with the predictions of neoclassical choice theory. LOGHRGW and the two

TABLE 3.10
OLS TOTAL WORKING HOURS EQUATIONS - TOTHRWK DEPENDENT
(Standardized Beta Coefficients, absolute value of t-Statistic in Parentheses)

Data Set	R ²	F Statistic degrees of freedom	Independent Variables						
			Int.*	BLACK	HISPANIC	AGEYRS	MARRY	GRADE	EXP
Total Sample	.389	28.16 ^d (487)	d (3.26)	-.202 ^d (3.90)	-.125 ^b (2.38)	.145 ^c (3.02)	.150 ^d (3.67)	.120 ^c (3.17)	.250 ^d (5.24)
Primary workers only	.387	11.99 ^d (211)	d (3.24)	-.141 ^a (1.88)	-.097 (1.27)	.017 (0.21)	.208 ^d (3.34)	.102 ^a (1.69)	.325 ^d (4.34)
Secondary workers only	.398	15.36 ^d (258)	-- (1.09)	-.268 ^d (3.51)	-.165 ^b (2.18)	.225 ^d (3.52)	.130 ^b (2.21)	.139 ^c (2.66)	.212 ^d (3.23)

Independent Variables

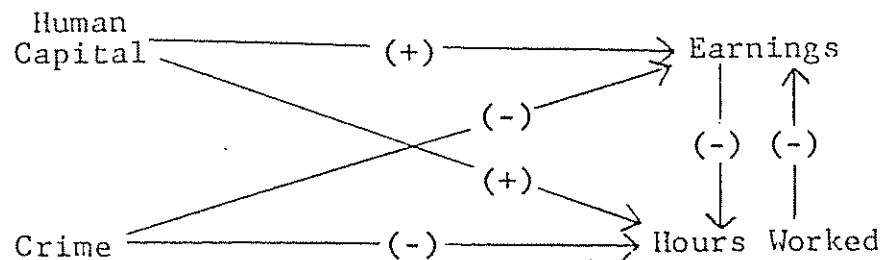
Data Set	LOGHRGW	GOVTRANS	PERSSUP	PRIARR	INCA RTMN
Total Sample	-.182d (4.52)	-.191d (5.13)	-.279d (6.87)	-.086b (2.11)	.041 (1.00)
Primary workers only	-.152b (2.25)	-.148c (2.54)	-.243d (3.70)	-.123b (2.09)	.002 (0.03)
Secondary workers only	-.186d (3.60)	-.211d (4.12)	-.290d (5.36)	-.042 (0.73)	-.074 (1.27)

*The Intercept does not have a standardized beta coefficient; only its level of statistical significance is indicated.
aSignificant at .10 level; bSignificant at .05 level; cSignificant at .01 level; dSignificant at .001 level.

supplementary income source variables GOVTRANS and PERSSUP are negatively associated with working hours, while GRADE and EXP, the human capital variables, are positively associated.

But PRIARR, the prior arrest variable, behaves according to the segmentation prediction, having a negative association with hours worked by primary workers, but no significant association with hours worked by secondary workers. Whether this is due to the operation of a tradeoff between crime and legal work in the primary market, or reflects the insensitivity of secondary employers to potentially negative credentials, it indicates that prior arrests have negative relationships with hours worked only in the primary market.

There remains a substantial interpretation problem with both the analyses of earnings levels and hours worked. In the neoclassical framework, hours and earnings are mutually and negatively interdependent, while for segmentation theory hours and earnings are features of jobs as much as reflections of worker preferences. Recall the schematic hypothesized neoclassical relationships for labor market variables outlined in chapter 2:



This model suggests a simultaneous relationship between earnings and hours, and implies that the single equation ordinary least squares models discussed above may be inadequate to capture the strengths and significance of the various relationships.

In a parallel fashion, the segmentation perspective suggests that earnings and work hours are heavily determined outside of worker characteristics. This is especially true for hours worked. In most jobs, whether primary or secondary, workers are not free to adjust their hours in a marginal fashion. Work hours are determined institutionally, with fixed work weeks offered by employers. Workers may have some relative freedom to accept varying wage levels. Further, it is less likely in an institutional or segmentation vision that employers offer varying combinations of hour and wage packages. Hours are usually fixed, and wages vary. Finally, primary jobs are more stable; more hours are a hypothesized characteristic of jobs. Thus from this point of view, the relationships between earnings and hours may be recursive for individuals; that is, hours are set by institutional factors, while individuals retain some autonomy in responding to wage offers.

If modelled incorrectly in single equation models, these types of simultaneous interaction could be lost or distorted. Accordingly, I carried out a series of simultaneous analyses of earnings and hours worked, both in a two stage least squares format as suggested by neoclassical choice

theory, and in a recursive model with hours determined prior to individual earnings, as suggested by institutional considerations. The results of these analyses are presented in the following section, and they shed some different light on the relationships between hours, earnings and crime for primary and secondary workers.

Simultaneous Analyses of Earnings and Hours

Two sets of equations were analyzed: a simultaneous system using two stage least squares (2SLS), and a recursive system that first estimated hours, and then used the hours estimation in the analysis of earnings. As discussed above, a neoclassical choice model is most compatible with the simultaneous approach, seeing earnings and hours as mutually determined in the context of an individual's equilibrium labor supply decision. Institutional and segmentation approaches are more compatible with the recursive approach, seeing hours and conditions of work as set largely through institutional means. Earnings also have a large institutional component, but can be affected by variations in individual characteristics, especially in the primary labor market.

Overall, the institutional segmentation approach is strongly supported by the analysis, while the simultaneous neoclassical approach is not. Table 3.11 summarizes the results. The 2SLS analysis of earnings and hours worked produced the strange result in primary and secondary markets

of no independent variables being significant on hours at a .10 level or better. For the pooled sample, only one variable, GOVTRANS, met the .10 criterion. In contrast, the recursive model found significant explanatory power for TOTHRSWK in all three runs. LOGHRGW is very similar between the two analyses, and both TOTHRSWK and LOGHRGW resemble the OLS run discussed previously, although with some significant differences. If one were to accept the 2SLS results, then hours worked would be virtually random. This finding goes against the theoretical and empirical grain of so much research in labor economics that it is hard to accept. Additionally, the F statistic for hours in the 2SLS analyses is highly significant. At the very least, these results do not strongly support the neoclassical model, as the 2SLS findings would imply that hours are random with respect to human capital, demographic factors, wealth and work history.

I have more confidence in the recursive model, both because of the robustness relative to the OLS results (and to the 2SLS results on LOGHRGW), and because of the support it provides generally for the theoretical predictions of the segmentation approach. The 2SLS results would have to be accepted with a lack of empirical robustness and a lack of conformity with theoretical predictions. Thus the remainder of this chapter will concentrate on the recursive results, and compare them to the OLS findings.

TABLE 3.11
SUMMARY OF 2SLS AND RECURSIVE ANALYSES OF EARNINGS
AND HOURS BY LABOR MARKET SECTOR

Dependent Variables and Data Set	2SLS			Recursive*			
	R ²	F statistic (degrees of freedom)	Significant independent variables**	R ²	F statistic (degrees of freedom)	Significant independent variables**	
Total Sample	(1) TOTHRSWK	356	24.4 ^d (486)	GOVTRANS	.363	27.8 ^d (488)	BLACK, HISPANIC, AGEYRS, GRADE, EXP, MARRY, GOVTRANS, PERSSUP, PRIARR
	(2) LOGHRGW	.233	13.4 ^d (486)	INTERCEPT, BLACK, HISPANIC, GRADE, EXP, PERSSUP	.277	14.3 ^d (488)	INTERCEPT, BLACK, HISPANIC, GRADE, EXP, PERSSUP
Primary only	(1) TOTHRSWK	.002	.05 (211)	None	.370	12.44 ^d (212)	INTERCEPT, MARRY EXP, GOVTRANS, PERSSUP, PRIARR
	(2) LOGHRGW	.344	10.05 ^d (211)	INTERCEPT, BLACK, AGEYRS, GRADE	.357	11.76 ^d (212)	INTERCEPT, BLACK AGEYRS, GRADE
Secondary only	(1) TOTHRSWK	.081	2.07 ^b (257)	None	.366	14.92 ^d (259)	BLACK, HISPANIC, AGEYRS, GRADE, EXP, MARRY, GOVTRANS, PERSSUP
	(2) LOGHRGW	.129	3.48 ^d (257)	INTERCEPT, BLACK, GRADE, EXP, PERSSUP	.123	3.65 ^d (259)	INTERCEPT, BLACK, EXP

*For recursive analyses, TOTHRSWK was analyzed first, and an estimated hours variable (ESTHRS) generated for the analysis of LOGHRGW.

**Significant at .10 level or better

bSignificant at .05 level; dSignificant at .001 level

Table 3.12 presents the results of three recursive analyses, which first analyze hours worked (TOTHRSWK) then compute hours based on that analysis (ESTHRS), and finally analyze LOGHRCW using ESTHRS rather than TOTHRSWK. (The mean values of TOTHRSWK and ESTHRS for the three analyses are shown in table 3.13.)

The results indicate different determinants of hours and earnings between the two labor markets. In the primary market, MARRY and EXP are positively and significantly associated with hours worked, while GOVTRANS, PERSSUP and PRIARR are negatively associated. (PERSSUP is marginally associated; its t-statistic has a significance level of .106.) In the secondary market, hours are positively influenced by AGEYRS, MARRY, GRADE and EXP--three human capital variables affecting hours worked rather than wages. Earnings in the primary market are positively influenced by AGEYRS and GRADE, and negatively by BLACK. Earnings for secondary workers are much more random (the explained variance of LOGHRCW is much lower for secondary workers than for primary), and are positively influenced only by EXP; like the primary market, being black has a negative effect on earnings levels for secondary workers.

These results correspond fairly closely to the labor market segmentation predictions about hours and earnings. Wealth holdings or outside sources of income, represented by GOVTRANS and PERSSUP, are negatively associated with hours worked. In the case of GOVTRANS, primary workers receive

TABLE 3.12
RECURSIVE ANALYSES OF HOURS AND EARNINGS BY LABOR MARKET SECTOR
(Standardized Beta Coefficients, absolute value of t-Statistic in Parentheses)

Data Set	R ²	F Statistic (degrees of freedom)	Independent Variables					
			Int.*	BLACK	HISPANIC	AGEYRS	MARRY	GRADE
(1) TOTHRSWK	.363	27.83 ^d	b	-.159 ^c	-.102 ^a	.131 ^c	.142 ^d	.083 ^b
Total Sample		(488)	(2.33)	(3.06)	(1.92)	(2.67)	(3.41)	(2.20)
(2) LOGHRGW	.227	14.34 ^d	d	-.238 ^d	-.126 ^b	.081	.047	.206 ^d
		(488)	(4.32)	(3.67)	(2.05)	(1.45)	(0.88)	(4.55)
(1) TOTHRSWK	.370	12.44 ^d	b	-.103	-.079	-.015	.205 ^d	-.145
Primary only		(212)	(2.82)	(1.39)	(1.03)	(0.19)	(3.26)	(1.14)
(2) LOGHRGW	.357	11.76 ^d	a	-.235 ^c	-.109	.212 ^c	-.011	.222 ^d
		(212)	(1.73)	(2.88)	(1.35)	(2.66)	(0.10)	(3.40)
(1) TOTHRSWK	.365	14.92 ^d	---	-.235 ^c	-.145 ^a	.233 ^d	.116 ^b	.119 ^c
Secondary only		(259)	(0.19)	(3.04)	(1.88)	(3.56)	(1.93)	(2.24)
(2) LOGHRGW	.123	3.65 ^d	d	-.188 ^a	-.119	-.029	.082	.133
		(259)	(4.18)	(1.67)	(1.21)	(0.30)	(1.14)	(1.58)

Data Set	Independent Variables					
	EXP	ESTHRS	GOVTRANS	PERSSUP	PRIARR	INCA RTMN
(1) TOTHRSWK	.205 ^d	**	-.191 ^d	-.261 ^d	-.092 ^b	-.041
Total Sample	(4.32)		(5.05)	(6.33)	(2.22)	(0.98)
(2) LOGHRGW	.249 ^d	-.012	***	-.107	.032	-.002
	(3.37)	(0.09)		(1.62)	(.531)	(0.04)
(1) TOTHRSWK	.294 ^d	**	-.145 ^c	-.255 ^d	-.131 ^b	.008
Primary only	(3.96)	(2.47)	(3.41)	(2.22)	(0.13)	
(2) LOGHRGW	.161	.084	***	-.091	.072	-.040
	(1.05)	(0.34)		(0.92)	(0.84)	(0.66)
(1) TOTHRSWK	.165 ^c	**	-.213 ^d	-.267 ^d	-.047	-.081
Secondary only	(2.51)		(4.08)	(4.87)	(0.79)	(1.35)
(2) LOGHRGW	.261 ^c	-.037	***	-.138	-.021	.030
	(2.68)	(0.21)		(1.47)	(0.30)	(0.41)

*The Intercept does not have a standardized beta coefficient; only its level of statistical significance is indicated.

**ESTHRS is estimated total hours worked based on the equation for TOTHRSWK.

***GOVTRANS is excluded for identification in all single equation analyses of LOGHRGW. GOVTRANS was insignificant statistically.

aSignificant at .10 level; bSignificant at .05 level; cSignificant at .01 level; dSignificant at .001 level.

TABLE 3.13

ACTUAL (TOTHRSWK*) AND ESTIMATED (ESTHRS) HOURS
WORKED, RECURSIVE ANALYSES

Variable and data set		Mean	S.D.	Std. Error	Variance
Pooled	TOTHRSWK	2199.69	2050.44	83.09	4204318.99
	ESTHRS	2220.11	1256.38	56.24	1578501.97
Primary	TOTHRSWK	2508.70	2073.33	126.65	4298703.24
	ESTHRS	2525.44	1255.70	84.09	1576772.56
Secondary	TOTHRSWK	1951.92	2006.10	109.60	4024440.15
	ESTHRS	1967.68	1256.51	76.47	1578820.38

*Mean values of TOTHRSWK are higher than those reported in table 3.2 above. The analysis here only include those who had a non-zero wage, e.g. those with some work history. The data in table 3.2 are for all sample members, including the roughly 14 percent who reported no work during the two years prior to the instant arrest.

more unemployment insurance when not working. There is a large representation of construction workers in the primary sample, which is a seasonal job with higher pay and provides eligibility for unemployment insurance. The negative values on GOVTRANS for primary workers may be indicating this correspondence.

Being married is positively associated with work hours for primary workers; men responsible for earning income for household support are more likely to be stable workers. EXP is also positively associated; to some extent this may be an artifact, as EXP is a subset of TOTHRSWK. But race and ethnic status, age, and school grade have no impact on hours worked for primary workers.

On the crime side, PRIARR is negatively associated with hours worked. Whether PRIARR represents a negative arrest credential or a proxy for time spent in crime, this finding corresponds to the economic choice model. PRIARR does not affect earnings levels, lessening the strength of the negative credential argument. And it affects neither hours nor earnings in the secondary market, supporting the segmentation prediction that primary workers may trade off crime and legal work, but secondary workers mix the two with no trade-off.

Earnings for primary workers follow rather closely with human capital predictions, except for the failure of EXP to be associated with LOGHRGW. AGEYRS and GRADE are positively and strongly associated with LOGHRGW. Being black is a

penalty in both the primary and secondary market, although in the primary market it affects only hours worked and not earnings. This indicates that blacks have a harder time gaining employment in either market, but if they can get into primary jobs, there may not be extensive earnings discrimination.*

Turning to the recursive analysis for secondary workers, a different pattern emerges. Hours worked for secondary workers are much more variable, and are affected by many more factors than hours for primary workers. Being black or Hispanic is a negative factor in the secondary market, somewhat undercutting the segmentation prediction of high substitutability in the secondary market. GOVTRANS and PERSSUP are also negative, as they are for primary workers. MARRY and EXP are positive, again in a similar way to the effects for the primary market, although both the coefficient sizes and significance levels are weaker for secondary workers.

The striking difference from primary workers involves the effects of AGEYRS and GRADE, indicating that these variables are associated with hours in the secondary market, and not with earnings, as in the primary. Segmentation theory has hypothesized that earnings levels are flat and nonvariable in the secondary market, while jobs are very unstable--

*Lazear (1979) has found that blacks may still suffer discrimination on the slope of earnings profiles relative to whites, even though entry-level wages have equalized somewhat due to affirmative action.

that is, fewer and more variable hours. This observation seems born out by these findings, although there do seem to be race, age, and credential impacts on hours worked in the secondary market.

Earnings for secondary workers also support the segmentation approach. Again, being black is a negative factor, although to a lesser extent than for primary workers. Being Hispanic is not associated significantly with earnings. In fact, the only other significant variable for LOGHRGW in the secondary market is EXP, which is not significant for primary workers. This indicates that stability of employment is associated with higher earnings for secondary workers. ESTHRS has no impact on earnings, so secondary workers, if they can make any earnings gains at all, seem able to make them by staying in one job. Human capital factors have no significant association with earnings for secondary workers, unless one were to interpret EXP as firm-specific human capital. However, this interpretation is undercut by EXP's lack of impact on earnings in the primary market. Crime variables, as mentioned above, have no significant associations with hours or earnings for secondary workers.

D. Summary

Overall, the analyses of the determinants of earnings and hours for the arrestees sample have produced results that tend to support segmentation analyses more than neo-classical choice models of labor market outcomes. Weaker

support has been found for the impacts of crime on labor market outcomes, although what impacts have been discovered are in line with segmentation predictions.

Earnings per hour have been found to be responsive to human capital variables more in the primary market than in the secondary, although longest job tenure has positive pay-offs in the secondary market. But since such tenure does not have significant effects in the primary market, at least in the recursive analyses, its interpretation as a human capital variable is weakened. The OLS finding did show a significant positive impact from EXP in both markets. Being black has a negative impact on earnings in both markets, indicating earnings discrimination across the two sectors. Hours worked is negatively associated with earnings in the OLS runs, but any significant association disappears when a system of equations is considered. Thus there is some support for neoclassical choice models of labor supply, but it is not very robust for this sample.

Hours worked in the two years prior to the instant arrest show a similar pattern. The OLS results would support a neoclassical model; hours are positively affected by human capital factors, and negatively by earnings levels and outside income sources. The recursive model again provides more support for a segmentation approach, with human capital factors affecting hours in the secondary market, but not earnings. Again, being black is a negative factor on hours

in all analyses, except in the recursive analysis for primary workers. The 2SLS simultaneous analysis undercuts the neoclassical choice model, as it finds no significant associations between any independent variables and hours worked, not even earnings. So the simultaneous determination of hours and earnings hypothesized by neoclassical models of labor supply is not well supported by my analyses. A stronger case seems to be made for an institutionalist/segmentation approach where hours are determined differently in the two sectors, and earnings are then responsive to human capital factors in the primary market, but not in the secondary. However, none of these findings are so clear as to be definitive.

On the relationship between criminal activity and labor market experience, the only impact found was a negative relationship between prior arrests and hours worked for primary workers. Incarceration time showed no significant negative impacts on earnings or hours worked for either labor market segment; the same was true for conviction to arrest ratios, before this variable was deleted due to its collinearity with arrests. This finding of significance for primary workers lends support to the segmentation approach to crime, but the relative weakness of the finding must be considered. Crime does not seem to be traded off by secondary workers, as it has no impacts on earnings or hours worked. The prior arrest variable may also indicate the sensitivity of primary employers to arrest records, insofar

as these are reported to employers. The next chapter turns to an analysis of criminal activity in more detail; as will be seen, there is not overwhelming evidence for simple economic tradeoffs in relation to crime from either a neo-classical choice or a labor market segmentation approach.

CHAPTER FOUR: CRIME AND ECONOMIC TRADEOFFS

This chapter analyzes the crime experiences of the arrestees' sample. Multivariate analyses are conducted on the determinants of the expected present value of the instant arrest, and supplementary analyses are performed on the determinants of prior arrests and incarceration time in the two years preceeding arrest. As in chapter 3, the effects of human capital, labor market segmentation and demographic variables on crime are examined, along with the impacts of earnings and hours worked.

Prior to the presentation of the multivariate analysis, arrest patterns are examined by labor market sector with attention to age and race differences. After that discussion, computation of expected net dollar returns for particular crimes is discussed. This computation is necessary to test the hypotheses laid out in chapter 2.

A. Income Oriented Crimes and Labor Market Segment

Involvement in income oriented crimes highlights some of the differences between primary and secondary workers, differences that are further clarified when the impacts of race/ethnicity and age in each labor market are examined. For instance, table 4.1 shows that secondary workers are

more likely to be arrested for income oriented charges, although the difference is not massive.

Table 4.2 shows the age distributions for those with income oriented arrests. There is a general fall in income oriented crimes with increasing age, with the exception of the 25-34 year old primary worker group. And in all but one age category, secondary workers are more likely than primary workers to have an income oriented arrest charge. Again, the anomaly appears with the 25-34 year old group, suggesting a need to pay attention to the 25-34 year old primary worker group with this relatively high and discontinuous level of income oriented arrests.

Race and ethnicity distributions, shown in table 4.3, suggest one possible solution to the patterns of table 4.2. For blacks and for whites, secondary workers have higher levels of income oriented arrests than primary workers, although blacks have higher levels of income charges in both sectors. But for Hispanic workers, this pattern does not hold; indeed, Hispanic primary workers have a slightly higher level of income oriented arrests than Hispanic secondary workers. This is most glaring with Hispanic primary workers, as the level of income oriented arrests for Hispanic secondary workers is not out of line with the levels for blacks and whites.

Recall that blacks in the sample had the highest levels of labor market distress as measured by employment indicators. If there is a relationship between labor market

TABLE 4.1

INCOME ORIENTED CHARGES BY LABOR MARKET SECTOR

Number of income oriented charges in the instant arrest	Labor Market Sector	
	Primary	Secondary
One or more	.578	.662
None	.422	.338

TABLE 4.2

SAMPLE MEMBERS WITH ONE OR MORE INCOME ORIENTED
CHARGES BY AGE AND LABOR MARKET SECTOR

Age	Labor Market Sector	
	Primary	Secondary
16-19	.670	.804
20-24	.563	.607
25-34	.622	.552
35+	.282	.377

TABLE 4.3

SAMPLE MEMBERS WITH ONE OR MORE INCOME ORIENTED ARREST
CHARGES BY RACE/ETHNICITY AND LABOR MARKET SECTOR

Race/Ethnicity	Labor Market Sector	
	Primary	Secondary
Black	.569	.697
Hispanic	.693	.623
White & other	.474	.587

distress and income oriented crimes, then the pattern shown in table 4.3 supports that relationship, again with the exception of the Hispanic primary workers.

Nevertheless, this one anomaly should not take attention away from the dominant pattern. Secondary workers are more often arrested for income oriented charges than primary workers, suggesting a relationship between labor market sector and income oriented crime. Younger workers are more often arrested for income oriented charges, but within age groups secondary workers are generally more often arrested for income oriented charges than primary workers of the same age.

These data on income oriented charges and labor market sector support a general relationship between secondary worker status and income oriented crime. But it could be that secondary worker status is a proxy for unemployment. The data could thus be seen as merely another expression of a simple relationship between income oriented crimes and unemployment.

Table 4.4 shows three labor force indicators by labor market segment and income oriented arrest charges. Primary workers, as expected, have higher employment to population ratios and labor force participation rates than secondary workers, and lower unemployment rates. And both primary and secondary workers' arrest types seem to be sensitive to labor market status.

TABLE 4.4

SELECTED LABOR FORCE INDICATORS, PRIMARY AND
SECONDARY WORKER STATUS, AND INCOME ORIENTED
ARREST CHARGES IN THE INSTANT ARREST

Labor Market Segment and Income Oriented Arrest Charges	Employment to Population Ratio	Labor Force Participation Rate	Unemployment Rate
Primary workers with 1 or more income oriented arrest charges	.554	.850	.348
Primary workers with no income oriented arrest charges	.661	.859	.231
Secondary workers with 1 or more income oriented arrest charges	.450	.785	.427
Secondary workers with no income oriented arrest charges	.570	.799	.284

Primary and secondary workers arrested for income oriented offenses have lower employment to population ratios than those in their sectors arrested for nonincome offenses. In the primary market, those arrested on income charges have an employment to population ratio of .554 compared to a ratio for primary nonincome arrestees of .661. Secondary workers arrested for income charges have an employment to population ratio of .450, while secondary nonincome arrestees have a higher ratio of .572. Labor force participation and unemployment rates show a similar pattern; within labor market sectors, those suffering are more likely to show labor market distress.

Yet table 4.4 also shows that labor market segment seems to exercise effects somewhat distinct from labor market status measured by the three conventional indicators. Labor force participation is higher for primary than for secondary workers, regardless of the type of arrest. But primary workers arrested for income oriented crimes have roughly the same employment to population ratio, .554, as secondary workers arrested for nonincome crimes, .572.

This effect of segments and unemployed status is best illustrated by the unemployment rate. The highest unemployment rate is found among secondary workers arrested for income oriented crimes, 42.7 percent. The next highest rate is among primary workers arrested for income oriented crimes, 34.8 percent. Then come secondary workers arrested

for nonincome crimes, with an unemployment rate of 28.4 percent, and finally primary workers arrested for nonincome crimes, with an unemployment rate of 23.1 percent.

As labor force participation rates are different by sectors, but not very different by arrest types, this pattern of unemployment rates calls attention to employment status at the time of arrest. Those not working are more likely to be arrested for income oriented charges, both within labor market sector and across sectors. But secondary workers have higher levels of labor market distress, especially in terms of employment, and this shows in their higher levels of unemployment relative to primary workers arrested on the same types of charges. Thus lack of work is tied to income oriented crime in both labor market sectors, but being in the secondary market seems to compound these difficulties and is tied to a greater incidence of income oriented crime.

Further clarification of the differences between labor market sectors regarding crime can be shown by examining selected variables in relation to labor market segment. Table 4.5 lists mean values and standard deviations for selected crime variables by labor market segment. The variables are:

YGAIN*--the expected gross dollar gain from the instant arrest;

YLOSS*--the expected foregone income from the likely punishment for the instant arrest on an income oriented charge;

ZLOSS*--the expected foregone income from the likely punishment for the instant arrest on a nonincome oriented charge;

PRIARR--the number of prior arrests per month free in the two year period prior to the instant arrest;

GSCONV--the ratio of convictions to arrests in the two year period prior to the instant arrest;

INCARTMN--months incarcerated in the two year period prior to the instant arrest.

Table 4.5 shows that secondary workers commit income crimes with a slightly higher expected return, and slightly lower expected losses. Expected losses from nonincome crimes are higher for primary workers than for secondary. (The very low expected loss amounts are due to the discounting of potential foregone labor income by the probabilities of an incarceration sentence. In some cases, these probabilities are virtually zero.) Secondary workers have slightly higher levels of prior arrest; the low levels of this variable are due to the fact that it is a per month average, and that many sample members have no prior adult arrests. (Juvenile records, because of legal protections, were not available.)

The last two crime variables deserve some attention. Secondary workers have a higher conviction-to-arrest ratio than primary workers, and have spent more time incarcerated

*Computation of the gain and loss variables is described later in this chapter.

TABLE 4.5

MEAN VALUES OF SELECTED CRIME VARIABLES
BY LABOR MARKET SEGMENT
(Standard Deviation in Parentheses)

Crime Variables	Total Sample	Primary Only	Secondary Only
YGAIN	\$207.91 (159.33)	\$189.95 (154.47)	\$219.52 (161.01)
YLOSS	\$10.66 (23.66)	\$15.26 (31.43)	\$7.61 (18.46)
ZLOSS	\$151.65 (644.01)	\$216.88 (712.76)	\$111.33 (600.37)
PRIARR	.049 (.095)	.038 (.082)	.058 (.105)
GSCONV	.529 (1.21)	.410 (.996)	.590 (1.22)
INCARTMN	.649 (2.39)	.403 (1.88)	.761 (2.47)

than primary workers. This suggests some differential treatment by the criminal justice system of secondary workers, although whether that treatment is due to employment status, age, race, or type of crime cannot be determined here. Future research using this data base could get the case outcomes for the instant arrest, and examine the various factors which enter into conviction and sentencing patterns.*

Table 4.6 examines the zero-order correlations between these crime measures and PCTPRIM, a variable which measures the amount of working time an individual spent in primary jobs. A similar pattern emerges, with all of the correlations being significant at the .10 level or higher. PCTPRIM is negatively associated with YGAIN, the expected take from an income producing crime. It cannot be determined here whether primary workers commit crimes with a lower dollar return, higher risks of punishment, or both. YLOSS and ZLOSS, the expected losses to income and nonincome crime respectively, are positively associated with PCTPRIM. As the loss is measured in potential foregone labor income, this is to be expected; primary workers work more consistently and for higher wages.

* For a review of recent literature on possible biases in criminal justice processing and some empirical evidence on those issues, see McDonald (1981).

TABLE 4.6

ZERO-ORDER CORRELATIONS OF SELECTED
CRIME VARIABLES WITH PCTPRIM
(prob $|R|$ under $H_0: \rho = 0$)

Crime Variables	Correlation with PCTPRIM
YGAIN	-.082 ^a (.072)
YLOSS	.152 ^c (.001)
ZLOSS	.085 ^a (.082)
PRIARR	-.089 ^c (.013)
GSCONV	-.066 ^a (.068)
INCARTMN	-.077 ^b (.032)

^aSignificant at .10 level

^bSignificant at .05 level

^cSignificant at .01 level

All three criminal justice variables are in significant negative association with PCTPRIM. Percent time in the primary market is associated with fewer arrests, lower conviction-to-arrest ratios, and lower amounts of prior incarceration.

Table 4.7 shows the zero-order correlations of the crime variables with different independent variables that were defined in chapter 3. The table shows these relations by labor market sector, and different correlational patterns emerge.

For primary workers, age is associated with some measures of crime, while for secondary workers it is not. Younger primary workers are arrested for lower payoff income crimes, and have lower expected losses, reflecting differential earnings by age in the primary market. Younger primary workers also have lower conviction-to-arrest ratios. For secondary workers, none of these variables are significantly correlated.

Being black or of Hispanic origin has no significant zero-order correlations with crime variables in the primary market. This remains true for Hispanics in the secondary market, but not for blacks. Blacks in the secondary market are arrested for lower paying crimes; they have higher levels of prior arrest and incarceration time. This last finding suggests differential sentencing or parole practices, as blacks do not have a significantly higher conviction-to-arrest ratio. The higher levels of arrest or

TABLE 4.7

ZERO-ORDER CORRELATIONS BETWEEN SELECTED VARIABLES
AND CRIME VARIABLES, PRIMARY AND SECONDARY WORKERS

(Probability that $|R|$ under $H_0: \rho = 0$ in Parenthesis)

Crime Variables	Primary Workers					
	YGAIN	YLOSS	ZLOSS	PRIARR	GSCONV	INCARTMN
BLACK	-.026 (.739)	-.114 (.166)	.002 (.981)	.084 (.149)	.056 (.342)	.045 (.446)
HISPANIC	.101 (.195)	.059 (.478)	-.057 (.466)	-.023 (.697)	.007 (.907)	.040 (.492)
AGEYRS	-.212 ^c (.007)	.184 ^b (.025)	.071 (.370)	-.089 (.134)	-.119 ^b (.045)	-.008 (.893)
MARRY	-.038 (.616)	.003 (.974)	.018 (.814)	-.113 ^b (.053)	-.143 ^c (.014)	-.081 (.168)
TOTHRSWK	-.148 ^a (.069)	.324 ^d (.0001)	.185 ^b (.018)	-.189 ^c (.002)	-.214 ^d (.0001)	-.063 (.301)
XHRGW	-.122 (1.45)	.194 ^b (.020)	.010 (.899)	-.031 (.618)	-.053 (.391)	-.044 (.477)
Crime Variables	Secondary Workers					
	YGAIN	YLOSS	ZLOSS	PRIARR	GSCONV	INCARTMN
BLACK	-.112 ^b (.049)	.004 (.950)	.095 (.128)	.079 ^a (.085)	.063 (.171)	.102 ^b (.023)
HISPANIC	.026 (.650)	-.028 (.635)	-.073 (.246)	-.025 (.579)	-.019 (.676)	-.062 (.176)
AGEYRS	.015 (.801)	.047 (.427)	.046 (.468)	-.052 (.266)	-.052 (.263)	.007 (.877)
MARRY	-.107 ^a (.058)	.113 ^b (.053)	.176 ^c (.005)	-.068 (.137)	-.047 (.301)	.016 (.733)
TOTHRSWK	.038 (.518)	.366 ^d (.0001)	.193 ^c (.002)	-.118 ^c (.011)	-.090 ^a (.055)	-.107 ^b (.022)
XHRGW	.026 (.662)	.269 ^d (.0001)	.168 ^c (.008)	-.044 (.360)	-.033 (.495)	-.024 (.618)

aSignificant at .10 level; bSignificant at .05 level;
cSignificant at .01 level; dSignificant at .001 level.

more serious nonincome arrests may account for the great amount of incarceration time, as blacks are being for income crimes of lower payoff.

Hours worked appears to be significant in many cases, but this conclusion should be tempered. Hours worked is a component of computing the expected loss variables, which measure the expected lost labor income discounted by the probability of an incarceration sentence. The same is true for the expected wage rate, which also enters into the expected loss computations.

But it should be noted that hours of work are negatively associated with prior arrests in both markets. They are also strongly associated with a lower conviction-to-arrest ratio for primary workers, and less strongly for secondary workers. Hours of work are slightly related to incarceration time for secondary workers; more incarceration time means less time outside in the labor market cet. par. But the failure of this variable to be significantly associated with incarceration time for primary workers also suggests a possible differential sentencing impact for secondary workers based on their lower hours of work.

Associations of the crime variables with the earnings variable are likely due to the use of the earnings variable in computing the expected losses. It should be noted, however, that primary workers do not show a significant association with wage levels and expected losses for nonincome crimes, while secondary workers show this relation. This

may be due to differences in the types of nonincome crimes between the two sectors, or it may indicate that the criminal justice probabilities dominate expected losses for primary workers, but not for secondary ones. This interpretation is supported by the relatively weaker impact of hours worked on nonincome losses for primary workers than for secondary workers.

Finally, marital status shows very different patterns of association between the two markets. It is related to lower levels of prior arrest and conviction for primary workers, while it is tied to the gain and loss variables for secondary workers. Marital status seems to have more association with prior criminal activities for primary workers and more association with the instant arrest for secondary workers.

These simple correlations suggest that crime has different patterns of association with other variables between primary and secondary markets. For a fuller exploration of these patterns, I used a variety of analyses. But before presenting those results, it is necessary to discuss a problem in examining economic models of crime--the computation of expected dollar gains and losses associated with criminal activity.

B. Estimating Monetary Gains and Losses from Crime

Analyzing criminal activity in terms of its expected value is called for by the economic choice model, although

assigning expected dollar amounts to crimes must consider the theoretical limitations noted in chapter 2. To the extent that crimes do not have strict dollar equivalents for returns and penalties, then the neoclassical economic choice model cannot make determinate predictions about relative impacts and directions of effect from changes in positive and negative returns.

However, assigning some dollar figure to crime is still critical for examining the neoclassical choice and labor market segmentation approaches to crime. This section describes the computation of expected present value in dollars for the most severe income oriented arrest charge of each individual in the sample.

Ehrlich noted that the differential returns to crime "cannot be estimated unless one is able to identify a control group representing potential offenders and study its alternative income prospects (1973, p. 538)." Ehrlich, like most economists who study crime, used macro level data, and he used an aggregate variable to represent positive returns to crime. He selected median family income by state, which he held to be a proxy for the median value of transferrable goods and assets. This value was seen as the primary determinant of payoffs to various crimes.

Attempts to analyze crime with individual level data have also suffered problems in this regard. For instance, Witte (1980), in her study of a group of prison releasees in

North Carolina, did not obtain estimates of the positive returns to crime, and was forced to conduct her analysis without them.

Heineke (1978) developed an elaborate utility maximization model to investigate substitution among crimes, and between crime and legal work. He estimated these phenomena cross-sectionally, developing estimates of both positive and negative incentives to crime. His estimates took a variety of factors into account, and represented the best attempt to develop gain and loss estimates. Unfortunately, the estimates still suffered from some incautious assumptions and from the inherent difficulties of using macro level data. My computations generally follow Heineke's with some important differences.

1. Positive Returns to Crime

For positive returns to robbery, burglary, and larceny, Heineke used data provided by the FBI Uniform Crime Reports. These data are gathered from local police agencies, describing the number of offenses that are reported to the police, and the estimated loss in dollar value for each offense. The data are aggregated over various geographic areas and a composite figure derived.

There are several difficulties with this approach. First, although dollar estimates are reported by local police, thieves obtain both money and goods, with a heavy preponderance of the latter. Such goods cannot be sold by

the thief for the estimated legal market replacement price, but must be heavily discounted when the thief sells directly to consumers or to a fence. Any estimate of dollar return to an offender ought to account for this discounting "fence factor." Second, the types of offenses included under the FBI definitions are quite broad, in scope and dollar value. It might be that the wide range of dollar returns to a particular activity (e.g. robbery, ranging from gas station holdups to bank robberies) are skewed by including extreme low or high points. Taking account of these issues, estimates are developed below for the positive returns for income-oriented crimes in Brooklyn during 1979.

Like Heineke, I have used police data to estimate positive returns. However, my sample members were arrested for a wide range of criminal activities, not only robbery, larceny, and burglary (although these charges accounted for 79 percent of all property charges in the sample). Further, although the FBI data are gathered by descriptive titles of crime ("robbery," etc.), my criminal charge data are categorized by New York penal law numbers, which do not always correspond to FBI definitions. Also, my data on negative sanctions for crimes are organized by penal law category. Thus I first had to assign FBI definitions to New York State penal law categories.

I then computed average dollar returns per individual offense. Data for Brooklyn were obtained for 1979 from the New York State Division of Criminal Justice Services

(1980). These data provide the numbers of offenses known to the police during 1979, and the estimated total dollar loss by type of crime. The data only provide estimated losses for three specific major property offenses--burglary, robbery, and larceny. The computations for these crimes are described first.

Several problems were encountered in computing the estimates. For example, a variety of events with very different returns come under the headings of "robbery," "burglary," "larceny," etc. Returns from robberies range from convenience store robberies (average loss, \$62.20) to bank robberies (average loss, \$3998.51). The range is less extreme for burglary and larceny. When rates are so dispersed, it is better to use median values.

Median values for robbery, burglary and larceny are shown in table 4.8. In most cases, the values are not very far from the means, suggesting that the presence of outliers does not significantly disturb the average returns, due principally to the relatively low frequencies of outlier crimes. In all three cases the median equals the mode.

Upward Biases in the Dollar Figures - the "Fence Factor"

The figures presented in table 4.8 are higher than most researchers report. Some of this discrepancy is due to the difference between the replacement costs of stolen goods and the actual amount received by the criminal. Most wealth

TABLE 4.8

MEDIAN AND MEAN VALUES AND RANGES OF DOLLAR LOSSES
FOR THREE MAJOR PROPERTY CRIMES, BROOKLYN, 1979

Type of Crime	Median Loss	Mean Loss	Range
Robbery*	\$376.65	\$431.55	\$62.20-\$860.96
Burglary	\$1194.27	\$1269.14	\$907.46-\$1386.86
Larceny	\$225.13	\$315.75	\$83.82-\$551.14

*Excludes bank robbery; this exclusion slightly lowers the mean value, but does not affect the median.

TABLE 4.9

DISCOUNTS ON STOLEN PROPERTY*

Item	List Retail Price	"Hot" Price	Discount Factor
Scrap metal, 100 lbs. of iron pipe	\$3.76	\$1.00	27%
Watch	\$400.00	\$100.00	25%
IBM Selectric Type-writer, new	\$1000.00	\$250.00	25%
Color console TV, new	\$750.00	\$200.00	27%
Heating oil pumped from abandoned building	.87 gal.	.50 gal.	57%
New ten speed bike	\$140.00	\$30.00	21%
Auto tires, slightly used	\$45.00	\$10.00	22%

*Sources include a New York Daily News 1981 series on fencing and examples from the field notes gathered by the Vera Institute's Employment and Crime Project.

stolen is real property; very little is liquid. Thus it is necessary to adjust the dollar figures by a discount factor to account for the mix of cash and real property and the fact that fences and consumers pay much less for "hot" goods than they do for list price items in stores.

New York State data show the mix of cash and real property stolen in Kings County in 1979 as 8 percent cash and 92 percent real property. Cash can be taken at its full value; real property must be adjusted by a "fencing factor."

There are no definitive analyses on discounting by fences or consumers of stolen goods. However, in reviewing specific accounts on the subject, surprisingly consistent numbers appear. Table 4.9 reports various estimates of this discount. These examples are only illustrative. Nonetheless, they suggest a fairly common discount for a wide variety of goods, especially given the different circumstances and types of commodities mentioned. West's informants told him that "thirty percent is the going rate--if you get less than that, you're a chump and being taken (1974, p. 92)." West also reported that when thieves act directly as small businessmen and sell to consumers, the range of discount runs from about 33-50 percent; when selling hot goods to legitimate stores and fences, the discount is about 20-33 percent; to pawnshops, about 10-15 percent.

I have chosen a 30 percent rate for adjusting the non-currency component of theft. This is slightly higher than most reported rates, but the total dollar figures involved

are low enough that a 5 percent variation in discounting makes little difference. Again, no further systematic adjustment would be possible without an extensive survey of stolen goods, list prices, and actual prices received by thieves. The 30 percent factor is also within the range reported from ethnographic research on drug users and their returns from crime.

The formula for computing the discounted median take per crime, T_d , is: $T_d = .08 T + (.92 T \times .30)$, where T = median take as presented in table 4.8, T_d represents the discounted median take, .08 is the average percentage of cash stolen, .92 is the average percentage of real property stolen, and .30 is the discount factor for real property. Adjusted median takes for robbery, burglary, and larceny are:

Robbery: $(.08)(\$376.65) + (.92)(\$376.65)(.30) = \$134.09$

Burglary: $(.08)(\$1269.14) + (.92)(\$1269.14)(.30) = \$451.81$

Larceny: $(.08)(\$225.13) + (.92)(\$225.13)(.30) = \$80.15$

These per event returns from single crimes may be biased upwards, due to the relatively high fence factor and possible overreporting of dollar losses by crime victims.

Estimating Returns to Other Income Producing Offenses

There are other income producing offenses that have no dollar information provided by the FBI reporting system. These include drug sales, gambling and book making, and

arson. The FBI system gives estimates for the number of offenses known to the police but provides no dollar estimates. In addition to the three major property offenses discussed above, I have examined the income producing crimes of arson, criminal impersonation, promoting prostitution (pimping), and drug and marijuana sale.

The largest dollar return for arson goes to landlords who practice insurance fraud. However, most arrests are of "torches," low level criminals who do the actual dirty work of setting fire to a building for a fixed fee or some percentage of the insurance money.

A recent series on arson in the New York Times (1980) quoted a "torch" as charging \$400 to burn down a building. (The Times series is instructive on the profits to be made from arson; landlords who hired a friend of this "torch" in one case took out \$500,000 in inflated insurance policies, and after the fire sought a settlement of \$130,000. They were offered \$72,000 by the insurance company, which they refused. In another case, an arsonist-landlord turned informer said that he paid between \$500 and \$600 for a torch job, and collected an insurance settlement of \$23,000.)

Drug sales present an additional difficulty. Most drug sales are carried out in a quasi-entrepreneurial fashion, with the seller buying or receiving a supply of drugs on credit. The seller is responsible for returning this amount, and anything above that amount is kept.

The best return estimates come from observations of drug dealers provided by Bruce Johnson (1981), coordinator of the New York State Department of Substance Abuse Services' ethnographic study of drug use and sales. Drug sellers have a highly variable market, with many transactions, but low dollar amounts per transaction. For instance, a very rough estimate of marijuana dealers' returns is \$5 per offense; these dealers often sell single joints for a dollar each. As of 1981, an ounce of marijuana was retailing in New York City for approximately \$50. In field notes of the Vera Institute's Employment and Crime Project, a street marijuana dealer told about turning "\$15 into \$55" in one day, giving a \$40 profit (1981).

Heroin sales on the street are also quite variable in return. Heroin sellers on the street are often users themselves, who sell enough to pay off their connection, and keep the remainder of the drug for their own use; these people are known as "jugglers." Transactions on the street involve variable numbers of bags; buyers set the sale by telling the seller the amount of cash they have available, and the seller then negotiates an amount. Very provisional estimates from Johnson's project indicated that a street dealer might buy a "bundle" (approximately 25 \$10 bags) with a theoretical value on the street of \$250, for between \$150-\$175. A total cash resale value of \$225 is more likely. Taking the most conservative estimates (lowest return and highest cost estimate), a "bundle" will produce

\$50 in profit. Interestingly, this provides a ratio of net gain (\$50) to cost (\$175) of .22, which is remarkably close to the ratios of property discount reported in the above section on estimating fenced property values.

In addition to arson and drug sales, I investigated returns to pimping and prostitution. Although none of our sample members is charged with prostitution, which is primarily a female offense in New York, some understanding of the prostitution market is needed to estimate the returns to promoting prostitution, or pimping, an almost exclusively male offense.

According to Carl Weisbrod (1981), the Director of the Mayor's Office of Special Enforcement in the Times Square area of New York City, an average street level trick costs about \$20. But prostitution, like drug dealing, is quasi-entrepreneurial. A daily take by a prostitute is called a "trap," and each woman is expected to produce a trap of \$250-\$300 per day. All of this is turned over to the pimp, who provides living expenses, and any legal costs which are incurred by arrests. He then keeps the remainder. (A trap does not have to be produced by tricking alone; the prostitute can rob the customer, or "john," and often does; the important thing for the prostitute is producing the expected trap by whatever means.)

Again, returns to a pimp are highly dependent on what he lays out for living expenses for his "stable" of women, and how large the stable is. Weisbrod estimated, very tentatively, that a stable runs between 1 and 3 women on

average; he had no way of estimating the amount that a pimp spends. Assuming a two woman stable, a pimp can average gross returns of \$500-\$600 per day. With a 25 percent net return, the pimp would clear between \$125-\$150 per day.

Final Dollar Estimates

The estimates of the returns per criminal even for income producing crimes are listed in table 4.10. These figures may still be biased for several reasons. For crimes reported to the police, especially burglary, the original dollar losses may be overstated. Higher income persons more frequently report crimes to the police, since they are more likely to take advantage of tax deductions for theft, and also are more likely to have insurance coverage for property loss. This also may lead them to overstate their losses for purposes of insurance fraud. Individuals sometimes report being encouraged in this overreporting by police, who tell crime victims that chances of recovering stolen property are virtually nil, and that inflating losses for insurance and tax purposes is the only way to get money back.

2. Estimating Losses from Punishment

The other problem in estimating net returns to crime is calculating the costs of criminal justice sanctions. The literature in economics is better developed here, primarily because economists have been more concerned in their empirical work with estimating deterrence effects. Aggregate

TABLE 4.10
ESTIMATES OF DOLLAR RETURNS PER CRIME

Crime	Dollar Return
Robbery	\$134.09
Arson	400.00
Grand Larceny	80.15
Burglary	451.81
Criminal Impersonation	n.a.
Gambling Records	n.a.
Promoting Prostitution	137.50
Non-marijuana drug sale	71.57
Marijuana sale	40.00
Petit Larceny/CPSP*	80.15

*Petit larceny in New York State involves amounts less than \$250, as well as other mitigating factors. As the estimated return for grand larceny is well below \$250, the figure for grand larceny is also used here.

studies, as well as micro-level data sets, usually take system probabilities of arrest, conviction, and sanction, and use these variables to explain variation in crime rates. This procedure seems more defensible with micro-level data sets, where it is reasonable to assume that the behavior of one individual criminal will not change the immediate parameters of the system.

However, most studies do not assign a dollar value to punishment. In part there are good theoretical reasons for this. As Block and Lind (1975) demonstrated, if it is assumed that there may be significant non-monetary costs to criminal activity itself, as well as punishment, then using only money losses misspecifies deterrent effects. But it is still necessary to try and estimate foregone earnings due to punishment, even while recognizing that these foregone earnings may not exhaust the effects of criminal justice sanctions.

Heineke (1978) attempted aggregate estimation of these foregone earnings based on estimates of overall unemployment rates and mean wage levels in various SMSA's. This is probably the best that can be done with aggregate data, but it still presents problems. The occupations that offenders are engaged in are fairly diverse, although mostly concentrated in secondary labor market jobs. Thus a selection of a relatively low wage rate would be more appropriate, as the variations between low wage occupations will be less extreme. On the other hand, hours vary significantly in the

secondary market, and computing labor incomes using an unemployment rate may overstate the foregone income from a prison sentence. As my data set has individual occupation and employment data, I computed the earnings and employment levels on an individual basis.

Criminal Justice Punishment Probabilities

The overall probability that an individual will be punished for a crime can be viewed as a multiplicative probability, that accounts for the probability of arrest, conviction, and sentence to prison. In New York State, there are two possible routes to incarceration. One runs through the criminal courts: an arrested case is disposed in criminal court, usually through a plea, and then sentenced. The other route runs through the superior courts (called the Supreme Court in New York), and passes through arrest, indictment by a grand jury, disposition, and sentencing. The criminal court is confined to misdemeanor or lesser dispositions, and can only sentence a convicted person to a local jail term of one year or less. Sentences to state prison (a year or more) are imposed in the Supreme Court. Thus the probabilities are mutually exclusive: if a local jail sentence is imposed, there is no state term, and vice versa.* This requires that two separate probabilities be computed for each criminal event.

*Theoretically, it is possible to be sentenced consecutively to state and local time, but this rarely occurs in practice.

Formally, the probabilities of a jail sentence from criminal court will be expressed as:

$$p(a_i) \times p(d_i) \times p(c_i) \times p(j_i) \text{ for crimes of type } i,$$

where:

$p(a)$ = the probability of arrest;

$p(d)$ = the probability of lower court disposition, given arrest;

$p(c)$ = the probability of conviction, given lower court disposition;

$p(j)$ = the probability of a jail sentence given conviction.

Similarly, the probability of a state prison term will be expressed as:

$$p(a_i) \times p(i_i) \times p(c_i) \times p(p_i) \text{ for crimes of type } i,$$

where:

$p(a)$ = the probability of arrest;

$p(i)$ = the probability of indictment given arrest;

$p(c)$ = the probability of conviction given indictment;

$p(p)$ = the probability of a prison sentence given conviction.

The variables are defined as:

$p(a_i)$ = the clearance rate for crime type i ; this rate expresses the ratio of arrest to crimes known to the police, or a/k ;

$p(d_i)$ = the percentage of arrests disposed in criminal court, d/a ;

$p(i_i)$ = the ratio of indictments to arrests, i/a ;

$p(c_i)$ = the ratio of convictions to dispositions (c/d) in criminal court, and the ratio of convictions to indictments (c/i) in Supreme Court;

$p(j), p(p)$ = the respective probabilities of a jail or or prison sentence (d/j in criminal court, and c/p in Supreme Court).

(It can be readily seen that each expression could be reduced for computational simplicity. The overall probability for a jail term from criminal court is:

$$\frac{a}{k} \times \frac{d}{a} \times \frac{c}{d} \times \frac{j}{c} \text{ which reduces to } \frac{j}{k};$$

For Supreme Court the probability is:

$$\frac{a}{k} \times \frac{i}{a} \times \frac{c}{i} \times \frac{p}{c}, \text{ which reduces to } \frac{p}{k}.)$$

Sources for each variable are described below.

Probability of Arrest, $p(a)$

This probability is drawn from data provided by the New York State Division of Criminal Justice Services (1980). It is the clearance ratio for various types of crimes or the ratio of solved cases to the number known to the police. In some cases, it is not possible to use the clearance ratio. For certain types of crimes, the level of complaints reported are only those that the police have generated by their own activity (e.g. drug cases and prostitution). In such cases, the clearance ratios are impossibly high, reflecting the fact that the denominator of the ratio is based only on police activity and not on independent complaints called in to the police. For instance, marijuana possession in Kings County, 1979, shows 375 total complaints, and 372 clearances. This would give a risk of arrest of .99 for

marijuana possession, when the actual risk is just the opposite--less than .001, according to all testimony that police and others give. In cases such as this, the probability of arrest will be expressed as .001.

Probability of a Local Jail Sentence, $p(j)$

There are no data which allow estimation by type of offense for the probabilities of disposition, conviction, and sentence to local jail in New York City (a term of one year or less). No record keeping agency in New York can estimate these by offense type. Given this problem, percentages will be used that express the average probability of the events leading to a local jail term; as discussed below, separate estimation of the probabilities by separate charge type is possible for the path leading to a state prison sentence.

The most reliable information on probabilities of a local jail term comes from a Vera Institute of Justice study (1981). By taking a sample of cases and tracking them through the system, Vera researchers were able to estimate flows of cases through the steps in processing. In a recent update of the study, these estimates were applied to data for the year 1977. Discussion with criminal justice personnel and examination of partial data indicated that the probabilities for 1977 are very similar to those for 1979. (Indeed, one of the major findings of the recent Vera update was that the overall probabilities had not changed materially between 1971 and 1977.)

The three average probabilities are: probability of disposition in the criminal court, given arrest (mostly cases that do not go to the grand jury for a possible felony indictment); the probability of conviction through plea or trial given disposition; and the probability of a jail sentence given conviction.

Probability of a State Prison Sentence, $p(p)$

Data for the estimates here are available by type of offense for most crimes. There are three separate stages: probability of indictment (non-indicted cases are disposed in the lower courts as discussed above); probability of conviction, by either plea or trial given indictment; and probability of sentence to state prison given conviction.

Data on the probability of indictment were provided by DCJS, the New York State Division of Criminal Justice Services (1980). The probabilities used here are those for Kings County, 1979. DCJS also provided conviction and sentence information by charge type.

A few cautionary notes are in order. New York State penal law, like that of most states, has two dimensions: type and severity. "Type" refers to the underlying type of offense (e.g. robbery, burglary, assault), while "severity" is a seven-letter classification that controls the allowable sentence for a crime of a given severity, an A felony being the highest and a B misdemeanor being the lowest. Thus a person convicted of a class C burglary is liable to the same

penalty as a person convicted of any other class C (e.g. drugs or robbery). It would be desirable to control the data estimates here for severity as well as type, as many cases are reduced in severity from initial arrest charge to disposition through various bargaining and institutional pressures, but this cannot be done.

So the estimates presented here will be for separate types of offenses only, with no control for charge severity or reductions. This should not substantially affect the results. Sentences to prison are the main variable affected by charge severity, and the data that will be used present median sentences by type of offense. The use of median sentences is the best proxy for the ideal, but unobtainable, separate estimates by charge type and severity. To get such disposition information, a researcher would have to obtain a sample of cases and track them through the system from arrest to release from prison, an Augean task.

A second caution involves use of averages for certain offenses. Although DCJS gives data by charge type, it does not do so for all offenses. Some crimes are so little represented in the Supreme Court (e.g. promoting prostitution, criminal possession of stolen property) that no separate data are available. In these cases, I used the average probabilities of indictment, conviction, and sentence to state prison for all crimes in Kings County, 1979. (This procedure should not affect the computation of monetary estimates of crime gains and losses in any significant

way, as the final result is a cumulative probability from arrest to sentence. These cases where average probabilities are used are very few in number. For indictments, the average rate will be used in 1.8 percent of the total cases in Kings County, 1979; for convictions, 1.3 percent of the cases, and for sentences to state prison, 0.6 percent.)

Estimating Dollar Losses Associated With the Sanction Variables

After the probabilities of punishment were estimated, I then estimated the dollar loss associated with different punishments. This involved estimating the expected time that would be served in jail or prison for specific offense types, and computing the opportunity cost of foregone labor income associated with that sanction. This was done on an individual level basis by examining the labor market portion of the data file. I estimated the average amount of employment in the two year period prior to the arrest, to get approximate expected level of employment for an individual. This was multiplied by the wage rate received for that employment, weighted by the percent time spent at each job. Multiplied by the expected length of the prison sentence for the type of crime, and discounted by a present value formula, this computation gives the best possible present value estimation of the expected monetary loss associated with a particular crime type.

Formally, the expected dollar loss will be computed as the probability of punishment times:

$[E(e) \times E(r)] \times E(s_i) \times D$, where:

$E(e)$ = expected working time in one year;

$E(r)$ = expected wage rate;

$E(s)$ = expected length of incarceration time for crime type i ;

D = present value discount formula.

The variables are defined as:

$E(e)$ = percent weeks actually worked in the two years preceeding arrest;

$E(r)$ = the gross earnings rate received for work in the preceeding two year period, weighted by the amount of time worked at each job if more than one job;

$E(s_i)$ = the median time served in local jail or state prison for crime type i ;

$E(D)$ = discount rate; finance company unsecured loan rates as published in the Federal Reserve Bulletin.

The first two data elements are generated from within the sample, based on the individual's own experience. This computation illustrates, once again, the advantage of individual-level data. In contrast, Heineke, in his aggregate cross-sectional work, multiplied mean legal earnings for an SMSA by the unemployment rate for those of high school age or older. But many of those arrested for street crimes are in secondary labor market jobs, where the earnings are below average; additionally, their employment is more unstable, something not captured in a single yearly estimate of unemployment. On wage levels and hours worked, Heineke's method would likely overstate legal earnings available to street criminals, and thus overstate the dollar losses associated with incarceration.

Time served data for state prison sentences are from DOCS, the New York State Department of Correctional Services (n.d.). State prisoners have a maximum term that represents the total time owed to the state. Prisoners also are eligible for conditional release (CR), which allows release after two-thirds of the maximum has been served, barring loss of CR time for disciplinary infractions. Many prisoners are not released on their first parole date, but hardly any do the maximum behind bars: For these reasons, the median time before CR release will be used to represent the expected time served on a state prison sentence. This is available for most crime types; for those where it is not available, the median CR time for unspecified felonies will be used.

No parallel data are available for New York City sentences. Local sentences served previously by sample members were computed to serve as possible proxies; these were potentially unreliable, due to a small N. I used the alternative of 8 months as a proxy for all local jail sentences, 8 months being two-thirds of the maximum possible local time, 1 year.

The choice of a discount rate variable follows Heineke's (1978, p. 181) plausible suggestion that the "social and financial backgrounds of property crime offenders" would make finance company rates the most applicable.

Final computation of the expected dollar losses was done by multiplying the probability of punishment in lower

and superior courts times the expected lost labor income from an average sentence in each of those courts. As cases are disposed in one of the two courts, the probabilities add to unity, so the two resulting dollar figures are added together to produce an estimate of expected dollar loss for a particular crime, given an individual's prior labor market experience.

C. Multivariate Analysis of Expected Net Returns from Crime

This section presents the results of ordinary least squares regressions on the estimates of monetary gains and losses--the net benefits--from the instant arrest. Analyses were carried out for all types of crimes, and for income and nonincome arrests separately. The analysis was then done for each net benefits measure for the two different labor market sectors.

The expected net benefits to a particular crime were computed as described in the preceeding section. This led to three dependent variables:

YNETBEN--the expected net benefits, in dollars, of an income oriented crime (this equals YGAIN-YLOSS);

ZNETBEN--the expected net benefits, in dollars, of a nonincome crime (it is assumed that there is no dollar equivalent for gains from nonincome crimes, so ZNETBEN is only the loss variable, ZLOSS);

XNETBEN--the expected net benefit for all sample members (when both an income and a nonincome charge of equal weight were included in the arrest, XNETBEN is the higher in dollar terms of YNETBEN and ZNETBEN).

Thus YNETBEN examines only those persons arrested on an income oriented charge, ZNETBEN represents those without an income oriented charge, and XNETBEN pools income charges only, nonincome charges only, and those with both types of arrest charges.

The independent variables used are the same ones used in chapter 3. Hourly earnings levels are included in the set of independent variables, but XHRGW is used here rather than its log, LOGHRGW, which was the dependent variable in the analysis in chapter 3. The use of XHRGW allows inclusion of those cases without any reported work in the two years prior to arrest, where XHRGW is, of course, zero. As the log of zero is not defined, LOGHRGW would exclude these cases.

Recapitulation of the hypotheses regarding net payoff to crime is needed here. Recall that the neoclassical choice model holds legal opportunities to be determined by levels of human capital, and that legal opportunities so determined are then traded off against criminal activity. Thus the higher the levels of human capital and labor market success, the higher a particular crime ought to be in net payoff to compensate for the greater foregone legal opportunity and the higher risk of loss.

Segmentation theory, while also positing a tradeoff between economic opportunity and crime, claims that success in the secondary market is limited and bears no significant relation to human capital variables. If such tradeoffs

exist as envisioned by the neoclassical choice model, they should be mostly found in the primary market.

Table 4.11 shows the results for analysis of XNETBEN for the pooled sample, for primary workers only, and for secondary workers only. As can be readily seen, the amount of variance explained is not very high, although it is significant in all three equations. Most of the independent variables did not reach a minimum confidence interval of .10, based on a one-tailed t test. There are virtually no comparable studies in the literature, so it is hard to assess the magnitude of explained variance.

The pooled sample run on XNETBEN shows two variables with significant impact: MARRY, a dummy variable indicating marital status at the time of the arrest, and EXP, the duration of the longest job. But the analyses of the two separate labor market sectors indicate that these effects are actually distinct for the two sectors, and are only found in the pooled sample due to the mixing of primary and secondary workers.

EXP, the duration of the longest held jobs, is the only independent variable with a significant impact on XNETBEN for primary workers. The longer a job for primary workers, the lower the expected net benefit. This may be due to EXP's ties to total hours worked; total hours worked is part of the computation of expected losses, which enters directly into the computation of all of the net benefits variables. However, this interpretation is less plausible when it is

TABLE 4.11

OLS NET BENEFITS--XNETBEN DEPENDENT
(Standardized Beta Coefficients , absolute value of t-Statistics in parenthesis)

Data Set	R ²	F Statistic (degrees of freedom)	Independent Variables					
			Int.*	BLACK	HISPANIC	AGEYRS	MARRY	GRADE
Total Sample	.063	3.19 ^d (518)	-- (1.31)	-.070 (1.14)	.010 (0.16)	-.005 (0.10)	-.119 ^c (2.51)	-.029 (0.64)
Primary workers only	.085	1.74 ^a (206)	-- (0.13)	-.052 (0.56)	.031 (0.32)	.007 (0.07)	-.042 (0.55)	.005 (0.06)
Secondary workers only	.087	2.61 ^c (300)	^a (1.84)	-.053 (0.63)	.016 (0.19)	-.050 (0.69)	-.206 ^d (3.28)	-.068 (1.16)

Data Set	Independent Variables						
	EXP	XHRGW	GOVTRANS	PERSSUP	PRIARR	INCA	TMN
Total Sample	-.102 ^a (1.84)	-.049 (1.03)	.056 (1.20)	.029 (0.61)	.056 (1.17)	.020 (0.42)	
Primary workers only	-.281 ^c (3.04)	.071 (0.86)	.029 (0.37)	-.022 (0.28)	.049 (0.68)	.011 (0.15)	
Secondary workers only	.044 (0.63)	-.114 ^b (1.95)	.070 (1.14)	.070 (1.15)	.047 (0.73)	.027 (0.42)	

*The intercept does not have a standardized beta coefficient; only its level of statistical significance is indicated.
^aSignificant at .10 level; ^bSignificant at .05 level; ^cSignificant at .01 level;
^dSignificant at .001 level.

noted that EXP has no significant impact for secondary workers in the analysis of XNETBEN.

Results presented below on the analysis of YNETBEN also make it more likely that the impact of EXP on XNETBEN is not merely an artifact of XNETBEN's computation. Jobs of shorter duration seem to be traded off against dollar benefits from crime, suggesting that for primary workers, job stability diminishes the net payoff of the crime, at least in dollar terms. Rather than a simple opportunity cost tradeoff, which would suggest higher values of XNETBEN in association with EXP, it may be that primary workers are more risk sensitive, and less willing to risk a good stable job for a serious crime. EXP may also in part reflect an individual preference for stable work, and thus may be a proxy for risk averse behavior. Of course, stability of work can exercise nonmonetary impacts, especially in the primary sector where there is more variation on job stability.

Another variable which can be interpreted along stability lines, although more social than job stability, is MARRY. And for secondary workers, MARRY has a significant negative association with XNETBEN, indicating that being married is associated with less lucrative crimes. Considering labor market variables and XNETBEN, EXP is not significantly associated with XNETBEN for secondary workers, but XHRGW, the average gross hourly wage, is. Like EXP for primary workers, XHRGW has a negative association, again

confounding a simple opportunity cost tradeoff model. In that view, higher wages should be traded off against higher crime returns, to compensate for the foregone legal activity. But like EXP for primary workers, the negative direction of XHRGW suggests that secondary workers are less likely to commit higher paying crimes if they receive a higher wage.

One problem with the results in table 4.11 is the pooling of income oriented and nonincome oriented crimes. If economic tradeoffs as hypothesized by the neoclassical framework are operating, then they would most likely be found with income oriented crimes. Table 4.12 presents the results of analyzing YNETBEN, the expected net benefits from income oriented crimes. Again, the analyses are carried out for the pooled sample, and for the two labor market sectors independently.

At first glance, there is no change from the analysis of XNETBEN, at least if only the pooled data are considered. MARRY and EXP are negatively associated with YNETBEN, just as they were with XNETBEN, and no other independent variables meet the minimum confidence interval. (The equation itself is not significant, unlike the pooled equation for XNETBEN.)

But examination of the two labor market sectors again shows differences. For primary workers, EXP is not significant. Instead, GOVTRANS, a variable indicating receipt of government transfer payments when not working, and PRIARR,

TABLE 4.12

OLS NET BENEFITS--YNETBEN DEPENDENT
(Standardized Beta Coefficients , absolute value of t-Statistics in parenthesis)

Data Set	R ²	F Statistic (degrees of freedom)	Independent Variables					
			Int.*	BLACK	HISPANIC	AGEYRS	MARRY	GRADE
Total Sample	.047	1.47 (325)	d (4.08)	-.103 (1.20)	-.034 (0.39)	.039 (0.54)	-.103 ^a (1.73)	-.074 (1.24)
Primary workers only	.132	1.58 (114)	b (2.44)	.013 (0.10)	.140 (1.08)	-.072 (0.61)	.015 (0.16)	-.018 (0.19)
Secondary workers only	.096	1.91 ^b (199)	c (2.79)	-.218 ^a (1.89)	-.200 ^a (1.67)	.146 (1.57)	-.194 ^c (2.53)	-.086 (1.14)

Data Set	Independent Variables					
	EXP	XHRGW	GOVTRANS	PERSSUP	PRIARR	INCA RTMN
Total Sample	-.154 ^b (2.29)	-.021 (0.35)	.007 (0.11)	-.042 (0.67)	.068 (1.07)	-.068 (1.05)
Primary workers only	-.147 (1.32)	-.065 (0.61)	-.222 ^b (2.29)	-.074 (0.70)	.183 ^a (1.83)	-.143 (1.45)
Secondary workers only	-.196 ^b (2.30)	.128 ^a (1.75)	.127 ^a (1.64)	-.032 (0.42)	.012 (0.15)	-.043 (0.52)

*The intercept does not have a standardized beta coefficient; only its level of statistical significant is indicated.

aSignificant at .10 level; bSignificant at .05 level; cSignificant at .01 level; dSignificant at .001 level.

prior arrests in the two years preceeding arrest, turn up as significantly associated with YNETBEN. GOVTRANS is negatively associated with YNETBEN, again confounding a simple opportunity cost framework. If GOVTRANS represents another form of noncriminal income, then a simple tradeoff model would anticipate a positive association between GOVTRANS and YNETBEN. Again, the negative associaton of GOVTRANS suggests that receipt of government transfer payments has a dampening effect on crime payoff measured in net dollar take.

PRIARR has a weak but significant positive associaton with YNETBEN, indicating that for primary workers, prior arrests are associated with higher returns on the current arrest. This fits better with a simple neoclassical choice model. Whether PRIARR indicates higher "criminal human capital" leading to higher return crimes, or a higher potential risk of apprehension and punishment, leading to a higher opportunity cost for crime, the positive association is the only indicator of a simple tradeoff model for primary workers.

For secondary workers, the picture on YNETBEN is very distinctive from all of the other analyses, suggesting that involvement in income oriented crimes for secondary workers is relatively unique. Both BLACK and HISPANIC are negatively associated with YNETBEN; blacks and Hispanics are arrested for income crimes with lower net returns than whites. This could be evidence of discrimination by the

police if all race and ethnic groups commit the same types of income offenses.

Analyses of income oriented crime type indicates that this is not the case, however. Blacks in the sample are almost two-and-a-half times as likely to be arrested for robbery as Hispanics or whites, and robberies are treated more severely in the penal law and in the sentencing practices of courts. Conversely, whites and Hispanics are about twice as likely to be arrested for burglary as blacks. All three groups have roughly equal probabilities of arrest for grand larceny. The place where Hispanics are overrepresented is low payoff crimes like petty larceny, which take up a much higher proportion of their arrests than whites and a slightly higher proportion than blacks.

So blacks generally are arrested for lower payoff crimes than whites and Hispanics, especially robbery. Robbery is a relatively risky activity in terms of punishment, exacerbating the low net returns. For Hispanics, the low payoff crime is petty larceny, which while not as severely punished as robbery also has low positive returns. Thus the negative associations for blacks and Hispanics on YNETBEN in the secondary market may be due to different types of crimes they engage in, all of which have low net returns.

On labor market variables, secondary workers have negative associations between YNETBEN and EXP. Again, this negative direction indicates that a simple opportunity cost tradeoff is not occurring. More hours worked are associated

with lower payoff crimes, once again suggesting a sensitivity to risk of loss and an impact of stability from longer hours. MARRY is also negative and significant for secondary workers, with its possible interpretation of greater social stability.

One puzzle involves the relationship between YNETBEN and XHRGW, the average gross hourly wage. For the XNETBEN run, this relationship was negative and significant. For the property crimes, however, it is positive and marginally significant. It may be that income oriented criminals fit the rational choice model better than the pooled sample, as the positive relationship between YNETBEN and XHRGW fits a simple opportunity cost tradeoff model. However, the volatility of the sign change should limit confidence in easy interpretations of this variable, especially since its relation with nonincome crimes is insignificant.

Finally, GOVTRANS is positively associated with YNETBEN for secondary workers. This is the other variable which fits a conventional tradeoff model; its direction is doubly confusing when compared to GOVTRANS for primary workers, where the association was negative. One source of this difference is the type of transfer payments received in each sector. Primary workers are more likely to have unemployment insurance when not working, while the major source of transfer income for secondary workers is welfare, often received through the woman in a household, usually a common-law wife or mother. So unlike the dampening effect that

GOVTRANS has on severity for primary workers, it is tied to higher payoffs of income crimes for secondary workers. As receipt of welfare usually indicates more economic distress in a household, and often the lack of employment for males in the household, then the positive association between GOVTRANS and levels of income crime for secondary workers is less surprising. It is often not the male's income so much as that of the woman in the household. It is less likely to be accessible to the men, especially younger males who are receiving support in female-headed households.

The analyses of YNETBEN thus indicates different relationships for primary and secondary workers. There is more evidence for simple opportunity cost tradeoffs between levels of income oriented crime and other factors in the secondary market. For primary workers, there is some evidence that the associations run in the opposite direction. Factors such as higher wages, more stable work, unemployment benefits and marital stability reduce the net payoffs of crime. Being married and having stable work also seem to have this effect for secondary workers.

Finally, nonincome crimes were examined separately for each labor market, to see if they could be explained along the lines posited by either economic approach. In general, little explanatory power was found in analyzing ZNETBEN, the expected loss from an incarceration sentence for a nonincome crime.

Table 4.13 shows the results for the three data sets. Secondary workers were the only group where the overall analysis reached even a marginal level of significance. There, once again, MARRY has a relatively strong negative impact, indicating that married secondary workers are arrested for less risky nonincome crimes. EXP is significant for primary workers, again suggesting that the stability of a primary job may be an important factor in lessening the losses associated with nonincome crime.

D. Arrest History and Labor Market Experience

This section presents analysis of prior criminal history in relation to demographic factors and labor market experiences. The time examined is the two years prior to the arrest that brought the individual into the sample. Only those who have complete adult criminal histories for the period are included in the analysis; that is, only those who were 18 years of age or older at the time they entered the sample.

Use and Interpretation of Tobit Analysis

The dependent variable is PRIARR, the number of arrests adjusted for time at risk. The numerator of PRIARR is the total number of arrests in the two year period, and the denominator is 24-INCARTMN, where INCARTMN is the number of months incarcerated in the two years. The distribution of PRIARR ranges from 0 to 1, with a mean of .06 for the entire sample.

TABLE 4.13

OLS NET BENEFITS--ZNETBEN DEPENDENT
(Standardized Beta Coefficients , absolute value of t-Statistics in parenthesis)

Data Set	R ²	F Statistic (degrees of freedom)	Independent Variables					
			Int.*	BLACK	HISPANIC	AGEYRS	MARRY	GRADE
Total Sample	.044	1.29 (307)	-- (0.65)	-.101 (1.32)	-.022 (0.02)	.085 (1.13)	-.123 ^b (1.93)	-.003 (0.06)
Primary workers only	.078	0.98 (128)	-- (0.58)	-.073 (0.63)	-.019 (0.16)	.086 (0.69)	-.020 (0.20)	-.013 (0.13)
Secondary workers only	.099	1.67 ^a (167)	-- (0.01)	-.095 (0.91)	.031 (0.30)	.083 (0.84)	-.025 ^c (2.96)	-.038 (0.47)

Data Set	Independent Variables					
	EXP	XHRGW	GOVTRANS	PERSSUP	PRIARR	INCA RTMN
Total Sample	-.090 (1.24)	-.026 (0.41)	.042 (0.69)	.050 (0.76)	.034 (0.56)	.029 (0.47)
Primary workers only	-.299 ^c (2.50)	.115 (1.09)	.082 (0.90)	-.042 (0.39)	.047 (0.50)	.035 (0.40)
Secondary workers only	.094 (1.01)	-.120 (1.53)	.007 (0.09)	.121 (1.43)	.028 (0.33)	.029 (0.34)

*The intercept does not have a standardized beta coefficient; only its level of statistical significance is indicated.
^aSignificant at .10 level; ^bSignificant at .05 level; ^cSignificant at .01 level; ^dSignificant at .001 level.

Many individuals in the sample had no prior arrests; thus PRIARR for them is 0. No one was incarcerated for the entire 24 months, so there is no problem of division by 0 in the creation of PRIARR (INCARTMN ranges from 0 to 21.5). The distribution of PRIARR, ranging from a cluster at 0 upward to a maximum of 1, makes use of ordinary least squares (OLS) analyses problematic. The problem is analogous to the problem of sample truncation discussed above in chapter 3.

OLS analysis on a variable like PRIARR would estimate a regression line only for those observations above the limit; in this case, where PRIARR was greater than 0. OLS regression assumes a normal and homoschedastic distribution of the error term, and by extension of the dependent variable. The bounded nature of a variable like PRIARR violates the assumption of constant variance on the error term, and OLS analysis would produce a relatively poor fit in the region of the limit. Tobin (1958) showed that OLS in this type of case would tend to underestimate parameters for low values of the independent variables, and overestimate parameters for high values.

To correct for this problem, Tobin developed a procedure which has become known as "tobit" analysis, the name stemming from analogies with probit and logit procedures, maximum likelihood approaches to analyzing dichotomous dependent variables. The tobit procedure partitions the data into the limit and nonlimit observations, forms a log-likelihood function for the entire sample, and then maximizes

this function with respect to possible values of coefficients for the independent variables. I have used a version of tobit analysis which follows a procedure developed by Fair (1977), and was originally programmed for SAS by Gust and Klemm (1980). (I have found it necessary to make some minor alterations in the program provided by Gust and Klemm.)

Parameter estimates in tobit analysis are not standardized, and cannot be interpreted in the same way as standardized beta coefficients. Direction of effect is more critical. Similarly, the "t-ratios" to test the significance of individual coefficients are not true t-ratios, but represent the ratio of a coefficient to its standard error, where the standard error is derived from the diagonal elements of the negative inverse of the second derivatives' matrix. Amemiya (1973) has shown that these ratios are normally distributed regarding the null hypothesis that the coefficient is zero, and that the distribution of the variables is approximately normal for large samples.

Finally, tobit analysis has no real analog to the R^2 of OLS, but does provide a test of overall equation significance analogous to the F statistic for OLS. This is the -2 log likelihood test, also called the likelihood ratio test. The procedure computes the value of the maximized likelihood function including all independent variables, and then computes this value for a model with only the constant term as an independent variable. The logs of these two functions

are then computed, and the difference between the log of the value for the constant only and the log of the whole model is multiplied by -2. This -2 log likelihood statistic is distributed approximately chi-square, where the degrees of freedom for the chi-square is the number of independent variables without the constant.

Empirical Analysis of Prior Arrests

Table 4.14 presents the results of a tobit analysis on PRIARR for the entire sample and for each labor market segment. Recall that the simple neoclassical choice model predicts a negative relationship between work time and time spent at crime, but suggests a positive relationship between legal earnings rates and payoffs from criminal activity based on an opportunity cost framework. The labor market segmentation approach suggests that if such relationships are found, they will only be found in the primary market.

Table 4.14 presents the tobit results on PRIARR for the total sample and for each group of workers identified by labor market segment. For the total sample, the overall equation is highly significant, and three variables are significantly associated with prior arrests per month free: BLACK, AGEYRS and TOTHRSWK. The latter two are negatively associated, with TOTHRSWK having the strongest association. (Remember that the parameter estimates in table 4.14 cannot be read like standardized beta coefficients; it is the ratio of the estimates to their standard errors that indicates the strength of the relationship.)

TABLE 4.14

TOBIT ANALYSES OF PRIARR BY LABOR MARKET SEGMENT
(Unstandardized Parameter Estimates, absolute value of "t-Ratios" in Parenthesis)

Data Set	Likelihood Ratio Test	Independent Variables					
		Constant	BLACK	HISPANIC	AGEYRS	MARRY	GRADE
Total Sample	590.35 ^d	.173 ^b (1.97)	.054 ^a (1.66)	-.001 (0.14)	-.004 ^b (2.13)	-.016 (0.49)	-.005 (0.90)
Primary workers only	480.36 ^d	.248 ^a (1.89)	-.004 (0.09)	-.063 (1.09)	-.003 (1.17)	-.022 (0.42)	-.009 (1.08)
Secondary workers only	182.64 ^d	.119 (0.96)	.107 ^b (2.41)	.055 (1.11)	-.004 ^a (1.70)	-.019 (0.47)	-.004 (0.45)

Data Set	Independent Variables				
	EXP	TOTHRSWK	XHRGW	GOVTRANS	PERSSUP
Total Sample	-.00002 (1.29)	-.000003 ^d (3.76)	-.002 (0.51)	-.014 (0.49)	-.005 (0.22)
Primary workers only	-.00002 (0.94)	-.000003 ^c (2.77)	.0001 (0.02)	.021 (0.46)	-.023 (0.59)
Secondary workers only	-.00001 (0.86)	-.000002 ^b (2.14)	-.005 (0.90)	-.037 (0.98)	.012 (0.41)

^aSignificant at .10 level; ^bSignificant at .05 level; ^cSignificant at .01 level; ^dSignificant at .001 level.

For primary workers, the negative association with TOTHRSWK remains, but the other significant relationships disappear. The effects of AGEYRS and BLACK reappear for secondary workers, and TOTHRSWK retains its significance for secondary workers. Thus total working hours is significantly and negatively associated with prior arrests for both groups of workers. Being black and older is tied to more arrests in the secondary market only; age and race do not have a tie to prior arrests for primary workers.

These separate effects of race, age and work time indicate that there may be distinctive age and race effects regarding crime in the secondary market. Blacks work less, as do younger males; this indicates collinearity between TOTHRSWK, age and race. Thus so the three coefficients could be seen as expressing a simple relationship between less work and more arrests, dispersed somewhat through race and age effects. But primary workers show no significant impacts from age and race on PRIARR, while retaining the impact of TOTHRSWK. (Indeed, the variable BLACK is almost perfectly independent from PRIARR in the primary market--its "t-ratio" confidence interval is .928.)

There is thus evidence in both markets of a time allocation effect--more working hours are tied to fewer arrests. Neoclassical hypothesized opportunity cost tradeoffs between earning levels and prior arrests are not found; the estimates for XHRGW, the average hourly gross wage when working, are very weak, and the sign fluctuates between

markets. Older black secondary workers have more arrests than other secondary workers, cet. par.

These results are somewhat parallel to the analysis of the instant arrest presented in the preceeding section. There, little evidence was found for short-term opportunity cost tradeoffs (except among workers arrested on income producing crimes), and some evidence was found for negative relationships between work stability and expected net benefits from crime. Taken with the tobit results for PRIARR, this suggests important crime dampening effects on severity and frequency from stable work.*

More information can be gleaned from the tobit analyses through a result presented by McDonald and Moffitt (1980). They showed that tobit effects could be decomposed into the probability of being above the limit on the dependent variable and change in the value of the nonlimit observations on the dependent variable. In the case of PRIARR, this can be interpreted as seeing how impacts on arrests would be spread between being arrested at all and marginal changes in arrest frequency.

Table 4.15 presents the results of decomposing the PRIARR analyses according to the procedures outlined by

*As the variable measuring work stability in the net benefit analysis was EXP, the duration of the single longest job, these comparisons cannot be made more directly. TOTHRSWK could not be used in the analysis of the expected net benefits, as it entered directly into their calculation.

TABLE 4.15
 FRACTION OF SAMPLES ABOVE LIMIT FOR PRIARR, AND
 DECOMPOSITON OF TOTAL TOBIT EFFECTS

Data Set, PRIARR Dependent	Fraction of Sample Above Limit	Fraction of Mean Total Response Due to Response Above Limit
Total Sample	.440	.325
Primary Only	.367	.288
Secondary Only	.500	.368

McDonald and Moffitt. It shows the percentage of each sample above the limit (e.g. where PRIARR is greater than zero), and the fraction of the mean total response due to these nonlimit observations. Thus 44 percent of the total sample had one or more prior arrests in the two years preceeding the instant arrest. Thirty-two and a half percent of the total changes in PRIARR resulting from a change in the independent variables would derive from marginal changes in numbers of arrests, and 67.5 percent of the change would come from changes in the probability of being arrested at all. ($100 - 32.5 = 67.5$.) More of the changes for the pooled sample are tied to the probability of having any arrests at all rather than marginal increases in arrest rates.

This remains true for the two labor market groups, although less so for the primary workers. There, approximately 37 percent of the sample had some arrests, and about 71 percent of changes in arrest rates for that group would stem from initial adult arrests. For secondary workers, only about 63 percent of changes in the arrest rate would come from new arrests, and about 37 percent would be due to marginal increases in arrests for those with prior arrests. Put more intuitively, it means that reductions in hours worked for primary and secondary workers would have its largest impact on first arrests. Reductions in hours worked, rising age, and being black would also have large

effects on marginal increases in arrest for those with previous records, about 37 percent of the total change. Secondary workers are more likely to have adult arrest records, and are more likely to manifest continued increases in arrests than primary workers, due to changes in the independent variables.

E. Summary

Relatively weak explanatory power was found for either economic model of crime, as indicated by the low levels of variance explained on expected net benefits. The best results were obtained for income oriented crimes. But even there, the results did not generally accord with standard neoclassical predictions.

For primary and secondary workers, the length of a job is often associated with lowered crime payoffs. Like the impact of transfer payments on income crimes for primary workers, this goes against the grain of a simple opportunity cost model, and suggests that factors stemming from stable jobs and income may be important in lessening the severity of crime for this group.

For secondary workers, household stability as represented by marital status is a key factor associated with lower levels of crime payoff. For income crimes, race and ethnicity matter for secondary workers, along with job stability. Earnings levels and transfer payments have a positive association with crime severity, unlike their

impacts for primary workers. This is likely due to the different types of transfer payments in the two markets--unemployment insurance in the primary, and welfare in the secondary. Again, this finding suggests that institutional arrangements and household composition issues matter for dampening crime. Transfer payments, like many other factors, cannot be simply reduced to their net dollar impact on behavior.

When the tobit analyses of prior arrests are considered, they strengthen the idea of distinctive crime relationships for the two labor markets. Primary workers are less likely to have prior arrests than secondary workers, and the only significant impact on those arrests is a negative relationship with time worked. This relationship to time worked also holds for secondary workers, although age and race also have significant associations with prior records for secondary workers.

Time on jobs matters both in dampening the net payoffs of instant arrests and the frequency of prior arrests. However, it is not clear that this is due to a simple time allocation effect of work. Marital status, transfer payments, and job stability (EXP) as differentiated from total work hours (TOTHRSWK) also have significant relationships to crime, relationships that probably cannot be reduced to a simple economic tradeoff model. It may also be that total hours worked matters as much in terms of overall social stability as it does in terms of individual cost-benefit

calculations. These issues cannot be resolved from a purely quantitative analysis; the complexity of the relationships outlined here suggests further exploration of this multidimensionality of crime and work. Labor market sector and hours of work matter, and the above analysis shows this; it does not, however, tell exactly why and how these factors matter.

CHAPTER FIVE: CRIME, THE LABOR MARKET AND ECONOMIC RESEARCH

This study has found relatively weak support for simple tradeoffs between crime and legal work, tradeoffs which are envisioned in both neoclassical and labor market segmentation views of crime. Of more interest, however, are the complex interactions between crime and labor market position. After summarizing the results of the empirical work, I consider deficiencies in the current research program on crime, suggest issues for incorporation in future research, and outline the policy implications of economic research on crime.

A. Summary of Empirical Results

Crime's Relationship to Legal Work

Chapter Three presented analysis of the earnings and work time of the arrestees. The explanatory power of neoclassical labor economic theory and labor market segmentation approaches were contrasted there, and the impacts of crime on labor market outcomes were explored.

The empirical work lends stronger, although qualified, support to the segmentation view of labor market dynamics. Earnings were found more responsive to human capital variables in the primary labor market. The length of the

longest held job was significantly associated with earnings in the secondary market, which could support a human capital view that sees job specific training as an important contributor to higher earnings. But as schooling was not significantly associated with earnings for primary workers, the human capital approach would have to clarify why particular forms of human capital would only get significant returns in particular markets. (This summary is based on the recursive analyses of earnings and hours. For the reasons stated in chapter 3, I think that these results are the most reliable empirically.)

Aside from human capital impacts, there is evidence that blacks receive lower earnings in both labor markets. This somewhat weakens the segmentation view, which at its simplest sees labor as perfectly substitutable in the secondary market. However, it lends no comfort to a neoclassical vision of labor market dynamics, where discrimination ought to be eliminated or attenuated by the operation of competitive market forces. Work hours do not have a significant impact on earnings in the recursive analyses, although they do in the single equation analysis of earnings. Again relying more on the recursive results, this finding lends little support to the neoclassical model of labor-leisure trade-offs.

The recursive findings suggest that hours of work are determined somewhat differently than earnings. Some factors affect both earnings and hours in the pooled sample analysis, but distinctions appear when each labor market segment

is considered separately. Hours in both markets have negative associations with government transfers and personal income supports. Consistent positive impacts on hours stem from being married and having longer job tenure.

The determinants of earnings also vary between markets. Being black, as mentioned, has a negative impact on earnings in both markets. There is no other consistent effect on earnings for both primary and secondary workers. Primary workers have positive payoffs to age and years in school, while secondary workers have positive payoffs only to job tenure.

The influence of crime on earnings or hours is very limited in these analyses. Only one statistically significant impact was found: primary workers show a negative association between arrest rates and hours worked, a finding that would conform to a simple time allocation model. But no such relationship was found between hours and arrest rates for secondary workers. And incarceration time had no significant relationship with hours or earnings in either market, somewhat undercutting the time allocation view.

Thus for this sample of arrestees, hours worked seem determined in a different manner than earnings. Those with more work report less reliance on government transfers and personal supports in both markets. Marital status has a positive association with work hours in both markets. Job tenure has a positive association with hours, although this is somewhat of an artifact; a worker with a single long job

is more likely to have higher overall working hours. Hourly earnings are lower for blacks in both markets. Schooling and age have a positive payoff in the primary market, while job tenure is associated with higher earnings rates in the secondary market. Primary workers have lower arrest rates associated with more work hours; this is the only significant impact of crime as an independent variable.

Crime as a Dependent Variable

Two types of analysis were carried out using some measure of crime as a dependent variable: analysis of the seriousness of a crime, measured by the expected net returns, and analysis of the prior arrest rate. In both these analyses, very little evidence was produced that supports a simple economic model of crime. This is true both for the neoclassical choice model and for the segmentation view. The most statistically significant results were obtained for income oriented crimes, but even there the results do not support a simple tradeoff model.

Job tenure is a key factor in lower crime payoffs for both markets. Lower payoff crimes are associated with transfer payments for primary workers and being married for secondary workers. Transfer payments have a positive association with crime payoffs for secondary workers, which could either be evidence of an opportunity cost tradeoff or a comment on their household composition, as secondary worker households are more likely to be receiving welfare.

Analysis of prior arrest rates also resulted in relatively low explanatory power. Primary workers have lower prior arrest rates than secondary workers; total time worked is negatively associated with arrest rates for both markets. In addition, in the secondary market, increased age and being nonwhite also have significant positive associations with arrest rates.

Social Stability, Economic Tradeoffs and Crime

Neither vision of the labor market and its associations with crime is decisively supported by the empirical research. There is more reason to support a segmentation view of the determinates of earnings and hours, but not much that supports a vision of distinctive criminal tradeoffs within each.

To some extent, this stems from the fact that in the two economic theories, while they have sharply different ideas and implications about the determination of legal labor market outcomes, are rather similar on their view of crime. Crime is viewed by both theories as a quasi-employment, one that might be traded off or mixed with legal opportunities. It is over the determination of legal opportunities where the two theories collide, but they both share the rather simple vision of the relationship between crime and legal work. To borrow a phrase from older Marxist analysis, both theories have a rather "economistic" vision about the associations between crime and legal work.

The empirical research presented here suggests that if such tradeoffs exist, they are more complex than either economic theory suggests. Taken as a whole, this research suggests that an important explanatory locus might be social stability factors. For instance, transfer payments and personal supports are negatively associated with work hours for all workers; steady work means less reliance on these alternative income sources. Being married and holding a single job longer are positively associated with work hours for all workers in the sample. Schooling and being older have a positive relationship with earnings for primary workers, while job tenure has a positive effect for secondary workers. Blacks are discriminated against on earnings in both markets, and on hours in the secondary market.

Now look at the factors associated either with less severe crimes or with lower prior arrest rates: total work time and longer job tenure in both markets, transfer payments for primary workers, and being married for secondary workers. Again, work time seems key, both in terms of total work hours and in terms of longest job tenure. When taken in conjunction with the effects of transfer payments and marital status, this suggests a cluster of social stability factors, factors that this research indicates are associated with less severe and fewer crimes.*

*Transfer payments are associated with higher crime payoffs for secondary workers, but this may also be an indicator on social stability. These payments are principally welfare

I should caution against too strong an interpretation of the empirical findings. The above discussion is meant to be suggestive, not conclusive. For this empirical research, like other work on crime and similar topics, is shadowed by a series of methodological and theoretical problems. These problems form a necessary backdrop to the empirical results discussed here, and caution against a simple interpretation of the analysis.

B. Ambiguities of Empirical Research

There are three interrelated sets of issues to consider when assessing policy-oriented econometric work. The first involves the quantification and econometric analysis of the problem--being sure that the data represent the underlying variables of interest, and that the techniques chosen for analysis are appropriate. The second problem involves the relation between econometric analysis and theoretical models; how are the variables of interest identified, and how does the researcher ground or justify the choice of those items and exclusion of others? Finally, there remains the problem of theoretical models per se, and the use of relatively simple theoretical ideas to capture complex behaviors such as work and crime. These three areas will be

payments in that market, payments which are most often made to female household heads. Welfare is received by households that are often in desperate financial straits; without stigmatizing welfare recipients, it is still possible to view receipt of inadequate public assistance as a sign of greater social instability.

considered briefly here; no attempt is made to be exhaustive on these subjects, but considering them provides a necessary frame for the empirical findings presented in the preceeding chapters.

Data Construction and Econometric Technique

Assuming knowledge of variables of interest, researchers must find data that represent those variables and select appropriate statistical techniques for analyzing the relationships among those variables. But neither step is as clear cut as the vast bulk of econometric research would make it seem.

In data construction, recall the discussions about the use of self-reported arrest data versus official police records. Self-report data suggest that many more crimes are committed by individuals than are cleared by the police through arrests. Even if a researcher assumes that the police do not make significant numbers of unfounded arrests (an invalid and dangerous assumption), different types of crimes and different types of criminals will have different ratios of crime commission to arrest. But using self-reported crime data offers no easy solution, due to the wide variation in events of interest, the problem of getting honest responses, and the problem of inaccurate memory. Even if honest responses can be guaranteed, accurate memory cannot. If the variations in timing, frequency and type of crimes are important, then self-reported data have myriad potential problems.

To some extent, researchers can make use of statistical checks for reliability, but these statistical checks cannot solve the underlying problem. How can a researcher be sure that the data punched in numeric form into a computer file are accurate representations of the variables to be examined? It is possible to hypothesize about various forms of bias and test for them; this work, although in nascent stages, holds some promise for problems such as truncated or self-selected samples. But purely technical solutions to such problems are less plausible as the complexity of the behavior increases.

A parallel problem exists in the use of econometric techniques. Through prepackaged computer software systems, even novice researchers have access to a dizzying variety of estimation techniques. There is extensive debate in econometric literature about the statistical properties of various estimators, a debate that grows daily in size and complexity. In the study of crime, debates over the use of ordinary least squares regression versus other forms of maximum likelihood estimation are rapidly appearing. (My use of tobit analysis to study the prior arrest rates of the sample is based on some of these econometric considerations.)

Think, for a moment, about the range of data gathering assumptions and technical considerations required in an econometric analysis of crime and employment such as those presented here. The analysis in chapter 4 on the expected

net monetary benefits from a single crime provides a good example. Taking guidance from a theory that suggests crimes are at least partially weighed in terms of their expected dollar benefits, I tried to compute a plausible dollar figure for such events. I had to use aggregated data collected by official state agencies on the number of crimes and the amounts lost, data that are subject to many doubts and potential inaccuracies. These figures were then discounted for a real property "fence" component and assigned a present value based on a Federal Reserve series on interest rates and a plausible possible jail or prison sentence.

In modelling the possible losses from crime, I used aggregated system data on the probabilities of arrest, court track, conviction, sentence to incarceration, possible and plausible sentence lengths, and the self-reported labor market activity of the arrestee. The resulting dollar figure was then used in ordinary least squares regressions to try and see what factors could help explain variation in the figures.

I think the work was done with extreme caution and care; but reliance on official data in some cases, self-reports in others, and the methods used in constructing the dollar figures required many assumptions and judgements on my part concerning the most accurate way to represent the data. The same goes for the choice of econometric techniques. I experimented with many other permutations of data and technique, but in the end had to be guided by my own sometimes intuitive judgements.

Theory and Technique

Conventional methodological thinking deals with these problems in two ways--focus on technique or positivist claims about theory. On the problem of econometric technique, the greatest attention is given to the statistical properties associated with particular variables or data sets. While such considerations can often point out the limits on previous analyses, they do not by themselves solve the problems. In the sort of complexities that researchers deal with when confronting problems like crime and employment relations, there are no unambiguous statistical solutions (McGahey, 1982).

Another related way of trying to deal with such problems is through the use of theory. Theory, it is often claimed, predicts the issues of interest, and hence suggests the variables and data to be used. Further, theory provides predictions that can be tested through the use of appropriate econometric techniques. This is a view of economic and social analysis that dominates the field, drawing on a particular reading of positivist philosophy and the history of science.

But in the study of crime, neoclassical labor supply theory has reached the point where it can make no determinate predictions. Further, this conclusion stems from the internal logic of the theory itself, not from any empirical refutation or alternative vision of the process. Yet econometric work on individual labor supply models of crime

continues apace. (As noted above, the labor supply model is reaching the same dead-end in other areas, notably in the study of taxation and experiments aimed at altering labor market behavior.) Why has the neoclassical research program on crime led to this theoretical cul-de-sac, and the partial and inconclusive empirical results reviewed in chapter 1?

One possible response is that the neoclassical program is in its infancy, and will develop over time. Netzer employed such an argument in considering economic analysis of the arts:

The principal explanation [of variable quality work] lies in the state of 'economics of the arts' as a field. It is a new field...New fields in any discipline are likely to be marked by all sorts of false starts. Economists being a conservative lot, new fields in economics are likely to attract only a few of the ablest people and a fair number of others who are trying to make their marks where the competition is less than fierce...passion [that attracts people to the field] cannot help affecting the work that is done in several ways. First, advocates are prone to make unacceptable leaps over logical chasms...Second, advocates are likely to focus on the wrong questions...Third, advocates are likely to see their fields as something unique, quite unlike other fields of economic inquiry (1981, p. 1121).

The implication is that over time, the community of economists sorts out the good work from the bad, a vision that fits well with the positivist claims for theory. Thus an observer might claim that economic research on crime, as in other fields, will stabilize and advance through the application of standard maximization theory and empirical research.

The problems, however, do not lie exclusively in the novelty or uniqueness of the field. It is not just models of crime, but individual models of labor supply per se that are in disarray. (Indeed, the problems in the neoclassical labor supply model can be seen as a subset of the problems with individual choice models and their reliance on tenuous equilibrium theories.)

Labor market segmentation approaches to crime do not entirely resolve this problem, although they represent a considerable advance. By seeing a sharply divided institutional labor market, the segmentation perspective is aware that tradeoffs against legal work are likely to be very different in different markets. It grows out of the institutionalist tradition in economics, a tradition that is centered on the limits to neoclassical choice models. Starting with the work of Veblen and Commons, extended through the concerns of post-war labor economists like Dunlop and Lester, and continued through contemporary cross-fertilization with radical and Marxian theory, labor market segmentation approaches are more compatible with historical and sociological analysis. Thus the segmentation approach to differing legal and illegal opportunities is reminiscent of sociological work like that of Cloward and Ohlin (1960), whose work on delinquent gangs used the idea of differential access to opportunity structures in terms that seem compatible with segmentation theory.

But there are still critical limits to the segmentation research program. Like earlier macrosociological approaches, the segmentation approach has a dangerous tendency to a form of determinism, economic in this case. This has weakened it for study of issues like individual labor supply or variations in criminal behavior, unlike marginalist approaches in neoclassical economics, where the major focus is precisely on marginal variations in individual behavior. Like conventional economic theory, segmentation theorists tend to view activities like crime as another form of employment. While much crime has a vital income-oriented dimension, viewing crime as a quasi-employment risks losing the multidimensionality of both crime and employment, leading them to be viewed much like the neoclassical choice between income alternatives.

Limits on Economic Analysis

It is this narrowing of focus in both economic approaches that is of most concern to me regarding the empirical work conducted here. Crime, like many other social processes, is an extraordinarily complex intersection of multidimensional individual behavior patterns, complicated institutional environments like the criminal justice system and the labor market, and the ever changing interrelations between those spheres. Individual choice models that attempt to partition certain aspects of behavior and hold all other factors constant are of limited applicability in such complex situations.

Mack (1981) offered some useful ideas about the efficacy of microeconomic decision models, aligning behavior along a continuum of complexity. At one pole would be a consumer's choice between two similar commodities--two types of grape jelly, for example. Here the decision is among two fairly similar commodities; most of the choice elements can be given monetary equivalents; the expected relative utilities can be known due to previous experiences; the time involved is minimal, so opportunity costs are not especially relevant; the consequences of the decision are short-term, so the dynamic nature of the interaction between the individual decision and the future is of little import; and the decision is primarily that of one individual. In short, the decision comes close to the model of neoclassical consumer choice: monetary equivalents for utilities, preference rankings of utility in a fairly well specified or observable behavior function, low or zero opportunity costs, the smallness assumption, a highly individuated decision process, and little need to consider risk preferences or possible moral or possible psychic costs.

At the other end of the continuum, Mack placed events like significant voluntary household moves, which involve many family decision makers, multiattribute utilities with highly imprecise monetary equivalents, significant but unknown long-term consequences of the action, and many dimensions of expectation and risk.

Criminal behavior belongs at this complex end of the continuum. Crimes result from a diverse set of different and complicated factors, even within the same category of offense. There is a world of difference between a killing committed between family members in the heat of an argument and a murder for hire by a professional hitman, yet both are parts of the same penal law section, albeit with some allowances for mitigating circumstances. In empirical studies, the two killings are both murders. Even for crimes with more homogeneity, like street robberies, the motivations and settings for the crimes involve a variety of different interacting factors.

The theoretical challenge is thus to incorporate insights from other disciplines and types of data in a theory building process aimed at a more complex and well-rounded theory of social behavior. The segmentation framework seems to me to hold more promise for such expansion, as it is rooted in an institutionalist tradition that takes account of the role of social complexity, and draws on Marxian approaches to socioeconomic processes that coexist and comeingle over time.

But an issue like crime remains somewhat intractable, due to the complex interaction of social and individual determinants of behavior. In considering the relationship between Marxism and psychoanalysis, Heilbroner discussed a fundamental puzzle facing psychology and social theory. Psychological theories "extend over a very limited range of

predictions about behavioral responses to changes in punishments and rewards. In other words, no determinate theoretical model undergirds the mass of empirical work that ostensibly tests theory. Policy efforts aimed at altering criminal behavior have also shown ambiguous effects, whether those efforts were directed at raising legal returns or raising the risks and severity of punishment.

Bearing in mind the methodological cautions of the previous section, I want to assess the findings of this study in the context of other important factors regarding crime and labor market behavior. Even given the broad problems described in the preceeding section, I feel it is possible to indicate relevant conclusions and suggest future directions for research and policy.

Direct and Indirect Relationships Between Employment and Crime

The research reported here, along with other work, suggests that a labor market segmentation model better describes the employment experiences of the arrestees sample than a neoclassical human capital model. The conclusion is not without cautions; education seems to have a positive payoff in the primary market, while job stability is important in the secondary market. As discussed above, a range of factors such as marriage, age, job tenure and receipt of transfer payments have generally dampening effects on crime. Many of these "social stability" factors are associated with primary sector employment, suggesting that the

importance of such employment might not be in its direct income effects on crime but on its indirect effects on stability, which then might help reduce crime. It is likely that causation is multidirectional here. Stable behavioral factors are more likely to be found among primary workers, which may make them more desirable as primary workers, provide job networks and skill training to them, and so forth (Doeringer and Piore, 1971; Lin, Ensel and Vaughn, 1981).

Employment experiences and crime are also multidimensional. In addition to the research here, interviews with jailed misdemeanants conducted by the Vera Institute of Justice discovered a multitude of ties between employment experiences and criminality.

Some offenders shifted the type and frequency of their crime depending on whether or not they were working. During periods of employment, they would commit less time-consuming crimes, or fewer income-generating crimes. Some offenders appeared to use a job as a "cover" in order to lessen police suspicion concerning their activities; for example, drug dealers are more easily able to explain large amounts of cash in their pockets if they claim employment. Some offenders used employers as the chief victims for their crimes, stealing from the job and in some cases carrying on gambling and drug schemes at the work site. Of course, many offenders illustrated the simple, direct relationship between employment and crime: they engaged in crime after loss of a job or after failure to locate and secure satisfactory employment (Sviridoff and Thompson, 1979).

But the analysis of these interviews and ethnographic research done by Vera Institute again disclosed that a host of non-employment variables contribute in indirect ways both to employment difficulties and to criminal behavior. The quantity and quality of jobs available to various groups living together in local neighborhoods influence the ways that people form households, regulate public behavior, and use public services such as schools, welfare, police, and social programs. The resulting neighborhood atmosphere in turn helps shape the incentives for residents to engage in employment and in income-oriented crime.

I think there are three essential issues that condition and constrain tradeoffs between legal and illegal opportunity, punishment and crime: the age-graded nature of socioeconomic behavior, the structure of and secular changes in the labor market, and the variegated income strategies of the urban poor.

Crime and employment are clearly linked to age, although in ways that are hard to understand. We have known for a long time that three things happen for most young males in their early twenties: arrests drop dramatically, participation in the labor market rises sharply, and family and household formations take place. To some extent, that is all we know. The interrelationships among these concurrent events are hard to model and explain in any parsimonious fashion.

Criminal behavior tends to moderate in severity and frequency in the late teens and early twenties, and ceases

altogether for many males in their early twenties. (I emphasize male behavior here, since males account for over 90 percent of FBI index arrests.) Additionally, labor market behavior changes dramatically for most young men in this period. Younger workers are in what Osterman (1980) has called a "moratorium" period, where work is exploratory and somewhat sporadic. Younger workers are often target earners, aiming at a particular short-term income rather than a long-term earnings stream, and often mix work with school or other pursuits.

These young workers typically have jobs that are low-paying, sporadic and dead end--"secondary" jobs in a dual or segmented labor market. It is these secondary jobs that are expanding in the U.S. economy; this secular change in job opportunities is the second critical factor to consider. The employment expansion of the 1970s was concentrated in sectors that have minimal skill requirements, unstable hours, and few prospects for advancement. McDonald's now employs almost two-and-a-half times as many workers as U.S. Steel.* Entry level blue collar manufacturing jobs and public service employment are disappearing from the economy, especially in older urban areas. These were the jobs that provided entry to the stable primary market for the older brothers and fathers of today's urban youth. In their place are less stable secondary jobs, many of which are segregated

*Prior to U.S. Steel's acquisition of Marathon Oil.

by gender, in such sectors as health care, food service, clerical work and cleaning.

Young urban workers are in a serious bind as they enter their late teens and early twenties. The traditional routes to employment stability that encouraged household and family formation are not there; the labor market opportunities remaining are atrophied and less rewarding. Many of the urban poor have devised a strategy of mixing many different income sources to compensate for the low income available from any single source.

This mixing of income strategies is the final important factor to consider in understanding criminal participation. Many of the urban poor supplement secondary employment with a variety of other wealth sources: government transfers, support from family and friends, barter and crime. A strict tradeoff between unemployment and crime for the urban poor is misleading. We instead need to focus on the issue of underemployment--how people actually cope with the world of intermittent secondary employment. A simple tradeoff between legal work and crime, as envisaged by some versions of the economic choice model, ignores these complexities. It has led to false expectations about the potential impacts of employment programs and punishment, and some recent discouragement with the results of those policy efforts.

Recent Labor Market Policy and Crime

It is fair, I think, to characterize recent employment and training efforts aimed at averting crime as human

what we call social behavior" and "we have no means of aggregating these individual behavioral attributes into patterns of social behavior"; conversely, "we face a theoretical impasse in trying to proceed from mass behavior to that of the single individual (1975, p. 415)." This impasse seems unlikely to be solved by purely theoretical or empirical work on a grand scale; middle-range theory building on a variety of subjects, such as crime, holds more promise for first illuminating where our understanding is lacking, and then proceeding towards more general approaches when warranted.

C. Labor Market Structure, Social Stability, Economic Choice and Crime

It is now over ten years since Becker (1968, p. 207) promised to use economic theory to help develop "optimal public and private policies to combat illegal behavior." Even allowing for economists' specialized use of the term "optimal," it is clear that this promise has been considerably diminished. Rather than policy recommendations stemming from theoretically informed research findings, much economic research on crime has produced what Tropp characterized as an "inconclusive and chaotic mass of data (1978, p. 24)."

Research has been plagued by formidable empirical problems. And the underlying model of criminal behavior, based on neoclassical choice theory and individual models of labor supply, has been found incapable of making determinate

capital programs. That is, the program efforts all assumed that raising human capital in some form would improve labor market outcomes; these better outcomes, in turn, would be traded off against crime. But many programs did not make substantial changes in long-term employment; hence the hypothesized tradeoff against crime was not observed. It remains to be seen what effects a substantial improvement in employment would have on reducing crime. For those in the secondary labor market, low earnings may lead to crime and employment being complements, not substitutes. And if programs do not act to alter the long-run employment prospects of individuals, then it is not surprising that many programs do not lower arrest rates, even during program participation. If they do not change labor market position, programs then resemble short-term, low-paying dead end jobs (McGahey, 1980b; Zall and Bethiel, 1979).

One path for future labor market policy would be consideration of how to improve jobs, and test the crime reducing impact of improved employment. For if the segmentation theorists are correct, human capital programs will have little impact on raising job outcomes. If the labor market does not operate according to the vision of human capital theory, then programs aimed exclusively at changing the characteristics of workers are misguided.

Liberal and conservative policies alike have assumed that there is some set of individual deficiencies that account for most underemployment, be it lack of skills, poor

attitudes, or whatever. "Supply-side" economics has enjoyed a recent vogue in discussions of macroeconomic policy. But labor economics has been dominated by "supply-side" explanations, where some alleged deficiency in the characteristics of labor supplied explains underemployment.

Future economic policy needs to consider improving available employment opportunities and matching program and policy efforts to the needs of particular age groups. A job program that would be appropriate for a 15 year old who is still involved in school should not be the same as a program for a 24 year old ex-offender. Their needs and social positions are so different that an employment policy merely providing employment of any sort will probably have discouraging results. The same is true for a policy that is premised on an exclusive human capital vision about deficiencies in individual characteristics.

Deterrence Policy and Crime

The disappointing results of employment programs highlight the complex responses individuals make to incentives and the difficult social and economic structural issues that surround labor market dynamics. But issues related to increasing deterrence through changes in punishment are no less complex, and recent policy results no less ambiguous. Whether through changes in police patrol, determinate mandatory prosecution policies, or legislative alterations in the

structure of the penal law, recent efforts to change criminal behavior through increased deterrence efforts offer little comfort to policy makers.

To some extent, this is because a simple deterrence policy requires the same behavioral model as simple employment policies. If punishments are raised, crime will go down, all other things being equal.

Unfortunately for this vision, as in the labor market, all other things are never equal. Individuals respond to changes in punishment in ways as complex as they respond to positive incentives. Again, issues of age and socioeconomic structure seem critical. Young offenders, especially those in poor urban areas, have access to relatively well-developed illegal markets, and poor access to primary legal ones. Rather than being simultaneous with legal work, crime often precedes any involvement in the legal labor market.

Further, knowledge about the concrete behavior required for crime is well known, possibly better than the activities required in legal jobs (Bullock, 1973). But young urban males may entertain grandiose ideas about the available dollar returns that can be made from crime, whereas they know that waged work is low paying and without future prospects.*

*The young urban poor are not the only ones with these fantasies. Many commentators and social researchers have the misleading idea that street crimes are a very lucrative occupation. In reality, most street crime is ill-paying, unstable and dangerous work; "successes" in this area are few and far between, however one-sidedly publicized.

In this context, changes in punishments may have little or no impact. With little to lose, there may be virtually no opportunity costs attached to punishment for the extremely poor. The ethnographic research at the Vera Institute does indicate sensitivity to changes in possible punishment with increasing age. This sometimes leads to cessation of crime, but often leads to switching into less risky lines of criminal work.

Additionally, making real and lasting changes in the punishments delivered by the criminal justice system is a very complex task. The criminal justice system has been rather aptly described as a "Rubik's Cube," where a change in one facet produces changes in others, changes which are often unintended and sometimes offset any intended effects. Just as institutional factors confound the simple vision of a labor market that rewards individual productivity, a host of complex institutional factors confounds the relationships between police, district attorneys, judges, corrections officers, and probation and parole personnel.

There is little knowledge on how to make simple increases in the probability of apprehension or punishment, especially apprehension, where the rate of arrest for most street crimes is extremely low. But without such changes in apprehension, policies of deterrence that would substantially lower the volume of crime are chimeras.

Even seemingly more tractable efforts to punish more severely and certainly those who are apprehended have run into difficulties. Tremendous public expenditures would be

required and important consitutional issues involved in significantly increasing the amount and length of prison sentences. But there is no compelling reason to think such increases would dramatically lessen street crime. This is doubly true given the lack of opportunity cost for many young criminals.

Certainly the results reported here give no comfort to proponents of deterrence policies, since deterrence theory relies on a simple economic tradeoff model of criminal behavior. As indicated above, I find little support for any such simple tradeoff. In further research using these data, I will make more direct tests of the effects of deterrence variables on crime type and severity.

Future Research and Policy Efforts

This rather gloomy discussion about the usefulness of economic research on crime should be tempered. The analyses of aggregate data that produced such inconclusive empirical results were a necessary first step in following the research program indicated by Becker and others. Employment policy interventions, which I have characterized as "human capital" programs, did not derive their main impetus from economic theory; the human capital vision of the labor market is one that has powerful resonance for most Americans. For it says that American society basically distributes rewards in an equitable and comprehensible fashion, and that policies could be designed to correct inequities. And the

recent interest in deterrence seems to me fueled primarily by people's legitimate personal fear of crime, as well as concern about the threatened deterioration of social peace.

The bright promise offered by Becker fourteen years ago came at a time when other social theory seemed unable, in Wilson's words, to "supply a plausible basis for the advocacy of public policy (1975, p. 53)." Economists offered a welcome approach that treated the problem of crime without reference to complex and specialized theories of deviance; Becker held out the possibility of "optimal public and private policies to combat illegal behavior (1968, p. 207)."

But we now must assess that theory's relevance for policy with less enthusiasm. As I have suggested, some of the problems are tied to the inability to extend the simple choice theory very far. It is a truism that people respond to incentives. Like all truisms, it is true, and that counts for something. But also like all truisms, it doesn't get you very far.

My reading of the economic research on the crime problem agrees with formal neoclassical critiques of the labor supply version of criminal behavior, up to a point. We could make unambiguous predictions about crime control policy through changes in positive or negative incentives if we knew how people translate incentives into monetary terms, how they respond to risk, and what role psychic and moral costs play in decisions about social behavior. But we know very little about those elements, and my judgement is that

further formal extensions of individual maximization models will not provide much help to researchers or policy makers.

The research presented here, and the considerations outlined in this chapter, lead to no easy policy directions concerning crime. Better and more stable jobs should be experimented with, for their potential crime reducing impacts as well as for many other reasons, social equity being foremost. Simple employment policies that do not consider the quality of employment provided are unlikely to have lasting impacts on labor market and crime behavior. The same is true for simple punitive deterrence policies. Through the theoretical openings created by labor market segmentation theory, economists studying crime should make links to other disciplines and sources of data, to ground insights about responses to incentives in the real world of the economy and society.

APPENDIX A

SAMPLE LABOR MARKET QUESTIONNAIRE
AND RAP SHEET

Now, I'd like to ask you about all your jobs in the last 2 years... (or, if you haven't worked in the last two years, your most recent job)

VERA ID

* RECORD ALL TIME WORKED.....DATES, AND TIME LINE.

THEN; IF WORKING.....GO TO 4

NOT WORKING.....GO TO 5

NEVER WORKED...GO TO 10

start _____ to _____ (probe for dates)
 _____ / _____ / _____ dates in 4 or 5
 _____ / _____ / _____ End or Today

6. Current or Most Recent Job....	<u>name of business</u>
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Start _____ to _____ End _____
(probe for dates) (date in 7)

8. Prior Job..... name of business

to / / (probe for dates)
to / / (dates in 9)

9, Other Jobs.....

* SHADE IN MONTHS WORKED (TIME LINE)

RECORD NO WORKED PERIOD BELOW.

[illegible]

NO WORKED PERIOD

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	101	102	103	104	105	106	107	108	109	110	111	112	113	114	115	116	117	118	119	120	121	122	123	124	125	126	127	128	129	130	131	132	133	134	135	136	137	138	139	140	141	142	143	144	145	146	147	148	149	150	151	152	153	154	155	156	157	158	159	160	161	162	163	164	165	166	167	168	169	170	171	172	173	174	175	176	177	178	179	180	181	182	183	184	185	186	187	188	189	190	191	192	193	194	195	196	197	198	199	200	201	202	203	204	205	206	207	208	209	210	211	212	213	214	215	216	217	218	219	220	221	222	223	224	225	226	227	228	229	230	231	232	233	234	235	236	237	238	239	240	241	242	243	244	245	246	247	248	249	250	251	252	253	254	255	256	257	258	259	260	261	262	263	264	265	266	267	268	269	270	271	272	273	274	275	276	277	278	279	280	281	282	283	284	285	286	287	288	289	290	291	292	293	294	295	296	297	298	299	300	301	302	303	304	305	306	307	308	309	310	311	312	313	314	315	316	317	318	319	320	321	322	323	324	325	326	327	328	329	330	331	332	333	334	335	336	337	338	339	340	341	342	343	344	345	346	347	348	349	350	351	352	353	354	355	356	357	358	359	360	361	362	363	364	365	366	367	368	369	370	371	372	373	374	375	376	377	378	379	380	381	382	383	384	385	386	387	388	389	390	391	392	393	394	395	396	397	398	399	400	401	402	403	404	405	406	407	408	409	410	411	412	413	414	415	416	417	418	419	420	421	422	423	424	425	426	427	428	429	430	431	432	433	434	435	436	437	438	439	440	441	442	443	444	445	446	447	448	449	450	451	452	453	454	455	456	457	458	459	460	461	462	463	464	465	466
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	Wks/mos in 12.
12 Prior Period of No Work.....	

13 ALL Other Periods of No Work..... wks/mos in 13

* IF WORKING GO TO 4
NOT WORKING GO TO 5
NEVER, GO TO 10

INTERVIEWER'S REPORT

1a. Start time _____ am pm

End time _____ am pm

2a. Number in holding pen

Start _____ End _____

3. Interviewer's Appraisals

3a. Confidence in truthfulness, of
D's responses:

1...high

2...moderate

3...low

9...DK...Can't evaluate

3b. D's ability to understand
questions:

1...high

2...moderate

3...low

3c. D's ability to articulate
responses:

1...high

2...moderate

3...low

3d. Language of interview:

1...English

2...Spanish

3...Mix of English and Spanish

3e. D's cooperativeness was

1...high

2...moderate

3...low

3f. Did D show signs of drug or
alcohol intoxication?

1...Yes

2...No

9...DK...can't evaluate

3g. Was interview disrupted by ex-
traneous noises or other dis-
tractions in the holding pen?

1...Yes, disrupted

2...Somewhat disrupted

3...No

3h. Are there any special problems
with any of the item? Which ones?
Any other comments?INTERVIEWER: _____
name

TODAY'S DATE: _____

*ENTER INFORMATION IN SAMPLING LOG.

INTERVIEWER REPORT

1. DEMOGRAPHIC

1a. Are you now

- 1...married, have a
- 2...common law wife; or
- 3...are you separated
- 4...divorced
- 5...widowed, or have you
- 6...never been married?
(*GO TO 1f)

1b. When did you marry (most recently) or establish your common law relationship?

____ Month ____ Year

* IF R IS CURRENTLY MARRIED (SEE 1a), GO TO 1f

1c. When were you divorced (widowed, separated)?

____ Month ____ Year

1d and 1e. DELETED

1f. Have you ever been in the service?

- 1...Yes
- 2...No(*GO TO 1i)

1g. What type of discharge did you receive?

- 1...General
- 2...Medical
- 3...Undesirable
- 4...Honorable
- 5...Dishonorable
- 6...Still in
- 7...Other

8...NR

9...DK

1h. When were you in the service?

From ____

To ____

____ Month ____ Year

*GO TO 2

1i. Did you ever try to enlist?

- 1...Yes
- 2...No

COMMENTS:

2. SCHOOL/TRAINING

2a. What is the highest school grade that you have completed?

2b. Do you have a GED or diploma?

1...Yes

2...No (GO TO 2d)

7...Other _____

8...NR

9...DK

2c. What type do you have?

1...High School diploma

2...GED

3...AA/AS

4...BA/BS

7...Other _____

8...NR

9...DK

2d-1. Do you think that having a diploma/degree has (would have) helped you to find or hold a job?

1...Yes

2...No(*GO TO 2e-1)

2d-2. Do you think that it (would) has help(ed)

1...a great deal or

2...just a bit

2e-1. When were you in school last?

_____/_____
month year

*IF MORE THAN 2 YEARS AGO, GO TO 2i 21 & 2m. DELETED

2e-2. When in the last 2 years were you in school?

_____/_____ to ____/_____ and

_____/_____ to ____/_____

2f. What were your hours?

_____ to _____

2g. How many days a week?

_____ days/wk

2h. Do you intend to return to school in September?

1....Yes

2...No

3....NR

5....DK

* IF STILL IN SCHOOL, GO TO 2k

2i. Did you leave school voluntarily?

1...Yes

2...No

8...NR

9...DK

2j. Why did you (have to) leave school?

11...received diploma/degree

12...lacked ability (poor grads)

13...didn't like school

14...family responsibilities

15...military

16...left to find work

17...left for a job - p

21...arrested, trouble with police

22...drugs

23...expelled, no reason

07...other _____

08...Nr

09...DK

2k. What was the name of the last school you attended?

COMMENTS:

2. SCHOOL TRAINING (cont.)

2t. Where did you get your training?

- 1...Rehabilitation program
(*GO TO 2v) _____
- 2...Institution (Jail or Prison) (*GO TO 2v) _____ name
- 3...Regular school
(*GO TO 2v) _____
- 4...Business/tech school
(*GO TO 2v) _____
- 5...Employer training
(*GO TO 2u) _____ name
- 7...Other _____ name
- (*GO TO 2v)
- 8...NR 9...DK

2n. Deleted

2o. Have you been in a job training program in the last 2 years?

- 1...Yes, currently
- 2...Yes but not currently
- 3...No (*GO TO 3)

2p... During the last 2 years when were you in training?

____/____ to ____/____

____/____ to ____/____

2q. What skills did you learn at the last program?

2r. What are (were) your hours?

_____ to _____

2s. How many days a week?

_____ days

2u. How did you learn at the employer training program? Were you

- 1...enrolled in formal company training class
- 2...taught by supervisors
- 3...taught by co-workers, or did you
- 4...pick up the skills on your own?
- 7...Other _____
- 8...NR 9...DK

2v. Did you complete the training program?

- 1...Yes
- 2...No, still enrolled
- 3...No
- 8...NR 9...DK

2w. Have you used the training on a job?

- 1...Yes 2...No
- 8...NR 9...DK

3. CUE

3a. Do you now have a job?

1...Yes (*GO TO 3c)

2...No

3b-1. Did you do any work for pay
in the last week?

1...Yes

2...No (*GO TO 3d)

3b-2. How many hours did you work
for pay in the preceding week?*IF 20 OR MORE, TREAT AS
CURRENT JOB; GO TO 3c.IF LESS THAN 20, DO NOT TREAT
AS CURRENT JOB; GO TO 3d.

3c. Do you have more than one job?

1...Yes (*REPORT PRIMARY ONLY
AND GO TO WORK SHEET-
RECORD ALL JOBS)2...No (*GO TO WORK SHEET-
RECORD ALL JOBS)

3d. Have you ever worked?

1...Yes (GO TO WORKSHEET -
RECORD MOST RECENT
JOB)

2...No (GO TO 10)

4. CURRENTLY WORKINGNow I'd like to talk to you about
your current job; the job you
started on

*COPY FROM WORK SHEET

4a. When in the last 7 days did you
work?

1...M

5...F

2...T

6...Sa

3...W

7...Sn

4...Th

Last Date Worked
(Month and Day)4b. Were you absent at all from work
last week?

1...Yes

2...No (*GO TO 6 -
NEXT PAGE)

4c. Why were you absent?

*GO TO 6 (NEXT PAGE)

5. NOT CURRENTLY WORKINGNow I'd like to talk to you about
your most recent job; the one you
started on

*COPY FROM WORK SHEET

and ended on

*COPY FROM WORK SHEET

GO TO 6 (NEXT PAGE)

CURRENT _____ MOST RECENT _____ JOB _____

6a. Where did (do) you work?
(Firm name and address)

6b. What kind of business was that--
what did they make or do?

_____ P

6c. What were your most important
activities at that job?

_____ P

6d. Is that a

- 1...privately owned company,
- 2...private agency (like day labor)
- 3...government, or a
- 4...govt. program or were you
- 5...self-employed
- 6...working without pay
- 7...other _____

6e. What were your typical hours?

_____ to _____

6f. How many days a week did you
usually work? How many hours/day
were you paid for?

_____ days _____ hrs/day/paid

* DETERMINE HOURS PER WEEK.
IF 35 OR MORE, GO TO 6h-1

6g. Did you prefer to have full-time
work?

- 1...Yes
- 2...No
- 8...NR
- 9...DK

6h-1. How much did you make?

\$ _____ per _____

6h-2. Is that what you took home?

1...Yes

2...No \$ _____ per _____

6i. Were taxes deducted from your pay?

1...Yes

2...No

8...NR

9...DK

6j. Did your position offer the
following benefits: Did it have--

Yes No DK

- a) medic. insur. ...1...2...9
- b) life insur. ...1...2...9
- c) paid vacatn. ...1...2...9
- d) paid sick days ...1...2...9
- e) pension ...1...2...9

6k. Did you ever receive a raise or
promotion at that job?

1...Yes, raise

from _____ to _____

(*GO TO 6n)

2...Yes, promotion

from _____

to _____ (*GO TO 6n)

3. No

8...NR

9...DK

6l. Do you think that it was possible
to get a raise or promotion in
the job position you held?

1...Yes

2...No

(why) _____

8...NR

9...DK

* IF CURRENTLY NOT WORKING GO
To 6n

6m. What would you say your chances are for a raise or promotion on this job? Would you say that you have--

- 1...a good chance,
- 2...fair chance, or a
- 3...poor chance for a raise or promotion

6n. How did people get promotions and raises at that job?

- 1...length of time there
- 2... contacts
- 3...skills
- 4...attitude
- 5...ethnicity
- 7...other _____

8...NR 9...DK

6o. While at that job, did (have) you pick(ed)-up any skills on your own that you think might help you to get a better job?

- 1...Yes 2...No
- 8...NR 9...DK

* IF YES, SPECIFY

6p. Would you say that most of the time you

- 1...Took instructions from supervisor

title of supervisor

no. under supervision P

- 2...worked on your own, or

- 3...supervised others

no. supervised on the job

6q. Were people at that firm union members?

- 1...Yes 2...No *IF NO GO TO 6t
- 8...NR 9...DK

6r. Were you a member of that union?

- 1...Yes _____
(specify)

- 2...No

6s. Deleted

6t. How did you find out about this job?

- 01...friends 21...at job?
- 02...relatives 22...at job?
- 03...private agency
- 04...NYSES
- 05...ad
- 06...direct application
- 10...neighborhood/community off.
- 11...rehab. program
- 12...probation/parole
- 13...asked by former employer
- 07...other _____
- 08...NR

6u. During the time that you worked at this job, were there any full weeks (in the last 24 months) that you did not work, not counting your vacation?

- 1...Yes
- 2...No (*IF R IS CURRENTLY WORKING, SEE CUES AT END OF 6w; IF R IS NOT WORKING, GO TO 6x-1.)

6v. About how many weeks were there that you did not work?

_____ weeks

6w. What was the main reason for not working during those weeks?

- 01...school
- 02...family reasons
- 03...illness
- 04...incarceration
- 05...lay-off
- 06...strike
- 10...drug-alcohol problem
- 11...did not want to work
- 07...other _____
- 08...NR
- 09...DK

*IF CURRENTLY WORKING, CHECK WORK SHEET:

IF CURRENT JOB STARTED MORE THAN 2 YEARS AGO, GO TO SUPPLEMENT

IF PRIOR JOB IN LAST 2 YEARS, GO TO 7.

IF NO PRIOR JOB IN LAST 2 YEARS, GO TO 11.

*IF NOT CURRENTLY WORKING, CONTINUE
TH 6x-1 AND 6x-2.

6x-1 Why did you leave that job?

- 11...Quit, found other job
- 12...Quit, because of employment conditions
- 13...Quit, wages too low
- 14...Quit, problem with boss, co-workers
- 15...Quit, illness/disability
- 16...Quit, interfered with school
- 17...Quit, to enter military
- 18...Quit, family reasons

21...Arrested

31...Lay off

32...Temporary job

34...Discharged or fired

_____ P

_____ P

07...Other _____ P

08...NR

6x-2 Did you leave voluntarily?

- 1...Yes 2...No
- 8...NR 9...DK

*IF CURRENTLY NOT WORKING, CHECK
WORK SHEET:

IF NO PRIOR JOB IN LAST 2 YEARS
 GO TO 10.

IF PRIOR JOB IN LAST 2 YEARS,
 CONTINUE TO 7.

7. PRIOR JOB IN LAST 2 YEARS

Now I'd like to talk to you
 about the job you had before
 that, the job that started on

_____/_____/_____
 *COPY DATE FROM WORK SHEET
 and ended on

_____/_____/_____
 *COPY DATE FROM WORKSHEET

8. PRIOR EMPLOYMENT WITHIN 24 MONTHS

8a. Where did you work? (Firm name and address)

8b. What kind of business was that -what did they make or do?

_____ P

8c. What were your most important activities at that job?

_____ P

8d. Was that a

- 1...privately owned company
- 2...agency (like day labor)
- 3...government
- 4...gov't program
- or were you:
- 5...self-employed
- 6...working without pay
- 7...other _____

8e. What were your typical hours?

_____ to _____

8f. How many days a week did you usually work? How many hrs/dy were you paid for?

_____ days _____ hrs/day/pd

* DETERMINE HOURS PER WEEK.
IF 35 OR MORE GO TO 8h

8g. Did you prefer to have full-time work?

- 1...yes 2...No
- 8...NR 9...DK

8h-1. How much did you make?

\$ _____ per _____

8h-2. Is that what you took home?

1...Yes

2...No \$ _____ per _____

8i. Were taxes deducted from your pay?

1...Yes

2...No

8...NR

9...DK

8j. Did your position offer the following benefits: Did it have

Yes No DK

a) medic. insur. ...1...2...9

b) life insur. ...1...2...9

c) paid vacatn. ...1...2...9

d) paid sick days ...1...2...9

e) pension ...1...2...9

8k. Did you ever receive a raise or promotion at that job?

1...Yes, raise

from _____ to _____

(*GO TO 8n)

2...Yes, promotion

from _____

to _____

(*GO TO 8n)

3...No

8...NR

9...DK

8l. Do you think that it was possible to get a raise or promotion in the job position you held?

1...Yes

2...No

8...NR

9...DK

8m. Deleted

8n. How did people get raises and promotions at that job? P

1...length of time there

2...contacts

3...skills

4...attitude

5...ethnicity (reverse)

7...other _____

8...NR

9...DK

8o. While at the job, did you pick up any skills on your own that you think might help you to get a better job?

1...Yes 2...No

8...NR 9...DK

*IF YES, SPECIFY

8p. Would you say that most of the time you

1...Took instructions from a supervisor,

title of supervisor

no. under supervision P

2...worked on your own, or

3...supervised others

no. supervised on the job

8q. Were people at that firm union members?

1...Yes 3...No *IF NO GO TO 8t

8...NR 4...DK

8r. Were you a member of that union?

1...Yes _____

specify

2...No

8s.

DELETED

8t. How did you find out about this job?

01...friends 21...at job?

02...relatives 22...at job?

03...private agency

04...NYSEs

05...ad

06...direct application

10...neighborhood/community off.

11...rehab. program

12...probation/parole

13...asked by former employer

07...other _____

08...NR

8u. _____, during the time that you worked at this job, were there any full weeks (in the last 24 months) that you did not work, not counting your vacation?

1...Yes

2...No (*GO TO 8x)

8v. About how many weeks were there that you did not work?

_____ weeks

8w. What was the main reason for not working during those weeks?

- 01...school
- 02...family reasons
- 03...illness
- 04...incarceration
- 05...lay-off
- 06...strike
- 10...drug/alcohol problem
- 11...did not want to work
- 07...other _____
- 08...NR 09...DK

8x-1. Why did you leave that job?

- 11...Quit, found other job
- 12...Quit, because of employment conditions
- 13...Quit, wages too low
- 14...Quit, problem with boss, co-workers
- 15...Quit, illness/disability
- 16...Quit, interfered with school
- 17...Quit, to enter military
- 18...Quit, family reasons

21...Arrested

31...Lay off

32...Temporary job

34...Discharged or fired

07...Other _____

08...NR

8x-2. Did you leave voluntarily?

- 1...Yes 2...No
- 8...NR 9...DK

9. OTHER EMPLOYMENT IN LAST 2 YEARS

*CHECK WORK SHEET:

COPY DATES OF ALL OTHER
EMPLOYMENT IN LAST 2 YEARS
HERE:

Start _____

End _____

Start _____

End _____

Start _____

End _____

*IF CURRENTLY WORKING, GO TO 11.

*IF NOT WORKING, GO TO 10

*IF R WORKED FOR ENTIRE 2 YEAR PERIOD, WITH NO PERIODS OF NOT WORKING, GO TO SUPPLEMENT

10. PERIOD OF NOT WORKING WITHIN
LAST 4 WEEKS:

10a. Have you looked for work in
the last 4 weeks?

- 1...Yes
2...No (*GO TO 10h)
8...NR

10b. What specifically have you done
to find work in the last four
weeks? (Check all)

- 01...friends 21...at jobs
02...relatives 22...at jobs
03...private agency
04...NYSEs
05...ads
06...employers directly
10...neighborhood/community off.
11...rehab program
12...probation/parole
13...former employer
07...other _____
08...NR

10c. What kind of work are you
looking for? _____

- 1...Any job (*GO TO 10e)
8...NR (*GO TO 10e)
9...DK (*GO TO 10e)

10d. Do you have any experience with
that kind of work?

- 1...Yes 2...No
8...NR 9...DK

10e. How many hours would you want
to work?

_____ per _____

10f-1. What is the lowest wage you
would accept for that kind of
job? (for the hours in 10e)

\$ _____ per _____

10f-2. Is that

- 1...before or
2...after taxes?

10g. What would you say has been your
biggest problem finding work?

- 0...No problems
1...lack of jobs
2...no skills
3...no education
4...no experience
5...criminal record
6...drug/alcohol problem
7...other _____
8...NR 9...DK
(*GO TO 10i)

10h. What is the main reason that you
are not looking at this time?

- 01...school
02...health--ill/disabled
03...personal/family reasons
04...lacks skill/education/
experience
05...employers think too old/
young
06...criminal record
10...believes no work avail.
11...incarcerated
12...waiting for good job
13...had job lined up
14...military
15...vacation/not interested/
doesn't want to work
16...lacks funds/clothes/tools
for job search
07...Other _____
08...NR 9...BK

10i. How have you supported yourself in the last 4 weeks?

1...Unemployment Insurance

2...Family 6...Savings

3...Welfare 10...Spouse

4...Odd Jobs 11...Institu.

5...Friends 12...Other
 Transfer

7...Other

8...NR 9...DK

COMMENTS:

CURRENT OR MOST RECENT PERIOD OF
NOT WORKING

* CHECK WORK SHEET:

FIND MOST RECENT PERIOD OF NO
WORK: COUNT UP WEEKS (MONTHS)
OF NO WORK.

*IF CURRENTLY WORKING

Before you found your current
job, you did not work for

_____ wks mos
(* COPY FROM WORK SHEET)

in the last 2 years

*IF NOT WORKING SAY

Since you left your last job,
you haven't worked for

_____ wks mos
(* COPY FROM WORK SHEET)

in last 2 years

11a & 11b DELETED

11c. During the time that you were
not working, did you look for
work at all?

1...Yes

2...No (*GO TO 11g-2)

11d. When you looked for work, what
did you usually do? (check all)

01...friends	21...at job
02...relatives	22...at job
03...private agency	
04...NYSES	
05...ads	
06...employers directly	
10...neighborhood/community off.	
11...rehab program	
12...probation/parole	
13...former employers	
07...other _____	
08...NR	

11e. When you looked for work during
that period, did you ever use
any of these other methods?

Did you check with:

	Yes	No	DK
friends	...1...	2...	9
relatives	...1...	2...	9
private agency	...1...	2...	9
NYSES	...1...	2...	9
ads	...1...	2...	9
employers	...1...	2...	9
neigh/community	..1...	2...	9
rehab. program	...1...	2...	9
probation/parole	..1...	2...	9
former emplrs1...	2...	9

11f. During this (that) time (from
11) about how many weeks
have (did) you look for work?
That excludes of course weeks
that you were incarcerated,
ill, with family responsibil-
ities, or time that you were
not interested in work because
of school or some other reason:

Looked for _____ wks/mos.

* IF R LOOKED FOR WORK FOR
THE ENTIRE TIME THAT HE WAS
NOT WORKING, GO TO 11h.

11g-1. If you looked for work
for (weeks in 11f) that
leaves

_____ weeks/mos. that
you did not look.

11g-2. What was the main reason
for not looking during
that time?

- 1...school
- 2...health--ill/disabled
- 3...personal/family reasons
- 4...lacked skills/education/
experience
- 5...employers thought too
young/old
- 6...criminal record
- 10...believe no work avail.
- 11...incarcerated
- 12...waiting for good job
- 13...had job lined up
- 14...military
- 15...vacation/not interested/
didn't want to work
- 16...lacked funds/clothes/
tools for job search
- 07...other _____
- 08...NR 9...DK

11h. During the time that you were
not working, how did you support
yourself? (Check all)

- 1...Unemployment Insurance
- 2...Family 3...Welfare
- 4...Odd Jobs 5...Friends
- 6...Savings 10...Spouse
- 11...Institution
- 12...Other Transfer
- 7...Other _____
- 8...NR 9...DK

*CHECK WORK SHEET:

IF THERE IS A PRIOR PERIOD OF
NO WORK, GO TO 12.

IF THERE IS NO PRIOR PERIOD OF
NO WORK, GO TO SUPPLEMENT

12. PRIOR PERIOD OF NO WORK

* CHECK WORK SHEET
FIND PRIOR PERIOD OF NO WORK
ON WORK SHEET. COUNT-UP TIME
NOT WORKED IN THE PRIOR PERIOD

* IF CURRENTLY WORKING, SAY:

Between the time you started your
prior job and left the other job
you had before that, you did not
work for

_____ wks/mos
*COPY FROM PRIOR PERIOD
NOT WORKING ON WORK
SHEET

in the last 2 years

* IF NOT CURRENTLY WORKING, SAY:

Between the time you started your
most recent job, and left the prior
job, you did not work for

_____ wks/mos
*COPY FROM PRIOR PERIOD
NOT WORKING ON WORK
SHEET

in the last 2 years.

12a & 12b. DELETED

12c. During that time, did you look
for work at all?

1... Yes 2...No (GO to 12e-2)

12d. During that time (from 12)
, about how many weeks did
you look for work? That ex-
cludes of course weeks that you
were incarcerated, ill, with
family responsibilities, or
time that you were not inter-
ested in work because of school
or any other reason.

Looked for _____ wks/mos.

* IF R LOOKED FOR WORK FOR
THE ENTIRE TIME THAT HE WAS NOT
WORKING, GO TO WORK SHEET

*IF THERE ARE OTHER PERIODS OF NOT
WORKING, GO TO 13.

*IF THERE ARE NO OTHER PERIODS OF
NOT WORKING, GO TO SUPPLEMENT.

12e-1. If you looked for work for
(weeks in 12d), that leaves

_____ wks/mos
that you did not look.

12e-2. What was the main reason for
not looking during that time?

- 01...school
- 02...health--ill/disabled
- 03...personal/family reasons
- 04...lacked skills/education/
experience
- 05...employers thought too
young/old
- 06...criminal record
- 10...believed no work avail.
- 11...incarcerated
- 12...waiting for good job
- 13...had job lined up
- 14...military
- 15...vacation/not interested/
didn't want to work
- 16...lacked funds/clothes tools
for job search
- 17...Other _____
- 08...NR 9...DK

* CHECK WORK SHEET

*IF THERE ARE OTHER PERIODS OF NOT
WORKING, GO TO 13

*IF THERE ARE NO OTHER PERIOD OF
NOT WORKING, GO TO SUPPLEMENT

13. OTHER PERIOD OF NOT WORKING* CHECK WORK SHEET

FIND OTHER PERIODS OF NO WORK
ON WORK SHEET. COUNT UP OTHER
PERIODS OF NO WORK AND

SAY:

Other than the times we've just
talked about, there were

_____ wks/mos

* COPY FROM OTHER PERIODS
NOT WORKING ON WORK SHEET

in the last 2 years that you
did not work.

13b. DELETED

13c. During that (those) time(s), did
you look for work at all?

1...Yes 2...No (GO TO 13e-2)

13d. During that time (13)
about how many weeks did you
look for work? That excludes
of course weeks that you were
incarcerated, ill, with family
responsibilities, or time that
you were not interested in work
because of school or any other
reason.

Looked for _____ wks/mos.

*IF R LOOKED FOR WORK FOR THE
ENTIRE TIME THAT HE WAS NOT WORKING,
GO TO SUPPLEMENT

13e-1. If you looked for work for
(weeks/mos in 13), that leaves

_____ wks/mos

that you did not look.

13e-2. What was the main reason for
not looking during that time?

01...school

02...health--ill/disabled

03...personal/family reasons

04...lacked skill/education/
experience

05...employers thought too
young/old

06...criminal record

10...believed no work avail.

11...incarcerated

12...waiting for good job

13...had job lined up

14...military

15...vacation/not interested/
did not want to work

07...other _____

08...NR

09...DK

*GO TO SUPPLEMENT

SUPPLEMENTS

*IF VERA ID IS ODD, GIVE JOB MOBILITY SUPPLEMENT (A&B).

*IF VERA ID IS EVEN, GIVE EXPECTATIONS, ATTITUDES, AND
BARRIERS SUPPLEMENT (C&D).

A. JOB MOBILITY-FIRST & LONGEST JOB

*IF R IS STILL IN SCHOOL
☐ CHECK HERE AND
 GO TO OTHER SUPPLEMENT (C&D).

A1. Now I'd like to ask you about
 your first job after you left
 school. When did it start and
 end?

____/____/____ to
 Start

____/____/____
 End

0...Never worked(*GO TO OTHER
 SUPPLEMENT)

*IS THIS THE SAME JOB RE-
 PORTED IN ITEM 6 OR 8?

1...Yes, same as 6(*GO TO B)

2...Yes, same as 8(*GO TO B)

3...Deleted

4...No, different job

A2. What kind of business was that
 --what did they make or do?

A3. What were your most important
activities at that job?

A4. Was that a

1...privately owned company,

2...private agency (like day
 labor

3...government, or a

4...gov't program

or were you:

5...self-employed

6...working without pay

7...other _____

A5. What were your typical hours?

_____ to _____

A6. How many days a week did you
 usually work? How many hours
 a day did you get paid for?

_____ days _____ hrs /day/pd

* DETERMINE HOURS PER WEEK.
 IF 35 OR MORE, GO TO A8-1.

A7. Did you prefer to have full-time
 work?

1...Yes

2...No

8...NR

9...DK

A8-1. How much did you make?

\$ _____ per _____

A8-2. Is that what you took home?

1...Yes

2...NO \$ _____ per _____
 (Take Home)

A9. Were taxes deducted from your pay?

1...Yes

2...No

8...NR

9...DK

A10. How did you find out about this
 job?

01...friends

21...at job?

02...relatives

22...at job?

03...private agency

04...NYSES

05...ad

06...direct application

10...neighborhood/community off.

11...rehab. program

12...probation/parole

13...asked by former employer

07...other _____

08...NR

All-1. Why did you leave that job?

- 11...Quit, found other job
- 12...Quit, because of employment conditions
- 13...Quit, wages too low
- 14...Quit, problem with boss, co-workers
- 15...Quit, illness/disability
- 16...Quit, interfered with school
- 17...Quit, to enter military
- 18...Quit, family problems

21...Arrested

31...Lay off

32...Temporary job

34...Discharged or fired

07...Other _____

08...NR

All-2. Did you leave voluntarily?

1...Yes 2...No

8...NR 9...DK

* CONTINUE TO B.

B. LONGEST HELD JOB

B1. Now I'd like to talk to you about the job you've held longest. When did it start and end?

____/____/____ to
Start

____/____/____
End

*IS THIS THE SAME JOB REPORTED IN ITEM 6 OR 8 OR A?

1...Yes, same as 6(*GO TO 14)

2...Yes, same as 8(*GO TO 14)

3...Yes, same as A(*GO TO 14)

4...No, different job

B2. What kind of business was that --what did they make or do?

B3. What were your most important activities at that job?

B4. Was that a

- 1...privately owned company,
- 2...private agency (like day labor)
- 3...government
- 4...gov't program

or were you:

- 5...self-employed
- 6...working without pay
- 7...other _____

B5. What were your typical hours?

_____ to _____

B6. How many days a week did you usually work? How many hours a day did you get paid for?

_____ days _____ hrs /day/pd

* DETERMINE HOURS PER WEEK.
IF 35 OR MORE, GO TO B8-1.

B7. Did you prefer to have full-time work?

1...Yes 2...No

8...NR 9...DK

B8-1. How much did you make?

\$ _____ per _____

B8-2. Was that what you took home?

1...Yes

2...No \$ _____ per _____

B9. Were taxes deducted from your pay?

1...Yes 2...No

8...NR 9...Dk

B10. How did you find out about this job?

01...friends 21...at job?

02...relatives 22...at job?

03...private agency

04...NYSEs

05...ad

06...direct application

10...neighborhood/community off.

11...rehab. program

12...probation/parole

13...asked by former employer

07...Other _____

08...NR

B11-1. Why did you leave that job?

- 11...Quit, found better job
 - 12...Quit, because of employment conditions
 - 13...Quit, wages too low
 - 14...Quit, problem with boss, co-workers
 - 15...Quit, illness/disability
 - 16...Quit, interfered with school
 - 17...Quit, to enter military
 - 18...Quit, family problems
 - 21...Arrested
 - 31...Lay off
 - 32...Temporary job
 - 34...Discharged or fired
-
-

07...Other _____

08...NR

B11-2. Did you leave voluntarily?

- | | |
|---------|--------|
| 1...Yes | 2...No |
| 8...NR | 9...DK |
-

* GO TO 14 - LAST PAGE.

c. BARRIERS TO EMPLOYMENT

C1. Have you ever been turned down for a job for which you had applied?

1...Yes

2...No (#GO TO C5)

8...NR 9...DK

C2. What do you think was most often the reason for being turned down?

1...physical appearance

2...health/disabilities

3...lacked skills

4...lacked education

5...lacked experience

6...employers thought too young/old

10...criminal record

11...incarceration history

12...racial discrimination

13...deleted

14...drug/alcohol history

7...Other _____

8...NR 9...DK

C5. Can you think of anything about yourself that would make an employer not want to hire you?

1...physical appearance

2...health/disabilities

3...lacked skills

4...lacked education

5...lacked experience

6...employers thought too young/old

10...criminal record

11...incarceration history

12...racial discrimination

13...no problems

14...drug/alcohol history

07...Other _____

08...NR 9...DK

C6. Which of the following things do you think have helped you, harmed you, or made no difference in terms of your ability to find a job.

For example,

a) your age—has it

1...helped you

2...harmed you, or

3...made no difference

9...DK

in terms of your ability to find a job?

a) your age1...2...3...9

b) your skills...1...2...3...9

c) your ability to speak English..1...2...3...9

d) your ability to read English..1...2...3...9

e) your health...1...2...3...9

f) your sex.....1...2...3...9

g) your level of education.....1...2...3...9

h) your race.....1...2...3...9

i) your employment history..1...2...3...9

* CONTINUE TO D

D. JOB ASPIRATIONS AND ATTITUDES

1. If you had your choice, what type of job would you most like to have?
- _____
- _____

D2. Have you done that type of work before?

1...Yes

2...No _____

D3. Do you know people who do that kind of work?

1...Yes

2...No

D4. Do you think you have an

1...Excellent 2...Good

3...Fair 4...Poor

chance of getting that job?

8...NR

9...DK

Why?

D5. How did you feel about your last (current) job? Did you

1...Like it very much

2...Like it fairly well

3...Dislike it somewhat

4...Dislike it very much

8...NR

9...DK

0...Never Worked

D6. What are the things you liked (or, if never worked, think you would like) about your (last/current) job?

D7. What are the things you didn't like (or think you won't like) about your (last/current) job?

* GO TO 14 - LAST PAGE.

D1-D7

14. END OF INTERVIEW

14a. If you were in a position of authority, what would you do to improve the employment situation of people like yourself?

14b. What do you think you'll be doing five years from now?

*GO TO CONSENT TO CONTINUE/RECONTACT FORM.

DATE
TIME
FAX NO

STATE OF NEW YORK
DIVISION OF CRIMINAL JUSTICE SERVICES

TRAN NO
PAGE
B#
DOB
RAC
SEX
HGT
SOC
FBI

CONFIDENTIAL TO: NYCPD HDQ

NAME

! NYSID

NAMES USED BY SUBJECT

Coded

Int

10/14/80

< < < < < CRIMINAL HISTORY > > > > >

ARREST DATE
INFORMATION

ARREST CHARGES

DISPOSITION AND
CORRECTIONS DATA

ARR DT/PL [REDACTED] SELLING DANGEROUS DRUG-3RD
KINGS COUNTY 220.30 PL CLASS D FEL
NCIC 3599!
CRM DT/PL [REDACTED]
KINGS COUNTY POS 25/MORE CIG CONT DANG DRUG!
220.15 PL CLASS D FEL
NCIC 3599!
ARR #/AGY [REDACTED]
NYCPD PCT [REDACTED]
POSSESSION OF LOADED FIREARM
265.05 PL CLASS D FEL
NCIC 5212!

- - DISPOSITION - -
UNKNOWN CHIM CRT KINGS
DKT # [REDACTED]
INITIAL REPORT OF DOCKET NO.

ARR DT/PL [REDACTED] POS 25/MORE CIG CONT DANG DRUG!
KINGS COUNTY 220.15 PL CLASS D FEL
NCIC 3599!
CRM DT/PL [REDACTED]
KINGS COUNTY
ARR #/AGY [REDACTED]
NYCPD PCT [REDACTED]

NO DISPOSITION REPORTED

ARR DT/PL [REDACTED] SELLING DANGEROUS DRUG-2ND
KINGS COUNTY 220.35 PL CLASS C FEL
NCIC 3599!
CRM DT/PL [REDACTED]
KINGS COUNTY
ARR #/AGY [REDACTED]
NYCPD PCT [REDACTED]

- - DISPOSITION - -
SUP CRT KINGS CO
IND # [REDACTED]
CONVICTED UPON PLEA OF GUILTY
THE FOLLOWING CHARGE(S):
ATTEMPT AT CLASS D FELONY
110.00 PL CLASS E FEL
SENT: 3 YRS MAX
IN FULL SATISFACTION OF:
SELLING DANGEROUS DRUG - 3RD
220.35 PL CLASS C FEL
POSSESSION DANGEROUS DRUG- 6TH
220.05 PL CLASS A MISD

(CONT. NEXT PAGE)

DATE
NAME

NYSID

PAGE 2
TRAN NO

< < < < < < CRIMINAL HISTORY > > > > > >

ARREST DATE INFORMATION	ARREST CHARGES	DISPOSITION AND CORRECTIONS DATA
----------------------------	----------------	-------------------------------------

		-- CORRECTIONS DATA -- RELEASED TO PAROLE UNKNOWN ID ASSIGNED: INTERSTATE DISCHARGED ADMISSION TERM: 3 YRS MAX CORR FAC ID
--	--	---

ARR DT/PL KINGS COUNTY	PROMOTING GAMBLING-1ST 225.10 PL CLASS E FEL NCIC 3999	DISPOSITION -- CRIM CRT KINGS DKT # BENCH WARRANT ISSUED
CRM DT/PL KINGS COUNTY	POSSESS GAMBLING RECORDS-1ST 225.20 PL CLASS E FEL NCIC 3912	
ARR #/AGY NYCPD PCT	UNLAW POSS MARIHUANA 221.05 PL CLASS VIOL NCIC 3562	CRIM CRT KINGS DKT # ADJ CONTEM OF DISM-CPL 170.55
CE CON#		
FAX NO		CRIM CRT KINGS DKT # DISM. UNDER 170.55 THE FOLLOWING CHARGE(S): UNLAW POSS MARIHUANA 221.05 PL CLASS VIOL

ARR DT/PL KINGS COUNTY	CRIM POSS CONTRL SUBST 220.06 PL CLASS D FEL NCIC 3599	DISPOSITION -- CRIM CRT KINGS DKT # CONVICTED UPON PLEA OF GUILTY
CRM DT/PL KINGS COUNTY	POSSESS GAMBLING RECORDS-2ND 225.15 PL CLASS A MISD NCIC 3912	THE FOLLOWING CHARGE(S): UNLICENSED DRIVER 05090 VTL CLASS INFR
ARR #/AGY NYCPD PCT	OPER VEH WHILE LIC SUSPENDED 05110 VTL CLASS U MISD NCIC 5499	SENT: CONDITIONAL DISCHARGE IN FULL SATISFACTION OF: OPER VEH WHILE LIC SUSPENDED 05110 VTL CLASS U MISD
CRT CON#		
FAX NO		CRIM CRT KINGS

(T. NEXT PAGE)

APPENDIX B: CONVERSION OF LABOR MARKET SEGMENTATION SCHEME INTO 1970 CENSUS CATEGORIES

Gordon's original scheme did require some adaptation for use in this analysis, as his work was done with 1960 occupational codes. My data are coded with 1970 Census occupational codes. It was necessary to transform Gordon's scheme into those codes, based on a list of the 1960 codes and their assignments to one of four segments, as well as new occupational titles that were added to the 1970 scheme. The four segments are independent primary--professional and technical; independent primary--craft; subordinate primary; and secondary. The first three groups together make up primary jobs.

In checking assignments of the new occupational titles, some discrepancies arose; additionally, there were some titles in the 1970 scheme that Gordon did not assign. I crosschecked all of Gordon's original assignments and produced a unique segment score for each occupation listed in the Census coding manuals (U.S. Department of Commerce, Bureau of the Census, n.d.).

When the Census Bureau added new occupational titles, they not only changed the numbers for most occupations, but also made numerous revisions among categories. (For a complete discussion of these changes, and a listing, see a

technical paper, U.S. Department of Commerce, Bureau of the Census, n.d.) The technical paper lists the 1970 codes, and then shows the 1960 elements that make up the 1970 category. It also shows the percentage from each 1960 category that went into the 1970 category, and the total number of workers in the 1970 category by their respective 1960 codes. The technical paper thus facilitated assigning segments for 1970 codes.

In some cases, there was only one underlying 1960 code for the 1970 scheme. In those cases where Gordon had assigned the 1960 code a segment score, the score was applied to the 1970 category. (For example, the category of "Judges" in 1970, #030, was entirely drawn from the 1960 category of "Lawyers and Judges," #105. This 1960 code was assigned a segment score of I--independent primary, professional and technical. Thus the 1970 Judges group was also assigned as I.)

In other cases, more than one 1960 group fed into the 1970 group, but both 1960 subgroups had the same segment score; the 1970 group thus got that segment score. (1970s "Chemical Technicians," #151, was drawn from 1960s "Technicians, other engineering and physical sciences," #191, and "Technicians, n.e.c. (not elsewhere classified)," #192. Both of these 1960 groups were III--subordinate primary, and so "Chemical Technicians" in the 1970 scheme were assigned a III.)

Not all assignments were so clear cut. Some 1970 codes drew on several 1960 categories which were assigned to different segments. In these cases, the category which provided the majority of the 1970 group was used to assign the segment score. (For example, "Accountants," #001 in 1970, was drawn from the 1960 groups of "Accountants and Auditors," #000, segment I; "Statisticians and Actuaries," #174, segment III; and "Clerical and Kindred Workers, n.e.c.," #370, segment III. In this case, 1960s "Accountants and Auditors" provided around 96% of the 1970 category, so the 1970 category was assigned to segment I.)

The Census technical paper was not exhaustive. Six occupations were listed in the 1970 Census scheme but not in the technical paper. Fortunately, all of these titles could be clearly assigned to segments based on information provided by Gordon. In most cases, there were unambiguous one-to-one correspondences between 1960 and 1970 titles. (In one case, the title changed slightly: 1960s "Health Workers, n.e.c." was taken to be 1970s "Health Practitioners, n.e.c.," #073, segment I.)

There are some slight differences between the occupational list provided by the Indexes and the list I produced. The Indexes give 437 total categories, while my list provides segment assignments for 428 occupations. The differences are discussed below.

First, the Indexes provide 12 codes for occupational allocation to a two-digit group. When the Census coders

cannot assign an occupation a three-digit code, the Bureau allocates the worker to one of the twelve two-digit groups on the basis of the demographic characteristics of the two-digit group which most closely approximates that of the worker. As Gordon's segment scheme does not operate at the two-digit level, there can be no assignment of these codes to segments. I did employ a similar assignment procedure for three-digit occupations in a few cases, based on work done at the University of Wisconsin (Featherman, Sobel and Dickens, 1975). Additionally, the Census reserves one code for "Occupation Not Reported"; I had no missing occupational detail, as the Vera interviewers got occupational information for all reported jobs. Finally, the Census provides one code in 1970, #580, for "Former Members of the Armed Forces"; this code is not assigned to any segment on the attached sheet.

Gordon's segmentation scheme contains separate codes within the "Salesman and Sales Clerks, n.e.c." category. The Census Bureau used five subdivisions of this category when coding occupations. The general category is #280, and the five subdivisions are #281-285. My list assigns #280, and also the five subdivisions, #281-285, to segments. However, the count of 437 occupations in the Census Indexes only includes the single category of #280.

Thus the 437 occupations of the Indexes and the 428 of my list are reconciled in the following manner:

	437	Index occupational codes
minus	12	Allocation categories (#196, 246, 296, 396, 586, 696, 726, 796, 806, 846, 976, 986)
minus	1	"Occupation not reported" category (#995)
minus	1	"Former member of the Armed Forces" category (#580)
plus	5	Subdivisions within the salesman category (#281-285)
equals	428	Occupational codes

As noted, there are 428 occupational codes assigned to segments in my adaptation of Gordon's scheme.

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