Ex-Offenders and the Conformist Alternative: A Job Quality Model of Work and Crime*

CHRISTOPHER UGGEN, *University of Minnesota*

Criminologists from diverse theoretical perspectives have long asserted that the quality of employment is more strongly associated with criminal behavior than its presence or absence. By this argument, "good jobs" or "meaningful work" are necessary to induce offenders to desist from crime. This paper constructs a satisfaction-based measure of job quality using data from the National Supported Work Demonstration and the 1977 Quality of Employment Survey and tests whether employment in high quality jobs reduces the likelihood of criminal behavior among offenders. After statistical corrections for selection into employment, job quality is found to reduce the likelihood of economic and non-economic criminal behavior among a sample of released high-risk offenders. None of the most salient alternative explanations—sample selection, human capital accumulation, personal expectations, external labor market effects, or prior criminality—appear to diminish the job quality effect.

In recent years, criminologists have detailed the effects of employment on crime (Crutchfield and Pitchford 1997; Ploeger 1997; Sampson and Laub 1990, 1993; Wright, Cullen and Williams 1997), the effects of crime on occupational attainment (Hagan 1991; Nagin and Waldfogel 1995), and the reciprocal relation between unemployment and crime (Baron and Hartnagel 1997; Thornberry and Christenson 1984). Much of this work hypothesizes some relation between the quality of employment and criminal behavior. Only "high quality" jobs with "adequate hours and pay" (Allan and Steffensmeier 1989:107) or "satisfying employment" (Witte 1979:30; Shover 1996:129) leading to "viable work careers" (Duster 1987:309) are thought to affect recidivism.

Because individuals self-select into employment, however, it is difficult to separate job effects from the pre-existing characteristics of workers. When jobs have been exogenously determined, as by random assignment in experimental research, they have been of uniformly low quality (Piliavin and Gartner 1981; Uggen 1998). Would the provision of *high* quality jobs reduce criminal behavior among released offenders? I address this question by analyzing a sample of released prisoners. All new releasees share a similar social position as they reenter civil society, though they differ in human capital and post-release support. Over time, some will enter the paid labor force and some will return to crime. This paper uses sample selection techniques to model these processes jointly in an effort to separate job effects from the "person effects" that predict job entry. I then test whether exposure to high quality jobs, net of background and alternative employment measures, reduces subsequent criminal activity.

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Crime and the Quality of Employment

Anomie Theory

Robert K. Merton's (1938) anomie theory posits an inverse relationship between job quality and crime at both societal and individual levels of analysis. Merton holds that the combination of universal cultural success goals and an unequal distribution of legitimate means to their attainment produces a societal condition of anomic stress. This stress engenders high crime rates in stratified societies (such as the contemporary United States) in which material success and social standing are prized by all yet unattainable to many. Merton thus predicts that societal variation in social mobility processes affects aggregate crime rates and the distribution of crime across social groups (Merton 1995; Messner and Rosenfeld 1997a:53).

At the individual level, Merton offers a model of "choice-making among socially structured alternatives" (Rosenfeld 1989:454; Stinchcombe 1975). Persons' social positions, which derive largely from their occupation, partially determine their reactions to socially structured strain. In Merton's typology, conformity to success goals and legitimate means is the most common role adjustment, marked by the pursuit of economic and social ends through educational preparation and labor market achievement. The ritualist response, in which success goals are abandoned but devotion to legitimate means persists, is adopted by those dutifully laboring in unrewarding positions with no hope of advancement—so-called dead-end jobs. The retreatist adaptation is manifest in a rejection of both goals and means, typified by those chronically unemployed discouraged workers who withdraw from the labor force completely.

This paper is primarily concerned with criminal innovation as a mode of adaptation. Innovators spurn undesirable employment, opting instead for illicit pursuit of success goals.[1] Merton links job quality and crime in his discussion of Chicago's near north side, where legitimate opportunities had been confined to low-quality manual work that could not compete with the income and status available from crime. When the legitimate means to success goals are thus blocked, criminal motivation rises from the "triumph of amoral intelligence over morally prescribed 'failure'" (Merton 1938:679).

Two conceptual sticking points in anomie theory concern Merton's emphasis on economic processes and his ambiguous treatment of "legitimate means." First, critics charge Merton with overemphasizing pecuniary or economic goals (e.g., Kornhauser 1978:150), and challenge the theory's applicability to extra-economic or "nonutilitarian" (Farnworth and Leiber 1989:264) personal crimes. This analysis attempts to isolate the economic and non-economic effects of job quality on disaggregated economic and non-economic crime outcomes. Second, with regard to legitimate means, it is not clear whether Merton refers to opportunities or outcomes, to access to rewards or to the rewards themselves (Rosenfeld 1989). Though opportunity can be conceived as human capital, expectations, or background characteristics, the most conspicuous indicator of employment alternatives is the job itself. Job quality therefore represents a gradient of both opportunities and outcomes; the quality of one's job indicates the likely range of future movement as well as one's present social location.

Because opportunities are unevenly distributed throughout society, those holding jobs of higher quality will be more satisfied with their positions and less likely to innovate in pursuing success goals than those lacking such opportunities. As people participate in economic role

1. Rebellion, the final individual adaptation, is comparatively rare and refers to the rejection and replacement of both goals and means. Anomie theory invokes these adaptations to explain variation in crime rates by social structure (cf. Bernard 1987; Messner 1988; Messner and Rosenfeld 1997a). Individual-level tests of "strain theory" typically emphasize the discrepancy between perceived aspirations and expectations (Burton, Cullen, Evans and Dunaway 1994; Farnworth and Leiber 1989; Jensen 1995; Kornhauser 1978). Although the subjective strain approach may clarify the social psychological mechanisms connecting work with crime or identify new sources of strain (Agnew 1992), it de-emphasizes individuals' objective social positions (cf. Black 1976; Uggen and Kruttschnitt 1998).

behavior, they may shift from deviant to conforming role alternatives (or vice versa). Correctional policies, such as post-release work programs, attempt to induce such a shift by expanding the legitimate opportunities of releasees.

Herein lies the conceptual attractiveness of employment programs for deviant and disadvantaged populations, as well as their ultimate practical limitations. Anomie and differential opportunity (Cloward 1959; Cloward and Ohlin 1960) theories suggest that criminal motivation recedes when the gates of legitimate opportunity open. Due to an array of political and economic constraints, the best that policy efforts such as Mobilization for Youth (Marris and Rein 1973) and Supported Work (Manpower Demonstration Research Corporation 1980) could offer was the ritualist proposition of honest work for mean wages. When these programs met with questionable success (if not outright failure) in reducing crime, their proponents argued that the low quality program jobs failed to meaningfully alter workers' social positions (e.g., Orr et al. 1996:219). Although some offender subgroups may benefit from marginal work, social programs providing short-term, low-wage jobs generally fail to transform criminal innovators into working ritualists, much less successful conformists (Cook 1975; Piliavin and Gartner 1981). Whether providing jobs of higher quality might have a greater impact on crime remains an unanswered question.

Social Control and Reintegrative Theories

Unlike anomie and opportunity theories that posit job quality works by decreasing the motivation to commit crime, social control perspectives assume that criminal motivation is relatively constant across individuals and groups (Hirschi 1969:25–26; Kornhauser 1978:24, 47). Instead, the varying strength of formal and informal social controls is thought to regulate criminal and conforming behavior. Job quality remains important, however, insofar as it represents informal social control or a "stake in conformity" (Toby 1957). Those with higher quality jobs will be least likely to jeopardize their positions by engaging in crime; conversely, those with marginal jobs will have the least incentive to avoid crime. Therefore legitimate opportunities are only important insofar as they strengthen or weaken controls. Although some social control theories do not assign a causal role to job quality (Gottfredson and Hirschi 1990; Hirschi 1969), other control-based perspectives have conceptualized employment as an indicator of commitment (Sampson and Laub 1993) and social integration or interdependence (Braithwaite 1989). For example, job characteristics are important operational measures in tests of Sampson and Laub's age-graded social control theory, though it is not "employment *per se*" (1990:611) or "by itself" (Laub and Sampson 1993:304) but rather the adult social bonds associated with work that increase controls and decrease crime.

Reintegrative theories also suggest that quality employment is necessary to weave released offenders into the economic and social fabric. Braithwaite (1989) argues that employment indicates interdependence and, hence, responsiveness to informal social controls such as shaming. Moreover, those in jobs of poor quality have greater access to subcultures that supply illegitimate opportunities (1989:106; Crutchfield and Pitchford 1997). From this integrated perspective, job quality works both by increasing controls and by decreasing motivation. This analysis will not provide a critical test of anomie and social control interpretations of the relation between job quality and crime, although some of the evidence presented has implications for both of these theories. Instead, I will estimate economic and extra-economic job quality effects on crime and attempt to isolate these estimates from the processes of job search and job entry.

Job Quality

For motivational and control theories of crime, both economic and extra-economic aspects of employment are important in assessing job quality. Though he clearly emphasizes

economic inequality and monetary success, Merton identifies extra-economic aspects of social position, such as prestige, personal achievement, and social ascent (1957). This notion of a multifarious "status-set" (Merton 1957; 1995:58) distinguishes anomie theory from both structural Marxist theory and economic models of criminal choice. In pure form, the former theories view occupation solely in terms of class position and the latter either treat extra-economic job characteristics as psychic returns (Block and Heineke 1975) or discount them altogether (Becker 1968). Anomie theory, in contrast, posits occupation as a means to manifold cultural success goals, calling for a conception of job quality as an overall affective orientation toward a position. To the extent that one's job is economically rewarding and personally satisfying, one discerns progress toward success goals and is unlikely to adopt a deviant role adaptation. Extra-economic job characteristics also drive control-based interpretations of job quality effects on crime. For Sampson and Laub (1993:18), the social investment and role reciprocities associated with high-quality employment are at least as important as monetary rewards in creating and strengthening informal social controls.

This extra-economic conception of job quality is a central concern of the job satisfaction literature (e.g., Kalleberg 1977; Kalleberg and Loscocco 1983; Spector 1997). In particular, the 1977 Quality of Employment Survey (QES) developed a facet-free index constructed from global questions such as "How satisfied are you with your job?" and facet-specific evaluations of work dimensions such as comfort, challenge, rewards, relations with coworkers, resource adequacy, and promotions (Quinn and Staines 1979). Both the strongest pressures toward deviation and the weakest informal social controls should be felt at the bottom of the occupational hierarchy. Not surprisingly, inmates tend to be drawn from the social groups least satisfied with their work: African Americans are less satisfied than whites; younger workers are less satisfied than older workers; and unskilled and operative workers are less satisfied than craft and professional workers (Quinn and Staines 1979; U.S. Department of Justice 1993).[2]

Selection into Employment

Although job quality measures may operationalize legitimate opportunity among employed persons, many offenders are not employed. Individual self-selection into employment is thus an analytic problem that must be remedied to determine the effects of job quality on crime. More importantly, however, job entry is a general substantive process with specific implications for offenders. The labor economics literature on job search (cf. Holzer 1987; Mortensen 1986) and labor supply (cf. Mincer 1993) suggests that the unemployment rate, African-American race, age, poor health, and welfare transfer payments are positive predictors of unemployment duration (Petterson 1997; Polachek and Siebert 1993) and negative predictors of job entry (Blau 1992; DiPrete 1981; Stratton 1993). Education, work experience, marriage, and the number of dependents, on the other hand, have weak negative effects on the duration of joblessness and positive effects on the probability of employment.

Standard indicators of social position (age, race, sex, and the number of dependents), human capital (work experience and education), previous wages (see Blau 1992), and the external labor market (local unemployment rate) are appropriate for modeling job entry among ex-offenders. Some aspects of this basic model must be adapted, however, to conform to the offender population under study. Because few offenders are eligible for unemployment

2. Standard indicators of occupational standing, such as Duncan's (1961) Socioeconomic Index (SEI) and Treiman's (1977) Standard Scale Occupational Prestige Scores might also be used to gauge the effects of occupation on crime. Although prestige scales are consistent with Merton's conception of shared success standards, they measure neither the specific job characteristics that distinguish good jobs from poor ones nor workers' affective reactions to their jobs. As a prestige-anchored index (Featherman and Stevens 1982; Haug 1977), socioeconomic status is also a problematic indicator of job quality. Because SEI is determined by income and education level, and returns to education for deviant groups appear to be quite low (Witte and Reid 1980), SEI may further attenuate the effect of occupation on crime. The satisfaction-based job quality measure thus remains the best available indicator of job quality.

compensation, for example, welfare benefits and work program status are better indicators of support for unemployment. Illegitimate experience and opportunities (Cloward and Ohlin 1960; Cullen 1988; Steffensmeier 1983) are also likely to be negative predictors of job entry among offenders. Criminal history and substance use, for example, represent both a stigmatized social position constraining legitimate mobility (ex-offenders are less likely to be hired) and illicit means that may be mobilized (ex-offenders have the skills and associates to return to criminal activity). In sum, illegitimate opportunities, social position, human capital, expectations, post-release benefits, and the external labor market are likely to affect job entry among offenders.

An empirical test of the job quality hypothesis requires data offering variation in both legitimate opportunities and illicit behaviors. Although job quality varies greatly in the general population, for example, its effects may not be generalizable to a population of serious or chronic offenders. Moreover, prior tests of differential opportunity theory suggest that strain is criminogenic only when social deprivation is harsh and immediate (Bernard 1987; McCarthy and Hagan 1992; see also Burton et al. 1994). For the purposes of this paper, an offender sample is therefore preferred over a national probability sample. On the other hand, to obtain variation on the independent variable, detailed information on job quality and job entry must be known and at least some of the offenders must have obtained high quality jobs. The National Supported Work Demonstration Project (Piliavin and Gartner 1981) may offer the most detailed information collected to date on the employment history and criminal conduct of ex-offenders.

Data, Measures, and Hypotheses

Data

The data to be analyzed were collected as part of the National Supported Work Demonstration Project. Between 1975 and 1979, 2268 offenders participated in this job creation experiment (Manpower Demonstration Research Corporation 1980; Piliavin and Gartner 1981). In keeping with anomie and opportunity theories, Supported Work's "guiding principle" was to provide opportunities so that those "severely handicapped for employment may be able to join the labor force and do productive work, cease engaging in socially destructive or dependent behavior and become self-supporting members of society" (Manpower Demonstration Research Corporation 1980:1). To be eligible for the program, ex-offenders must have been recently incarcerated, currently unemployed, and employed for no more than three of the preceding six months. Following random assignment, half of those sampled were offered minimum wage employment in crews with 6–8 other offenders, while the remainder were assigned to control status. A total of 1448 ex-offenders were present for the three interviews at baseline, nine months, and eighteen months.[3] Assignment to the work program generally failed to reduce crime among offenders and most participants quickly left their assigned jobs (Piliavin and Gartner 1981), although some subgroups appeared to benefit from the experience (Uggen 1998). Proponents of Supported Work have argued that the jobs did not go far enough in shifting the social position of offenders. Both motivational theories and control theories suggest a more radical shift in occupational position is necessary to affect criminal offending.

Job quality information is available for 445 respondents who found employment outside

3. To determine weather sample attrition systematically biased parameter estimates in the program evaluation, Brown (1979) utilized a Heckman (1976) procedure to estimate and correct for sample selection. He found no biasing on outcomes such as self-reported arrest.

Table 1 • *Comparisons of Job Entrants with Non-Entrants on Selected Variables*

Characteristic	*Job Entrants (n = 488)*	*Non-Entrants (n = 1011)*
Age in years	25.2	25.3
	(6.5)	(5.8)
Percent male	94.6	94.5
Percent african american**	81.1	85.7
Percent white**	8.9	5.9
Percent hispanic	9.8	8.4
Education in years**	10.5	10.3
	(1.8)	(1.8)
Work experience in years**	1.5	1.1
	(2.4)	(1.8)
Prior earnings in dollars#	$54.70	$45.86
	($111.40)	($99.60)
Percent reporting poor health	3.7	4.0
Percent pressured to find work**	18.1	13.7
Experimental status***	29.2	58.0
Number of dependents	0.4	0.4
Welfare payments	24.13	23.00
	($74.30)	($59.20)
Number of prior arrests**	7.9	9.7
	(9.8)	(13.5)
Percent reporting prior crime for money	80.1	78.1
Frequency of alcohol use*	1.8	1.9
	(1.4)	(1.3)
Percent reporting prior heroin use#	42.7	47.0
Site unemployment rate#	7.7	7.9
Percent at Chicago site	13.8	12.2
Percent at Hartford site*	12.0	15.6
Percent at Jersey city site***	7.3	11.9
Percent at Newark site***	26.0	18.7
Percent at Oakland site	22.2	20.3
Percent at Philadelphia site	10.4	10.5

#p < .10 *p < .05 **p < .01 ***p < .001 (one-tailed *t*-tests of group differences)

of the Supported Work program by the nine-month interview. Table 1 compares employed ex-offenders with those who failed to find their own jobs. Among both job entrants and non-entrants, most respondents were young African-American males with less than a high school education and a history of serious drug use and criminal involvement. Those who found jobs tended to have fewer arrests and greater education and work experience than those who did not enter employment. Not surprisingly, those assigned to the experimental work condition were less likely to find jobs on their own. Although Supported Work participants represent a sample of criminal innovators, they also constitute a group of job seekers with multiple barriers to employment. In short, their legitimate opportunities are quite restricted.

Measures

Crime. The primary substantive dependent variable in this study is a dichotomous self-reported crime measure. Because the resumption of criminal behavior signals a rejection of legitimate opportunity and the likely revocation of parole, the most important recidivism outcome is the commission of any criminal offense following employment. The self-reported crimes included offenses such as burglary, theft, drug sales, assault, and prostitutional, though not drug use. (See the appendix for a list of all activities considered crimes for this analysis.) To determine whether job quality effects are specific to economic or utilitarian offenses, I also disaggregate the crime outcome into economic (money-producing) crimes and non-economic crimes. Reverse record checks suggest that self-reported crime data are reasonably reliable and valid by social scientific standards (Hindelang, Hirschi and Weis 1981), although discrepancies between self-report and official data have been observed for African-American males with official criminal records (see also Huizinga and Elliott 1986:324). Supported Work conducted an independent reverse record check in three sites, comparing official records of participants in Hartford, San Francisco, and Oakland with self-reported interview data. Schore, Maynard, and Piliavin (1979) found 45 percent underreporting of the incidence or frequency of arrest, but only 20 percent underreporting of the prevalence of arrest. That is, persons were more likely to underestimate the number of times they had been arrested than to err in reporting whether they had been arrested at all. Under these conditions, a dichotomous recidivism indicator is preferable to a frequency scale.[4]

One important measurement concern is whether response bias in self-reported offending may have been systematically related to job quality. Although it is impossible to verify the self-report data with certainty, there is some indirect evidence that this was not the case. Those assigned to Supported Work program jobs, rather than those who found their own jobs, would have had the greatest incentive to underreport their criminality. Although the original investigators were keenly aware of such potential biases and maintained a strict separation between local program staff and research staff, participants may still have feared that confessing criminal behavior might jeopardize their participation in the program (Manpower Demonstration Research Corporation 1980:45). Nevertheless, no evidence of underreporting differentials between workers and non-workers appeared in the reverse record checks on arrest data, or in income reported in Social Security records, welfare department files, and other official sources (Manpower Demonstration Research Corporation 1980:49). In fact, the initial investigation found no work program effect on self-reported crime and arrest. The overall null finding is evidence against systematic response bias, unless one assumes that there was actually significantly *greater* offending among workers than among non-workers. Although the possibility remains that those with the best non-program jobs may have underreported relative to those with the worst non-program jobs, there is no evidence that this was the case among Supported Work participants.

Job Quality. To indicate legitimate opportunity (as well as stake in conformity), job quality must tap the overall desirability of occupations rather than the respondents' individual characteristics of respondents (see also Jencks, Perman and Rainwater 1988). Here I am less interested in participants' subjective assessments of their jobs than in the behavioral consequences of a more or less satisfying position as judged by incumbents. I use transformed Quality of Employment Survey (QES) mean overall job satisfaction scores to measure job quality (Quinn

4. An event history analysis of the time until recidivism failure was also conducted for the substantive model presented below which treated employment as a time-varying covariate (cf. Uggen 1998). Because this paper is less concerned with the duration structure of recidivism than with isolating the effects of job quality on crime and distance, it presents the "static" selectivity models.

and Staines 1979). Because "satisfaction" typically refers to a subjective attitudinal indicator, however, I refer to the transformed QES aggregate measure as "job quality" rather than job satisfaction.

Anomie theory assumes that occupational achievement is a universal cultural success goal (Kornhauser 1978:146; Merton 1938), but are perceptions of job quality shared by offenders and non-offenders alike? The Quality of Employment Survey satisfaction measure was expressly developed to measure job satisfaction across all workers in a national sample. In fact, QES researchers critiqued previous job satisfaction measures developed on homogeneous populations as "too occupation-specific, 'white collarish,' or 'blue collarish.'" (Quinn and Staines 1979:206). Research on the prestige of low-status occupations (Guppy and Goyder 1984) and the Supported Work sample to be analyzed (Matsueda et al. 1992) also suggests that low-status workers and offenders are aware of such global assessments and generally share them. Matsueda and colleagues, for example, report a rank-order correlation of .73 between the NORC prestige scale (Featherman and Hauser 1975) and the prestige ratings of conventional occupations made by Supported Work participants (Matsueda et al. 1992: 759).

In order to link the Supported Work and the QES data, I first convert the third edition *Dictionary of Occupational Titles* code for each occupation into one of eight major classifications: professional and technical, managerial and administrative, sales, clerical, craft, operative, laborer, and service occupational groups. Within these groups I further categorize workers by skill level and industry, two important aspects of labor stratification that affect job quality (Crutchfield and Pitchford 1997).

Next, I assign occupations within each category the mean overall satisfaction rating for the 1977 QES shown in Table 2. A constant is added to each score and the result divided by 100 to obtain a positive metric for data transformations and to scale job quality to a magnitude similar to other variables in the analysis. The resulting job quality scores range from 0 (for operatives in nondurable goods manufacturing industries) to 1.08 (for skilled craft workers outside the manufacturing or construction industries), with a mean score of .57 (approximately equal to the .56 score for food service workers). As an interval measure, job quality provides a gradient that would be lost were occupational groups modeled as a set of dummy variables. The carpenters classified as construction industry craft workers, for example, have higher quality jobs than the "miscellaneous construction workers" classified as laborers. The number of offenders finding work in each category is shown in the right-hand column of Table 2. Most offenders held jobs as operatives, laborers, and service workers, though a sizable number found skilled craft, clerical, sales, and managerial or professional work.

Other Independent Variables. Because selection into a quality job is a non-random process, I model job entry as well as criminal behavior. Indicators of the respondent's health and perceived pressure to find work are included in the job entry equation. The participants' baseline social position is reflected in the job entry equation by their age, race, sex, number of dependents, prior earnings, and welfare receipt. The human capital measures of education and work history (both measured in years) should translate these status origins into legitimate opportunity. The external labor market conditions both the quality and availability of work and is measured by the unemployment rate at each program site. In specifying legitimate opportunity for the crime equations, I retain the traditional economic employment measures, job tenure, and wage to isolate the extraeconomic effects of job quality (Liker 1982). I also wish to adjust both job and work effects for individual differences in "criminal capital" (Grogger 1994; Matsueda and Heimer 1997) or criminality (Gottfredson and Hirschi 1990) at program entry. The number of times arrested and a self-reported indicator of prior economic crime are included as criminal history indicators, as are measures of prior alcohol and heroin use. Aside from the employment measures taken at the nine-month interview, all independent variables were measured at the initial baseline interview.

Table 2 • *Mean Overall Job Satisfaction Scores by Occupation and Industry*

Occupation	*Transformed Job Quality Score*	*Number of Job Entrants*
Professional and Technical Workers (n = 11)	.93	
Education	1.02	2
Health Services	1.05	0
Other Areas	.83	9
Managers, Administrators and Proprietors (n = 9)	.87	
Wage and Salaried	.77	9
Self-Employed	.85	0
Sales Workers	.90	21
Clerical Workers (n = 39)	.74	
Office Machine Operators, Receptionists, Secretaries, Stenographers, and Typists	.86	12
All Other Clerical	.69	27
Skilled Craft Workers (n = 66)	.79	
Manufacturing	.69	13
Construction	.67	32
Other Industries	1.08	21
Operatives (n = 131)	.26	
Durable Goods Manufacturing	.29	38
Nondurable Goods Manufacturing	0.00	32
Transportation	.33	17
Other Industries	.56	44
Laborers	.35	57
Service Workers (n = 111)	.66	
Food Services	.56	44
Other Services	.69	67

Source: Adapted from Quinn and Staines 1979:224–227.

Hypotheses

Both motivational and control theories, as well as the empirical generalizations of prior research, suggest the following hypotheses. First, *employment in high quality jobs will decrease the likelihood of criminal behavior net of prior criminality and substance use, other indicators of social position, and economic employment measures.* In the initial Supported Work demonstration, as well as other experimental jobs programs for releasees, those given jobs showed no reduction in crime relative to those in the control group (Manpower Demonstration Research Project; Piliavin and Gartner 1981; Uggen 1998). Thus, the provision of low-quality jobs has largely failed to convert innovators into ritualists or conformists. In the current analysis, however, I assess the entire range of employment obtained by participants. Persons with jobs of better quality should have greater legitimate opportunity, stronger informal controls, and, hence, lower rates of law violation. Those holding high-quality jobs will be less likely to reoffend; those lacking such opportunities are likely to continue criminal activity unabated. If statistically controlling for the effects of prior criminality renders job quality effects spurious, this would provide evidence against anomie theory and supporting theories emphasizing the stability of criminal propensity (e.g., Gottfredson and Hirschi 1990). If wage and tenure render the effects of job quality spurious, this would provide evidence in favor of economic conceptions of crime (Block and Heineke 1975; Grogger 1994).

Second, *human capital variables are expected to predict job entry, but will not affect crime net of job quality, since the job itself should mediate the effects of education and work experience.* Because Supported Work participants were screened on the basis of their poor work histories, the sample range on these variables is limited. In previous research, however, marginal increases in human capital have had no significant effect on either wages or work stability among ex-offenders (Witte and Reid 1980). Thus, the effect of job quality on crime will not be diminished by factors such as education and work history. In contrast, prior criminality should be negatively related to job entry and positively related to subsequent crime.

Third, *the negative effect of job quality on crime is expected to hold when statistical corrections for sample selectivity are introduced.* Because many participants did not obtain jobs, the effects of job quality may be biased due to unmeasured characteristics of the job-holding subsample. If the subsample somehow "creams" those least likely to engage in crime from the overall group—in terms of prior work history, education, or other characteristics—external validity is compromised. In this case, the analysis will not be generalizable to any population of interest. Moreover, internal validity will also be compromised, since the estimates will be biased even if generalizations are limited to the workers (Berk 1983). The greatest threat to a job quality model—the most compelling rival hypothesis—is that individuals self-selected themselves into jobs on the basis of some unmeasured factor. Thus, the relation between job quality and crime may be spurious due to a common cause, such as ambition, self-control, or initiative. Controlling for factors such as criminal history will only speak to competing hypotheses if the subsample of jobholders is representative of the total sample. A well-specified correction for sample selectivity, however, will allow generalization of the findings to the offender group as a whole.

Estimation

When a dependent variable is dichotomous, simple linear regressions will not yield estimates with desirable statistical properties. I therefore use a univariate probit procedure to estimate initial models of job entry and criminal behavior. Probit analysis assumes an underlying outcome variable, here defined as the propensity to enter employment or commit crime, that is observed as a dichotomous response (Maddala 1983).

The most direct way to measure job quality's effect on crime is to simply exclude participants who did not obtain jobs from the analysis. Because employment information is only available for working members of the sample, however, there is no assurance that job quality effects found among employed participants would hold among those who failed to find jobs. Moreover, if jobholders are characterized by some unmeasured trait associated with both work and crime, such as ambition or self-control, the estimated job quality effects among this subsample could be seriously biased. I therefore estimate two-equation probit models that allow for interdependence in the processes leading to job entry and crime. The first equation predicts the likelihood of criminal behavior and the second equation models the selection process predicting entry into a job, and hence entry into the subsample to be analyzed. If the unmeasured factors driving criminal behavior also impel offenders into (or out of) the labor force, the error terms for the work and the crime equations will be correlated. For example, if the jobholders are less impulsive and impulsivity is positively associated with crime, this would be reflected in a negative correlation (ρ) between the error terms of the job entry and crime equations. Introducing this correlation into the crime equation corrects for selectivity by adjusting the estimated job quality effects (as well as the effects of the other independent variables) among jobholders for the unmeasured factors affecting the propensity to enter employment.[5]

The bivariate probit selectivity model is a generalization of the Tobit model, in which Y_{2i},

5. For a general introduction to selectivity models, see Winship and Mare (1992). For applications and extensions of the bivariate probit selection model, see van de Ven and Van Praag (1981), Dubin and Rivers (1989), Hardy (1989), and Liao (1995).

representing job entry, determines whether the effect of job quality on Y_{1i} (criminal behavior) is observed (Liao 1995; van de Ven and Van Praag 1981; Winship and Mare 1992). This model takes the form:

1. Criminal Behavior: $Y^*_{1i} = \beta_1 \mathbf{X}_{1i} + \varepsilon_{1i}$
   $Y_{1i} = 1$ if $Y^*_{1i} > 0$; 0 otherwise
2. Job Entry: $Y^*2_i = \beta_2 \mathbf{X}_{2i} + \varepsilon_{2i}$
   $Y_{2i} = 1$ if $Y^*2_i > 0$; 0 otherwise

where, for the ith observation, Y^*_1 represents the propensity to commit crime (an unobserved continuous latent variable), Y_1 represents observed self-reported criminal behavior, Y^*_2 is the latent propensity to work, Y_2 is a dichotomous job entry indicator; $\mathbf{X}_1$ is a vector of values on the independent variables (including job quality) that are observed when $Y_2 = 1$, $\mathbf{X}_2$ are values on the independent variables in the job entry equation, β_1 and β_2 are vectors of coefficients, and ε_1 and ε_2 are error terms with correlation $(\varepsilon_1, \varepsilon_2) = \rho$ (see Liao 1995:91). This correlation represents the interrelatedness of the two processes, or the association between the probabilities of job entry and crime net of the effects of the independent variables in the model.

Results

Results of the Selection or Job Entry Model

The first step in the analysis is to estimate the job entry selection equation. As indicated in the discussion of Table 1, the employed participants reported greater education and experience and fewer arrests than the balance of the sample. The probit estimates in Table 3 confirm that human capital accumulation appeared to aid respondents in entering the paid work force. As predicted by economic models of labor supply, those with educational credentials and labor force experience, and those who reported feeling pressure to find work were most likely to gain employment.

African Americans were less likely to find jobs than non-African Americans. As expected, long arrest records reduced the probability of employment, although those who had committed crimes for money were significantly more likely to be working, net of the other covariates. The comparatively large effect of experimental status is an artifact of the program design: members of the control group were not offered supported employment and were thus much more likely to find their own jobs.[6] In sum, the job entry equation shows that those with higher levels of human capital and lower levels of criminal history and substance use at time *1* were more likely to enter work at time *2* than those who failed to find their own jobs. After discussing the bivariate relation between job quality and crime below, I estimate the effects of job quality at time *2* on crime at time *3* after adjusting for this job entry selection process.

The Bivariate Relation between Job Quality and Crime. Figure 1 shows the percentage of those committing a crime by occupational group. The bivariate relation is quite consistent with theoretical expectations. Operatives, for example, are coded as low in job quality and are most likely to report crime (31–41% depending on industry). Skilled craft workers and professional and technical workers, on the other hand, have the highest job quality scores and the lowest proportion of recidivists. Though sales workers received very high job quality scores, they are

6. I also estimated each model with product terms for the interaction of experimental status and each of the covariates. Since the interaction terms were not significantly different from zero, I aggregate controls and experimentals in the analyses. When the analysis is limited to those in the control group, estimates are comparable in magnitude to those reported here, though sample size and the statistical power to detect effects are reduced.

Table 3 • *Univariate Probit Estimation of Job Entry*

Variable	*Coefficient*	*Standard Error*
Intercept	0.179	0.297
Age in years	−0.008	0.007
Male	0.104	0.166
African american	−0.304***	0.100
High school graduate	0.168*	0.086
Work experience in years	0.062***	0.021
Prior earnings (in hundreds of dollars)	0.027	0.036
Poor health	0.070	0.202
Pressured to find work	0.212**	0.106
Experimental status (v. control status)	−0.780***	0.077
Number of dependents	0.056#	0.041
Welfare payments (in hundreds of dollars)	−0.002	0.063
Number of prior arrests	−0.007**	0.003
Prior crime for money	0.261***	0.100
Frequency of alcohol use	−0.030	0.029
Prior heroin use	−0.095	0.082
Site unemployment rate	−0.032#	0.020
Number of cases: 1334		
Log-Likelihood: −812.16		
Chi-Squared (16): 143.18***		

#p < .10 *p < .05 **p < .01 ***p < .001 (one-tailed tests)

among the most likely to report crime. Perhaps the relatively high satisfaction level among sales workers may be less applicable to Supported Work participants than to QES respondents. Although Quinn and Staines gathered QES satisfaction data from a probability sample of those currently working in each industry, employed offenders had been working in these positions for less than nine months. If there is a high rate of early attrition out of sales positions—if many of the new hires fail to earn a sufficient commission, for example—these jobs may be satisfying only to sales "survivors." Overall, however, Figure 1 suggests a negative relation between job quality and crime. About 10 percent of the working offenders held jobs in skilled craft, skilled clerical, and professional/technical categories and fewer than 10 percent of these workers reported criminal activity.

Results of the Crime Model with and without Selection

Table 4 reports estimates for uncorrected models predicting the probability of any new crime among employed respondents and two-equation models corrected for selectivity. Probit coefficients can be interpreted as the effects of a unit change in the independent variable on the cumulative normal probability of the dependent variable, or as effects on z-scores. Model **A** fits only a constant, indicating that the probability of criminal behavior relative to desis-

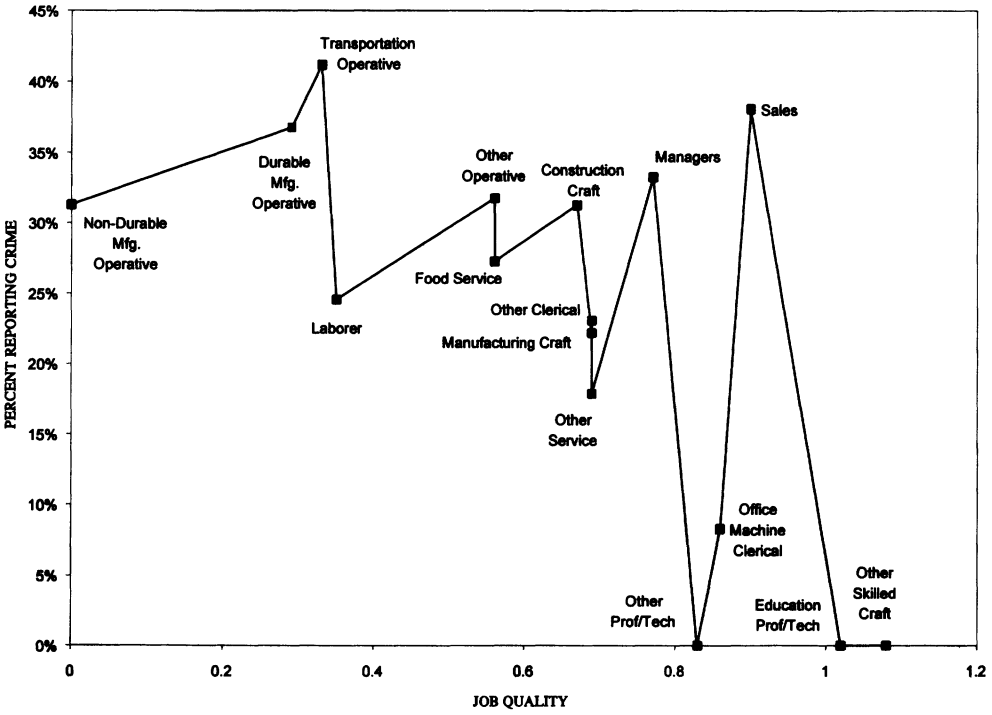

Figure 1 • ***Job Quality and Crime***

tance (or non-criminal behavior) is .26, which is the probability associated with a *Z*-score of −.65 (see Hardy 1989). Model **B** adds job characteristics, prior criminality and substance use, and site indicators. This equation estimates the effects of job quality net of the amount and severity of prior deviance and local labor market conditions. Job quality, tenure, and pay are all negatively related to crime, although only the job quality indicator meets usual significance criteria. As expected prior arrests, prior economic crime, and prior alcohol use have a strong positive effect on criminal behavior. Model **C** adds factors related to job entry, such as human capital (education and experience), social position (age, race, sex, and dependents), and welfare receipt. Although these factors have little individual predictive power in the crime equation, the overall improvement in fit over Model **B** is statistically significant.

The bivariate probit selection models **A′**, **B′**, and **C′** test whether unmeasured characteristics that increased the probability of employment decreased (or increased) the likelihood of criminal behavior. If unmeasured criminal propensity was lower among workers than among non-workers, for example, this would be manifest in a negative correlation (ρ) between the error terms of the job entry and crime equations. Alternatively, if workers had *higher* levels of unmeasured criminal propensity, then ρ would be positive. Model **A′** gauges the effects of unmeasured traits that increase the likelihood of job entry on the probability of criminal behavior. Here, the value of ρ is large in magnitude, positive in direction, and statistically significant. Therefore, unmeasured factors associated with job entry *increased* the probability of criminal behavior.[7] This result is perhaps more consistent with a Mertonian account of offend-

7. Because this result is observed among those assigned to both the experimental condition and the control group, the positive correlation does not appear to be an artifact produced by the availability of Supported Work program jobs.

Table 4 • *Probit Estimation of Crime with and without Selectivity Correction*

	Uncorrected Univariate Probit			*Bivariate Probit with Selectivity*		
Variable	*A*	*B*	*C*	*A′*	*B′*	*C′*
Intercept	−.650***	−.969**	−.193	−1.151***	−1.188**	−0.031
	(.064)	(.457)	(.933)	(.158)	(.588)	(1.478)
Job quality		−.813***	−.829***		−.928***	−.823**
		(.291)	(.316)		(.324)	(.363)
Job tenure (months)		−.057*	−.050#		−.054#	−.049
		(.030)	(.033)		(.357)	(.038)
Job pay ($100)		−.037	−.041		−.039	−.041
		(.036)	(.038)		(.039)	(.041)
Age (years)			−.031*			−.031#
			(.018)			(.022)
Male (v. female)			.411			.397
			(.388)			(.455)
White (v. Af.-Am.)			.147			.115
			(.266)			(.381)
Hispanic (v. Af.-Am.)			−.520*			−.544#
			(.310)			(.366)
Education (years)			−.029			−.032
			(.051)			(.056)
Experience (years)			−.020			−.026
			(.057)			(.081)
Prior earnings ($100)			−.036			−.039
			(.079)			(.091)
Poor health			−.286			−.029
			(.408)			(.398)
Experimental (v. Ctl)			.128			.205
			(.172)			(.614)
Any dependents			.039			.027
			(.200)			(.235)

ers as frustrated strivers than with a social control view of offenders as unattached or uncommitted. The constant term in the corrected equation is larger in absolute magnitude than in the uncorrected model: the probability of crime net of unmeasured factors related to job entry was only approximately .125, the probability associated with a Z-value of -1.15.

The addition of job indicators and prior crime and substance use variables to the equation in Model **B′** dramatically reduces the magnitude and significance of ρ. This reduction suggests that unmeasured traits related to both job entry and crime are mediated in large part by job quality and prior deviance. Moreover, the estimates for the latter characteristics are quite similar in size and significance to the uncorrected coefficients in Model **B**. Finally, the estimate for ρ in model **C′** is further reduced and changes sign with the addition of human capital and social position measures. Aside from the low self-control hypothesis of stability in offending, perhaps the most compelling rival hypothesis to the job quality argument concerns the human capital measures of education and work history.

Desistance may result not from job quality but from pre-existing assets that predict the

Table 4 (*continued*)

	Uncorrected Univariate Probit			*Bivariate Probit with Selectivity*		
Variable	*A*	*B*	*C*	*A′*	*B′*	*C′*
Any welfare			.047			.047
			(.248)			(.266)
Prior arrests (#)		.007	.010#		.005	.010#
		(.007)	(.007)		(.007)	(.008)
Prior money crime		1.045***	1.100***		1.161***	1.069**
		(.280)	(.321)		(.376)	(.481)
Prior alcohol use		.174***	.153**		.154**	.155**
		(.057)	(.061)		(.062)	(.064)
Prior heroin use		.245#	.374**		.293*	.381**
		(.154)	(.167)		(.161)	(.190)
Chicago (v. San Fran)		−.468#	−.621*		−.488#	−.615#
		(.317)	(.358)		(.350)	(.401)
Hartford site		−.795**	−.908**		−.819**	−.899**
		(.335)	(.368)		(.352)	(.410)
Jersey city site		−.310	−.427		−.307	−.407
		(.362)	(.398)		(.384)	(.481)
Newark site		−.125	−.307		−.218	−.299
		(.266)	(.301)		(.285)	(.349)
Oakland site		−.177	−.359		−.270	−.355
		(.272)	(.291)		(.291)	(.323)
Philadelphia site		−.177	−.433		−.298	−.429
		(.356)	(.401)		(.393)	(.464)
ρ Covariance (ε_1, ε_2)				.546***	.273	−.135
				(.182)	(.256)	(.992)
Number of cases	442	419	397	442,1334	419,1334	397,1334
Log-Likelihood	−252.32	−202.73	−185.49	−966.73	−930.92	−926.06
Fit improvement χ^2df		99.2_{13}***	34.5_{11}***		71.6_{13}***	9.7_{11}

#p < .10 *p < .05 **p < .01 ***p < .001 (one-tailed tests)

occupational level that one attains. Yet the job quality effects remain despite the inclusion of human capital variables in Models **C** and **C′**. Moreover, education and work experience have comparatively little predictive power in these models. Though these factors are undeniably related to job entry, they have no independent effect on criminal behavior. Thus job quality effects persist in models that include diverse background and employment variables. As anticipated, illegitimate opportunity in the form of prior criminal behavior and drug use increases the likelihood of crime. The results in Table 4 show little evidence that the single equation estimates are biased by selectivity and the correlation between the disturbance terms of the job entry and crime equations (ρ) in Model **C′** is not significantly different from zero at −0.135.[8]

8. To determine whether the exclusion of participants due to missing data biased the analysis, I reestimated each model using mean imputation of missing data (analysis not shown). For each model, the results are nearly identical to those presented here. As a check for robustness, I also reestimated all models using job data from the 18-month interview (Time 3) and crime data for a 27-month interview (Time 4). Again, the results are substantively similar to those presented in Tables three and four.

Table 5 • ***Probit Estimation of Economic and Non-Economic Crime***

	Uncorrected Univariate Probit			*Bivariate Probit with Selectivity*		
Variable	*A*	*B*	*C*	*A*	*B*	*C*
Intercept	−.751***	−1.446**	−1.193	−1.230***	−.494	1.548#
	(.066)	(.478)	(.961)	(.080)	(.542)	(1.15)
Job quality		−.570*	−.519*		−1.355***	−1.609***
		(.293)	(.311)		(.372)	(.422)
Job tenure (months)		−.046#	−.039		−.035	−.012
		(.031)	(.033)		(.038)	(.041)
Job pay ($100)		−.027	−.023		−.098*	−.101*
		(.037)	(.038)		(.047)	(.050)
Age (years)			−.033*			−.042*
			(.018)			(.024)
Male (v. female)			.336			
			(.382)			
White (v. Af.-Am.)			.272			−.828*
			(.265)			(.411)
Hispanic (v. Af.-Am.)			−.370			−.631#
			(.314)			(.418)
Education (years)			−.023			−.068
			(.052)			(.065)
Experience (years)			−.022			.011
			(.059)			(.077)
Prior earnings ($100)			−.028			−.089
			(.080)			(.121)
Poor health			−.564			−.441
			(.442)			(.623)
Experimental (v. Ctl)			− .006			.300#
			(.177)			(.222)
Any dependents			.047			.069
			(.083)			(.111)

Interpreting Job Quality Effects. Sample selection models are certainly no panacea for selectivity bias; in some cases they actually worsen estimates by introducing additional distortion (Stolzenberg and Relles 1990; Winship and Mare 1992). For present purposes, however, the bivariate probit model demonstrates the robustness of the job quality effects: both the corrected and the uncorrected equations yielded maximum likelihood estimates of approximately −0.82. What is the practical interpretation of this effect? Unlike an ordinary least squares regression coefficient, this probit estimate cannot be interpreted as the uniform effect of a unit change in job quality on the probability of criminal behavior. Nevertheless, by fixing each of the independent variables at some point the effects of a unit change in job quality on the probability of crime may be estimated. When all of the independent variables are set to their means in the bivariate selection model **C'**, the probability of criminal behavior is 0.28. The mean value of job quality among these offenders is .57, which is approximately equal to the job quality score for food service workers (.56). A shift from food service work to skilled craft work in other industries (with a job quality score of 1.08) decreases the probability of

Table 5 (*continued*)

	Uncorrected Univariate Probit			*Bivariate Probit with Selectivity*		
Variable	*A*	*B*	*C*	*A*	*B*	*C*
Any welfare			.077			.157
			(.116)			(.134)
Prior arrests (#)		.007	.010#		.013*	.016*
		(.007)	(.007)		(.008)	(.008)
Prior money crime		1.114***	1.062***		.399#	.474
		(.307)	(.332)		(.308)	(.374)
Prior alcohol use		.169**	.162**		.132*	.115#
		(.058)	(.062)		(.073)	(.082)
Prior heroin use		.106	.217		.358*	.597**
		(.159)	(.172)		(.195)	(.224)
Chicago (v. San Fran)		−.309	−.348		−.411	−.755
		(.325)	(.367)		(.387)	(.436)
Hartford site		−.759*	−.778*		−.780*	−1.160**
		(.358)	(.389)		(.434)	(.484)
Jersey city site		−.140	−.189		−.009	−.375
		(.372)	(.408)		(.401)	(.464)
Newark site		−.025	−.056		−.336	−.809*
		(.272)	(.307)		(.325)	(.379)
Oakland site		−.078	−.058		−.506#	−.784*
		(.277)	(.295)		(.336)	(.369)
Philadelphia site		−.021	−.087		.127	−.441*
		(.365)	(.414)		(.393)	(.465)
Number of cases	442	419	397	439	416	394
Log-Likelihood	−236.34	−193.05	−178.40	−151.51	−121.89	−109.48
Fit improvement χ^2df		86.58_{13}***	29.30_{11}**		59.24_{13}***	24.82_{10}**

#p < .10 *p < .05 **p < .01 ***p < .001 (one-tailed tests)

criminal behavior by approximately 11 percent when other independent variables are held constant at their means. A shift from food service work to a less satisfying job as a manufacturing operative in nondurable goods, in contrast, increases the probability of criminal behavior by approximately 14 percent when all other independent variables are set to their mean values.

In sum, in both corrected and uncorrected equations, jobs of high quality reduce the likelihood of recidivism among released offenders. The initial univariate model therefore appears more tenable and the selection equations support the validity of the subsample analyzed.

Disaggregated Results for Economic and Non-Economic Crime

The preceding analysis shows the effects of job quality on a general recidivism outcome. Yet job effects may vary with the severity, type, or frequency of offending. Although not all of these distinctions can be examined in this paper, at least one important specification can be

modeled with these data. If the mechanism linking job quality and crime is pecuniary, we should expect larger job quality effects in predicting economic than non-economic criminal activities. Alternatively, if job quality affects non-economic crime as well as crimes for money, this would provide some evidence that quality employment reduces criminal motivation or increases social controls more generally. Economic crimes include such offenses as burglary, robbery, drug sales, fraud, and prostitution while non-economic crimes include behaviors such as assault and arson (a complete list of crimes included in these classifications is contained in the appendix). Whereas approximately 26 percent of the sample reported at least one of the enumerated offenses in the post-employment period, about 23 percent and 11 percent reported economic and non-economic criminal activities respectively. Because of the distribution of these variables and the relatively small sample size, the full selectivity models cannot be estimated for each offense subtype. Nevertheless, the "any crime" analysis in Table 4 suggests that the uncorrected estimates are unlikely to be significantly biased by selectivity. I therefore present uncorrected models for the two subtypes below.

Table 5 shows estimates from separate (univariate) probit equations predicting economic and non-economic crime. Equation **C** in the economic crime model shows a job quality effect of $-.52$ which corresponds to a marginal effect of approximately .13. A shift from food service work to skilled craft work in other industries therefore decreases the probability of economic criminal behavior by approximately 7 percent when other independent variables are held constant at their means. Age, prior economic crime, alcohol use, and residence in the Hartford site (relative to the San Francisco site) are also significant predictors of crimes for money. When each variable is set to its mean, those with prior money crimes are about 27 percent more likely to commit a subsequent economic offense.

Somewhat surprisingly, both the job quality effect and the wage effect appear to be stronger for non-economic than for economic crimes. This result is due in large part to a high prevalence of economic offenses among workers in high-quality sales and management jobs (figures available from the author). Therefore job quality effects on crime are not limited to economic or utilitarian criminal activity and the mechanism linking job quality and crime is not exclusively economic. Race, prior heroin use, and several site variables also emerge as significant in the non-economic crime models. (The sex coefficient was omitted from the non-economic crime equations since no female offenders committed these offenses.)

Summary and Conclusion

Programs attempting to expand offenders' legitimate opportunities have generally provided jobs of low quality that fail to significantly alter the participants' social position. Therefore, proponents of anomie, opportunity, and adult social bond theories have argued that such programs are not an adequate test of the theories. In the absence of randomized assignment to jobs of varying quality, however, self-selection processes complicate efforts to separate job quality effects from the effects of pre-existing worker assets.

This research has modeled and partially corrected for these selection processes using statistical procedures for sample selectivity. The results show a strong and robust job quality effect on economic and non-economic criminal behavior. High-quality jobs decrease the likelihood of criminal behavior net of prior criminality and substance use, other indicators of social position, and alternative employment measures. The negative effect of job quality observed in standard uncorrected models holds equally well in models that correct for self-selection into the job-holding subsample. Moreover, job entry and job quality, in large part, mediate the effects of human capital and demographic factors on crime.

Nevertheless, the evidence is not so overwhelming as to inspire certainty in the effects of job quality and a more refined investigation is necessary to answer several remaining questions. Since bias in self-report offending may be systematically related to job quality, investiga-

tions examining official offending measures are needed to replicate these results. Future research should seek to identify the distinctive features of high-quality jobs and assess their impact on the frequency or incidence of crime as well as its prevalence. At the individual level of analysis, more detailed information on the type and timing of both work and criminal activity are needed to understand the social-psychological mechanisms connecting job quality and crime. Do jobs of high quality diminish criminal motivation, increase social controls, or both?

The positive relation observed between job entry and crime offers some support for a Mertonian view of the offenders as frustrated strivers. Alternatively, the strong job quality effects on non-economic crime seem to favor a social control or reintegrative interpretation. According to these theories, good jobs should provide a stake in conformity that inhibits assault and burglary alike. At the societal level of analysis, cross-national comparative work across varying historical periods and labor market conditions is necessary to examine the effects of labor market structure upon *rates* of criminal behavior (Messner and Rosenfeld 1997b). Yet the practical limitations of the data analyzed in this paper should not obscure the finding that an extra-economic measure of job quality predicts crime among high-risk offenders and that sociological theories of anomie and informal social control provide sound frameworks for interpreting these effects.

Aside from its scientific implications, this research raises potentially important policy questions. Assuming for the moment that the selectivity problem has been completely overcome by statistical correction and that assigning offenders to high quality jobs would indeed reduce their likelihood of recidivism, what is to be done with this information? How can policymakers justify allocating the best jobs (or the training required to access them) to the least deserving members of a large and needy underclass population? Even if cost-benefit analysis revealed a net economic gain to such a program, it would be difficult to justify on equity grounds without extending such opportunities to other groups. Perhaps work programs for ex-offenders would be most effective if they were embedded in a comprehensive, and correspondingly costly, national employment and training strategy (Currie 1985; Levitan and Gallo 1988; Skocpol 1997).

Without regard to the desirability of such large-scale efforts, it has become increasingly clear that marginal increases in human capital do little to reduce crime among offenders in the absence of quality employment. Work in an occupation that incumbents judge to be satisfying, however, may give rise to a reasoned reassessment of one's alternatives. This paper suggests that entry into high quality jobs may increase social controls, decrease the motivation to commit crime, and thereby alter the relative attractiveness of legal and illegal activities.

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Appendix • Listing of Explanatory and Dependent Variables

Explanatory Variables

Job Quality	A continuous variable coded to Quinn and Staines 1977 job satisfaction score for the occupation/industry category of respondent's longest non-Supported Work job (see Table 2).
Job Tenure	A continuous variable equal to the duration of the respondent's longest non-Supported Work job in months.
Job Wage	A continuous variable equal to the monthly wage of respondent's longest non-Supported Work job.
Education (Years)	A continuous variable equal to the years of schooling completed.
High School Graduate	A dichotomous variable coded 1 for high school degree or above.
Experience	A continuous variable for the length in years of the longest job ever held by respondent.
Prior Earnings	A continuous variable equal to the average monthly earnings in the twelve months prior to entering Supported Work.
Poor Health	A dichotomous variable coded 1 if respondent reported poor health.
Pressure to Work	A dichotomous variable coded 1 if respondent was pressured to find work.
Experimental Status	A dummy variable equal to 1 if respondent was assigned to the experimental group and 0 if assigned to the control group.
Number Dependents	A continuous variable equal to the number of dependents reported.
Any Dependents	A dichotomous variable coded 1 if dependents are greater than 0.
Welfare Payments	A continuous variable for the amount of general assistance, AFDC or other welfare respondent received in the month prior to program entry.
Prior Arrests	A continuous variable equal to the number of times the respondent had been arrested prior to program entry.
Prior Money Crime	A dummy variable equal to 1 if respondent reported ever committing a crime for money.
Prior Heroin Use	A dichotomous variable equal to one if respondent reported having ever used heroin.
Prior Alcohol Use	An ordinal variable equal to 4 if respondent reported drinking every day, 3 if a few times per week, 2 if a few times per month, 1 if less often, and 0 if respondent reported never drinking.
Unemployment Rate	A continuous variable equal to the unemployment rate for each site.

Dependent Variables

Offenses Constituting Economic Crime Measure:
Numbers, other gambling
Burglary or breaking and entering
Boosting, shoplifting, stealing from cars or trucks
Selling marijuana or other drugs
Robbery, holdups or stick-ups
Selling or fencing stolen goods
Mugging or snatching purses
Cashing or forging stolen checks or credit cards
Con games, fraud, swindles or jostling
Pimping or prostitution
Illegal sales of alcohol, selling booze

Offenses Constituting Non-Economic Crime Measure:
Burning a car or truck
Fighting with a gun or knife
Fist fighting
Destroying or damaging property
Concealing a crime
Homicide
Assault, attacking a person
Carrying a concealed weapon
Rape
Burning a building
Arson

Offenses Constituting Any Crime Measure:
All of the above economic and non-economic offenses