SUBSTANCE USE, DRUG TREATMENT, AND CRIME: AN EXAMINATION OF INTRA-INDIVIDUAL VARIATION IN A DRUG COURT POPULATION

DENISE C. GOTTFREDSON, BROOK W. KEARLEY, SHAWN D. BUSHWAY

This study examines the association between substance use and crime by modeling change within subjects over an 11 month period in a sample of 157 chronic drug-using offenders. For this sample, increased substance use—cocaine or heroin use as well as alcohol use—was significantly related to increases in self-reports of income generating but not violent crime. The study also demonstrates a significant effect of drug treatment in the last month on income generating crime, but not on violent crime and that the effect of drug treatment on income generating crime is mediated by reductions in drug use. This work refines prior work by showing that drug use effects vary by crime type and by providing further evidence that drug treatment reduces cocaine and heroin use, which leads to a reduction in property crime. It is the first study to examine variability over time in all three components (drug treatment, drug use, and crime) while adequately controlling for individual level propensity variables.

INTRODUCTION

Addiction and drug-related crime are two of the most intractable social problems facing the United States. The link between drugs and crime has been a topic of sustained interest to scholars and policy makers alike, as is evidenced by a large volume of literature—from seminal works such as Terry and Pellens' *The Opium Problem* (1928) to more contemporary works such as Tonry and Wilson's *Drugs*

Denise C. Gottfredson, Ph.D., is a professor at the University of Maryland Department of Criminal Justice and Criminology. Gottfredson's research interests include delinquency and delinquency prevention, and particularly the effects of school environments on youth behavior. **Brook W. Kearley** is a researcher at Social Solutions, Inc. Her primary research interests include the evaluation of drug treatment processes and outcomes, the organization of service delivery systems, and research methods. **Shawn D. Bushway**, Ph.D. is an associate professor at the University at Albany, School of Criminal Justice. His current research focuses on the economics of crime, the process of desistance, and the distribution of discretion in the criminal justice system.

and Crime (1990). Within this research there is considerable debate regarding the dynamics of the drug-crime relationship. Three explanations for the relationship have emerged: (a) drug use leads to crime, (b) crime leads to drug use, and (c) the drug/crime relationship is explained by a set of common causes (Gorman & White, 1995). These explanations are not mutually exclusive.

Goldstein (1985) identified three ways in which drug use leads to criminal activity. First, the pharmacological model, proposes that the effects of intoxication (e.g., disinhibition, poor judgment, cognitive-perceptual distortions) and its byproducts (e.g., withdrawal, enhancement of psychopathological disorders, sleep deprivation) cause criminal behavior. Second, the economic motivation model assumes that drug users commit income-generating crimes such as robbery, burglary, and drug sales in order to support their drug habits. The third model, the systemic model, suggests that the system of drug distribution and use is intrinsically linked with violent crime through activities such as "turf" skirmishes, assaults to collect debts, and robberies of dealers or buyers. Recent reviews of the drug crime literature point out that empirical support exists for all three explanations (MacCoun, Kilmer, & Reuter, 2003; White & Gorman, 2000).

Those who believe that crime leads to drug use assume that deviant individuals are more likely than nondeviant individuals to find themselves in social situations in which drug use is condoned and/or encouraged and that involvement in such a subculture provides the context for drug use (Collins & Messerschmidt, 1993; White, 1990). In addition, some researchers propose that deviant individuals may use drugs as a form of self-medication or to provide an excuse for their deviant acts (Collins, 1993; Khantzian, 1985).

The common cause explanation of the drug-crime relationship posits that drug use and crime do not have a direct causal link but instead are related by a number of common causes. These common causes include genetic or temperamental traits, antisocial personality disorder, parental alcoholism, and poor relations with parents (White, Brick, & Hansell, 1993). Subcultural norms, which promote "street" behavior, may also reinforce both criminal behavior and drug use (Gorman & White, 1995). Additional explanations include environmental causes, such as poor, densely populated neighborhoods that lack social capital and situational causes such as bars and sporting events where there is an increased number of motivated offenders and suitable targets (Ensminger, Anthony, & McCord, 1997; Fagan, 1993; Sampson, Raudenbush, & Earls, 1997; Skogan, 1990).

This study tests hypotheses about the association between substance use and crime by modeling change within subjects over time among a sample of drug offenders. In addition, it tests the mediating mechanism through which involvement in drug treatment reduces criminal activity in this population. No prior studies have examined variability over time in all three components (drug treatment, drug use, and crime) while adequately controlling for individual-level propensity variables.

THE DRUG-CRIME RELATIONSHIP

Several studies have provided support for a "drugs to crime" causal relationship. In a summation of 25 years of research conducted at UCLA's Drug Abuse Research Center, Anglin and Perrochet (1998) concluded that crime was an inherent part of illegal drug use and that the commission of property crimes almost always increased to support dependence level use of heroin, cocaine, crack, amphetamine and even marijuana. In a similar review of over 30 years of research in Baltimore, Nurco (1998) noted that during periods of narcotic addiction individual crime rates were six times higher than during nonaddiction periods. The author also found support for different types of addicts: those who were more generally deviant and those who were economically motivated. Harrison (1992) also concluded that crime was greatly increased during periods of narcotic use. Numerous other studies echo these findings by showing an increase in crime during periods of drug use (Caulkins, Rydell, Schwabe, & Chiesa, 1997; Chaiken & Chaiken, 1990). Although this research on substances other than alcohol has generally emphasized the economic motivation model and effects of property crimes, some evidence suggests that certain drugs (e.g., amphetamines) may also increase violent crimes via a pharmacological effect (Gelles, 1994).

Research on alcohol use and crime, on the other hand, has more often suggested a direct pharmacological effect of alcohol use on violence and hence a larger effect on violent crimes than on property crime. A number of controlled laboratory studies have shown that alcohol intoxication is related to aggression when the subject is provoked (Bushman, 1997; Exum, 2002; Giancola et al., 2001). In addition, statistics related to alcohol use by violent offenders generally show that about half of all homicides and assaults are committed when the offender, victim, or both have been drinking (Collins & Messerschmidt, 1993). Rates of homicide and other violent crime have also been related to alcohol availability and per capita consumption (Cuellar, Markowitz, & Libby, 2003; Parker, 1993; Parker & Rebhun, 1995).

While these studies document an association between drug use and subsequent criminal involvement, the analytic techniques employed have, in general, not entirely ruled out preexisting differences in unmeasured factors as potential alternative explanations for the observed relationships. Some studies also fail to establish that the substance use actually preceded the crime (e.g., a cross-sectional association of violent behavior and alcohol use may as likely arise from an increase in alcohol use after a fight as vice versa). For these reasons, some researchers continue to believe that unmeasured variable(s) lead to both drug use and crime, and that once included in the model, the drug/crime relationship would be explained by these common causes.

In perhaps the most thorough examination of the causal relationship between alcohol use and violent behavior, Lipsey, Wilson, Cohen, and Derzon (1997) use meta-analytic techniques to summarize what is known about the causal relationship between alcohol consumption and violence. They examine laboratory studies of animals and humans in which alcohol consumption was experimentally manipulated as well as correlational studies relating alcohol consumption to violence. They conclude that although there is no doubt that alcohol use is positively correlated with violence, this relationship is confounded by other factors such as socio-demographics and individual temperament. They report that only a small number of studies have even attempted to control for these factors, and that even these studies have employed only a limited set of controls. Thus, they conclude, no confident causal interpretations can be drawn from this body of research, and any effect of alcohol use on aggression is likely to be person- and situation-specific.

Fagan (1990), reviewing literature that relates intoxication more generally to aggression, underscores the complexity of the relationship. He states that “rather than being a linear process, aggression following intoxication is more likely to be a reciprocal process in which expectancies and physiological factors, social norms, events in specific situations where substances are used, and cultural factors have multiple and recursive interactions leading to aggressive or nonaggressive behaviors when intoxicated” (p. 300). Indeed, recent laboratory research on the interplay of disposition and alcohol use is now beginning to show that alcohol use increases aggression only for persons with a high initial disposition towards aggression (Giancola, 2002).

Recent methodological advances in the modeling of panel data increase the ability to control at least for the enduring individual characteristics that might explain both drug use and crime. By controlling for “unobserved heterogeneity” in models of the change in criminal behavior over time, these models can rule out stable criminal propensity as a cause of variation over time in crime, although they cannot rule out unmeasured situational variables of importance. Horney, Osgood, and Marshall (1995) were among the first to employ such a model to examine the drug-crime issue. They found, in a sample of convicted felons, that the use of illegal drugs was significantly related to four measures of offending, including drug dealing, property crime, assaults, and a summary index of all three crime types. Heavy alcohol use was also positively related to all four types of crime, but significantly so only for property crimes. Using a similar model, Uggen and Thompson (2003) found that drug use, particularly cocaine and heroin use, led to significant increases in individuals’ illegal earnings in a sample of offenders, drug addicts, and youth dropouts. Jofre-Bonet and Sindelar (2001) also found that reduced drug use (as a result of treatment) reduced income generating crime. They used a "first differences" approach to studying pre- to post-change in a sample of addicts who received drug treatment, but only two data points were available in this study. It was necessary to assume that changes in drug use were uncorrelated with the error terms in the model, an assumption that may not be tenable.

Recent research on family violence has also employed within-subject designs to study the effects of alcohol use on violent behavior. Research on newlywed couples using assessments of both partners at multiple time points have shown that, among newlywed couples who reported both verbal and physical aggression during their first year of marriage, husbands' alcohol use was higher preceding violent aggression incidents than preceding nonviolent incidents (Leonard & Quigley, 1997). An analysis of follow-up data from the same couples at the time of their third wedding anniversary contrasted alcohol-related and nonalcohol-related violent events reported by the same couples (Testa, Quigley, & Leonard, 2003). This study found that wives (but not husbands) reported that violent episodes in which husbands were drinking included more acts of violence and were more likely to involve severe violence than violent events in which the husband was not drinking. The inconsistency in the husband and wife reports, though, leaves open the possibility that the results are explained by wives perceiving that their husbands are more violent when they drink when they in fact are not.

Using hierarchical linear modeling in which daily aggression and alcohol use equations (level 1) were nested within persons (level 2), Fals-Stewart (2003) analyzed 15 months of daily diary entries recording violent behavior and alcohol use made by men in domestic violence treatment programs and alcoholism treatment programs. He found that the likelihood of male-to-female physical aggression was substantially higher on days of drinking by male partners than on days of no drinking, even controlling for levels of relationship disharmony and the extent and severity of husband alcohol use in general. Finally, Murphy, Winters, O'Farrell, Fals-Stewart, and Murphy (2005) interviewed male alcoholic patients and their female partners about specific episodes of partner aggression. Only subjects who reported one or more husband-to-wife acts of physical aggression were included in the study, and the situations surrounding violent and nonviolent conflicts were compared within subjects. This study assessed quantity of alcohol consumed in the 12 hours preceding each conflict situation. The study found that the number of alcoholic drinks consumed, but not the use of other drugs in addition to alcohol use, was higher preceding violent than nonviolent conflicts. In this study, results were similar for both husband and wife reports. These within subject analyses of domestic violence and alcohol use help to rule out the common cause alternative explanation for the association between alcohol use and violent behavior among spouses. However, they do not address the generalizability of the effects to violent crimes in general, and they do not establish an effect for other drugs.

Drug Treatment/Drug Treatment Courts

Drug treatment is a popular strategy for reducing the levels of both addiction and crime. Numerous studies in a variety of populations have found that the use of sufficiently intensive and appropriately applied treatment reduces drug use and crime (Anglin & Perrochet, 1998; Rettig & Yarmolinsky, 1995). For example, in a study of 96 drug treatment programs nationally, researchers found that participation in long-term residential treatment significantly reduced the frequency of drug use and predatory illegal activities in the year following treatment (Fletcher, Tims, & Brown, 1997). At the macro level, research has shown that individuals who lived in areas with a greater supply of treatment services had lower probabilities of crime (Cuellar et al., 2003).

Notwithstanding this large body of research supporting the effectiveness of drug treatment for reducing crime, a National Research Council 2001 report on drug policy data and research suggested that claims for the effectiveness of drug treatment are "sometimes based on misleading or ambiguous research" (p. 244). They cite several artifacts that might provide alternative explanations for the observed findings in typical drug treatment studies, including selection and regression to the mean. To strengthen the research base for drug treatment policy, the committee recommended a shift towards greater funding of randomized controlled clinical trials in which treatment and no-treatment conditions are contrasted to test the effectiveness of drug treatment. They specifically recommended that such trials be carried out in criminal justice settings, in which the need for drug treatment is high and adding a drug treatment component (for randomly selected clients) to the existing criminal justice sanctions is feasible. Our study uses a criminal justice system-involved sample of chronic drug users.

Not surprisingly, much of the recent evidence favoring drug treatment as a crime prevention tool comes from such studies conducted within the criminal justice system. For example, Drug Treatment Courts (DTCs) combine sanctions, drug treatment, and probation services for drug-involved offenders in an attempt to reduce levels of substance use and crime. A few studies have randomly assigned clients to receive DTC services or not. Deschenes, Turner, and Greenwood (1995) compared randomly assigned DTC participants to three samples with varying levels of drug testing coupled with supervision and found that DTC participants recidivated at approximately the same rates as the comparison group samples. In this study, the DTC participants were more involved in treatment and counseling during the one-year follow-up period, but less involved in other constructive activities such as employment, community service, payment of fines and restitution, and formal education than the controls, suggesting that the treatment component of the program was not highly effective. Harrell, Cavanagh, and Roman (1998) evaluated a pretrial drug court by comparing offenders who were randomly assigned to receive either drug treatment, drug testing, and judicial monitoring (the drug court); drug testing with graduated sanctions and judicial monitoring; or drug testing and judicial monitoring only. The defendants on both the drug court docket and the docket that included graduated sanctions were significantly less likely to test positive for drugs in the month before sentencing compared with offenders who were not subject to sanctions for noncompliance. This suggests that the graduated sanctions element of the drug court program may be effective with pretrial releasees regardless of whether or not it is coupled with drug treatment. These two studies suggest that drug testing and sanctions may be as effective as a program that also involves mandatory treatment, although in both studies the treatment component was poorly implemented.

Reports from the Baltimore City DTC, also studied using an experimental design, show more positive results for drug treatment. This study has shown positive effects of the DTC on recidivism at 12, 24, and 36 months after being randomly assigned to study conditions (Gottfredson & Exum, 2002; Gottfredson, Najaka, & Kearly, 2003; Gottfredson, Najaka, Kearly, & Rocha, 2006). Gottfredson and colleagues (2005) also documented significant effects of participation in the DTC program on substance use, using client self-reports. In an attempt to isolate the effects of drug treatment per se in the effectiveness of the DTC program, Gottfredson et al. (2003) examined the differences in recidivism between DTC cases that received drug treatment and cases that did not receive treatment. The treated DTC cases had significantly lower rates of recidivism at the two-year follow-up than controls or untreated DTC subjects. In a survival analysis of the same court, Banks and Gottfredson (2003) found that drug treatment was the only significant predictor of recidivism. Gottfredson et al. (2006) extended these findings by using an instrumental variables approach to handle the endogeneity problem that arises when subjects self-select into different levels of treatment. These more conservative analyses again showed that recidivism was lowest among subjects who received more days of certified drug treatment and drug testing.

In summary, a vast amount of research suggests that (a) drug use increases crime, and (b) drug treatment reduces drug use as well as crime. However, relatively little of this research completely rules out selection artifacts as alternative explanations for the observed relationship. Within-subjects studies of change in substance use and crime over time have helped to rule out individual-level propensity variables as explanations of the drug-crime relationship, and randomized trials have provided evidence that drug treatment, at least in combination with other correctional interventions, reduces both drug use and crime. But no studies have yet examined variability over time in all three components (drug treatment, drug use, and crime) while adequately controlling for individual-level propensity variables.

Also, the existing literature has suggested that the relationship among these variables depends upon the types of drugs used and the types of crimes committed. More specifically, research summarized earlier suggests that the use of more addictive narcotics such as cocaine and heroin should increase income-generating crimes such as theft and robbery, but not necessarily violent crime. On the other hand, alcohol use is expected to increase violent but not necessarily property crimes. The studies that have examined within-subjects variation in drug use and crime have not focused on these differences. The within-subjects studies of alcohol use and violence have generally not included measures of other drug use (but see Murphy et al., 2005). Horney et al. (1995) combined all illegal drugs into one index, making it impossible to examine the effects of highly addictive drugs on different types of crime. Uggen and Thompson (2003) examined only cocaine and heroin use and illegal earnings (as opposed to different types of crimes), and Jofre-Bonet and Sindelar (2001) examined only income generating crimes. Horney et al. (1995) also reported that the effects of heavy drinking are stronger for increasing property than interpersonal crime, and Jofre-Bonet and Sindelar (2001) find that alcohol use increases "for profit" crime—both of which run counter to research summarized earlier. This study adds to existing literature by modeling change within-subjects of drug treatment, different types of substance use, and different forms of crime over an 11 month period in a sample of chronic drug users with significant criminal involvement. More specifically, it tests the following hypotheses:

1. Substance use increases crime.
    a. Cocaine or heroin use increases income-generating crime more than other types of crime.
    b. Alcohol use increases violent crime more than other types of crime.
2. Drug treatment reduces substance use.
3. Drug treatment reduces crime.
    a. Drug treatment reduces income-generating crime more than violent crime because it focuses more on cocaine or heroin use than on alcohol use.
4. The effect of drug treatment on crime is mediated by reductions in substance use.

METHODS

DESIGN

Data for this study is from a subset of the subjects in a larger study of the Baltimore City DTC. The evaluation of the Baltimore City DTC used an experimental research design. Eligible DTC offenders were randomly assigned to the DTC (treatment condition) or to standard adjudication (control condition). Randomization occurred between February of 1997 and August of 1998, at which time 235 clients had been assigned randomly to one of the two conditions. Details of this larger study are provided in prior reports (Gottfredson & Exum, 2002; Gottfredson et al., 2003; Gottfredson et al., 2006).

TRACKING AND INTERVIEWING

Data used in this study come primarily from interviews with the study participants approximately three years after they were randomly assigned to conditions. Gottfredson et al. (2005) summarize the tracking and interview procedures. One hundred fifty-seven research subjects were interviewed between February 2000 and November 2001. An additional 16 subjects were confirmed to be deceased.[1] Interviews were conducted in a private area, either in the offices of the Division of Parole and Probation, in jail or prison, or in a community location. The interviews lasted approximately 90 minutes, and subjects were paid $50 for their participation.

The interview response rate was 72% for both the DTC and control subject groups. Treatment cases were tracked for an average of 97.7 days prior to their interview, and control subjects were tracked for an average of 100.2 days. The differences in tracking days between the two groups were not statistically significant. Subjects who were and were not interviewed did not differ significantly in terms of race, gender, age, or criminal histories.

This study uses both treatment and control cases from the Baltimore City DTC study to examine how variation in drug treatment is related to variation in substance use and crime. During the three years following entry into the study, 71.2% of the DTC group received some form of treatment for substance abuse, as compared with 27.1% of the control group. Thus, while much of the variance in drug treatment is related to participation in the DTC, considerable treatment was received outside of this program. This study does not assess the effect of the DTC program on substance use and crime. Instead, it uses both DTC-induced and natural variation observed in the control group to examine the associations of interest.

MEASURES

This study uses monthly measures of (a) criminal activity broken out by type of crime, (b) substance use broken out by type of substance, and (c) drug treatment. These measures come from subject self-reports obtained in the interviews described earlier. The substance use and crime measures were collected using a monthly calendar in which the subjects estimated for each month the frequency of each behavior. For the crime measures, the interviewer asked subjects to look at a calendar covering the past 12 months and state whether or not they committed each of a series of different crimes (break-in, theft, auto theft, fraud/forgery, shoplifting, prostitution, robbery, selling drugs, assault, and gun use) during that period. (Because the interview sometimes occurred during the 12th month on the crime and substance use calendar, the 12th month sometimes contains only a partial month's data. For this reason, only the first 11 months of the calendar data are used in this study.) Subjects responded "no", "yes, 1-10 times," or "yes, 11 or more times" for each crime type. All of the crime variables except "selling drugs" were used to create a variable for each month indicating whether they ever committed that crime in the past month.[2] Seven of the variables (break-in, theft, auto theft, fraud/forgery, shoplifting, prostitution, robbery) were combined to create a variable for each month indicating whether or not the individual committed an "income-generating" crime, and the other two (assault and gun use) were similarly combined to create a measure for violent crimes.

The substance use measures were similarly constructed. Interviewers asked each subject whether or not they used a series of different drugs in the past 12 months. In our analyses, we examine whether or not the subject reported using alcohol and whether or not the subject reported using cocaine or heroin during each month.[3] Although we initially intended to assess the effects of cocaine and heroin separately, the use of these two substances was too highly correlated (r=.713 for days of cocaine use and heroin use in the past 12 months) in our sample to justify this type of analysis. The two substances were therefore combined to create one dummy variable measuring the use of either substance during each month. Alcohol use was only moderately correlated with the use of cocaine and heroin (r=.172 and .214 for days of alcohol use with days of cocaine use and heroin use in the past 12 months).

To measure drug treatment, subjects were asked for the beginning and ending dates during the past three years the subject received each of the following types of treatment: residential, methadone maintenance, detoxification, intensive outpatient, AA/NA, and "other." These types of treatment were combined for each month to produce a dummy variable indicating whether or not the subject received any drug treatment.[4] Only those treatment episodes during the 11 months for which complete monthly crime and substance use data are available were used in this study.

Because incarceration in a secure facility can influence the opportunity to use substances and to commit crime, we control for subject self-reports of days spent in jail, prison, or detention centers. Although the interview assessed other types of confinement (e.g., in halfway houses and psychiatric facilities), these confinements were rare relative to jail, prison, and detention. A time-varying control variable measuring the number of days incarcerated each month is used as a control variable in all models. Subjects were asked for the beginning and ending dates during the past three years of all such episodes. Only those episodes during the 11 months for which complete monthly crime and substance use data are available were used in this study.

Validity of Self-Reports

Our study relies on self-reports of criminal involvement and substance use. Huizinga and Elliott (1986) demonstrate that underreporting of known crimes is highest in juvenile populations with characteristics similar to those of our sample (e.g., African American males) and that test-retest correlations of self-reports are lowest among individuals who have engaged in a greater amount of crime. It is often assumed that drug addicts' tendency towards untruthfulness automatically renders self-reports of crime from this population unusable. However, research on the validity of self-reports of substance use and crime among substance using populations has been mixed. Wish (1986), for example, found that nearly two thirds of a large sample did not answer accurately about their PCP use. But subsequent studies (e.g., Messina, Wish, Nemes, & Wraight, 2000; Mieczkowski, 1990; Wish, Hoffman, & Nemes, 1997) demonstrated that the degree of accuracy in self-reports of substance use depends upon factors such as the type of drug used, the seriousness level of the substance use, and the timing of the self-report. Studies of the validity of self-reports of crime among substance users have generally supported the validity of such reports (Amsel, Wallace, Matthias, Mason, & Lockerman, 1976; Ball, 1970; Stephens, 1972). Agreement rates between self-reports and official records of crime in drug using populations range from 55% to 96%, depending on the methodology used in the study and the type of criminal behavior.

These studies suggest that they may be less valid than self-reports of more general populations. Caution is generally urged when self-report data are used to make comparisons across subjects in samples that are heterogeneous with respect to these characteristics, as any differences observed might be due to measurement invalidity. Our study, however, examines within-subject variability over time, controlling for stable individual differences. Invalid reporting, therefore, is likely to be confounded with the study variables only to the extent that is varies over time within the individual. That is, if our subjects underreport their crimes more for those times when they were not using drugs than for the times when they were using drugs, or if they underreport their substance use and crimes more for those times during which they were receiving drug treatment, we would be at risk for misinterpreting an observed relationship in substantive terms when in fact it is due to invalid reporting. Although research on the differential validity of drug-addicts' self-reports of crime during times of substance use and abstinence is not available, some research has examined such differential validity of self-reports of substance use as a function of whether or not the subject is receiving drug treatment (Hinden et al., 1994; Sowder et al., 1993; Wish et al., 1997). These studies suggest that clients may be less likely to accurately report their substance use after treatment compared to prior to treatment engagement, but they also suggest that factors such as how and where self-report data are collected may confound the relationship. This issue is not likely to influence our results because the reports for all months were gathered at the same point in time.

Although we have access only to self-reports of substance use in our sample, we are fortunate to have both self-reports and official reports of criminal behavior. A prior investigation of the validity of self-reports of crime in our sample found that for 70% of the individuals in the study, an exact match was found in the official records for their self-reported crimes. Among cases for which exact matches were not found, as many subjects overreported as underreported their crimes (Rocha & Gottfredson, 2002). While this evidence cannot rule out the possibility that the validity of self-reports varied across the time points involved in the study in ways that are correlated with the variables of substantive interest, they do at least show a reasonable level of validity for the self-reports in general.

Analysis Strategy

We need a strategy that can make use of the 11 months of retrospective data to control for unobserved heterogeneity in models which link drug treatment, substance use, and crime. Two basic approaches are available for estimation of panel models with controls for unobserved heterogeneity: random effect and fixed effect models. These models are used with about equal frequency in sociology (Halaby, 2004), although reviews by Allison (1994), Firebaugh and Beck (1994) and Halaby (2004) all conclude that the fixed effect estimator is to be preferred over the random effect estimator in almost all cases. For example, Allison concludes his review by stating that "(c)aution in interpretation is always appropriate, but compared with alternatives (like the random effect model), (the fixed effect model) appears to be one of the most promising statistical methods for analyzing non-experimental panel data" (1994, p. 196).

The main difference between the two approaches is that the fixed effect estimator relies only on within individual variation to identify the estimate. This has intuitive appeal because it uses each person as his/her own control and focuses on correlation between change in the dependent and independent variable. The random effect estimator uses both between- and within-subject variation and relies on the assumption that the unobserved individual components are uncorrelated with all observed explanatory variables. As Allison (1994) points out, this is a problematic assumption when the main concern is selection bias (i.e., that unobserved factors are biasing the coefficient on the explanatory variable of interest).[5]

Because our dependent variables for crime and substance use are dichotomous, we use a logit framework. The model can be written as:

$$\Pr(y_{it} = 1 \mid x_{it}) = \log\left[\frac{\Pr(Y_{it} = 1)}{1 - \Pr(Y_{it} = 1)}\right] = \alpha + \beta X_{it} + v_t + \mu_i + \varepsilon_{it}$$

The conditional log-likelihood is:

$$L = \sum_{i=1}^{n}\left[\sum_{t=1}^{T_i} y_{it}x_{it}\beta - \log f_i(T_i, k_{1i})\right]$$

where

$$k_{1i} = \sum_{t=1}^{T_i} y_{it}$$

We estimated our models using STATA 7.0 with the *xtlogit* command.[6]

RESULTS

Table 1 shows descriptive statistics for the 156[7] subjects in the study sample. Approximately 78% of the sample is male, and 91% is African American. The average age of the subjects in February 1997 (the beginning of the larger study of the Baltimore City Drug Treatment Court) is 34.4. The mean number of arrests prior to randomization into the study for the sample is 11.9, and the mean number of prior convictions is 4.8. Approximately half of the sample was handled in the Circuit court (the court that handles felony offenses) and half in the District Court (which handles misdemeanor offenses). Fifty-nine percent of the cases in this study were randomly assigned to the drug treatment court.[8] The others were handled "as

Table 1
Descriptive Statistics for Sample

	Mean	Std. Deviation	Range
Age as of 2/1/97 (beginning of study)	34.43	7.62	18-57
Proportion male	.78	.42	0-1
Proportion African American	.91	.29	0-1
Number of prior arrests	11.88	7.80	0-55
Number of prior convictions	4.83	3.44	0-16
Proportion handled in Circuit Court	.49	.50	0-1
Proportion assigned to drug treatment court	.59	.49	0-1
Proportion in drug treatment prior two years	.53	.50	0-1
Total # of treatment days	86.65	143.30	0-696
Total # of treatment episodes	1.18	1.53	0-7

Note:.$N = 156$

usual" in the criminal justice system. Fifty-three percent of the cases in this study reported receiving drug treatment in the two-year period prior to the year about which the subject reported in the interview. Subjects had, on average, 1.2 episodes of drug treatment lasting approximately 87 days in that two-year period. Considering only those 83 subjects receiving drug treatment, the average number of episodes of drug treatment was 2.2, and the average days of treatment was 163 in that same two-year period.

Table 2 reports the descriptive statistics for the outcome variables for months 2 through 11. Month 1 is excluded from the analysis because we used lagged variables. Thirty-nine percent of the subjects admitted to using alcohol in the average month, while 34% admitted to using cocaine or heroin. Twenty percent of the subjects reported an income-generating crime and 4% a violent crime in the average month. Finally, 17% of the sample received drug treatment in the average month. Subjects spent 7.9 days in jail or prison in the average month. The ranges show much variability across the 10 months included in the analysis.

Substance Use and Crime

We predicted that cocaine or heroin use would increase income-generating crime (IGC) more than other types of crime and that alcohol use would increase violent crime (VC) more than other types of crime. Table 3 shows results from fixed effect logit regression models relating IGC and VC to each of the two lagged substance use dummy variables (cocaine/heroin use and alcohol use) partially supported this hypothesis.[9] The coefficient for lagged cocaine/heroin use is 2.03 for IGC and 1.40 for VC and is significantly different from zero only for IGC. The odds ratio in the IGC equation is larger than in the VC equation (OR=7.61 vs. 4.04). The probability of engaging in an IGC is 18.4% in a month during which the individual does not use heroin or cocaine in the previous month, but 63.1% in a month during which the individual does use heroin or cocaine in the previous month.[10] This pattern of

TABLE 2
DESCRIPTIVE STATISTICS FOR OUTCOME VARIABLES, MONTHS 2-11

	Mean	Std. Deviation	Range
Proportion used alcohol	.40	.49	0-1
Proportion used cocaine or heroin	.34	.48	0-1
Proportion engaged in IGCs	.20	.40	0-1
Proportion engaged in VC	.04	.21	0-1
Proportion received any drug treatment	.18	.38	0-1
Number of days spent in jail	7.90	13.08	0-31

Note: $N = 1560$

effects for cocaine or heroin use is consistent with our expectation that the effect of cocaine or heroin use on crime will be stronger for IGC than for VC. We were somewhat surprised that these substance use variables are not significantly correlated with VC, although the fairly large size of the coefficients implicates the smaller sample size in the VC models as a possible explanation.

The coefficient for lagged days of alcohol use is 2.35 for IGC and 1.53 for VC, and is significantly different from zero only for IGC. As with cocaine/heroin use, the odds ratio in the IGC equation is larger than in the VC equation (OR=10.45 vs. 4.62). The probability of engaging in an IGC is 16.1% in a month during which the individual does not use alcohol in the previous month, but 66.7% in a month during which the individual did use alcohol in the previous month.[10] This pattern of effects for alcohol use is not consistent with our expectation that the effect of alcohol use on crime will be stronger for VC than for IGC. The results for alcohol use are similar to the results for cocaine and heroin use.

DRUG TREATMENT, SUBSTANCE USE, AND CRIME

Table 4 shows the results from models relating drug treatment to substance use in the following month. In each of the logit models, treatment reduces alcohol use as well as the consumption of cocaine/heroin, although the effect is statistically significant only for the latter. The OR for cocaine/heroin use suggests that treatment in the prior month is related to a 90% in the odds of decrease in heroin/cocaine use in the following month. Individuals who had any treatment last month and spent seven days in jail this month had a 1.8% probability of using cocaine or heroin this month, while individuals without any treatment last month had a 14.7% chance of cocaine or heroin use. In keeping with our prediction that drug treatment should matter more for cocaine/heroin use than for alcohol use, we find that the OR for alcohol is .59, suggesting a 41% decrease in the odds of alcohol use for those in treatment the prior month. We find that individuals who had any treatment last month had an 11% probability of using alcohol this month, while individuals without any treatment last

TABLE 3
THE EFFECT OF DRUG USE ON CRIME: FIXED EFFECT LOGIT REGRESSIONS[A]

	Any Income Generating Crime (1)	Any Income Generating Crime (2)	Any Violent Crime (3)	Any Violent Crime (4)
Lagged cocaine or heroin use (ever in past month)	2.03***		1.40	
	(.51)		(1.05)	
	[7.61]		[4.04]	
Lagged alcohol use (ever in past month)		2.35***		1.53
		(.54)		(.98)
		[10.45]		[4.62]
Days of jail time	-.15***	-.15***	-.031	-.037
	(.025)	(.027)	(.042)	(.040)
	[.86]	[.86]	[.97]	[.960]
MONTHS (Month 2 omitted)				
Month 3	-.62	-.50	.43	.62
	(.61)	(.63)	(.92)	(.96)
	[.54]	[.6]	[1.53]	[1.85]
Month 4	-.55	-.53	.10	.12
	(.64)	(.66)	(.99)	(1.01)
	[.58]	[.59]	[1.11]	[1.13]
Month 5	-.27	-.36	.39	.66
	(.63)	(.64)	(.96)	(.97)
	[.76]	[.70]	[1.48]	[1.94]
Month 6	-.55	-.66	-.23	.19
	(.64)	(.65)	(1.04)	(1.02)
	[.58]	[.52]	[.79]	[1.21]
Month 7	-1.14	-1.31*	.66	1
	(.65)	(.67)	(.93)	(.93)
	[.32]	[.27]	[1.94]	[2.71]
Month 8	-.98	-1.26*	.46	.59
	(.67)	(.69)	(.99)	(.99)
	[.38]	[.28]	[1.58]	[1.81]
Month 9	.31	-.068	1.73**	1.78**
	(.63)	(.65)	(.88)	(.90)
	[1.37]	[.93]	[5.66]	[5.94]
Month 10	.054	-.25	1.73**	1.73*
	(.62)	(.63)	(.88)	(.90)
	[1.06]	[.78]	[5.65]	[5.64]
Month 11	-.58	-.92	1.49*	1.49
	(.66)	(.67)	(.89)	(.91)
	[.56]	[.40]	[4.44]	[4.42]
Log Likelihood Chi square	124.81***	129.19***	15.75	16.48
N	410[a]	410[a]	210[a]	210[a]

Note: Standard errors are in parentheses, odds ratios are in brackets.
[a] In a fixed effects logit, only those cases that change are used in the estimation. Forty-one of 156 experienced change in IGC, and 21 experienced change in the VC. *p<.10 ** p<.05 *** p<.01

month had a 17.3% chance of alcohol use. Similar models relating drug treatment in the current month with each substance use measure were estimated. These models (not shown) show larger effects, which are statistically significant for both alcohol and cocaine/heroin use. However, it is likely that these contemporaneous effects reflect in part a substance use effect of treatment.

Table 5 reports the effect of drug treatment last month on IGC and VC. The effect on treatment on ICG is large (OR=.19), negative and statistically significant as predicted. The model in the right-most column shows that the effect of drug

TABLE 4
THE EFFECT OF DRUG TREATMENT LAST MONTH ON CURRENT DRUG USE: FIXED EFFECT LOGIT REGRESSIONS

	Alcohol Use Logit	Cocaine or Heroin Use Logit
Any drug treatment last month	-.52	-2.26**
	(.77)	(1.14)
	[.59]	[.10]
Days of jail time	-.21***	-.20***
	(.036)	(.030)
	[.81]	[.82]
MONTHS (Month 2 omitted)		
Month 3	-.66	-.49
	(.69)	(.67)
	[.52]	[.61]
Month 4	-.52	-.24
	(.69)	(.69)
	[.60]	[.79]
Month 5	.090	.38
	(.67)	(.65)
	[1.09]	[1.47]
Month 6	-.33	.059
	(.68)	(.68)
	[.72]	[1.06]
Month 7	-.082	-.45
	(.67)	(.69)
	[.92]	[.64]
Month 8	-.083	-.78
	(.65)	(.70)
	[.92]	[.46]
Month 9	.62	-.068
	(.64)	(.71)
	[1.86]	[.93]
Month 10	.19	-.97
	(.62)	(.70)
	[1.21]	[.38]
Month 11	-.30	-.76
	(.66)	(.74)
	[.74]	[.47]
Log Likelihood Chi square (*df*=11)	121.54***	167.98***
N	420[a]	460[a]

Note: Standard errors are in parentheses, odds ratios are in brackets.
[a] In a fixed effects logit, only those cases which change are used in the estimation. Forty-two experienced a change in alcohol use, and 46 experienced a change in either cocaine or heroin use.
** $p<.05$ *** $p<.01$

TABLE 5
THE EFFECT OF DRUG TREATMENT LAST MONTH ON CURRENT CRIME: FIXED EFFECT LOGIT REGRESSIONS

	Income Generating Crime	Income Generating Crime	Violent Crime
Last month drug treatment	-1.67**	-.95	-1.09
	(.68)	(.90)	(1.25)
	[.19]	[.39]	[.34]
Heroin or Cocaine use (ever in current month)		3.09***	
		(.89)	
		[21.95]	
Alcohol use (ever in current month)		3.42***	
		(.96)	
		[30.63]	
Days of jail time	-.19***	-.086**	-.044
	(.028)	(.031)	(.039)
	[.83]	[.92]	[.96]
MONTHS (Month 2 omitted)			
Month 3	-.70	-.76	.52
	(.64)	(.76)	(.96)
	[.50]	[.47]	[1.68]
Month 4	-.70	-.88	.074
	(.65)	(.80)	(1.01)
	[.50]	[.42]	[1.08]
Month 5	-.28	1.12	.57
	(.62)	(.83)	(.96)
	[.76]	[.32]	[1.76]
Month 6	-.41	-1.05	.098
	(.64)	(.84)	(1.02)
	[.66]	[.35]	[1.1]
Month 7	-.88	-2.22**	.93
	(.63)	(.90)	(.93)
	[.41]	[.11]	[2.53]
Month 8	-.83	-1.66*	.72
	(.65)	(.89)	(.99)
	[.44]	[.19]	[2.05]
Month 9	.39	-.35	1.94**
	(.61)	(.80)	(.90)
	[1.48]	[.71]	[6.95]
Month 10	.26	.19	1.93**
	(.60)	(.77)	(.90)
	[1.30]	[1.21]	[6.92]
Month 11	-.59	-.57	1.7*
	(.66)	(.80)	(.91)
	[.55]	[.57]	[5.48]
Log Likelihood Chi square	114.01***	191.29***	14.71
N	410[a]	410[a]	210[a]

Note: Standard errors are in parentheses, odds ratios are in brackets.
[a] In a fixed effects logit, only those cases which change are used in the estimation. Forty-one of 156 experience change in IGC, and 21 experience change in the violence. *p<.10 ** p<.05 *** p<.01

treatment is also large and negative for VC (OR=.34), but the coefficient is not statistically significant. This is as predicted.

Thus, we now have strong evidence (Table 4) that drug treatment reduces cocaine or heroin use in the subsequent month, and we also have evidence (Table 5) that drug treatment is correlated with a reduction in IGC. The key question is whether the effect of drug treatment on IGC is mediated through its effect on cocaine or heroin use. In column 2 of Table 5, we test this by adding current substance use and alcohol use into the model estimated in column 1. If drug treatment reduces crime indirectly through reductions in substance abuse, we should see a reduction in the size of the coefficient on drug treatment. The coefficient for drug treatment in the last month is no longer statistically significant in this model. Furthermore, the reduction in magnitude is impressive, moving the OR from .19 in column 1 to .39 in column 3.[11]

DISCUSSION

LIMITATIONS

Following the recommendation of a recent National Research Council 2001 report on drug policy data and research, this study uses a criminal justice system-involved sample of chronic substance users to study the effects of drug treatment on substance use and crime. The sample consists of 157 drug-involved subjects who were eligible for participation in the Baltimore City DTC. The sample is not necessarily representative of drug-involved offenders in general. In fact, it is not highly representative even of a DTC sample. An earlier report from the study (Gottfredson et al., 2003) documented that the Baltimore sample has a greater percentage of African Americans and heroin addicts than typical drug treatment courts. Nevertheless, it is one of a relatively small set of samples of chronic substance users with high crime involvement that has been exposed to drug treatment with a sufficient number of observations of each of these variables to facilitate within-subject analysis of variation. Aside from questions about its generalizability to the larger population of drug-involved offenders, it should be noted that one selection criteria used to screen for DTC eligibility was the absence of current or prior convictions for VC. Although we observed some variability on self-reports of VC in the sample, a more violent sample may have increased the probability of observing effects on VCs. In our sample, 37.2% of subjects reported any IGC and 14.7% reported any VCs during the 12 months covered in their interviews. The relative rarity of VCs most likely also reduced the power of the study to detect effects on VCs relative to IGCs.

Another limitation of the study is that the within-subjects models are misspecified to the extent they omit important time-varying situational variables. Lipsey et al.'s

(1997) meta-analysis of alcohol consumption and violence and Fagan's (1990) review of intoxication and aggression both underscore the complexity of the relationship. Although stable individual differences (which are controlled in our study) account for some of the relationship between substance use and crime, factors unique to situations in which substances are used are highly likely to interact with individual tendencies to produce criminal behavior, especially violent criminal behavior. Also, time varying statuses such as employment and the presence of a significant other may influence substance use and criminal behavior. Future studies should address this limitation by collecting data on and incorporating these situational variables into analyses.

This study, like several others that examine the relationship between substance use and crime, relies solely on self-reports of crime and substance use. Although we summarized evidence that self-reports of crime in this sample are credible, we are unable to rule out the possibility that the subjects' self-reports of crime for periods of elevated substance use are less valid than for periods of abstinence. The probability of this pattern of differential validity is reduced by the design of our data collection which obtained all self reports at one point in time, but the possibility remains that subjects underreported their crimes more for those months when they recalled not using drugs than for those months when they recalled using drugs.

More is known about the differential validity of self-reports of substance use as a function of whether or not the subject is receiving drug treatment. Several studies have found substantial variation in the accuracy of client self-reports of substance use at different times in the treatment process. Three studies (Hinden et al., 1994; Sowder et al., 1993; Wish et al., 1997) have found self-reports to be less accurate after treatment than at the early stages of treatment, raising doubts about the suitability of using self-report data for studying within-person variability over time when the drug treatment status of the subjects changes over the period of study. However, the studies also suggest that other situational variables may confound the relationship. The demand characteristics of the self-report situation (e.g., whether or not the subject perceives benefits to admitting to use) (Sowder et al., 1993), the residential vs. community-based setting (Hinden at al., 1994), and the methodology used to collect self-reports (Wish et al., 1997) are alternative explanations for the variability in self-report accuracy observed at different points in the treatment cycle. Fortunately, because we collected data on substance use, crime, and treatment for all months at the same point in time using the calendar methodology, any differential validity observed as a function of time since treatment is removed.

In short, self-report data gathered from chronic substance users may not be perfectly valid, and it is possible that recall of crime varies with the level of substance use. However, it is unlikely that the validity of reports of substance use and crime varies with drug treatment, the other variable of substantive interest in this study.

CONCLUSION

Notwithstanding these limitations, this study adds to the knowledge concerning the association between substance use and crime by modeling change within-subjects over time, comparing effects for different types of crimes and substances, and by testing the mediating mechanism through which involvement in drug treatment reduces criminal activity in a sample of substance users with a history of elevated criminal behavior. This is the first study to examine variability over time in drug treatment, substance use, and crime while adequately controlling for individual-level propensity variables. In the absence of random assignment to different levels of substance use and to substance abuse treatment, individual-level variables that determine both levels of these independent variables and levels of crime provide credible alternative explanations for associations found. By examining the association of change in the independent variables of interest with change in the dependent variables within-individuals, we are able to rule out this class of alternative explanations for our findings.

The study finds that substance use is related to increased levels of crime. Specifically, the use of alcohol and the use of cocaine or heroin are related to increases in IGC crime. None of the effects of substance use on VC are statistically significant, but the magnitudes of the coefficients suggest a smaller effect of each type of substance use on VC. The lower power available in the VC equations makes these smaller effects difficult to detect. We conclude that the predominant effect of substance use on crime is to increase nonviolent crimes, most likely to generate money to purchase drugs.

We did not find support for the hypothesis that the effect of alcohol use would be larger on VC than on IGC. This finding reinforces the Lipsey et al. (1997) meta-analysis showing that the positive association between alcohol use and violence is largely explained by other factors related to both alcohol use and violent behavior, such as socio-demographics and individual temperament. These enduring individual characteristics are controlled in our study through the use of a within-subjects design. The results are also consistent with Horney et al. (1995), who used a within-subjects design to demonstrate effects of drinking alcohol on property crime but not on violent crime, and with Jofre-Bonet and Sindelar (2001), who used a within-subjects design and found an effect of alcohol use on IGC. The results are at odds with prior domestic violence research (summarized earlier), showing an effect of husband alcohol use on the level of violence in domestic situations, suggesting that these effects of alcohol may not generalize to violent crime more generally.

The study also hypothesized that drug treatment would reduce IGC more than VC crime because it focuses more on hard drug use than on alcohol use. The study demonstrates a significant effect of drug treatment in the last month on IGC, but not on VC (Table 5). Drug treatment in the previous month reduces the probability of using cocaine or heroin in the current month from 14.7% to 1.8%. The magnitude of the treatment effect is higher for cocaine and heroin than for alcohol use in the subsequent month, as predicted (Table 4).

Finally, we investigated the extent to which the effect of drug treatment on crime is mediated by reductions in substance use. Evidence for such a mediating effect is found for IGC. The effect of drug treatment in the last month on IGC is diminished in magnitude and is no longer statistically significant when measures of current month substance use are added to the model.

Our work adds to the literature that has used other methodologies and different measures to conclude that substance use increases crime. It refines prior work by showing that substance use has an effect primarily on property crimes. It provides further evidence that drug treatment is effective for reducing substance use, but it also shows that, at least as treatment was delivered in the context of the BCDTC, the effect is more pronounced on cocaine and heroin than on alcohol use. This effect on cocaine and heroin use leads to a reduction in property crime.

Implications of our work are primarily that efforts to provide substance abuse treatment for chronic, drug-involved offenders should be redoubled. Substance abuse treatment is likely to reduce property crime. Significant effects of substance abuse treatment on violent crime were not observed in our sample, but the limited amount of change in violent crime observed in our sample diminished the likelihood of observing such effects. Within-subjects designs used with larger samples, samples not selected for their limited involvement in violent crimes, and with a longer observation period might be used to further explore the effects of substance use treatment on violent crime.

NOTES

1 Nine of the deceased were in the drug court group, representing 6.5% of treatment cases, and seven were in the control group, representing 7.3% of control cases. Based on medical examiner's reports, the major cause of death among the subjects was acute narcotic intoxication. Other causes of death, such as sepsis and AIDS, were thought to be correlated with intravenous drug use.

2 Between 93% and 99% of respondents responded "no" to these monthly crime measures. The predominance of "no" responses precluded a finer examination of the counts of crimes in each month.

3 Interviewers asked about the use of several other substances as well. In this population, the use of substances other than cocaine, heroin, and alcohol was rare. Also, substance use tended to be regular. We experimented with different ways of dichotomizing reports of use, but the main distinction in any given month was between those who did not use at all and those who did.

4 In the study of the effectiveness of the DTC (Gottfredson et al., 2006), the most common types of treatment were outpatient and intensive outpatient treatment, with 21.2% and 14.5% of treatment and control subjects receiving each. In addition, 8.9% of subjects received residential drug treatment services. Among subjects who received any drug treatment services, clients averaged 192.6 days in treatment over the course of the three-year study.

5 This problem is circumvented by some users of random effect models by decomposing the explanatory variables into within and between components (Halaby, 2003).

6 Estimating this model using maximum likelihood estimation with dummy variables for the time and individual fixed effects results in inconsistent estimates of both α and β because of the "incidental parameter" problem in maximum likelihood estimation (Allison, 1994; Chamberlain, 1980). This problem, which is basically the result of having a dummy variable for each of N observations, is solved by working with the conditional likelihood using sufficient statistics for the nuisance parameters – the fixed effects μ_i. The sufficient condition in this case is the sum of the outcomes (Y_{it}) over all of the periods of the data (Allison, 1994; Chamberlain, 1980; see also Greene, 1997 ch. 19, for a textbook treatment). The individual-specific fixed effects (and the constant) drop out of the model and are not estimated but the estimates of β are consistent. Given that the fixed effect estimator relies only on within individual estimation, the likelihood is only calculated for cases in which there is some change over the period of observation—cases where the dependent variable is always zero or always one are dropped.

7 One case was dropped due to missing data on drug treatment.

8 All subjects originating in the Circuit Court were randomly assigned to the drug court and control conditions using a one-to-one ratio. In comparison, district court cases were randomly assigned using a two-to-one ratio. The number of treatment cases is therefore greater than the number of control cases for subjects originating in the district court.

9 In these models, we lag substance use so that we are more likely to capture the causal relationship from substance use to crime, rather than any simultaneous relationship between crime and substance use. This reduces our sample from 11 to 10 months because we have no lag for the first month. In the income equation,

we have a total of 410 observations because 41 subjects experienced some month-to-month change in their IGC. Because VC is less common, only 21 of the subjects experienced changes in violent offending, providing 210 observations for the violence equation. Readers interested in the contemporaneous effect of substance use on crime should refer to the results in Table 5. Finally, we include the number of days spent in jail in the current month in all models to control for exposure time.

[10] This predicted probability is for an individual whose fixed effect is zero, during the average month with seven days of jail.

[11] The careful reader of Table 5 might be alarmed by the large ORs for the substance use variables. For example, the OR for cocaine/heroin use in the current month on IGC in the current month is 22. This is much higher than the results reported earlier (OR=7.61). Recall however, that the earlier results were based on lagged drug use in order to make a stronger argument about causality. The large coefficients in Table 5 demonstrate the reality that virtually all of the cocaine/heroin users in our sample steal to raise money for drugs but actually have a very low rate of crime when they are not using drugs.

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