WORK AS A TURNING POINT IN THE LIFE COURSE OF CRIMINALS: A DURATION MODEL OF AGE, EMPLOYMENT, AND RECIDIVISM

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Sociologists have increasingly emphasized "turning points" in explaining behavioral change over the life course. Is work a turning point in the life course of criminal offenders? If criminals are provided with jobs, are they likely to stop committing crimes? Prior research is inconclusive because work effects have been biased by selectivity and obscured by the interaction of age and employment. This study yields more refined estimates by specifying event history models to analyze assignment to, eligibility for, and current participation in a national work experiment for criminal offenders. Age is found to interact with employment to affect the rate of self-reported recidivism: Those aged 27 or older are less likely to report crime and arrest when provided with marginal employment opportunities than when such opportunities are not provided. Among young participants, those in their teens and early twenties, the experimental job treatment had little effect on crime. Work thus appears to be a turning point for older, but not younger, offenders.

LIFE-COURSE transitions, such as entry into marriage or employment, may be "turning points" (Elder 1985) in the lives of criminal offenders (Sampson and Laub 1993; Warr 1998). Because researchers typically cannot assign people to these statuses, however, it is difficult to determine whether such transitions are causes or correlates of changes in offending. In fact, early or "precocious" transition to these adult roles appears to worsen problem behavior (Bachman and Schulenberg 1993), suggesting that work and marriage have age-specific effects rather than uniform effects on crime. Whether work is a turning point for offenders may also depend on other dimensions of time, such as the duration since release from prison. This investigation takes up three questions about causality, age-dependence, and the timing of criminal recidivism. Does work cause a reduction in crime, or is the association spurious? Do work effects depend on the life-course stage of offenders, or are they uniform across age groups? When are released offenders at greatest risk of recidivism?

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WORK, CRIME, AND CAUSALITY

Work is important for theories of crime because workers are likely to experience close and frequent contact with conventional others (Warr 1998) and because the informal social controls of the workplace encourage conformity (Sampson and Laub 1993). In particular, life-course theories suggest that employment is critical to explaining desistance or cessation from crime (Shover 1996). Because employment is so clearly manipulable, work effects are amenable to both scientific study and policy analysis. In fact, the provision of employment is one of few policy instruments at the state's disposal (Wilson 1975:59). It is much simpler to place parolees into varying work statuses, for example, than to randomly assign them to different spouses, neighborhoods, or peer associates.

For all of these reasons, studies of work effects on crime are prominent in the sociological literature (Berk, Lenihan, and Rossi 1980; Crutchfield and Pitchford 1997; Hagan 1991; Sampson and Laub 1990; Thornberry and Christenson 1984). This research has established several basic empirical findings. First, the meaning of work and its implications for crime appear to change at some point during the transition to adulthood: The bivariate association between employment and law violation is generally positive for juveniles (Bachman and Schulenberg 1993; Gottfredson and Hirschi 1990:138; Mihalic and Elliott 1997; Ploeger 1997; Wright, Cullen, and Williams 1997), but negative for adults (Farrington et al. 1986; Hagan and McCarthy 1997; Sampson and Laub 1990). Second, a high incidence of short jobless spells, rather than long-term unemployment, characterizes the work histories of criminal offenders (Cook 1975; Sullivan 1989). Although their earnings and employment levels are far below those of the general population, most inmates have at some point penetrated the paid labor force (U.S. Department of Justice 1993). Given this instability, researchers must address both changes in work statuses over time and the temporal sequencing of work and crime. Third, despite solid observational evidence showing strong effects of work on adult offending (Sampson and Laub 1990), most experimental efforts to reduce crime through employment have had null or disappointingly small treatment effects (Piliavin and Gartner 1981; Sherman et al. 1998). Although crime reduction among older offenders has sometimes been observed in financial aid programs (Lenihan 1977) or employment projects (Gartner and Piliavin 1988), adolescents generally derive far smaller benefits from these programs (Orr et al. 1996).

Taken together, existing theory and research point to a complex and perhaps conditional relation between work and criminal behavior. Whether this relation is causal remains unresolved, as does the direction of causality. Employment has been conceptualized as a cause of crime and conformity (Hagan 1993), a spurious correlate (Gottfredson and Hirschi 1990:139), and as a proxy indicator of a latent causal factor (Sampson and Laub 1990). These differing interpretations are partly due to the coincidence of employment with age and differing interpretations of the relationship between age and crime.

AGE-INVARIANT AND LIFE-COURSE CONCEPTIONS OF CRIMINAL BEHAVIOR

For most offenses in most societies, crime rates rise in the early teen years, peak during the mid- to late teens, and decline thereafter (Hirschi and Gottfredson 1983). Although the general contours of this "age-crime curve" are well established, the curve's interpretation has been hotly debated. Those in the life-course camp argue that the causes of crime are age-graded and variable over the life cycle (Greenberg 1985; Matza 1964; Sampson and Laub 1993; Shover 1996; Steffensmeier et al. 1989). From this perspective, the transition to employment partially explains the relationship between age and crime (Greenberg 1977; Grogger 1998; Pezzin 1995), and work effects will vary with the age of the worker. Shover (1996), for example, suggests that employment reinforces an emergent noncriminal identity among older offenders but not among younger offenders. These views imply that work is causally related to crime and that age interacts with work in its effects on crime.

In response to life-course arguments, Hirschi and Gottfredson (1983) argue that age affects crime directly and "does not interact with any known causal variables in its effect on crime" (p. 580). This "non-interaction hypothesis" (Tittle and Grasmick 1998: 337) posits that age does not interact with work or anything else in affecting crime.

With regard to employment, Gottfredson and Hirschi (1990) maintain that "employment does not explain, or help to explain, the reduction in crime with age" (p. 139). From this perspective, the association between work and crime is spurious due to a common cause, such as impulsiveness or opportunity. In contrast to life-course approaches that imply causality and interaction in the relation between age, work, and crime, Gottfredson and Hirschi's argument implies spuriousness and noninteraction: Work is not causally related to crime and age does not interact with work or other variables.

This debate has important implications for science and policy. The age-invariance argument suggests a focus on primary prevention (Gottfredson and Hirschi 1990: 269). By allocating resources to younger "at-risk" populations, society reaps the multiplier effects of a lifetime of industrious conformity. The life-course argument, in contrast, suggests age-graded correctional programs to reduce the social harm associated with recidivism. From the latter perspective, employment programs may be an important turning point in the criminal trajectories of older offenders.

The empirical standing of this debate is inconclusive. Life-course studies that report a negative correlation between work and crime (Sampson and Laub 1990) generally have strings attached—the association between work and crime is conditional on some characteristic of the jobs or the workers. In their age-graded theory of informal social control, Sampson and Laub (1990: 611) argue that it is not "employment *per se*," or "employment by itself" (Laub and Sampson 1993:304) that reduces crime, but rather the stability and commitment associated with work. Therefore, Sampson and Laub (1990) include measures such as work habits in their job stability index.

Those in the invariance camp, however, find such evidence unconvincing. If employment effects are conditional on good work habits, the putative "job effects" are tainted by "person effects" or preexisting worker characteristics. Presumably, people with good work habits would be less likely to commit crime in the first place than people with poor work habits, even in the absence of employment. To arbitrate between these positions, some mechanism to sort "job effects" from "person effects" is needed. Two such mechanisms are random assignment (Campbell and Stanley 1963; Cox 1958) and statistical corrections for sample selectivity (Winship and Mare 1992).

The relative superiority of experimental over nonexperimental methods is a matter of some debate in the program evaluation literature (Fraker and Maynard 1987; Heckman 1992; Heckman and Hotz 1989; Lalonde 1986). Unlike nonexperimental methods, however, a properly conducted experiment does not reduce the precision of estimates (Manski 1995) or require additional statistical assumptions (Heckman and Hotz 1989:862). Experiments on the effect of employment on crime convert uncontrolled variation in personal criminal propensities into random variation, allowing researchers to make probabilistic statements about whether job effects are due to chance (Rubin 1974). Therefore, designs that randomly assign people to work and nonwork statuses are preferred over nonexperimental designs, especially when a convincing model of selection into employment is unavailable (Winship and Mare 1992).

Work and the Duration Structure of Recidivism

Experimental studies of employment programs for offenders report that work effects may dissipate over time (Cave et al. 1993; Mallar et al. 1982). Moreover, the effect of employment is likely to be strongest when recidivism is most likely (Ekland-Olson and Kelly 1993; Schmidt and Witte 1988). For these reasons, the measurement of work treatment effects is another important design specification. To determine when and how employment affects recidivism, I use work effects that correspond to three distinct behavioral models. Assignment, participation, and eligibility specifications are distinguished by duration (short-term versus long-term) and whether they are conditional on participation in a work program.

Assignment effects. These effects are standard indicators of program effectiveness, determined by assignment to the work treatment or control condition. Assignment effects correspond to a *turning point* model:

Did the work experience permanently alter trajectories of offending? This is the most conservative test of work effects because it encompasses the overall impact of being in the program, long term and short term, regardless of the level of participation.

PARTICIPATION EFFECTS. In contrast to assignment effects, participation effects refer to the immediate contemporaneous impact of actually working in the job. This corresponds to an *involvement* or time allocation model of work and recidivism (Hirschi 1969:187). To use a metaphor from the drug and alcohol literature: Are respondents on the job when they fall off the wagon?

ELIGIBILITY EFFECTS. These effects fall somewhere between assignment and participation effects. They are limited-term measures of treatment impact during periods when those assigned to the program *could have* worked in program jobs. Eligibility effects correspond to a *job opportunity* model: Having a job available may provide a temporary safety net that reduces recidivism during the eligibility period, but work opportunity will not have lasting effects after the program ends.

DATA: THE NATIONAL SUPPORTED WORK DEMONSTRATION PROJECT

DESIGN OF THE EXPERIMENT

To test these job-treatment effects, I analyze data collected in a large-scale experimental employment program, the National Supported Work Demonstration Project. The program drew participants from the ranks of the underclass or ghetto poor. People were referred to the program by criminal justice, social service, and job-training agencies and randomly assigned to experimental and control conditions. From March 1975 until July 1977, over 3,000 persons with an official arrest history drawn from nine U.S. cities were randomly assigned to the control or treatment condition and completed initial (baseline) interviews. Those in the treatment group were offered minimum-wage jobs (mainly in the construction and service industries) in crews of 8 to 10 workers led by a counselor/supervisor. Members of both groups reported work, crime, and arrest information at nine-month intervals for up to three years.[1]

PROFILE OF THE PARTICIPANTS

The supported work project targeted criminal offenders, hardcore drug users, and youth dropouts with a history of both chronic and recent unemployment. To join the program, offenders were required to have been incarcerated within the past six months, addicts were required to have attended a drug treatment program, and at least half of the youth dropouts were required to have an official delinquent or criminal record. Screening for these conditions was conducted separately from the assignment process, so it would not compromise randomization (although generalizations may be limited to extremely disadvantaged persons with official criminal histories). Because this study examines recidivism or reoffending, only those with an official arrest history (85 percent of the sample) are included in the analysis.[2]

Table 1 shows mean values on personal characteristics for members of the control and treatment groups. In both groups, most subjects were young African American males with less than a high school education and an extensive criminal history. Those in the treatment group were slightly older, more likely to have a history of economic crime, and less likely to be assigned to the youth offender group.[3]

Because respondents were randomly assigned to the treatment (work) or control

[1] Of those scheduled for interviews, completion rates were 77 percent at nine months, 70 percent at 18 months, and 60 percent at 36 months. Incarcerated subjects were interviewed in jail or in prison. A sample selectivity analysis found no evidence that program effects on arrest and other outcomes were biased by sample attrition (Brown 1979; Piliavin and Gartner 1981).

[2] The results reported below also hold when persons who had no arrest history at the baseline interview are added to the analysis.

[3] To equalize the age distributions in the treatment and control groups, I reestimated all models after excluding members of the youth dropout group. Analysis of this subsample yielded the same substantive conclusions as those presented in Tables 2 and 3.

Table 1. Characteristics by Experimental Group: Participants in the National Supported Work Demonstration Project, 1975 to 1977

Personal Characteristics	Experimental Group: Control		Experimental Group: Treatment
Age (mean)	24.6 (6.6)	†	25.2 (6.6)
Percent male	92.0		91.6
Racial Group (Percent)			
African-American	76.2		76.9
White	11.1		11.7
Latino	12.3		11.0
Percent married	13.7		14.1
Years of education (mean)	10.2 (1.8)		10.3 (1.8)
Longest job duration (mean, in months)	15.2 (21.1)		17.1 (24.5)
Criminal History			
Percent with prior crime for money	74.8	†	79.2
Number of arrests (mean)	8.3 (11.4)		8.7 (11.7)
Project Site (Percent)			
Jersey City	18.1		18.9
Hartford	20.5	†	14.0
Philadelphia	16.2		18.2
Oakland	14.2		15.4
Chicago	12.4		13.9
Newark	10.0		10.7
San Francisco	5.6		6.4
New York	2.1		1.6
Atlanta	.7		.7
Sample Group (Percent)[a]			
Offender	52.6		53.8
Addict offender	28.5		31.2
Youth offender	18.9	†	14.9

Notes: Numbers in parentheses are standard deviations. Number of cases in the control group range from 1,937 to 2,210; for the treatment group, the number of cases ranges from 1,821 to 2,052.

[a] The combined offender sample includes all supported work participants who had been arrested at least once prior to program entry. This includes members of the groups called "ex-offender," "former addict," and "youth dropout" in the initial evaluation (Hollister et al. 1984).

† Difference between control and treatment groups is significant at $p < .05$ (two-tailed tests).

groups, program assignment is uncorrelated with work habits or other unmeasured "person effects" that might contaminate job effects. These data are therefore appropriate for testing hypotheses such as the Gottfredson-Hirschi age-invariance argument. Although supported work data have been carefully examined in a number of articles (Matsueda et al. 1992; Piliavin et al. 1986), the full continuous-time data arrays have yet to be analyzed. In light of the $100 million program cost (Hollister, Kemper, and Maynard 1984), the obstacles to replicating the National Supported Work Demonstration Project's experimental design, and the rich employment and crime information collected, these data are perhaps uniquely suited to unraveling the relation between age, work, and criminal recidivism.

SPECIFIC INDICATORS AND ANALYTIC STRATEGY

MEASURING ASSIGNMENT, ELIGIBILITY, AND PARTICIPATION

Assignment is measured by a fixed covariate coded 1 for those assigned to treatment and 0 for those assigned to the control group. The analysis of assignment effects is based on the classic true experimental design (Campbell and Stanley 1963; Cox 1958). If there had been no systematic departures from randomization, no attrition, and no time dependency in work effects, I could limit the analysis to assignment effects. Because these conditions were not met in the supported work project, I adjust assignment effects for several covariates, and analyze eligibility and participation as well.

Eligibility is measured as a time-varying dichotomous indicator for those assigned to the experimental condition. Those assigned to the treatment group were eligible for either 12 or 18 months of employment and were coded 1 during these months.[4] For follow-up interviews beyond their eligibility period, however, those assigned to the treatment group were ineligible and coded 0. All

[4] Those assigned in the Newark, Philadelphia, and Hartford sites were eligible for 18 months; those in the other sites were eligible for 12 months.

members of the control group, of course, were ineligible for all periods and were coded 0. Because the length of eligibility was predetermined by the research design, estimated eligibility effects are also based on a true experimental design.

Participation is modeled as a time-dependent explanatory variable, coded 1 during periods of work in a program job and 0 otherwise. Random assignment ensures that the control and the treatment groups have equal proportions of crime-prone individuals at the start of the experiment. Over time, however, criminal propensity (or any other correlate of crime) may be associated with project attrition. If crime-prone individuals are more likely to abandon treatment jobs, the pool of "job leavers" will be increasingly composed of more crime-prone people, and the pool of "job stayers" will be increasingly composed of less crime-prone people. Because these selection processes may bias participation effects on crime, I include in the participation models a time-varying term for having left supported work. And, because at least some of the "leavers" left for better opportunities outside the program, I also adjust program effects with time-varying terms for regular full-time employment and school participation. Although these statistical adjustments do not address selectivity as effectively as randomization would, the participation models illustrate the pattern of contemporaneous association (if not causation) between work, crime, and arrest.

Age and Other Independent Variables

I test for differences in survivor functions between the work treatment and control groups, stratifying by age of participants. After examining a number of different age classifications, I chose age 26 as a discrete cutting point for both empirical and theoretical reasons. First, the strongest effects on arrest in an influential financial aid experiment were found for those aged 26 and older (Mallar and Thornton 1978). Second, those older than 26 are specifically excluded from several large-scale programs, such as the Job Corps (Lattimore, Witte, and Baker 1990; Mallar et al. 1982). Third, the median age of U.S. prison inmates is approximately 26 (U.S. Department of Justice 1993). Fourth, labor force participation rates for men and single women peak between ages 25 and 34 (U.S. Census Bureau 1998:408). Finally, using an age threshold of 26 assures a sample of almost 1,000 offenders in each age category.

Apart from these empirical grounds for an age-26 cutting point, there are also theoretical reasons to select an age threshold that exceeds the age 18 to 22 range that characterizes the transition to adulthood in the general population. In view of their criminal histories and spotty employment records, standard transition-age norms are unlikely to apply to criminal offenders (Hogan and Astone 1986). Rather, theories of criminal embeddedness suggest that juvenile crime isolates adolescents from education and work networks that help initiate and sustain adult employment (Hagan 1993). By the age of 26, when crime rates have begun to decline, transitions to marriage and employment may facilitate cessation or desistance from crime.

I estimate models that include age (as both a discrete and continuous variable) and work effects. In addition, I also present models that adjust these effects for other explanatory variables. Covariance adjustment improves the precision of estimators and helps overcome the effects of incomplete randomization or selective attrition. Criminal history, measured by the number of arrests and a dichotomous indicator of prior economic crime, is included in the adjusted models. Controls for years of education, longest job duration, race, sex, marital status, and sample group are also entered in adjusted models, as well as program cohort (because stronger work effects were found among early enrollees [Hollister et al. 1984:39]), and the project site unemployment rate.

Dependent Variables

I analyze the timing of the first self-reported arrest after program entry. The first arrest is especially salient to released offenders, often representing a parole violation with serious consequences. Because theories of crime predict strong relationships between economic crimes and employment (Merton 1938), I also examine the timing of the first spell of illegal earnings.[5] This indicator is based on a semi-monthly self-reported array of crimes committed for money (e.g., burglary, robbery, drug sales). Comparing the results for arrest and illegal earnings helps disentangle the behavior of offenders from the actions of the criminal justice system. Police discrimination against unemployed suspects, for example, would inflate estimates of work effects on arrest but would not bias work effects on illegal earnings.

To evaluate the validity and reliability of the supported work data, Schore, Maynard, and Piliavin (1979) compared official arrest data with self-reported arrest data for three of the program sites. They found a 45 percent underreporting of arrest incidence, but only a 20 percent underreporting of arrest prevalence. Thus, people were more likely to understate the number of their arrests than to err in reporting whether they had been arrested at all. Nevertheless, Schore et al. (1979) found that experimental status was unrelated to underreporting, and there was no evidence that those assigned to jobs underreported in an effort to keep them. The follow-up observation period ranged from 18 to 36 months, depending on the date of enrollment into the program. All subjects were scheduled for an 18-month interview; at this point, 38 percent had been arrested and 32 percent reported illegal earnings. After three years, when all follow-up interviews were completed, approximately 54 percent of the respondents had been arrested and 38 percent had reported earning money illegally.

METHOD: EVENT HISTORY ANALYSIS

Event history analysis is suited to the study of work and recidivism. Relative to cross-sectional or panel designs, event history approaches (1) increase the precision of estimated work effects, (2) aid in determining the sequencing of work and crime, (3) appropriately model censored cases (i.e., those who never reoffend) over varying observation periods, and (4) allow for time-varying as well as fixed work effects. I estimate proportional and nonproportional piecewise models that specify an exponential baseline hazard (Tuma, Hannan, and Groeneveld 1979; Wu and Tuma 1991:362). In these specifications, the dependent variable is the natural logarithm of the hazard of arrest or illegal earnings, defined as an instantaneous probability.

Most recidivism studies operate on a "duration clock," modeling the time from prison release until parole violation, rearrest, or reconviction (Ekland-Olson and Kelly 1993; Hepburn and Albonetti 1994; Maltz 1984; Schmidt and Witte 1988). In most experiments, however, the analysis begins at the time of randomization, ensuring that the distribution of other time origins is approximately equal across the treatment and control groups. I estimated models on both clocks, but present results based on the experimental clock because my primary interest is the work treatment effects.[6] To assure the temporal ordering of work and crime, I lag participation indicators by two weeks (so that employment at time t predicts crime at time $t + 2$ *weeks*).

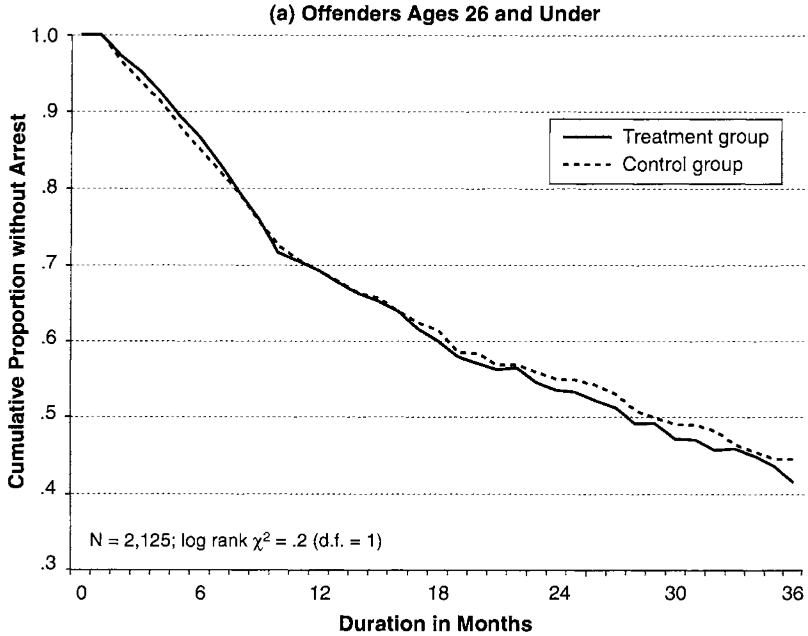
RESULTS

A NONPARAMETRIC ANALYSIS OF ASSIGNMENT EFFECTS

I use standard demographic life table methods (Namboodiri and Suchindran 1987) to examine the time until the first arrest and the first period of illegal earnings after the start of the experiment. These approaches are nonparametric in the sense that they make no distributional assumptions about the underlying recidivism process. To determine whether supported employment affects recidivism rates, the survival distributions for those assigned jobs are compared with the corresponding distributions for the control group. In the figures that follow, the horizontal axis represents time in months and the vertical axis represents the cumula-

[5] For both theoretical and practical reasons, I limit the analysis to the timing of the first arrest and the first period of illegal earnings. These first transitions are particularly critical for released offenders and are likely to differ qualitatively from subsequent transitions.

[6] The substantive conclusions of analyses run on the duration clock are identical to those discussed for the experimental clock.



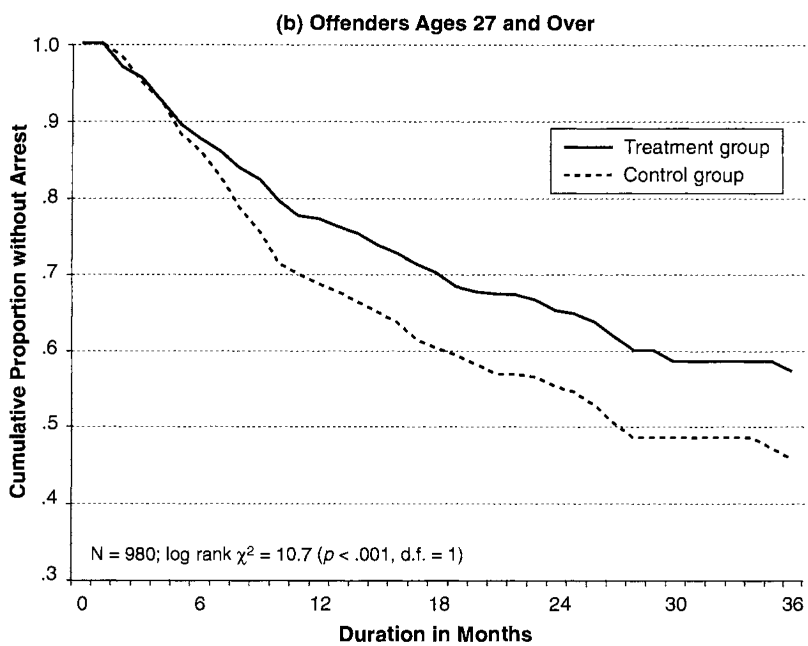


Figure 1. Time to Arrest for Younger Offenders and Older Offenders: National Supported Work Demonstration Project

tive proportion of those at risk of arrest who have yet to report an arrest. Both groups begin the experiment at "1" on the vertical axis because all respondents are at risk and none has yet failed by being arrested. I assess the statistical significance of the treatment effect with a log-rank test of survival-curve equality.

Figure 1a compares those in the experimental and control groups aged 26 and under; it depicts an ineffective treatment. The survival distributions are virtually indistin-

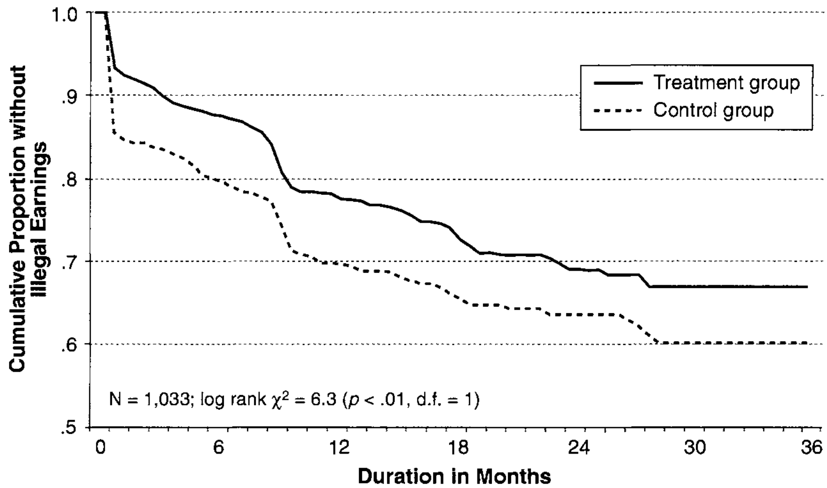


Figure 2. Time to Illegal Earnings for Offenders Ages 27 and Over: National Supported Work Demonstration Project

guishable for the first year, when 69 percent of those in both the treatment and control groups have yet to be arrested. After two years, about 55 percent of those in the control group remain free from arrest compared to only 54 percent of the treatment group. Although this gap widens slightly throughout the observation period, the difference between the distributions is neither statistically nor practically significant. Figure 1b depicts the curves for those aged 27 and older and paints a much different picture of program effectiveness. Here the line indicating the survival function for the treatment group diverges from the corresponding line for the control group after approximately six months. The difference gradually increases from 8 percentage points at one year to 11 percentage points after 3 years. As the chi-square statistic shows, this gap constitutes a strong and statistically significant treatment effect for employment in this older group.

Gottfredson and Hirschi (1990) argue that minor or trivial variations do not disprove the invariance thesis and that the empirical relation between employment and crime is "too small to be of theoretical import" (p. 164). What, then, is the practical significance of these statistical tests? The curves plot the estimated probability of arrest among offenders who have not been arrested by time *t*. If recidivism is defined as the probability of arrest after 18 months, Figure 1b shows 30 percent recidivism in the experimental group compared with about 40 percent in the control group. This 10 percentage-point difference corresponds to a 24-percent reduction in recidivism when the control group is taken as the base rate ([.300 –.397]/.397 = .24). In light of the widely reported failure of correctional treatment (Martinson 1974) and the high costs of the criminal justice system, a reduction in recidivism of this magnitude is noteworthy.

As was true for arrests, supported work assignment fails to reduce the rate of illegal earnings among the total sample or among the younger subgroup (figures not shown). For those aged 27 and over, by contrast, Figure 2 shows that the work program decreases recidivism among those in the experimental group relative to the control group. Among older offenders, the cumulative proportion reporting illegal earnings is about 7 percentage points lower for those assigned to the program than for those in the control group, a statistically significant treatment effect.

A COMPARISON OF THE DURATION STRUCTURES OF ARREST AND ILLEGAL EARNINGS

Comparison of Figure 1b and Figure 2 suggests differences in the duration structures of arrest and illegal earnings, as well as a more gradual work effect on arrest than on illegal earnings. Studies of official recidivism gen-

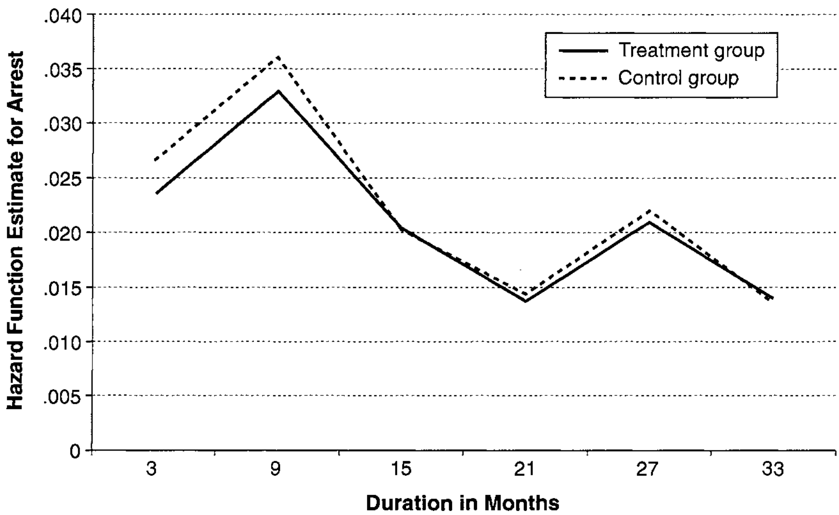


Figure 3. Time to Arrest for Offenders from All Age Groups: National Supported Work Demonstration Project

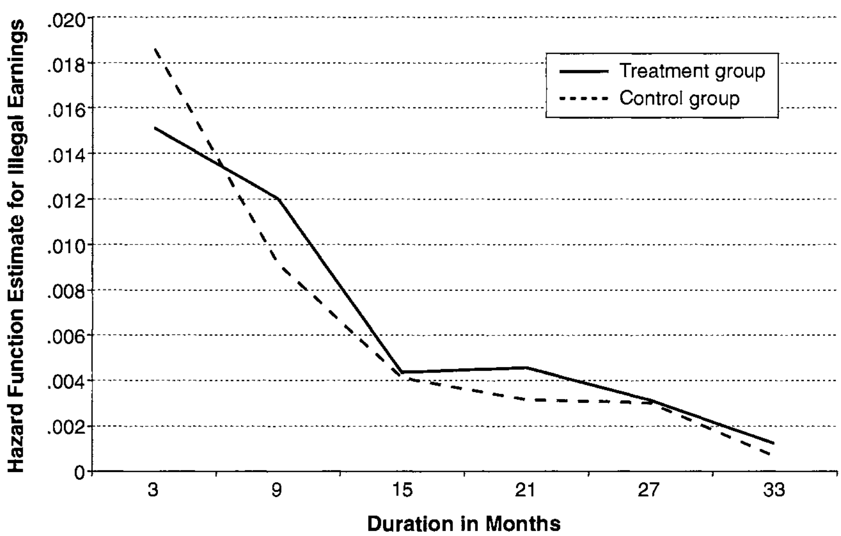


Figure 4. Time to Illegal Earnings for Offenders from All Age Groups: National Supported Work Demonstration Project

erally report rising hazard rates for the first year after release and declining rates thereafter (Chung, Schmidt, and Witte 1991; Ekland-Olson and Kelly 1993). Is this log-normal pattern due to an initial abstinence period or to lags in criminal justice processing? This puzzle is at least partially resolved by plotting the hazard functions of the two recidivism measures. Figure 3 shows the familiar rising and falling pattern observed when arrest or conviction is used to measure recidivism. The rate of first rearrest rises during the first nine months and begins an uneven decline thereafter.

In contrast to the arrest hazard, however, the hazard for illegal earnings (Figure 4) decreases monotonically, with the steepest decline occurring in the first year of the program. The greatest recidivism risk for illegal earnings therefore occurs immediately after release. To test whether this result is due to running the analysis on the experimental time clock rather than the duration-since-release clock, I limited the sample to recent prison releasees, starting the clock for both arrest and illegal earnings outcomes at the first month of release from prison. The discrepancies between the two recidivism outcomes for this subgroup are even more pronounced than those shown in Figures 3 and 4 (available on request from the author).

One interpretation of these results is that the rising arrest hazard reflects a delayed criminal justice response rather than an initial period of abstinence. For example, parole or police officers may not react to an offender's illegal activities until they become sustained or repeated. Alternatively, the two measures may be tapping different domains of activity, as Hindelang, Hirschi, and Weis (1979) have argued with regard to self-reported and official delinquency measures. Among supported work participants, the self-reported illegal earnings indicator may represent less serious offenses that are invisible to the criminal justice system. For example, whereas selling a small amount of marijuana to a close friend may generate illegal earnings, selling drugs on a street corner is more likely to result in arrest. Thus self-reports from adult offenders may suffer from the same domain limitations as do indicators of adolescent delinquency.

Whether the discrepancies in the hazard rates are due to delays in criminal justice responses or to domain differences, however, there is no discrepancy in the age-graded work effects: The survival curves show that supported employment decreases the likelihood of recidivism for both measures among older offenders. Although this finding is not incontrovertible, it is based on a solid experimental design and an analytic technique that makes few assumptions.

PROPORTIONAL MODELS OF ASSIGNMENT, ELIGIBILITY, AND PARTICIPATION

Tables 2 and 3 elaborate the nonparametric results, showing the main and interactive effects of age and work on arrest and illegal earnings, alongside estimates from models that adjust these effects for race, sex, marital status, program cohort, work history, prior economic crime, prior arrests, sample group, and site unemployment rate. Results from the adjusted models test the robustness of the experimental work effects, though they do not supplant them (Zeisel 1982).

ASSIGNMENT EFFECTS. Model 1 of Tables 2 and 3 shows the main effects of assignment to treatment and of age on arrest (in Table 2) and illegal earnings (in Table 3). For arrest (Table 2), the treatment assignment effect is close to zero (–.03), and the significant age coefficient (–.12) suggests that older offenders are less likely than younger offenders to be arrested. The joint assignment specification for Model 1 adds an age × treatment assignment interaction term, reflecting the predictions of the life-course model. This equation provides a much better fit to the data. Under this joint model, the assignment coefficient represents the effect of assignment to treatment among younger participants and the age coefficient represents the age effect within the control group. Both are nonsignificant and positive. The estimate for the age × treatment assignment interaction gives the difference in the treatment effect across age groups, providing a direct test of the age-invariance hypothesis. The significant negative interaction term for arrest in Table 2 (–.31) suggests a differential rather than an invariant treatment effect by age. The treatment effect on arrest for the older group is obtained by deviating the interaction coefficient from the assignment coefficient: The relative risk of arrest is about 22 percent lower for older assignees than for older controls ($1 - e^{(-.31+.06)} = .22$).[7] After covariate adjustment, the interaction term is slightly reduced but remains statistically significant.

In the analysis for illegal earnings shown in Table 3, the work treatment effect again differs across age groups, as reflected in the significant interaction terms of the joint and adjusted models. This finding holds net of prior criminal and employment history, marital status, and other background characteristics. Results for both arrest and illegal earnings thus support the conception of work as a turning point in the life course for older, but not younger, offenders.

ELIGIBILITY EFFECTS. Eligibility refers to the opportunity to participate in sup-

[7] To test whether older treatment assignees differ from older controls, a modified coding scheme is required. In this case, the difference between the two groups is statistically significant.

Table 2. Maximum-Likelihood Estimates of the Effect of Work on Self-Reported Arrest: Offenders from the National Supported Work Demonstration Project

Independent Variable	Model 1 (Treatment Assignment) Main	Joint	Adjusted	Model 2 (Eligibility[a]) Main	Joint	Adjusted	Model 3 (Participation[a]) Main	Joint	Adjusted
Age > 26	$-.12^{*}$ (.06)	.03 (.08)	.08 (.09)	$-.12^{*}$ (.06)	−.01 (.07)	.05 (.08)	$-.13^{*}$ (.06)	−.08 (.07)	−.02 (.07)
Treatment assignment	−.03 (.06)	.06 (.07)	.05 (.07)	—	—	—	—	—	—
Age × treatment assignment	—	$-.31^{**}$ (.12)	$-.28^{*}$ (.12)	—	—	—	—	—	—
Eligibility[a]	—	—	—	−.07 (.06)	−.02 (.07)	.01 (.07)	—	—	—
Age × eligibility	—	—	—	—	$-.32^{*}$ (.13)	$-.30^{*}$ (.13)	—	—	—
Participation[a]	—	—	—	—	—	—	$-.30^{***}$ (.08)	$-.22^{*}$ (.09)	$-.23^{*}$ (.09)
Age × participation	—	—	—	—	—	—	—	−.28 (.16)	−.26 (.16)
Left program[a]	—	—	—	—	—	—	.11 (.06)	$.11^{*}$ (.06)	.10 (.06)
Regular work[a]	—	—	—	—	—	—	$-.29^{***}$ (.07)	$-.29^{***}$ (.07)	$-.31^{***}$ (.08)
In school[a]	—	—	—	—	—	—	−.25 (.14)	−.25 (.14)	.24 (.14)
Chi-square	198	204^{**}	319^{***}	199	205^{**}	321^{***}	234^{**}	237^{***}	352^{***}
Degrees of freedom	3	4	16	3	4	16	6	7	19

Note: Piecewise proportional models; N = 3,102. Numbers in parentheses are standard errors. Overall significance tests are based on improvement in fit over baseline model with no covariates.

[a] Time-varying covariate.

$^{*}p < .05$ $^{**}p < .01$ $^{***}p < .001$ (two-tailed tests)

ported work, regardless of actual participation. As predicted by the "job opportunity" model, eligibility effects on arrest are stronger than are treatment assignment effects. Nevertheless, the eligibility results, shown in Model 2 of Tables 2 and 3, are consistent with the assignment results: The significant interaction terms in the joint and adjusted models indicate that the effects of eligibility depend on age. For the illegal earnings outcome (Model 2, Table 3), eligibility effects are slightly weaker than assignment effects, but they remain statistically significant. For both the arrest and illegal earnings outcomes, older offenders have lower rates of criminal behavior during periods when those assigned to the treatment group were eligible to work in the program.

Participation Effects. Model 3 of Tables 2 and 3 reports the effects of active supported work participation on arrest (Table 2) and illegal earnings (Table 3). Whereas assignment and eligibility effects were limited to older offenders, the main arrest model in Table 2 shows that program participation reduced the arrest rate among the total sample, with effects comparable to those of regular employment. The difference between regular employment effects and participation effects is that the latter are less likely to be biased by the self-selection of "good risks"—those with better employment prospects and lower risks of recidivism—into regular work. Moreover, the significant participation coefficient in the interaction model suggests that even younger offenders—a group that appears unresponsive to the work program un-

Table 3. Maximum-Likelihood Estimates of the Effect of Work on Self-Reported Illegal Earnings: Offenders from the National Supported Work Demonstration Project

Independent Variable	Model 1 (Treatment Assignment)			Model 2 (Eligibility[a])			Model 3 (Participation[a])		
	Main	Joint	Adjusted	Main	Joint	Adjusted	Main	Joint	Adjusted
Age > 26	–.11 (.07)	.07 (.09)	–.01 (.10)	–.11 (.07)	.03 (.09)	.01 (.08)	–.11 (.07)	.01 (.08)	–.07 (.09)
Treatment assignment	–.02 (.06)	.09 (.07)	.03 (.07)	—	—	—	—	—	—
Age × treatment assignment	—	–.36** (.13)	–.31* (.13)	—	—	—	—	—	—
Eligibility[a]	—	—	—	–.03 (.07)	.06 (.07)	–.01 (.08)	—	—	—
Age × eligibility	—	—	—	—	–.31* (.13)	–.28* (.14)	—	—	—
Participation[a]	—	—	—	—	—	—	–.06 (.07)	.06 (.08)	–.003 (.08)
Age × participation	—	—	—	—	—	—	—	–.39** (.15)	–.36* (.15)
Left program[a]	—	—	—	—	—	—	.06 (.09)	.06 (.09)	.01 (.09)
Regular work[a]	—	—	—	—	—	—	–.08 (.09)	–.08 (.09)	–.11 (.09)
In school[a]	—	—	—	—	—	—	–.01 (.13)	–.01 (.15)	–.03 (.14)
Chi-square	1,246	1,253*	1,495***	1,246	1,252*	1,494***	1,247	1,255	1,498***
Degrees of freedom	4	5	17	4	5	17	7	8	20

Note: Piecewise proportional models; N = 3,300. Numbers in parentheses are standard errors. Overall significance tests are based on improvement in fit over baseline model with no covariates.

[a] Time-varying covariate.

$^{*}p < .05$ $^{**}p < .01$ $^{***}p < .001$ (two-tailed tests)

der the eligibility or assignment specifications—are likely to avoid arrest during periods when they are actively working in the program. Although the *difference* in participation effects (represented in the interaction term) is not statistically significant in Model 3, participation reduces the rate of arrest for the elder group by approximately 39 percent in both the joint and adjusted models. Consistent with the involvement model of participation, leaving the program has a marginal positive effect on arrest and securing regular employment a strong negative effect on arrest, net of the other variables.

In contrast to the arrest results, the results for illegal earnings show an age-graded participation effect with a significant age × participation interaction in the joint models (Table 3, Model 3). The weak participation coefficients for the younger group in the joint and adjusted models mean that supported employment fails to reduce the rate of illegal earnings for younger offenders, *even during periods of active program involvement.* Also in contrast to the arrest models, regular employment and school attendance have little effect on the hazard of self-reported illegal earnings.

Tables 2 and 3 show that the effects of work on crime depend on the specification of the independent and dependent variables. For the arrest outcome, the program is effective for older offenders under all specifications and for both younger and older offenders under a model specifying contemporaneous participation. For the illegal earnings outcome, supported work reduces recidivism for older but not for younger offenders across the treatment assignment, eligibility, and participation models.

ALTERNATIVE SPECIFICATIONS

AGE. Age and each of the age × work interaction terms can also be modeled as *continuous* variables, corresponding to the view that offenders are increasingly amenable to employment with each passing year. Under this coding, the interaction is again negative and statistically significant, but the main experimental effect is positive and marginally significant (not shown). I present the categorical results in this article because plots of age, program status, and recidivism suggest a discrete rather than a continuous interaction. Moreover, the threshold is easier to interpret and more applicable for policy purposes. The models in Tables 2 and 3 assume that age matters most when the opportunity to work is initially offered—that amenability to treatment is fixed at program entry. Age may also be specified as time-dependent, assuming that people "age into amenability" during the experiment. Under this specification, the substantive results are almost identical to those reported in Tables 2 and 3.

NONPROPORTIONAL PARTICIPATION MODELS. The survival and hazard plots show that rearrest rates peak later than do crime rates and that treatment effects are strongest at the beginning of the experiment. I therefore reestimated the models in Tables 2 and 3 using nonproportional piecewise hazard models (not shown). For the arrest outcome, results for the nonproportional models parallel those for the proportional models, with work participation reducing arrest in both the main and joint models in each period. For illegal earnings, however, the beneficial treatment effects and age × treatment assignment interaction effects dissipate rapidly after the first year. In light of these results, and the small differences between the treatment assignment and eligibility models, it is unlikely that extending the supported work eligibility period would have had a marked effect on recidivism.

DISCUSSION AND CONCLUSION

This article has examined whether an experiment that provided jobs to criminals served as a turning point in their offending careers. In light of the rising number of released offenders rejoining civil society, it is important to identify such turning points for both scientific and policy purposes. This year, over 500,000 criminals will exit from state and federal prisons (U.S. Department of Justice 1999), and approximately 2 million will be released from probation or parole supervision. Given the high likelihood of recidivism among this group, it is good policy to seek the turning points that enhance desistance from crime and facilitate rehabilitation. Moreover, the identification of such contingencies is fundamental to scientific explanations of criminal behavior over the life cycle.

Work appears to be a turning point in the life course of criminal offenders over 26 years old. Offenders who are provided even marginal employment opportunities are less likely to reoffend than those not provided such opportunities. Employment in the National Supported Work Demonstration Project—a program critics deemed to be a failure—significantly reduced recidivism among offenders over the age of 26. Primary findings reported here indicate: (1) a differential work effect across age groups, lending support to a life-course rather than an age-invariant model of work and crime; (2) progressively larger effects of assignment, eligibility, and participation, particularly for the arrest outcome; and (3) the varying timing of recidivism as measured by self-reported illegal earnings and arrest.

I first examined the effect of program assignment within age categories using tests for the equality of survival distributions. Although the program failed to reduce crime across the entire sample, its impact was clearly age-graded: The job treatment significantly reduced recidivism among older participants. In contrast to the stylized cultural image of the "hardened criminal," these results suggest that older offenders are more amenable to employment interventions than younger offenders. Because these findings are based on a sound experimental design and on an analytic technique that makes few assumptions, the evidence is strong against the noninteraction hypothesis proposed by Hirschi and Gottfredson (1983). The suggestion that older offenders generally desist from criminal behavior is unremarkable.

These results are important because they show that older offenders given jobs are less likely to reoffend than those *of comparable age who were not provided these opportunities*. This age × treatment assignment interaction suggests that work hastens desistance from crime among older offenders.

The success of work programs in promoting desistance from crime for older offenders and their failure to prevent crime among adolescents indicate asymmetric or irreversible causation: The conditions that stop crime in adulthood are not simply the reverse of those that caused it in adolescence (Uggen and Piliavin 1998). Whereas parents, peers, and neighborhoods are inarguably among the initial causes of crime, for example, work and family factors take precedence in explaining desistance. The same general theoretical models of age-graded informal social control (Sampson and Laub 1993), peer association (Warr 1998), or criminal embeddedness (Hagan and McCarthy 1997) may well apply to both processes. Nevertheless, the transition out of crime is distinguished by its clear link to *adult* life-course progressions and transition-age norms. In fact, desistance from minor delinquency and crime may itself constitute a separate and important dimension of the multifaceted transition to adulthood.

A second contribution of this study is the identification of a range of work treatment effects on crime. I report treatment assignment, participation, and eligibility effects both before and after adjusting for personal characteristics related to both work and crime (i.e., sex, race, marital status, program cohort, education, work history, prior crime and arrests, and site unemployment rate). The substantive results suggest that maximizing participation rates—keeping those assigned to the experiment from dropping out of work—may increase program effectiveness, notwithstanding the "person effects" driving both participation and recidivism. As cautioned above, however, participation effects may be biased upward by selective attrition. On the other hand, assignment effects may be biased downward because the observation period exceeds the length of the program. Modeling program eligibility as a time-varying characteristic results in a more sensitive estimate of program effects that retains the experimental design and sidesteps some of the selectivity problems inherent in participation models.

For illegal earnings, results from all three models tell the same substantive story. In each case, the hazard of illegal earnings is significantly lower for older members of the treatment group than for older members of the control group. For arrest, the magnitude and even the direction of work effects varies with the specification: The assignment results (Model 1) show an age × treatment assignment interaction, the eligibility models (Model 2) show a stronger age × eligibility interaction, and the participation specification (Model 3) shows a strong effect for both younger and older participants. Although the assignment and eligibility results are comparable in this study, the conceptual distinction between a turning point model and a treatment availability model may be important in a variety of research settings. Moreover, the similarity of results in this case is relevant for policy purposes—extending the eligibility period would appear to have little additional effect on recidivism. Analysis of eligibility effects may be informative in other settings in which the follow-up period exceeds the duration of the program and participation is voluntary, such as predicting the time until employment in a welfare-to-work experiment.

My third main finding indicates different duration structures for self-reported illegal earnings and self-reported arrest. Although the risk of recidivism via illegal earnings is highest immediately upon release from prison, with no initial abstinence period, the risk of recidivism via arrest rises more gradually over the first post-release year. These differences could be due to lags in criminal justice processing or to the two outcomes (arrest and illegal earnings) tapping different domains of behavior. In either case, however, this finding highlights the potential importance of self-reported offending information in scientific research on recidivism and desistance. Although reliance on official statistics has been challenged in etiological studies of delinquency, most investigations of adult offenders accept such measures uncritically. When criminals are asked to report on their crime and arrest records, they tell us they are most likely to commit at least some forms of economic crime immediately upon release, but are most likely to be arrested some time thereafter. Although the age-graded job effects I examined hold across these two outcomes, recidivism studies can only be strengthened by validating arrest or conviction data with self-reported information on offending.

Because this analysis examines data from the American underclass in the late 1970s, it is unclear whether the results are generalizable to less marginalized populations in different labor markets. Nevertheless, these findings should encourage employment, training (Sampson and Laub 1996) and other interventions (Sherman et al. 1998) for serious adult offenders. These results also go to the heart of a much larger debate within the sociology of crime: Are crimes discrete events in the life course, or is criminality a stable characteristic of persons themselves? The latter view, that crime results from individual personality traits, implies that criminal propensity crystallizes sometime during socialization in early childhood and is virtually impossible to change thereafter. In contrast, this study provides clear evidence that a modest work experience later in life can change the behavior of older criminals.

This research highlights both the promise and the limitations of employment programs for criminal offenders. Although the National Supported Work Demonstration Project was a bold policy experiment, it constitutes a weak intervention for social-scientific purposes. That is, the program did not radically shift participants along stratification dimensions such as wealth, status, or power. By extending a basic job opportunity, policymakers hope to induce offenders simply to desist from crime. Because of an array of political, economic, and ethical constraints, however, the best that such programs can offer is honest work for meager wages. This study shows an age-graded response to this offer that supports a life-course conception of crime. Although programs providing marginal jobs are relatively unattractive to youth, they may provide the turning point toward a viable pathway out of crime for older offenders.

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