CRIMINOLOGY

DEALERS, THIEVES, AND THE COMMON DETERMINANTS OF DRUG AND NONDRUG ILLEGAL EARNINGS*

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Drug crime often is viewed as distinctive from other types of crime, meriting greater or lesser punishment. In view of this special status, this article asks whether and how illegal earnings attainment differs between drug sales and other forms of economic crime. We estimate monthly illegal earnings with fixed-effects models, based on data from the National Supported Work Demonstration Project and the National Longitudinal Survey of Youth. Although drug sales clearly differ from other types of income-generating crime, we find few differences in their determinants. For example, the use of cocaine or heroin increases illegal earnings from both drug and nondrug crimes, indicating some degree of fungibility in the sources of illegal income. More generally, the same set of factors—particularly legal and illegal opportunities and embeddedness in criminal and conventional networks—predicts both drug earnings and nondrug illegal earnings.

Research on illegal earnings often has emphasized the distinctiveness of drug offenses and the greater or lesser punishments associated with these

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crimes (Jacobs, 1999; Levitt and Dubner, 2005; Sullivan, 1989; Venkatesh, 2008). Much of this work has drawn a causal chain in which illicit drug use engenders urgent economic need, which in turn drives illegal earnings (Bennett, Holloway, and Farrington, 2008; Fischer et al., 2001; Inciardi and Pottieger, 1994; Manzoni et al., 2006). Several studies have suggested that the economic imperatives of illegal drug use are primarily addressed within the drug economy (Inciardi and Pottieger, 1994; Johnson and Natarajan, 1995; Manzoni et al., 2006). By this view, drug use would only lead to crimes such as burglary or theft when drug sales are insufficient to support drug consumption and other pressing needs (Inciardi and Pottieger, 1994; Manzoni et al., 2006). This article addresses this contention—that illegal drug use primarily leads to drug sales—by expanding on a previous study in which we found that illegal earnings rise substantially during periods of active use (Uggen and Thompson, 2003). We move beyond the previous study by testing whether the theoretically motivated determinants of drug earnings differ from the determinants of illegal earnings from crimes such as robbery and theft.

These differences have implications beyond the source of money to procure drugs; they also carry differences in social harm. The individual and societal costs resulting from predatory crimes such as burglary or robbery are arguably quite different than the costs associated with a *malum prohibitum* offense such as drug trafficking, in which voluntary exchanges occur. As Cohen, Piquero, and Jennings (2010) suggested, such considerations should inform the "allocation of scarce resources when considering the appropriate mixture of prevention, punishment, and treatment" (p. 282; see also Chaiken and Chaiken, 1984; Clarke and Cornish, 1985).

For those convicted of economic crime, the specific source of their illegal earnings clearly affects their life chances. On the one hand, some drug offenders are granted more effective treatment and correctional alternatives, especially since the rise of drug courts, which go beyond "treatment as usual" in the criminal justice system (Gottfredson, Najaka, and Kearley, 2003; U.S. GAO, 2011). On the other hand, the war on drugs and welfare reform movement of the 1990s both created special collateral sanctions for those convicted of drug offenses. For instance, under the Drug Free Student Loans Act of 1998, students convicted of drug crimes are ineligible for federal loans, grants, and other assistance (Blumenson and Nilsen, 2002). Similarly, welfare reform's Gramm Amendment imposed a lifetime ban on the receipt of food stamps and TANF for those convicted of drug felonies (Jayakody, Danziger, and Pollack, 2000). Analogous drug-related measures include the Department of Housing and Urban Development's "one strike and you're out" rules, which allow eviction of public housing tenants involved in drug-related crimes and termination of federal disability payments to individuals for whom drugs or alcohol was the primary reason for disability (Jayakody, Danziger, and Pollack, 2000). These consequences apply only to drug offenses, and there are few equivalent sanctions for any other type of conviction (Allard, 2002).

In light of the considerable differences in the consequences of drug convictions and the public policy assumption that drug offenses require specialized social responses, this study investigates differences in the predictors of income generated from drug and nondrug crime. This investigation does not suggest that individuals necessarily specialize in drug or nondrug offenses, although some studies have found some evidence of specialization (Osgood and Schreck, 2007: 300; Shover, 1996: 65). Subsequently, we will present descriptive information on those who specialize in drug and nondrug illegal earnings before proceeding to analyze the short-term determinants of each income source.

Short-term stints of criminal activity seem to be responsive to time-varying changes in situational constraints and opportunities (Deane, Armstrong, and Felson, 2005; Sullivan et al., 2006: 224). In the analysis to follow, we will suggest that these stints of criminal activity—whether they involve drug sales or other illegal economic activity—are a product of general economic needs, opportunities, and embeddedness in social networks and institutions. We first motivate and specify an integrated conceptual model of illegal earnings, building on our previous work (Uggen and Thompson, 2003). We then compare its relative capacity to predict earnings from drug sales and earnings from other income-generating crimes. These results will, thus, speak to scientific questions about the fungibility of illegal income sources, as well as to policy questions bearing on the distinctiveness of drug offenses and convictions.

CRIME AND MONEY

Most previous studies of illegal earnings have focused on a single drug-selling gang, have used prison inmates to assess illegal income retrospectively, or have employed cross-sectional research designs (Levitt and Venkatesh, 2000; Venkatesh, 2008; Wilson and Abrahamse, 1992). Much recent research has emphasized the distinctiveness of drug selling. Whereas studies of theft and burglary have called attention to thrill seeking and other extraeconomic considerations (Jacobs, 2000; Katz, 1988; Morselli, Tremblay, and McCarthy, 2006; Shover, 1996: 63), drug-selling gangs seem to devote greater attention to considerations of profit and risk (Curtis and Wendel, 2007). For example, such gangs tend to discourage illicit drug use and violence by their members because these behaviors draw police attention and could jeopardize potential profits (Jacobs, 1999; Johnson and Natarajan, 1995; Venkatesh, 2008).

Morselli and Tremblay (2004) distinguished between predatory and market offenses, noting that persons who commit predatory crime—such as robbery, burglary, and theft—generally have lower rates of offending (and earnings) than those who commit market offenses such as drug dealing (Peterson and Braiker, 1981; Tremblay and Morselli, 2000). Returns from market crimes are driven by market conditions and consensual exchanges among customers, producers, and sellers, whereas returns from predatory crimes are largely a function of offense frequency, target selection, and success in avoiding detection (Morselli and Tremblay, 2004). Although this line of research has contributed greatly to understanding the economics of crime, criminologists have yet to learn whether our models are uniformly efficacious in predicting returns from different types of crime or whether the predictors of remuneration are offense specific.

Analyses of crime-specific earnings have primarily been the province of ethnographic researchers. Whereas quantitative research tends to focus on global crime measures, qualitative work more typically addresses particular offenses, including fencing stolen goods (Steffensmeier, 1986), burglary (Cromwell, Olson, and Avary, 1991), robbery (Jacobs, 2000), dealing crack cocaine (Jacobs, 1996, 1999), and selling other drugs (Mohamed and Fritsvold, 2006). Such studies provide a revealing window into the factors giving rise to specific forms of law violation. Of course, this does not imply that individuals specialize in only one form of crime but instead that people have specific preferences based at least partly on prior experience and expertise, considerations of risk, and immediate situational factors (Shover, 1996; Sullivan et al., 2006).

The analysis that follows compares the predictors of monthly earnings from drug sales with those from other illegal activities, net of an individual-specific intercept term that captures preexisting differences across individuals (see, e.g., Bushway, Brame, and Paternoster, 1999). This investigation builds on our general model of criminal earnings, which emphasizes how illegal drug use creates a strong earnings imperative (Uggen and Thompson, 2003). The primary building blocks of this model include substance use, legal and illegal opportunities, embeddedness in criminal and conventional networks, and perceptions of risk and reward. Whereas this previous work considered total criminal remuneration, the current article tests for differences in the predictors of drug and nondrug economic crime.

TWO THEORETICAL PERSPECTIVES ON DRUG AND NONDRUG INCOME

GENERAL THEORIES AND THE FUNGIBILITY OF ILLEGAL EARNINGS

Generalized explanations of crime posit that the same basic conceptual and empirical model can explain returns from all forms of criminal behavior. Most economic perspectives, for example, suggest that criminal choice is based on the perceived risks and rewards associated with an offense (e.g., Becker, 1968). The various sources of illegal income are fungible to the extent that crimes return similar rewards at a given level of risk and opportunity. Other general explanations of crime, including those based on social control (Hirschi, 1969; Sampson and Laub, 1993) and self-control theories (Gottfredson and Hirschi, 1990), also make few offense-specific predictions. From each of these perspectives, the determinants of drug earnings also should predict earnings from other types of crime. Such an assumption is implicit, if untested, in research that has failed to distinguish among the various sources of illegal earnings, including our own work (Uggen and Thompson, 2003).

THE DISTINCTIVENESS OF DRUG CRIME

In contrast to the fungibility argument, drug crimes may be distinctive for many reasons. Drug crime is especially prevalent (Mumola and Karberg, 2006), is uncommonly remunerative (Fagan and Freeman, 1999; Levitt and Venkatesh, 2000), and invokes a markedly different social reaction than other economic crimes. Moreover, some ethnographic research has pointed to a career progression from "sneaky" crimes such as theft in the early teens, to robbery in the middle and late teens, to drug dealing in the late teens and thereafter (Sullivan, 1989: 202). This progression has been attributed to the sustainability and potential for income growth in drug sales (Levitt and Venkatesh, 2000; Sullivan, 1989: 177). In terms of the likelihood of arrest, both researchers and study participants have deemed the consensual exchange of drugs for money as considerably less risky than other types of income-generating crime (Jacobs, 1999: 100–1; Johnson and Natarajan, 1995; Shover, 1996: 122; Sullivan, 1989: 165). Taken together, such distinctions could alter the effects of several predictors in our general model.

Drug Use and Economic Crime

We have argued that the regular consumption of relatively expensive illegal drugs, such as cocaine and heroin, creates an "earnings imperative" that increases criminal activity (Uggen and Thompson, 2003). Drug users may be especially likely to earn money via drug sales, given their existing supply networks and incentives to subsidize their personal use (Maher, 1997). Assessing month-to-month change in criminal behavior among a prison sample, Horney, Osgood, and Marshall (1995) found an especially strong association between drug use and drug dealing. Drug use also was linked to property crimes and assaults in their study, although to a lesser extent. Heavy users seeking quick money may turn first to drug dealing and only secondarily to nondrug property crimes (Manzoni et al., 2006). If this pattern holds, then we should find an especially strong effect of drug use on drug earnings and a smaller—although still strong and positive—effect of drug use on nondrug income.

Legal Versus Illegal Income

Obtaining income illegally carries the risk of punishment, which individuals would not incur without a compensating advantage, such as a high financial return (Johnson, Natarajan, and Sanabria, 1993; Viscusi, 1986). As our previous examination of these data suggests, income-generating crime rises when legal avenues to obtain money are limited and respondents report little licit income. To the extent that we expect any differences in the effects of legal income on the two types of illegal earnings, they should be tied to the anticipated returns to different types of crime. The literature suggests that crime pays more than minimum-wage legal work only for some offenses or for drug dealers at the highest end of the selling hierarchy (Johnson and Natarajan, 2005; Levitt and Dubner, 2005; Levitt and Venkatesh, 2000; Venkatesh, 2008; Wilson and Abrahamse, 1992). On balance, we expect earned and unearned (e.g., social security and public assistance) legal income to decrease both drug and nondrug earnings.

Opportunity Structure

With regard to unemployment rates, Cantor and Land (1985, 2001) found evidence for both a positive "motivation" effect (see also Arvanites and Defina, 2006) and a negative "opportunity" effect on crime. As joblessness rises, the attractiveness of targets declines and the presence of guardians increases (Cantor and Land, 1985, 2001). High unemployment rates should thus reduce nondrug economic crimes (such as burglary and robbery) where guardianship and target attractiveness are most salient (see Cohen, Cantor, and Kluegel, 1981; Cohen and Felson, 1979) but increase drug income by virtue of greater motivation and, perhaps, demand on the part of buyers (see, e.g., Bachman et al., 1997). Respondents also have less opportunity to earn money illegally during spells of incarceration. Because drugs are actively traded within prisons, however, nondrug income may be reduced to a greater extent than drug income. Finally, some evidence suggests that perceived illegal opportunities may be more salient in explaining offenses that must be sought, created, or planned (Clarke and Cornish, 1985; Matsueda, Kreager, and Huizinga, 2006) than in explaining market offenses such as drug sales.

Criminal Embeddedness

We anticipate positive relationships between embeddedness in criminal networks and both drug and nondrug earnings, as criminal success in each arena requires networks of suppliers, customers, and intermediaries

(Hagan and McCarthy, 1997; Levitt and Venkatesh, 2000; Morselli, Tremblay, and McCarthy, 2006; Sutherland, 1965; Warr, 1998). Although we generally expect crime (and hence remuneration) to decline with age, we also anticipate some income return to experience. For drug income, which may require time or tenure to move up the ranks where earnings are higher, age and experience may yield greater remuneration (Levitt and Venkatesh, 2000; Sullivan, 1989: 177), with nondrug income declining rapidly with age.

Conventional Embeddedness

In contrast to criminal embeddedness, illicit income declines with ties to family, work, and school institutions (Hagan and McCarthy, 1997; Ihlanfeldt, 2007; Laub and Sampson, 2003; Sampson and Laub, 1993; Uggen and Thompson, 2003). Because drug sales are perceived as less risky than crimes such as robbery and burglary, drug earnings may be somewhat less responsive to conventional social controls (Hagan and McCarthy, 1997; Sullivan, 1989). Although education is generally thought to reduce illegal activity via increased human capital (Hagan and McCarthy, 1997), a school environment also provides a fertile market for drug sellers (Mohamed and Fritsvold, 2006). Income from drug sales may therefore rise with school attendance, in contrast to earnings from other illegal activities such as robbery, burglary, and theft.

Subjective Risks and Rewards

Although drug crimes may be severely punished, the certainty of punishment tends to be low, as there is little incentive for purchasers to report suppliers to the police (Viscusi, 1986). Experienced drug dealers thus perceive lower risks and higher returns than less experienced dealers and nondealers (MacCoun and Reuter, 2001; Reuter et al., 1990). If the demand for drugs is inelastic (whether because of dependency or other factors), then greater risk could actually drive illegal earnings higher during heavy enforcement periods (MacCoun and Reuter, 2001: 30; see also Reuter and Kleiman, 1986). Consequently, we might expect perceived risk to diminish nondrug criminal earnings more strongly than drug earnings. With regard to rewards, those with the greatest legal earning capacity should be least motivated to engage in crime, although such respondents also may possess the skill set required for higher illegitimate earnings (Levitt and Dubner, 2005; Levitt and Venkatesh, 2000; Venkatesh, 2008).

In sum, the research literature offers some reason to suspect differences in the predictors of drug and nondrug illegal earnings, although most general theories of crime (Gottfredson and Hirschi, 1990; Laub and Sampson, 2003; Sutherland, 1939) would predict relatively small shifts in the magnitude of predictors rather than a wholesale reversal of their effects.

Table 1. Descriptive Profile for Fixed Characteristics, National Supported Work Demonstration Project Data

Fixed Characteristics	Baseline Value (t_1)	Description
Male	89.3%	
African American	76.1%	
White	10.8%	
Hispanic or other	13.1%	
Youth sample	25.3%	"Youth dropout" sample
Addict sample	28.6%	"Ex-addict" sample
Offender sample	46.2%	"Ex-offender" sample
Number of cases	4,927	

DATA AND METHODS

DATA: NATIONAL SUPPORTED WORK DEMONSTRATION PROJECT

The data for this study were collected between 1975 and 1979 as part of the National Supported Work Demonstration Project (Hollister, Kemper, and Maynard, 1984), a job-creation program for persons with severe employment problems. The Supported Work (SW) data are well suited to examine how changing life circumstances affect illegal earnings. Because these data include information about month-to-month changes in legal and illegal activities, we specify and estimate fixed-effects models to examine how illegal earnings respond to changes in predictors such as employment status. Overall 4,927 "ex-offenders" (primarily referred by parole agencies in Chicago, Hartford, Jersey City, Newark, Oakland, San Francisco, and Philadelphia), "ex-addicts" (primarily referred by drug treatment agencies in Chicago, Jersey City, Oakland, and Philadelphia), and "youth dropouts" (referred from social service and education institutions in Atlanta, Hartford, Jersey City, New York, and Philadelphia) participated in the study. Respondents were given an initial baseline survey with follow-ups collected at 9-month intervals. All were tracked for at least 18 months, with subsamples followed for 27- and 36-month periods. The response rates varied from 77 percent (at 9 months) to 67 percent (at 36 months). Fixed characteristics for the sample are shown in table 1. Respondents were primarily African American males with extensive criminal histories and multiple barriers to employment.[1]

1. We compared characteristics of the SW respondents with state and federal prison inmates, represented in the 2004 Survey of Inmates in State and Federal Correctional Facilities (U.S. Department of Justice, Bureau of Justice Statistics, 2007). Relative to SW respondents, prison inmates are more likely to report illegal

DATA: NATIONAL LONGITUDINAL SURVEY OF YOUTH

We also use data from the 1997 National Longitudinal Survey of Youth (NLSY97) (Bureau of Labor Statistics, 2006) to conduct supplementary analyses. These NLSY data offer a more recent nationally representative sample of 8,984 people who were 12 to 16 years old in 1996. Round 1 of the survey was collected in 1997, with youth interviewed on an annual basis. We use the 1997–2003 data for a supplemental analysis of drug sales earnings.

DEPENDENT VARIABLES

For our fixed-effects analysis of remuneration, the dependent variable is the natural log of total dollars earned illegally each month from crime.[2] This variable is based on self-reports from Supported Work participants, who were asked the number of income-generating crimes they committed each month, the type of activity they did to earn this money, and the remuneration from each act. We report all legal and illegal earnings in constant 2004 dollars (U.S. Department of Labor, 2004). We first consider a global measure of *any* economic crime and then disaggregate drug and nondrug earnings. The nondrug earnings were generated from robbery, burglary, forgery, theft, shoplifting, prostitution, gambling, and similar offenses. Descriptive statistics for time-varying variables are displayed in table 2. For all respondents, including those who reported no illegal earnings, the average monthly illegal income is approximately $386 (in 2004 dollars). Among those who earned at least $1 illegally, the average illegal income is approximately $1,299, which consists of $492 from drug sales and $807 from nondrug income.

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earnings in the month before their incarceration (25 percent of inmates compared with 9 percent of SW respondents). Considerably more SW respondents reported welfare receipt compared with prison inmates in the month before their incarceration: 41 percent vs. 12 percent. Prison inmates also were more likely to be legally employed before their incarceration (66 percent vs. 29 percent), to be married (17 percent vs. 13 percent), to have higher educational attainment (10.8 years vs. 10.2 years), and to have more children (1.2 vs. 0.2) than SW respondents. Thus, SW respondents exhibit a lack of conventional embeddedness, although fewer report illegal income than the 2004 incarcerated population. SW respondents also reported more previous arrests (mean of 7.5 vs. 5.0 for prison inmates) and were more likely to be male (89 percent vs. 80 percent) and African American (76 percent vs. 43 percent) than were the incarcerated population (see also Uggen, Wakefield, and Western, 2005).

2. We added $1 to all earnings before logging to account for those without illegal earnings (the log of zero is undefined). Because most respondents reported relatively low earnings, we considered whether outliers were affecting our results. We top-coded any illegal earnings higher than the 90th percentile and reestimated our models. The results were similar to those presented in table 3 and did not alter the key findings presented in this article.

Table 2. Descriptive Profile for Time-Varying Characteristics (Months 1–36), National Supported Work Demonstration Project Data

Time-Varying Characteristics	Means or Percents	Description
Drugs and Money		
Illegal income (2004 $)	$385.87 (2155.81)	Average of monthly income from any illegal activity
Drug sale income (2004 $)	$146.04 (992.21)	Average of monthly income from drug sale income
Nondrug income (2004 $)	$239.84 (1784.74)	Average of monthly income from nondrug illegal activities (includes robbery, burglary, theft, shoplifting, prostitution, and gambling)
Illegal income among earners	$1299.07 (3802.68)	Monthly illegal income from respondents reporting at least $1 in illegal income
Drug income	$491.69 (1773.31)	Drug income among those who reported at least $1 in illegal income
Nondrug income	$807.47 (3204.06)	Nondrug income among those who reported at least $1 in illegal income
Drug use	7.7%	Monthly indicator for cocaine or heroin use
Earned legal income (2004 $)	$776.24 (1190.24)	Monthly legal earnings
Unearned legal income (2004 $)	$230.18 (395.92)	Monthly unearned income (Social Security, welfare, unemployment, etc.)
Opportunity Structure		
Incarceration status	11.8%	Monthly indicator for jail and/or prison
Unemployment rate	7.7%	Unemployment rate in each site
Illegal opportunities	1.22 (1.20)	How often nowadays do you have a chance to make money illegally? [3 = few/day; 2 = few/week; 1 = less than that; 0 = no chance]
Criminal Embeddedness		
Friend in full-time hustle	11.1%	Is closest friend unemployed and involved in drugs, hustles, or trouble with police? [% yes]
Arrests	7.46 (11.28)	Number of arrests
Age	25.71 (6.59)	Age
Conventional Embeddedness		
Ties to spouse/partner	20.3%	Cohabitation with spouse or partner
Regular employment	26.4%	Indicator of employment in a subsidized (nonprogram) job
Program employment	13.5%	Indicator of Supported Work employment
School attendance	5.1%	Indicator of school attendance
Subjective Risks and Rewards		
Perceived risk of prison	3.78 (1.50)	If you made $1,000 illegally, what do you think your chance would be of getting sent to prison if you were caught? [1 = low; 3 = 50/50; 5 = high]
Earnings expectations (2004 $)	$571.76 (263.82)	If you had to look for a job—keeping in mind your past experiences, your education, and your training—how much do you think you would earn per week, before taxes?

NOTE: Standard deviations are in parentheses.

INDEPENDENT VARIABLES

With regard to serious drug use, approximately 8 percent of respondents reported using either cocaine or heroin in any given month. We consider two legal income sources: legal earnings, which average $776 per month, and unearned legal income (including social security, unemployment, and public assistance), which averages $230 per month. Our drug earnings models also include nondrug illegal earnings as an independent variable (and vice versa) to test whether forms of illegal income are complements or substitutes. A negative coefficient would indicate that respondents with high nondrug illegal income (via burglary, for example) correspondingly reduce their drug sales. A positive coefficient, in contrast, would indicate complementarity and entrenchment in mutually reinforcing criminal activities.

We measure the structure of legal and illegal opportunity with three indicators. With regard to legal opportunity, we consider the local unemployment rate in each site. With regard to illegal opportunity, we use a dichotomous measure of incarceration in jail or prison for some portion of the month. Approximately 12 percent of the respondents were incarcerated at any given point during the study period. We consider also a more subjective measure of illegal opportunity: the frequency of perceived opportunities to obtain money illegally.

Other independent variables include measures of embeddedness in criminal and conventional activities. Approximately 11 percent of respondents indicated that their closest friend was involved in the full-time pursuit of drugs, hustles, or trouble with the police, which indicates association with delinquent peers. In addition, we use the number of arrests as an indicator of criminal experience. On average, respondents reported 7.5 arrests. We measure conventional embeddedness using links to conventional family, work, and educational institutions. At any given time, approximately 20 percent of respondents lived with a significant other, 26 percent were employed in a "regular" job they obtained on their own, and 14 percent were employed in a subsidized job as part of the Supported Work program. Finally, approximately 5 percent of respondents were enrolled in school.

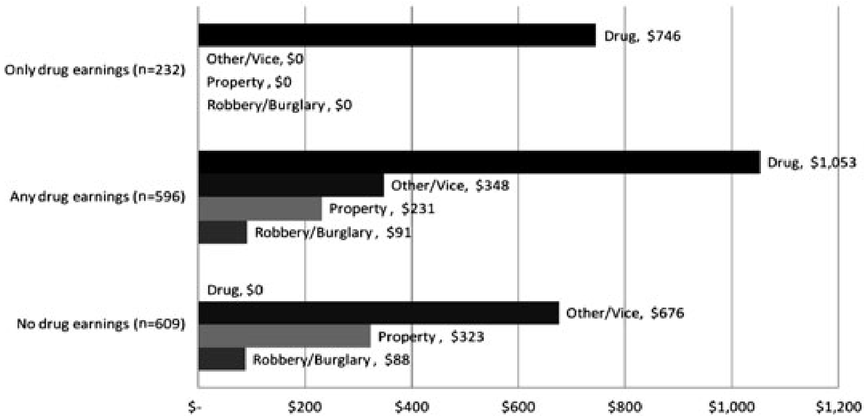
Our final set of independent variables concerns subjective risks and rewards. We measure respondents' perceptions of the likelihood of going to prison if caught committing an illegal act. Legal earnings expectations are indicated by the weekly amount respondents expect to obtain in legal employment. On average, respondents believed they could make $572 per week, or almost $30,000 per year, in a legal job (in 2004 dollars).

ESTIMATION AND RESULTS

AVERAGE MONTHLY EARNINGS

As shown in table 2, the average monthly illegal income for the Supported Work sample is $386, which is considerably less than average

Figure 1. Average Monthly Earnings by Offense Type, National Supported Work Demonstration Project Data



NOTE: "Property" refers to illegal income obtained via theft, shoplifting, and writing bad checks; "Other/vice" refers to illegal income obtained via victimless crimes such as gambling and prostitution.

monthly legal earnings ($776). The most common remuneration pattern—approximately 67 percent of the sample—involved only legal income. On average, this group reported $1,103 per month in income. The second most common category was a combination of legal and illegal income, with approximately 27 percent reporting this form of remuneration. Drug earnings made up 37 percent ($516) of their $1,381 average monthly illegal earnings, but they also reported $981 in legal income. Their average total monthly income of $2,362 significantly exceeded that of the 1 percent of respondents who relied exclusively on illegal earnings ($1,745, 31 percent of which came from drug sales). Also, 225 respondents (5 percent) reported no income, legal or illegal, during the study period. In sum, those who engaged in both legal and illegal income-generating behavior reported a higher monthly total income than those who specialized in either legal or illegal activity. Among respondents with any illegal earnings, drug income consisted of approximately one third of their total illegal earnings.

Because respondents who earn the most money are the ones engaging in a mixture of legal and illegal activities, we also consider whether this holds true for those reporting a diversity of illegal activities. Figure 1 compares 1) respondents who relied *solely* on drug sales for their income, 2) those who earned *any* money from drug sales, and 3) those *without* any drug income.

For each group, we report the average monthly income from drug sales, robbery/burglary, other property (including theft and writing bad checks), and other income-generating crimes (including vice crimes of gambling and prostitution).

Figure 1 shows that specializing in drug sales is not especially remunerative. Respondents relying *solely* on drug sales garnered the lowest total illegal earnings—approximately $746 per month. In contrast, respondents reporting *any* drug sales tended to report higher drug and nondrug earnings as well, averaging $1,723 per month from illegal sources. Those who *refrained* from drug sales but engaged in more predatory offenses such as burglary and robbery earned an average of $1,087. These descriptive results, thus, show that respondents engaging in a greater variety of illegal acts reported the highest income and that those who specialize in either drug distribution or nondrug earnings tended to report lower illegal earnings.

FIXED EFFECTS AND SEEMINGLY UNRELATED REGRESSIONS ESTIMATION

As noted, fixed-effects models are useful for examining how changes in circumstances affect illegal earnings. Most importantly, the fixed-effects estimator helps to correct for selectivity biases originating from stable characteristics that may be driving levels of independent variables as well as illegal earnings. That is, unchanging factors such as genetic endowment, cohort, or prior upbringing are statistically controlled because the model nets out fixed individual characteristics, including basic demographic variables such as race and gender. All models are estimated in Stata (StataCorp, College Station, TX), with standard errors corrected for clustering.[3] Each variable in this model can be conceptualized in terms of deviations from its person-specific mean value (Bushway, Brame, and Paternoster, 1999; England et al., 1988, Johnson, 1995; Uggen and Thompson, 1999; Waldfogel, 1997). To ensure proper temporal ordering, we lag all independent variables by 1 month, except for those that clearly exert a contemporaneous impact (incarceration status, in particular, but also the local unemployment rate, other sources of illegal income, and age).

The results of these fixed-effects models are displayed in table 3, which represents up to 36 months of data for Supported Work respondents 16

3. Models were estimated in Stata using the xtreg command with the vce (cluster clustvar) option. Clustering on the panel variable produces robust standard errors, which adjusts for serial correlation (Drukker, 2003; Wooldridge, 2002).

Table 3. Fixed-Effects Estimates of Logged Monthly Illegal Earnings from Specified Offense, with Robust Standard Errors, National Supported Work Demonstration Project Data

Variables	Total Illegal Income	Drug Sales Income	Nondrug Income
Drugs and Money			
Drug use	1.11**	.51**	.61**
	(.146)	(.114)	(.120)
Log earned legal income	−.01	−.00	−.01
	(.009)	(.007)	(.007)
Log unearned legal income	.01	.01	.00
	(.010)	(.008)	(.008)
Log other illegal income[a]		.12**	.14**
		(.017)	(.021)
Opportunity Structure			
Incarceration status	−.68**	−.25**	−.41**
	(.086)	(.057)	(.070)
Unemployment rate	−.02	−.00	−.02
	(.014)	(.011)	(.010)
Illegal opportunities	−.01	−.01	−.01
	(.028)	(.021)	(.020)
Criminal Embeddedness			
Friend in full-time hustle	.03	.02	.01
	(.109)	(.086)	(.087)
Arrests	−.14**	−.02	−.12**
	(.045)	(.028)	(.041)
Age	.004	.01	−.01
	(.041)	(.030)	(.031)
Conventional Embeddedness			
Ties to spouse/partner	−.12	.05	−.16
	(.107)	(.077)	(.087)
Regular employment	−.18**	−.09	−.11*
	(.065)	(.049)	(.049)
Program employment	−.25**	−.15*	−.15*
	(.082)	(.063)	(.060)
School attendance	−.01	.05	−.05
	(.091)	(.079)	(.067)
Subjective Risks and Rewards			
Perceived risk of prison	−.01	.02	−.03
	(.024)	(.018)	(.019)
Log earnings expectations	−.08	.01	−.13
	(.089)	(.061)	(.073)
Adjusted R^2	.483	.416	.493
Number of Observations	58,929	58,928	58,928

NOTES: Numbers in parentheses are standard errors. Shaded cells indicate statistically significant differences between types of income, estimated using seemingly unrelated regression (see table S.1).
[a]Includes all types of illegal income, excluding the dependent variable.
$^{*}p < .05$; $^{**}p < .01$ (two-tailed tests).

to 60 years of age.[4] The model in the first column shows the effects of the independent variables on *total* illegal income. The two other columns display crime-specific results to indicate whether embeddedness, opportunity, and subjective perceptions exert similar or different effects on income from drug sales and from nondrug criminal earnings.[5] To obtain significance tests for these differences, we estimated seemingly unrelated regressions (SURs), clustered on individual respondents. This approach allows for correlation in the error terms of the equations predicting drug and nondrug illegal earnings, permitting cross-equation tests of the equality of coefficients. We centered our independent variables on the individual means to match closely our fixed-effects estimation strategy. Significant differences between the predictors of drug earnings and the predictors of other illegal earnings are denoted by shading in table 3. The full SUR equations and chi-square tests (with 1 degree of freedom) for significant differences across equations are shown in table S.1 in the online supporting information.[6]

Our first model examines predictors of any illegal income, whether from drug sales or from nondrug offenses. Here, drug use increases illegal earnings, whereas incarceration, arrests (our proxy for criminal experience), and conventional work all reduce illegal earnings. Comparing estimates for drug and nondrug income in the second and third columns of table 3, we find

4. Because of concerns that the inclusion of zero-earners could alter the reported results, additional analyses were conducted on the subgroup of "earners"—those who reported at least $1 of any economic crime. The results (shown in table S.2 of supplemental online materials) mirror those reported in table 3, with slight differences. The primary difference is that the magnitude of the coefficients tends to be larger among the "earners" sample.

5. Some suggest that fixed-effects models understate effects, for "if the independent variable is an imprecise measure of the relevant factor, coefficient estimates from these models can be severely attenuated toward zero" (McKinnish, 2008: 352). McKinnish (2008) suggested comparing fixed effects and first-differences models to check for misspecification. Because our fixed-effects model estimates deviation from individual means and does not explicitly test the impact of specific changes, we conducted a supplemental analysis using a first-difference specification to consider the impact of short-term (month-to-month) changes in behaviors, attitudes, and social characteristics. In our standard first-difference models (in which 1 month elapses between observations), we find a similar pattern to that presented in table 3, although the coefficients are understandably smaller in first-difference specifications (shown in table S.3 of the supporting information). Nevertheless, the primary findings remain the same: Beginning to use (or resuming use of) an illegal substance increases both drug income and nondrug income. Examining the "other illegal income" measure, we find that higher drug sale income increases nondrug income significantly.

6. Additional supporting information can be found in the listing for this article in the Wiley Online Library at http://onlinelibrary.wiley.com/doi/10.1111/crim.2012.50.issue-4/issuetoc.

four significant differences: other illegal income, incarceration, arrests, and age. We anticipated a larger effect of drug use on *drug* income, but the results do not indicate a significant difference between the two forms of illegal income. Clearly, cocaine and heroin users do not limit their illegal activity to consensual exchanges of drugs and money. Instead, they are equally likely to increase their nondrug illegal earnings in the next month. One (admittedly post hoc) explanation for this pattern is that drug earnings are lost when dealers consume rather than distribute their product. Jacobs (1999), for example, noted that users are "forever cannibalizing their own supply, jeopardizing any chance they might have to move up the dealing ladder or to stay up there very long" (p. 35). Drug suppliers also may realize that users are less effective dealers, and limit the quantity of drugs supplied to active users, resulting in fewer opportunities to earn money from drug sales.

Although we expected legal income (both earned and unearned) to reduce all types of illegal income, we find no statistically significant impact, net of the fixed effect and other independent variables. Note, however, that these models also include terms for legal employment status. The legal earned income coefficient thus represents income changes in compensation or job changes, *net* of employment in a regular or Supported Work program job. We included a term for "other" forms of illegal income to learn whether nondrug economic crime is a complement or substitute for drug crime (and vice versa). The results clearly support the former interpretation, as illegal income from other sources increases crime-specific earnings dramatically. Drug income, however, exerts an especially large effect on nondrug income in these analyses (chi-square of the difference = 10.44).

Because our dependent variable is log transformed, the coefficients in table 3 can be interpreted in terms of a percentage change in the dependent variable (Wooldridge, 2009). For the independent variables in their original metric (e.g., drug use, incarceration status, and unemployment rate), this means that multiplying the estimated coefficient by 100 will yield a percent change interpretation. Thus, in the "any illegal income" model, respondents who use drugs in a given month increase their illegal earnings by 111 percent. For the effect of logged independent variables on logged dependent variables—a relationship referred to as "elasticity" in economics—we interpret the elasticity as the percent change in illegal earnings when the independent variable increases by 1 percent. Thus, a 1 percent increase in nondrug illegal income corresponds to a .12 percent increase in drug sale income. Even though several other coefficients have a statistically significant impact on illegal earnings, we should note that most predictors are considerably smaller in magnitude than that of drug use.

The opportunity structure measures in table 3 show a consistent incapacitative effect of incarceration. This effect is especially pronounced for nondrug earnings, which are reduced by approximately 41 percent during incarceration spells (testing for a difference in the incarceration coefficient, chi-square = 4.0). Because the risk of jail or prison is highest in months of intensive criminal activity, respondents generally report higher illegal income in the period immediately preceding incarceration. The local unemployment rate and our final measure of opportunity structure—changes in perceived illegal opportunities—do not significantly affect illegal earnings from either drug sales or nondrug crimes.

The peer associate indicator of criminal embeddedness, measured as ties to friends involved in a "full-time hustle," does not significantly affect illegal earnings. Although we expected criminal experience to increase illegal earnings, we find instead a negative effect of arrests on illegal income. In particular, each additional arrest is associated with approximately 12 percent less nondrug illegal income. Monthly illegal earnings seem unaffected by age in this sample, net of the other variables in the model.[7] We also sought evidence of offense-specific effects for our conventional embeddedness measures. Consistent with adult social bond perspectives, we find that supported employment reduces both forms of illegal earnings, although living with a spouse or partner is not a significant predictor in these models.[8] Thus, whereas conventional embeddedness is generally important in reducing illegal income, we observe a greater effect for employment than for family ties.[9] Finally, we find little effect of school attendance or perceived risks and rewards, net of the other variables in the model.

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7. We also estimated separate models with squared arrest and age terms to model curvilinear relationships. The results of these quadratic specifications are consistent with expectations of diminishing returns as respondents age and experience a greater number of arrests, but inclusion of these terms fails to improve model fit significantly (see table S.4 of the supporting information).
8. The experimental effect of supported employment has been examined elsewhere (see, e.g., Hollister, Kemper, and Maynard, 1984; Uggen, 2000). We conducted supplementary analyses of drug and nondrug earnings for both the job treatment and the control group, finding no significant differences.
9. When these models are estimated on raw income rather than on log income, however, ties to spouse or partner emerges as a stronger predictor (Uggen and Thompson, 2003). Because Horney, Osgood, and Marshall (1995) found that living with a wife is associated with lower offending whereas living with a girlfriend is associated with higher offending, we attempted to separate cohabitation from marriage. Unfortunately, the Supported Work month-to-month data only indicate whether respondents were living with a spouse/partner. What is available, however, is a measure of marital status at the beginning of data collection. We thus created "married" and "cohabiting" dummy variables based on marital status at baseline. Respondents who were married at baseline were considered married in each subsequent month that they reported living with a significant other. Respondents who were not married at baseline were considered to be cohabitating (and unmarried) in each subsequent month that they reported living with a significant

In sum, we can identify only a handful of significant differences in the predictors of drug income as opposed to nondrug income, and these generally represent differences in degree rather than differences in kind. For example, income from drug sales has an especially large effect on nondrug income. Nevertheless, the reverse also is true, albeit to a lesser extent. Incarceration also exerts a somewhat larger impact on nondrug income. Respondents who are incarcerated report less illegal income from all sources than other respondents, but their nondrug income declines most steeply during months of incarceration. The effects of arrest and age also differ significantly between the two models (chi-squares of 5.76 and 6.22, respectively), although age is not a significant predictor in either equation. Arrest, in contrast, is a statistically significant negative predictor of nondrug income but not drug income.[10]

GENERALIZABILITY OF THE SUPPORTED WORK FINDINGS

Our Supported Work data were collected from 1975 to 1979, which precedes the crack and methamphetamine eras and predates many social changes that may alter our results and interpretations. We, therefore, test our basic model's capacity to explain drug income among a more recent cohort—the 1997 National Longitudinal Survey of Youth (NLSY97) (Bureau of Labor Statistics, 2006). From this nationally representative sample of approximately 9,000 youth 12–24 years of age, we use the 1997–2003 data for the supplemental analysis shown in table 4.

For purposes of comparison, we trimmed variables that are unavailable in the NLSY from our basic Supported Work model. To make these data sets more comparable, we also eliminated any SW respondents who were older than 24 years of age and any NLSY respondents younger than 16 years of age. Our two new analytic samples thus consist of respondents 16–24 years of age in 1975–1979 (36 months of SW data) and respondents 16–24 years of age in 1997–2003 (7 years of NLSY data).

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other. We then substituted these two variables for our "ties to spouse/partner" measure in table 3. In our fixed-effects analysis with these new variables, neither the "married" nor the "cohabiting" dummies were significant in any of our models.

10. We also estimated Cragg or hurdle models for truncated regression. These do not include fixed effects but are useful for distinguishing the predictors of participation in illegal economic activity from predictors of the amount earned illegally. With regard to participation, drug use, incarceration, illegal opportunity, peer associations, arrests, age, employment, and perceived risk are all significant predictors in the expected direction. With regard to the amount earned, only drug use, earned and unearned legal income, perceived risk, and earnings expectations are statistically significant. Although we observed some differences in the factors predicting participation in drug and nondrug illegal earnings, there were again few differences in the amount earned illegally in each category (see tables S.5 and S.6 of the supporting information).

Table 4. Fixed-Effects Estimates of Logged Monthly Illegal Earnings from Drug Sales: 1975–1979 Supported Work Data versus 1997–2003 NLSY Data, with Robust Standard Errors

Variables	SW data (16–24 years of age)	NLSY data (16–24 years of age)	NLSY Data, Arrested Respondents[a] (16–24 years of age)	NLSY Data, Arrested Respondents[a] (12–24 years of age)
Drugs and Money				
Drug use	.51*	.14**	.10	.23**
	(.200)	(.046)	(.075)	(.071)
Log earned legal income	−.00	−.00	.01	.01
	(.007)	(.005)	(.014)	(.012)
Log unearned legal income	.00	−.00	−.01	−.02
	(.009)	(.004)	(.011)	(.010)
Log other illegal income[b]	.13**	.28**	.33**	.33**
	(.025)	(.019)	(.024)	(.020)
Opportunity Structure				
Unemployment rate	−.00	−.01	−.02	−.03**
	(.013)	(.004)	(.013)	(.010)
Criminal Embeddedness				
Arrests	−.07*	.04	.03	.08*
	(.035)	(.043)	(.042)	(.039)
Age	.06	−.003	−.01	.06**
	(.043)	(.01)	(.016)	(.012)
Conventional Embeddedness				
Ties to spouse/partner	−.03	−.00	.04	−.02
	(.104)	(.038)	(.119)	(.109)
Any employment	−.09	−.01	−.11	−.09
	(.057)	(.039)	(.109)	(.088)
School attendance	−.01	−.05*	−.12*	.01
	(.111)	(.021)	(.057)	(.045)
School × Age Interaction				
Respondent post-high-school age (19+ years of age)	−.04	−.05	−.11	−.24**
	(.060)	(.030)	(.075)	(.068)
School attendance × post-high-school age	−.01	.09**	.23**	.17*
	(.162)	(.028)	(.076)	(.069)
Adjusted R^2	.463	.312	.308	.287
Number of Observations	32,278	41,142	12,322	15,562

NOTE: Numbers in parentheses are standard errors.
[a]Limited to NLSY respondents who reported at least one arrest during the study period.
[b]Includes all types of illegal income, excluding the dependent variable.
*$p < .05$; **$p < .01$ (two-tailed tests).

We limit this analysis to drug sale income because the SW questions that inquired about other illegal earnings are not comparable with those in the NLSY survey. We then estimated the same basic model using both the SW and the NLSY data. Focusing on the first two columns of table 4, we find a congruent pattern between the two data sets, aside from some differences in the magnitude of the coefficients. In both cases, illegal drug use significantly increases drug earnings, both in the following month (SW data) and in the following year (NLSY data). This effect is larger for the SW participants than for the NLSY participants, which may reflect the shorter duration between measures in the SW data. In this analysis, logged nondrug illegal income exerts a larger impact in NLSY than in SW, although the relationship is positive in both cases.

Because the meaning of school attendance varies considerably between 16 and 24 years of age, we examine the relationship between school attendance and age in greater detail. These models, thus, include interactions for school attendance and a dummy for whether the respondent was older than high-school age (19 or more years of age). Among the SW respondents, being older and attending school had little impact on drug earnings. For NLSY respondents, however, those who began or resumed attending school after 18 years of age significantly increased their drug earnings in the following year. At the same time, however, attending school at a younger age (16–18) decreases drug sale income by 5 percent in the following year.

We next attempt to make the NLSY data even more similar to the SW data by selecting only respondents with a history of arrest. These previously arrested NLSY respondents, represented in the final two columns of table 4, are more comparable with the SW respondents who have extensive criminal histories. The final column of table 4 then adds the youngest NLSY respondents back into the analysis to show effects among the full NLSY age range. Among this subsample of arrested NLSY respondents, we observe results similar to those for both the total NLSY sample and the SW sample as well as some important differences. Arrests, for example, work in opposite directions: Each additional arrest reduces drug earnings in the following month by 7 percent in the SW data, whereas each arrest *increases* drug earnings by 8 percent in the following year in the NLSY arrested data (12–24 years of age). We suspect that this difference is in part a result of the highly selective Supported Work sample, in which virtually all participants had extensive official criminal histories. In such a sample, arrest is likely a better proxy for contact with the criminal justice system than for criminal experience. In contrast, the NLSY sample with younger respondents is more likely to consist of inexperienced offenders, and each arrest may represent accumulated criminal experience, culminating in a higher level of drug sale income.

We also find strong and consistent evidence—across all three NLSY models—for a significant difference in the effects of school attendance by age, as reflected in the positive interaction coefficients. This interaction seems especially important among those with histories of arrest and, presumably, with more extensive criminal networks. These NLSY findings seem compatible with market-based approaches to drug earnings. To the extent that drug sellers are embedded in criminal networks, find a customer base among fellow students, and avoid "cannibalizing" their own supply, their illegal earnings are likely to rise.

Because all stable characteristics are statistically controlled (and hence invisible) in our fixed-effects results, we also examined the race and gender distribution of drug earners and nondrug earners in the SW and NLSY samples. Compared with those reporting other forms of illegal earnings, drug earners tend more often to be White (in the NLSY data) and less often to be Black (in the SW data). To the extent that implicit racial biases affect perceptions about crime severity and dangerousness (Tonry, 2011), such race differences may account, in part, for beliefs about the distinctiveness of drug crimes. We did not find significant gender differences, however, in comparing drug earners versus nondrug earners in the two samples.

Regarding the generalizability of the Supported Work findings, we conclude that despite some differences, the general pattern is quite similar between the two data sets. The composition of the samples, of course, is considerably different—with the SW data characteristic of the urban underclass in the 1970s and the NLSY data representative of the general population in the 1990s to 2000s. Nevertheless, the basic model of drug earnings that held for Supported Work also seems to hold for a more contemporary and representative sample of respondents.

DRUGS, EARNINGS, AND SPECIALIZATION

Although our analytic strategy is not designed to answer questions about whether individuals specialize, this study speaks to literatures in offense specialization (e.g., Osgood and Schreck, 2007; Sullivan et al., 2006), showing how changing life circumstances affect remuneration for two forms of income-generating crime. We find common predictors for drug and nondrug earnings, suggesting that no bright line separates the two phenomena.[11] The strong general effect of cocaine and heroin use on illegal earnings challenges arguments that drug use only engenders victimless crime within the drug economy. We instead find evidence that cocaine and heroin use corresponds to an across-the-board increase in all forms of illegal earnings. We also find that conventional embeddedness and adult bonds to employment generally reduce illegal earnings, consistent with Sampson and Laub's (1993) informal social control theory, although ties to spouses and partners have less impact in our models. In sum, we find that different forms

11. The results presented in table 3 show few differences in the predictors of drug and nondrug income, although our supplemental analyses revealed some potentially distinctive predictors among the small number reporting robbery and burglary income.

of illegal earnings share common determinants, that most “drug earners” also engage in other forms of income-generating crime, that drug earnings and other illegal earnings are complements rather than substitutes, and that drug earners who also engage in other forms of income-generating crime earn more than those who specialize in the sale of drugs.

Focusing specifically on drug earnings, the positive relationship between school attendance and drug income—which is statistically significant only for older respondents in the general NLSY sample—may be partially explained by the drug markets that schools provide and, perhaps, the potential debt burdens associated with enrollment (Cureton and Bellamy, 2007). We also might note in this regard that student dealers are less likely than other students to perform well in school or to report long-term academic or vocational goals (Black and Ricardo, 1994; Little and Steinberg, 2006; Uribe and Ostrov, 1989). In contrasting the two data sets, we also found differences in the effect of arrests, our proxy for criminal experience. In Supported Work, arrests reduced illegal income in the following month. In the more advantaged NLSY sample, arrests had either a nonsignificant or a positive effect on drug earnings in the following year.

Although this analysis provides provisional answers to questions about drug and nondrug illegal earnings, we must note several important caveats. First, the validity and reliability of self-reported illegal earnings data are not well established; however, the Supported Work data may provide the best available information on changes in illegal earnings over time. Second, the offense data do not allow us to explore white-collar offenses such as fraud or embezzlement. Therefore, we cannot generalize these results beyond the urban poor and, to a lesser extent, the younger national cohort represented in the NLSY. Third, even our fixed-effects estimates may be biased by potential endogeneity. Prior levels of illegal earnings could be driving drug use, for example, which in turn increases illegal earnings. Finally, our measures of some hypothesized theoretical mechanisms are cruder than we would like. For example, our embeddedness measures are dichotomies that cannot speak to the strength of ties, whereas our deterrence measure (perceived risk of prison) is general rather than offense specific. Despite these important caveats, we believe that our main conclusions are sound and well supported by the data.

To make more definitive statements regarding similarity and dissimilarity across crime types, future research will require at least two data improvements:

1. More variation in the types of criminal activity available for analysis. This would include analysis of personal, noneconomic offenses and white-collar offenses.

2. More nuanced indicators of adult social bonds that represent deeper levels of attachment to peers, spouses, and employment (Laub and Sampson, 2003; Sampson and Laub, 1993: 21; Warr, 1998).

CONCLUSION

We began by asking whether and how illegal earnings attainment differs between drug sales and other forms of economic crime. Our results show that people sell drugs for much the same reasons they participate in other forms of economic crime—their illegal opportunities are expanding relative to their legal alternatives and they are increasingly embedded in criminal rather than in conventional networks. We, thus, find considerably greater evidence for fungibility than specialization, with common predictors explaining the earnings of dealers and thieves. These results speak to the specialization literature by suggesting that drug earnings and other illegal earnings are complements rather than substitutes. In particular, when people begin earning more from drug sales, they also begin earning more from other forms of illegal activity.

Cocaine and heroin use have a robust positive effect on both forms of illegal earnings, creating an earnings imperative that extends well beyond the voluntary transactions of the drug economy. Consequently, little in our analysis would suggest the negative effects of drug use are limited to willing participants. Instead, we find evidence that people sell drugs or commit other economic crimes for the same reason that Willie Sutton robbed banks—because that's where the money is (Sutton, 1976).

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SUPPORTING INFORMATION

The following supporting information is available for this article:

Table S.1. Seemingly Unrelated Regression Coefficients, with Independent Variables Centered on Individual Means, National Supported Work Demonstration Project Data

Table S.2. Earners Only Analysis: Fixed-Effects Estimates of Logged Monthly Illegal Earnings from Specified Offense, with Robust Standard Errors

Table S.3. First-Difference Models: Models Regressing One Month Differences in Monthly Illegal Earnings on Differences in Selected Variables

Table S.4. Fixed-Effects Estimates of Logged Monthly Illegal Earnings from Specified Offense, with Robust Standard Errors, with Arrest-Squared and Age-Squared

Table S.5. Truncated Regression Estimates (Predictors of Any Illegal Earnings) [Tier 1 from Cragg Models], with Robust Standard Errors

Table S.6. Truncated Regression Estimates (Expected Illegal Earnings Conditional on Any Illegal Earnings) [Tier 2 from Cragg Models], with Robust Standard Errors

Supporting Information may be found in the online version of this article.