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**Distinguishing Between Effects of Criminality
and Drug Use on Violent Offending**

**Final Report to National Institute of Justice
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By

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Distinguishing Between Effects of Criminality and Drug Use on Violent Offending

SUMMARY

Violent Offending and Drug Use¹

The alarming increase in lethal violence among young people in the U.S.—which is often attributed to drug use and drug trafficking—has prompted re-examination of the relationship between drugs and violent offending. While no national data exist, numerous local studies find a high prevalence of homicide deaths among identified drug addicts,² a high prevalence of substance use—typically alcohol—among victims of homicide,³ and a high proportion of persons testing positive for drug use among arrestees for violent offenses.⁴ Other studies report large increases in drug-related homicides⁵ or other violence⁶ associated with drug distribution.

¹ A number of excellent reviews are available of the very large body of research on the relationship between drug use and crime. See, for example, Goldstein (1989), Chaiken and Chaiken (1990), de la Rosa, Lambert, and Gropper (1990), and Harrison (1992). Miczek and Thompson (1983) and Fagan (1990) specifically address the relationship between drug use and aggression/violence.

² Fitzpatrick (1974) reports on addicts in federal treatment, Zahn and Bencivengo (1974) on addicts in Philadelphia, Monteforte and Spitz (1975) in Detroit, McCoy (1978) in Dade County, FL, Preble (1980) and Tardiff et al. (1986) in New York City.

³ McGuire (1983) reports on homicide victims in New York City in the 1970s. Relying on reports of medical examiners, NIDA (1985) reports 15-to-30% drug-related homicides in San Diego, New Orleans, Los Angeles, Philadelphia, Miami, and Detroit. The D.C. Office of Criminal Justice Plans and Analysis (1988/89) finds toxicology evidence of drug or alcohol use in over 60% of homicide victims in Washington, D.C. between 1985 and 1988.

⁴ Toborg et al. (1986) in Washington, D.C.; Wish et al. (1989) in New York City

⁵ Swerzey (1981) describes an increase in homicides associated with drug distribution in Harlem, New York in the late 1960s and early 1970s. New York City Police (1983) finds that 24% of homicides in 1981 were drug related. Anderson and Harrell (1990) report that census tract levels of homicide in Washington, D.C. were related to levels of drug offenses in the same tracts in 1980 and 1988. Rosenfeld (1990) finds that 22.5% of all homicides in St Louis from 1985 to 1989 were drug-related and 45% of these drug-related homicides involved drug distribution. Goldstein, et al. (1992) reports that 53% of homicides in New York City during 1988 and 42% of homicides in upstate New York during 1984 were drug related, with 74% of drug-related homicides in New York City being "systemic" homicides arising from distribution of powder and crack cocaine, while 59% of drug-related homicides in upstate New York were "psychopharmacological" associated with alcohol consumption. (See Goldstein, 1985 for tripartite conceptual model of relationship between drugs and violence.)

While a substantial body of evidence documents strong positive associations between heroin involvement and property offenses⁷, less is known about drugs other than heroin, and about links between drugs and violent offenses.⁸ Studies of narcotic addicts focus primarily on theft crimes. When they are examined, violent offenses by narcotic users or addicts typically occur at much lower levels than property crimes and do not differ significantly with level of drug use.⁹ Elevated levels of violent offending are more evident for non-narcotic drugs.¹⁰ Recent

(..continued)

⁶ Altschuler and Brounstein (1991); von Kammen and Loeber (1994).

⁷ The earliest studies were usually based on narcotic drug users in publicly funded drug treatment programs or processed by the criminal justice system. Focusing on relative crime type distributions of narcotic drug users, they noted an overwhelming predominance of property offenses among arrests and self-reported offenses by these drug users. See, for example, Finestone (1957), Inciardi and Chambers (1972), Jacoby, et al. (1973), Elliott and Ageton (1976), Johnston, et al. (1976), McBride (1976), and O'Donnell, et al. (1976). Other studies compare offending levels of drug users to nonusers (see note 13), and still others compare offending levels of addicts during periods of heavy and light drug use (see note 14).

⁸ While robbery is frequently included with assaultive violent offenses in studies of the relationship between drug use and crime, robbery actually shares many features in common with property offenses. From the perspective of the victim, the threat of violent harm in robberies is particularly salient. From the perspective of the offender, however, the acquisition of property may be the primary motivation. The patterns of robbery offending—especially frequencies of committing this offense by active robbers, and the duration of active careers in robbery—are more similar to other property offenses than to assaultive violent offenses (Cohen, 1986). Because of its ambiguous status, the present analysis treats robbery separately from more directly assaultive offenses.

⁹ Wish et al. (1980); Wish (1982); Speckart and Anglin (1986a); Nurco et al. (1986); Nurco et al. (1988)

¹⁰ In an analysis of self-reported drug use and offending by prison inmates, Chaiken and Chaiken (1984) report higher rates of assaultive violent offending by users of multiple drugs (especially when used in combination with barbituates) and users of psychotropic drugs (e.g., hallucinogens, PCP). Clayton and Tuchfield (1982) and Kandel, et al. (1986) report similarly higher rates of violence associated with use of amphetamines, PCP, and multiple drugs. Analyzing data on alcohol, cannabis, and cocaine use from the National Household Survey on Drug Abuse, Harrison and Gfroerer (1992) report that drug use by respondents is always associated with a significantly higher odds-ratio of "doing" and "being arrested" for property and violent crimes, with the strongest difference for cocaine use on arrests for both types of offenses. (While robbery is included in the category of violent offenses by Harrison and Gfroerer, violent offenses are heavily dominated by self-reported assaultive crimes in this sample from the general population of U.S. households.)

Spunt et al. (1990) and Goldstein et al. (1991) focus exclusively on violent offending by drug users in New York City. "Psychopharmacological" violent events (primarily involving consumption of alcohol) predominate for all ethnic and gender groups except black males for whom "systemic" violent events associated with drug distribution predominate. Notably, "economic compulsive" violent events that typify robberies are rare in these New York City samples. Excluding alcohol, heroin is the predominant drug in violent events among white drug users, while cocaine dominates among black drug users (Spunt et al., 1990). Among cocaine users in New York City, male "big users"—whose daily expenditure on cocaine exceeds the sample mean—display disproportionately higher involvement in assaultive violent events than other users in the sample (Goldstein et al., 1991).

studies of users and distributors of crack cocaine report elevated levels of both property and violent crime that are related to both crack use and crack dealing.¹¹ However, higher levels of violent offending by crack dealers often predate their involvement in drug distribution and suggest selection into this activity by individuals already inclined to violence.¹²

Focus on Changing Rates of Offending

In a departure from previous research that contrasts users and nonusers of drugs¹³, or compares broad periods of heavy and light drug use during long addiction careers¹⁴, the present study attempts to isolate more direct effects of drug use near the time of offending. The data are for a sample of adults arrested in Washington, DC from July 1, 1985 to June 30, 1986, and include their longitudinal arrest histories along with the results of urine drug screens administered following arrest.

¹¹ Inciardi (1990) examines offending reported by 254 hard-core, adolescent, drug-using offenders from Miami and Dade County, FL who are distinguished by their relative involvement in crack dealing, while Inciardi and Pottieger (1994) analyze adult crack users from treatment and street samples in the same Miami, FL metropolitan area. A series of studies of New York City drug abusers by Fagan, Johnson, and colleagues report results from the "Careers in Crack" project contrasting offending before and after initiation of crack use and involvement in crack dealing (Belenko et al., 1989; Chin and Fagan, 1990; Johnson et al., 1994; Johnson et al., 1995). While not specifically focusing on crack, Altschuler and Brounstein (1991) contrast offending patterns of juvenile drug users and juvenile drug sellers with non-drug involved juveniles in Washington, D.C. in 1987-88; they report a significant relationship between drug trafficking and assaultive violent offenses and no relationship between drug use and violence in this sample of juveniles.

¹² Inciardi (1990); Chin and Fagan (1990); Fagan and Chin (1990); Dembo, et al. (1990); von Kammen and Loeber (1994).

¹³ Chaiken and Chaiken (1984); Elliott and Huizinga (1984); Goldstein, et al. (1991); Harrison and Gfroerer (1992); Dembo, et al. (1994)

¹⁴ Studies of offending patterns by narcotic addicts during periods of varying levels of drug use include McGlothlin, et al. (1978), Ball, et al. (1981, 1982), Ball, et al. (1983), Shaffer, et al. (1984), Nurco, et al. (1985), Nurco, et al. (1986), Nurco, et al. (1988), Anglin and Speckart (1986, 1988), Speckart and Anglin (1986a). Relying on panel data from general population samples of juveniles, two studies examine temporal patterns in initiation of drug use and delinquency (Huizinga et al., 1989; van Kammen and Loeber, 1994). The second of these studies also examines changes in the frequency of offending following initiation and discontinuance of drug use and drug dealing.

Relying on the length of time intervals between arrests to measure individual offending levels, we look for changes in the *rates at which arrests occur* for various offense types, and the relationship of these rate changes to individuals' drug-use status at the time of successive arrests.¹⁵ Offending that is aggravated by drug use will occur at faster rates when drugs are used, while offending that is inhibited by drug use will occur at slower rates under the same circumstances. Any effects of drug use that are detected may reflect *psychopharmacologically* induced behavioral effects associated with alterations in mood, irritability, or inhibition as a result of ingesting drugs, or *situationally* induced behavioral effects arising from the social setting or context in which illicit drugs are obtained and used.¹⁶

Comparing Drug Users to Nonusers

Replicating earlier findings of self-reported offending, arrest rates in the present study—reflecting the annual number of arrests per offender—are higher when heroin *users* are compared to *nonusers* for property/theft and drug offenses (Exhibit 1). Null effects occur for personal-violence (i.e., assaultive crimes) and public order/vice offenses (including prostitution). Reverse effects—where heroin users have *lower* rates than nonusers—occur for predatory (robbery and burglary) offenses. Exhibit 1 also reports results for users and nonusers of non-narcotic drugs. Property/theft and drug offenses are the only crime types that consistently display higher arrest rates among users than nonusers for the drug types analyzed here—heroin, cocaine, and PCP.

[Exhibit 1 about here]

¹⁵ The later discussion of the strategy of analysis addresses the appropriateness of the arrest rate measure and other methodological considerations in the present analysis.

¹⁶ The present research focuses on identifying changes in offending levels that accompany drug ingestion, and does not attempt to distinguish between the physiological and situational mechanisms that give rise to these changes. It also does not address violent offending associated with trafficking in illicit drugs.

The differences in arrest rates between drug users and nonusers are most pervasive for PCP. PCP users (who comprise 27% of the current sample of adult arrestees in Washington, D.C.) display higher arrest rates than nonusers in personal-violence and predatory offenses, as well as the more broadly observed higher rates in property/theft and drug offenses. The difference is largest for predatory offenses, where rates among users are more than four times higher than rates among non-users.

Cocaine (primarily in crystal—"crack"—form) is distinctive from heroin, with no difference between offending rates of users and nonusers for property/theft and predatory offenses, and lower rates for public-order/vice and personal-violence offenses when cocaine users are compared to nonusers. Only drug offenses exhibit higher arrest rates among cocaine users than nonusers.

"Use" and "Criminality" Effects

Prior research on the relationship between drugs and crime has been unable to distinguish between "use" effects of drugs and "criminality" effects of drug users. "Use" effects refer to transitory effects arising from actual ingestion of drugs or the influence of the settings where drugs are used, while "criminality" effects refer to more enduring traits of individuals that contribute to both drug use and offending by the same persons. In the case of "use" effects, reductions in access to and use of illicit drugs can alter associated offending patterns. If, however, drug use is merely one of many behavioral manifestations of individual dispositions toward unconventional behavior, changes in the consumption of drugs are not likely to affect offending levels.

Transitory Effects of Drug Use

The principal innovation in the current research is to compare offending rates by the same individuals when they use and do not use drugs in order to explicitly assess the transitory effects of drug use on offending rates, while simultaneously controlling for the effects of more enduring attributes of the persons studied and broadly felt time trends that affect offending. The results for use effects, summarized in Exhibit 2, suggest that prior findings of elevated offending rates by heroin users in property/theft and drug offenses primarily reflect "criminality" effects in which individuals who offend at high rates are also disposed to use illicit drugs.

[Exhibit 2 about here]

Heroin Use

There are no indications that heroin use has aggravating effects on offending. The transitory effects of heroin use near the time of the offense tend to *inhibit* arrest rates for predatory, drug, and public order/vice offenses and have no effect on property/theft offenses. These inhibiting effects are manifested by declines in the detrended arrest rates on succeeding arrests involving drug use ($(V_i^{UU} / V_{i-1}^{UU}) / (V_i^{CC} / V_{i-1}^{CC}) < 1$). Raw arrest rates are essentially unchanged when using heroin on successive arrests, compared to a more than doubling in rates among cleans on succeeding arrests.¹⁷ Thus, in the absence of a general upward trend in arrest rates, the arrest rates of chronic heroin users would have declined by more than half.

Arrest rates for personal violence exhibit broad declines over time among heroin users, regardless of their current drug use status (Exhibit 3). While using heroin, the overall mean annual arrest rate for personal violence offenses is 0.110 (one arrest every 9.1 years) compared

¹⁷ Details of the changes in arrest rates are in the Appendix table.

to 0.157 (one arrest every 6.4 years) while not using heroin. Detrended personal violence arrest rates decline by 41 to 47% as offenders go on and off heroin,

$$\begin{aligned} (V_i^{CU} / V_{i-1}^{CU}) / (V_i^{CC} / V_{i-1}^{CC}) &= .53 \\ (V_i^{UC} / V_{i-1}^{UC}) / (V_i^{CC} / V_{i-1}^{CC}) &= .59 \end{aligned}$$

and decline by 65% over succeeding arrests involving heroin use

$$(V_i^{UU} / V_{i-1}^{UU}) / (V_i^{CC} / V_{i-1}^{CC}) = .35$$

[Exhibit 3 about here]

Cocaine Use

The transitory effects of cocaine use show evidence of broad *inhibiting* effects, with *lower* arrest rates when offenders use cocaine. The inhibiting effects on offending, that were evident between users and nonusers of cocaine (Exhibit 1), are more widespread when comparing arrest rates as the same sample of offenders use and do not use cocaine. When cocaine is used near the time of an offense, detrended arrest rates are lower for personal-violence (Exhibit 4), property/theft, and drug offenses. As in the case of heroin, raw arrest rates remain unchanged as offenders go from being clean to using cocaine, but the rates for cleans double on successive arrests. Thus, without the general upward trends evident among cleans, arrest rates for personal violence, property/theft and drug offenses would have declined from 40 to 50% as offenders went from being clean to using cocaine.

[Exhibit 4 about here]

Only the detrended arrest rates for predatory offenses increase after using cocaine on an earlier arrest, but this aggravating effect is observed regardless of drug use status on subsequent arrests. Nevertheless, the transition from using to not using cocaine

$[(V_i^{UC} / V_{i-1}^{UC}) / (V_i^{CC} / V_{i-1}^{CC})]$ is accompanied by a large 6.57-fold increase in the arrest rate for predatory offenses (Exhibit 5). This pattern is consistent with *withdrawal* effects in which rates of acquisitive predatory offenses (robbery and burglary) *increase* when users of cocaine (primarily in crack form) are not using this drug, and may be seeking financial resources to pay for the purchase of more drugs. A similarly large increase, however, also accompanies chronic cocaine use with predatory offending increasing 5.77-fold when offenders use cocaine on successive arrests $[(V_i^{UU} / V_{i-1}^{UU}) / (V_i^{CC} / V_{i-1}^{CC})]$.

[Exhibit 5 about here]

PCP Use

PCP is the only drug type for which higher arrest rates—like those observed when PCP users are compared to nonusers—persist when examining the transitory effects of PCP use near the time of the offense. Detrended arrest rates for personal violence, predatory, drug, and public order/vice offenses all increase with chronic PCP use on successive arrests. Property offenses are the only offense for which arrest rates decline when PCP is used.

A pure “use” effect is evident for predatory offending as the detrended annual arrest rate more than doubles as offenders go from being clean to using PCP

$[(V_i^{CU} / V_{i-1}^{CU}) / (V_i^{CC} / V_{i-1}^{CC})] = 2.69$], and then increase by another 2.29-fold when they continue to use PCP on successive arrests $[(V_i^{UU} / V_{i-1}^{UU}) / (V_i^{CC} / V_{i-1}^{CC})]$ in Exhibit 6). For personal-violence offenses, there is no evidence of a transitory “use” effect of PCP in the detrended rates. Instead, PCP use on an earlier arrest apparently increases subsequent arrest rates for these assaultive offenses regardless of whether PCP is used (3.56-fold increase in

$(V_i^{UU} / V_{i-1}^{UU}) / (V_i^{CC} / V_{i-1}^{CC})$) or not (2.31-fold increase in $(V_i^{UC} / V_{i-1}^{UC}) / (V_i^{CC} / V_{i-1}^{CC})$) near the time of the current offense (Exhibit 7).

[Exhibits 6 and 7 about here]

Conclusions

The results in the present study derive from changes in individual rates of offending as the same sample of offenders use and do not use drugs near the time of offending. The most compelling results are:

- broad *inhibiting* effects of heroin and cocaine use on most types of offending,
- *aggravating* effects on predatory offending (robbery and burglary) during *withdrawal* from cocaine use (primarily in crack form), and
- both short- and long-term *aggravating* effects of PCP use on most types of offending, including personal violence.

These results—based on illicit drug use in real-world settings and actual dose levels—are especially noteworthy because they confirm findings previously observed only in artificial experimental settings (Fagan, 1990; Miczek and Thompson, 1983).

The higher offending rates associated with narcotic drug use in prior research apparently reflect population heterogeneity in which enduring differences among offenders contribute to both higher offending rates and illicit drug use by the same persons. Once the underlying differences among offenders are controlled, it appears that occasions when drugs are used do not aggravate offending levels further. In the case of heroin and cocaine, using these drugs actually seems to inhibit individual offending in most crime types. PCP is a noteworthy exception to this pattern—using this drug does aggravate offending levels.

These results suggest specific policy implications for interventions that seek to reduce crime by reducing drug use. Interventions intended to reduce heroin and cocaine use are not likely to have an impact on offending levels, which are higher among users than non-users, but do not appear to be further aggravated by transitory effects from using the drugs. For these types of drugs, the sources of both chronic offending and drug use seem to lie partly in enduring differences among offenders, and partly in broad secular trends that increased offending independently of patterns of drug use. Furthermore, the transitory effects of heroin and cocaine use appear to be in the direction of inhibiting offending when using these drugs, and there is some evidence of aggravating effects of withdrawal from cocaine use on the acquisitive predatory crimes of robbery and burglary.

Strategies that selectively target interventions on reducing PCP use are likely to have a greater impact in reducing crime. Chronic use of PCP was associated with increases in offending rates that exceeded the general upward trends in offending. These aggravating effects occurred broadly in all but property/theft crimes. Efforts that successfully reduce PCP consumption show the greatest promise of reducing crimes induced by drug use.

Some cautions against over-reaching from these results are worth noting. The analyses relate most accurately to the experiences of one city during one time period, and may be peculiar to unique features of the study site. For example, the levels of PCP consumption among arrestees were unusually high compared to other cities. If users of PCP in the study sample would have been using some other drug in another site, then the results for PCP may derive from other features peculiar to those offenders and not specifically from their PCP consumption. The analysis also targets a period of substantial changes in illicit drug consumption habits as crack replaced heroin as the drug of choice among chronic users. Results during this transition period

may not apply to a more stable setting. Replications in other sites and time periods will provide a sounder basis for assessing the generality of the results.

The analyses also rely on naturally occurring variation in drug consumption and offending patterns. This is a source of weakness and strength in the analysis. One obvious weakness is the potential bias introduced by the requirement that offenders must have extensive arrest histories to be included in the analysis. This limits the sample to offenders who remain free from incarceration long enough to accumulate repeated arrests during the observation period, a bias that may over-represent less serious offenders, and less serious offense types in the analysis. Such a bias might account for a generally low prevalence of serious offenses in the analysis sample.

Our focus, however, is less on the general prevalence of serious offenses and more on the changes in these offenses as offenders use and do not use illicit drugs. If offenders who accumulate extensive arrest histories respond differently when they use drugs, in the extreme becoming less violent while serious offenders become more violent, then the current results would not reflect drug use effects among serious offenders. If, however, drug use has a similar influence on offending levels of both serious and less serious offenders, then the current results would remain valid.

Another source of weakness derives from the array of statistical controls that must be invoked to isolate drug use effects in an otherwise uncontrolled environment. The validity of the conclusions depends on the robustness of the findings to variations in the statistical assumptions. To this end, the current analysis considers a number of ways that the assumptions might fail. For the most part the results are encouraging, and the main conclusion of broad inhibiting effects of drug use on offending levels prevails despite potential biases to the contrary. This

underscores the potential value of pursuing similar statistically based strategies as a means of teasing out estimates of effects from data that are more directly relevant to real-world patterns of illicit drug consumption and offending.

Distinguishing Between Effects of Criminality and Drug Use on Violent Offending

SUPPORTING MATERIAL

1. *STRATEGY OF ANALYSIS*

Previous research on the relationship between drugs—usually heroin—and crime focuses heavily on “cross-sectional” analyses that compare offending rates of distinct samples of drug users and nonusers.¹⁸ Another major body of research relies on long-term retrospective self-reports by drug addicts spanning periods as long as 10 to 25 years in which offending rates are compared during extended periods of heavy and light drug use.¹⁹ While the latter studies purportedly examine the same individuals as they increase and decrease their intensity of drug use, the results often rely on contrasts in which as many as 10 to 15% of the samples *never* experience a non-addicted period.²⁰ Thus, long-term addicted individuals, with their likely higher offending rates, do not contribute to the offending rate estimated during non-addicted periods.

Such comparisons of offending rates—that rely on different offenders, and not differences in drug use—confound whatever direct behavioral effects drug use may have in stimulating or inhibiting offending with other indirect effects of offender heterogeneity in which individuals who are more likely to use illicit drugs are also more prone to offend. Motivated by thrill

¹⁸ See note 13.

¹⁹ See note 14.

²⁰ Of the studies identified in note 14, McGlothlin, et al. (1978), Nurco, et al. (1986), Nurco, et al. (1988), and Anglin and Speckart (1986, 1988) explicitly examine the same sample during periods of heavy and light use (usually distinguished by daily and less than daily use of narcotic drugs). The changes in offending relate specifically to drug or property offenses. When violent offenses are examined (in Nurco and colleagues), statistically discernible changes are not observed.

seeking, for example, some individuals might be inclined to pursue situations involving greater risks of physical danger and aggression, as well as being more likely to use illicit drugs that characteristically result in heightened levels of arousal and excitement. Likewise, highly impulsive individuals might be more inclined to put themselves in situations with a greater risk of violent encounters and also to seek the psychological high derived from drug use. Situations like these would reflect the indirect impact of common third causes that contribute to higher or lower levels of both drug use and violent offending by some individuals, and not direct pharmacological or contextual effects of drug consumption in altering individual behavior in ways that promote or stimulate violent offending.

The distinction between "use" effects in which higher or lower offending rates arise directly from the circumstances of drug consumption, and "criminality" effects in which varying offending rates arise from differences across individuals, is fundamental in the search for strategies that effectively change offending rates. In the presence of direct causal links between drug acquisition or consumption and violent episodes, for example, reducing drug use might also result in reductions in violence. When drug use and offending are linked primarily as collateral effects of some other cause, however, reducing drug acquisition and consumption is not likely to affect the associated levels of violence.

Value of Longitudinal Analyses

Longitudinal panel data—repeated observations of the same individuals over time—provides one means for isolating direct behavioral impacts of drug use from the indirect effects of underlying differences in individual inclinations for both violence and drug use. Examining the same individuals over time—contrasting their offending rates when they use and do not use drugs—effectively controls for persistent heterogeneity associated with enduring traits of

individuals, and allows the behavioral impact of drug use on violence and other types of offending to be isolated.

A longitudinal approach has been used in a limited way to compare offending before and after initiation of drug use or during long periods of heavy or light use of drugs. These period contrasts, however, provide only vague temporal links between actual drug ingestion and offending and are often limited to documenting co-occurrence of drug use and offending during observation periods that run from months to several years in duration. The current research exploits a longitudinal design to isolate changes in the rate of offending associated with drug use or not near the time of an offense.

Units of Observation

The intention is to follow the same individuals over time and monitor their drug use and offending. Relying on this longitudinal data, each individual will serve as his or her control for enduring differences in offending and drug use propensities so that the effects of drug use can be inferred from the changes in offending that accompany changes in drug use in the same individuals. Arrests are used to calibrate offending rates, and urine drug screens administered at the time of arrest indicate drug use status near the time of the offense that precipitated the current arrest.

Arrests as a Measure of Offending

Arrests are clearly only a sample of all the offenses that an offender commits. Nationally during the 1980's, for example, police reported an average of 3.61 robbery offenses per arrest and 2.26 aggravated assault offenses per arrest, and 6.96 burglary offenses per arrest (calculated by the author from FBI, annual). Including offenses that were not reported to the police, the ratio of offenses-to-arrests averaged 7.69 for robbery, 5.09 for aggravated assault, and 12.79 for

burglary over the same period (FBI, annual; BJS, 1992). Accounting for multiple offenders participating in the same offense reduces the risk of arrest per crime for individual offenders to 1-in-17.5 offenses for robbery, 1-in-12.8 offenses for aggravated assault, and 1-in-20.4 offenses for burglary.²¹

When the *arrest risk per crime*, q , is relatively stable over time for individual offense types, the patterns of change (or stability) that are observed in arrest rates will track the same patterns in offending. If offending rates (O) increase by some percentage, then arrest rates (A) will increase by the same amount ($O_{t+1}/O_t = O_{t+1}q/O_tq = A_{t+1}/A_t$). Furthermore, if the arrest risk per crime for an offense type remains stable for individual offenders, then changes in the frequency of arrests as individuals use and do not use drugs provide a reasonable basis for identifying increases or decreases in offending rates that accompany drug use.

Broad stability in the arrest risk per crime is not unreasonable. During the decade of the 1980s, for example, the annual variation in the ratio of arrests to victim reports of offenses (measured by the standard deviation divided by the mean) was under 20% for aggravated assault and under 10% for robbery and burglary. Drug induced variations larger than this will be detectable. Detrending the data will control further for systematic trends in the arrest risk per crime.

The assumption of stability in q over time for an offender is more problematic. The more likely scenario is that drug intoxication increases the arrest risk per crime when an offender

²¹ Averages are derived from data reported by the Federal Bureau of Investigation (annual) and the Bureau of Justice Statistics (1992). The average offenders per crime incident—2.3 for robbery and 2.6 for aggravated assault—are from Reiss (1980). Blumstein and Cohen (1979) first introduced the estimate of an offender's arrest risk per crime committed as $(A/M)/(O/R)$. A and O = the number of arrests and offenses, respectively, reported by police, M = number of multiple offenders per crime incident, and R = the rate of victims reporting crimes to the police.

uses drugs because offenders become less cautious about avoiding detection. In this event, arrest rate changes will include a bias in the direction of increased offending when drugs are used.

This bias will overstate the magnitude of aggravating effects and understate the magnitude of inhibiting effects of drug use on offending. The widespread *inhibiting* effects of drug use found in the current analysis are thus likely to be even larger than estimated.

Drug Screens at Arrest as a Measure of Drug Use²²

Drug screens are administered while the offender is being processed following an arrest. This raises several concerns about detection errors in identifying drug users at the time of the offense. The first potential source of error—arising from delay between committing an offense and arrest—is of minimal concern. Studies of the arrest process repeatedly find that when arrests do occur, they are highly likely to occur within 24 hours of the offense and usually at the scene of the offense (Greenwood, 1970; Greenwood, et al., 1977; Spelman and Brown, 1984).

Other concerns surround detection errors in the drug screen itself. Two types of detection error are possible: (1) failure to detect users who are mislabeled as drug-free, or clean, in the drug screen, and (2) incorrectly identifying some offenders as users because the drug screen continues to detect drugs that metabolize slowly for days or even weeks after ingestion. While these errors certainly do affect our ability to detect an offender's drug use status at the precise moment of an offense, the drug screens at arrest provide a reasonable measure of drug use within a narrow window of no more than a few weeks before an arrest. This represents a vast improvement over previous methods (e.g., observations of co-occurring drug use and crime

²² The Technical Appendix accompanying this report includes a detailed discussion of the potential sources of measurement errors in detecting drug use, their impact on the estimated effects of drug use on offending, and a strategy for calibrating the impact of detection errors on estimates of the effects of drug use on offending rates..

during periods that generally extend from one to several years, or contrasts between drug users and non-users) in detecting offender drug use near the time of the offense.

Furthermore, it is possible—using reasonable assumptions about error rates—to calibrate the impact of these detection errors on the desired estimates of the drug use effect (see Table A1). Whatever that magnitude, both types of classification errors—including actual users among detected cleans, or vice versa—reduce the differences observed between detected “users” and “cleans,” and thus tend to *understate* the influence of drug use on offending rates. Any bias resulting from the errors in detecting drug use is in the direction of finding *null* effects, and so the effects of drug use that are detected in the current analysis actually understate the true magnitude of these effects.

Method of Pairwise Comparisons

The principal objective is to measure the direct behavioral impacts of drug use on individual levels of violent offending over and above the effects of other offender propensities that either inhibit or encourage violence. The outcome measure is the individual arrest rate (arrests per offender per unit of time) at each arrest. The basic strategy for isolating drug use effects is to rely on longitudinal panel data—repeated observations of the same individuals over time—and compare arrest rates on pairs of arrests for the same individuals.

For the i^{th} arrest in an individual arrest history, V_i^D reflects the rate at which violent arrests occur among offenders characterized by their drug use status, D , on a pair of arrests ($D = CC, CU, UC, \text{ and } UU$ on arrests $i-1$ and i). So, for example, the rates V_{i-1}^{CU} and V_i^{CU} are the rate of arrests for violent offenses when an offender is clean, C , (i.e., not using drugs) on the first arrest in a pair, and the same rate when the offender is using drugs, U , on the second arrest

in the pair. Likewise, offenders remain clean on both arrests in *CC* pairs, go from using drugs to clean in *UC* arrest pairs, and continue using drugs in *UU* arrest pairs.

Changes in the individual arrest rate are reflected in the ratio:

$$V_i^D / V_{i-1}^D$$

Values less than 1 indicate declines in the arrest rate and values larger than 1 indicate increases in the rate. The analytical advantage of the ratios derives not from their handling of measured sources of variation—which can be controlled directly in multivariate analyses—but rather from their ability to control for the effects of *enduring unmeasured* sources of variation, or persistent population heterogeneity. Since the effects of these stable sources of variation are the same on every arrest, the simple ratio removes these effects entirely, and isolates the behavioral effects of influences that are changing over time.²³ Several variations of the ratio measure are of interest.

Time Trend

Broad based secular or temporal trends in offending levels that are independent of transitory drug use are reflected in the ratio:

$$Trend = V_i^{CC} / V_{i-1}^{CC}$$

Since we apply the ratio to arrest rates, the trend estimate also controls for broadly felt changes over time in the arrest risk per crime and in time served following an arrest. Trends upward or downward in the transformation of crimes to arrests and time served will be manifested in the

²³ The enduring effects of offender differences reflect *persistent population heterogeneity*, while the behavioral effects of changing circumstances reflect *state dependence*. An extensive body of literature by Nagin and colleagues addresses methodological developments in distinguishing between these factors and substantive results with respect to offending behavior (Nagin and Paternoster, 1991; Nagin and Farrington, 1992; Nagin and Land, 1993; Nagin and Paternoster, 1993; Nagin, et al., 1995; Nagin and Waldfogel, 1995, 1998; Nagin, et al., 1998; Nagin, 1999).

ratio of arrest rates. The ratio thus provides a basis for detrending other cross-time comparisons.²⁴

Episodic Effect of Drug Use

The episodic effects of drug use on the arrest rate are manifested when offenders go from being clean to using drugs in an arrest pair.

EPISODIC EFFECT OF DRUG USE --Type 1a :

$$R_{1a} = (V_i^{CU} / V_{i-1}^{CU}) / (V_i^{CC} / V_{i-1}^{CC}) \quad (1a)$$

The first ratio in equation (1a) compares violence levels for the same individuals when they are using and not using drugs. This ratio controls for fixed effects of time stationary factors that are related to violence, including enduring personality traits like thrill seeking and impulsivity that make individuals more or less inclined to use drugs *and* to be violent. Such stable sources of variation in violence levels are assumed to be the same on all arrests for the same individual, and so the ratio removes them and isolates the effects of factors that differ between arrests.

Drug use, however, is not the only factor that changes across arrests. Violence levels may also be affected by a combination of secular trends that are common to all individuals. These include factors like the emergence and increased availability of crack cocaine, changes in macro-economic conditions like rising unemployment, and changes in criminal justice effectiveness or policy that affect the risk of arrest per crime or the expected time served following an arrest. (e.g.,

²⁴ The maximum likelihood estimation strategy involves identifying a subset of offenders who have at least two arrests with drug tests during a fixed observation period. While this time window is reasonably long, extending from early 1984 to the end of 1990, the requirement of two arrests during this fixed interval will have a "squeezing" effect toward shorter inter-arrest intervals on the second arrest. This bias toward shorter intervals, and the associated increase in arrest rates, is explicitly accommodated in the likelihood function (see section on maximum likelihood estimation and the technical appendix). The trend ratio provides an additional control for any trend upward in individual arrest rates that might not be captured by the likelihood function.

explicit or defacto decriminalization of offenses involving small quantities of marijuana).

Assuming that the different subgroups of offenders represented in V^{CU} and V^{CC} are affected similarly by macro-temporal trends, the effects of broadly based secular changes are controlled by the second ratio in equation (1a).

Trends within a sample of offenders can also emerge from changes in individual circumstances that affect some but not all sample members (e.g., getting married or divorced, or losing a job). The analysis includes further controls for the effects of measured time-varying attributes of individuals that may impinge unequally on different subgroups of offenders. This is accomplished by specifying the individual arrest rate as:

$$V_i^D = \beta_0^D * \prod_j \beta_j^{X_j}$$

where β_0^D is the base arrest rate within subgroup D and the X_j are covariates representing other offender attributes j on arrest i . The ratio of R_{ia} in equation (1a) isolates the desired effect of drug use from secular trends and from other measured factors that may be changing independently of drug use for some offenders in a subgroup, as well as varying across different subgroups (e.g., marital or employment status at each arrest).

Withdrawal Effect when Not Using Drugs

Another version of episodic effects looks at changes in violence levels when individuals go from using drugs on one arrest to testing clean on a subsequent arrest.

(1b)

WITHDRAWAL EFFECT OF DRUG USE --Type 1b:

$$R_{1b} = (V_i^{UC} / V_{i-1}^{UC}) / (V_i^{CC} / V_{i-1}^{CC})$$

If there are no withdrawal effects and no residual lingering effects of prior drug use, the effects in equations (1a) and (1b) will be exact inverses of one another, $R_{1a} = 1 / R_{1b}$. In this event, the levels of offending are restored to previous non-use levels. Departures from this reciprocal relationship might include carryover effects that sustain some of the change that accompanies prior drug use. It is also possible that withdrawal from drug use has a more profound deleterious effect on offending than actual use.

Effect of Chronic Drug Use

The effects of continuing *chronic* drug use are reflected in the ratio:

EFFECT OF CHRONIC DRUG USE :

$$R_3 = (V_i^{UU} / V_{i-1}^{UU}) / (V_i^{CC} / V_{i-1}^{CC}) \quad (2)$$

As in all other contrasts, the ratio in equation (2) partials out the effects of broad based secular trends that are common to all offender subgroups and of measured time varying factors included among the covariates X_j .

2. DATA

The outcome variable is individual arrest rates reflecting the annual number of arrests an offender incurs. This rate is estimated from the length of inter-arrest intervals, with higher rates associated with shorter intervals. The strategy for detecting transitory effects of drug use employs the same offender at an earlier time as a control for enduring offender propensities. This is accomplished by contrasting individual arrest rates measured at two points in time for the same sample of offenders, e.g., V_{i-1}^{CU} and V_i^{CU} . Estimation of the required arrest rates thus requires that all offenders included in the analysis must have at least two arrests that are accompanied by results from the urine drug screen.

The required data are drawn from a random sample of adults arrested between July 1, 1985 and June 30, 1986 in Washington, DC. The data are individual arrest histories—including results from urine drug screens—for arrests that occur between the start of EMIT immunoassay drug testing in March 1984 and the end of data collection in August 1990. The offenders are drawn from a stratified random sample of 1,365 adults arrested on any charge in Washington, DC between July 1, 1985 and June 30, 1986. A total of 201 offenders satisfied the requirement of having at least two arrests that occurred outside the sampling window and also included drug test results.²⁵

The analysis derives separate estimates of individual arrest rates for several classes of offense types (Exhibit 8). Rates for *personal violence* are of primary interest. The other types are included to permit comparisons with related prior research. While robbery is typically included among violent offenses, the current analysis combines robbery and burglary to form the class of *predatory* offenses. These two offenses both involve elements of property loss together with potential or actual threats to personal safety during encounters between offenders and victims. An arrest is characterized by all the charged offenses, and so, for example, the same arrest may contribute to both personal violence and predatory offending levels if both rape and burglary are charged.

²⁵ The arrest history and drug test data come from the computerized case files of all adult arrestees maintained by the DC Pretrial Services Agency. A random sample of 1,365 arrestees was drawn from about 18,000 adults arrested in the 1985-86 sampling year. This sample was stratified to increase representation of demographic groups other than black males, who represented 73% of the total population of adults arrested in Washington, DC during the sampling year. In addition to oversampling whites and females, arrestees with urine screens and those with at least two prior arrests are also oversampled to increase the yield of offenders who have urine test results. The current analysis requires that offenders have at least two arrests in addition to the original sampled arrest. This results in further oversampling of offenders who have extensive arrest histories. The estimation strategy (described in a later section) includes explicit controls for these idiosyncratic features of sample selection.

[Exhibit 8 about here]

The urine drug screen tests for five types of drugs. The current analysis excludes amphetamines and methadone, and includes only those drugs with sufficient numbers of drug positive results to support the analyses, namely heroin, cocaine and PCP (Exhibit 8). Use of combinations of multiple drugs is pervasive in the sample of arrestees, and very few arrestees test positive for only a single drug.²⁶ While the analysis will not produce estimates of “pure” effects of individual drug types, the resulting estimates will reflect the effects of realistic patterns of illicit drug use among offenders.

The estimation of arrests rates is done separately by offense type and by drug type. In each estimate, individual arrests are classified either as using or clean with respect to a drug class. Exhibit 9 reports the estimation sample sizes in each drug class. While the samples of offenders using specific drug types are sometimes small, this is not a major concern because the estimation strategy estimates arrest rates while using, *U*, and while clean, *C*, as competing rates. In this competing rates formulation, observations of times to a *C* arrest also provide information about the time to a *U* arrest, namely time to $U > \text{time to } C$. Thus, information from *C* arrests also contributes to estimation of *U* arrest rates. The same information sharing also applies to arrest rate estimates for a relatively rare targeted offense type, e.g., personal violence, and its complement, e.g., not personal violence (Exhibit 10).

[Exhibits 9 and 10 about here]

²⁶ Individuals in the sample commonly use more than one drug, and relatively few are detected using only a single drug in the urine screen—14.6% of arrests involving heroin use, 31.6% of arrests involving cocaine use, and 40.5% of arrests involving PCP use test positive only for the identified drug type.

Exhibit 11 reports basic demographic attributes of individuals in the estimation sample. The requirement of at least two tested arrests (in addition to an arrest in the sampling window) increases the representation of offenders with long arrest histories. Almost 20% of the estimation sample had 10 or more prior arrests at the time of the first arrest in a tested pair, and this increases to 36% at the second arrest in the tested pair. While the estimation sample averages over 6 prior arrests at the first tested arrest and nearly 9 priors at the second tested arrest, the average was only 2.4 priors in the original stratified sample of arrestees from Washington, DC.²⁷ The requirement of long arrest histories also reduces the representation of white males in the estimation sample (down from 30.5% to 10.4%), but does not affect the relative proportions of the remaining race-by-sex subgroups. At an average of 28 to 30 years, age at arrest is similar in the estimation and original samples.

[Exhibit 11 about here]

3. *MAXIMUM LIKELIHOOD ESTIMATION*

Maximum likelihood techniques estimate individual arrest rates (measured by the annual number of arrests per offender) from the observed length of intervals between arrests. In general, the arrest rate declines as intervals increase in length. Estimation requires that we specify a likelihood function to describe the stochastic process that generates the observed data.²⁸ The current analysis follows a sample of offenders over time noting the length of intervals between arrests and the offense type and drug use status at each arrest.

²⁷ Note, the original stratified sample is also not representative of a typical pool of persons arrested in a year. Stratification to increase the representation of whites and females in the original sample also affects the mix of age and priors in the stratified sample.

²⁸ The details of the likelihood function and its derivation are in the technical appendix.

Exhibit 12 presents a schematic of an individual arrest history. Observations for each offender extend over 6.5 years from the start of routine EMIT testing of arrestees in March 1984 to the end of data collection in August 1990. Estimation focuses on the time-back intervals from a tested arrest to the immediately preceding arrest, t_1 and t_2 , for each arrest in a tested pair. The prior arrest is generally unconstrained: it can involve any offense type, be tested or not, and be any drug use status. It functions primarily as a signal of the start of the new arrest interval that ends at a tested arrest.

[Exhibit 12 about here]

The parameters of interest are Poisson arrest rates V_i^k and N_i^k , reflecting the rates of incurring violent and nonviolent arrests by drug use status k (for $k=CC, CU, UC, UU$) on arrest i (for $i=1,2$) in a tested pair.²⁹ In a competing rates formulation, the processes generating violent and nonviolent arrests run in tandem. When an arrest of either type occurs, both processes reset to begin a new interval. The likelihood function and associated parameter estimates for such a process are relatively straightforward.

Missing Drug Tests

The data, however, result from a more complicated process. While drug testing was in principle required following every arrest, about half the arrests do not have a completed urine drug screen. Missing tests usually occur for arrests that are handled through a desk appearance ticket issued at local precincts rather than through the central lockup and booking facility. The

²⁹ The Poisson arrest rate, V , derives from an underlying Poisson offending rate, λ , and a homogeneous arrest risk per crime, q_A , where $V = \lambda q_A$. The estimates developed here are for the *arrest* rates with no attempt to separately estimate the components λ and q_A .

missing tests introduce the possibility that the prior arrest that anchors the time back interval from a tested arrest could be either tested or untested. Thus, the likelihood function must be expanded to include the possibly different rates of incurring tested and untested arrests.

Two Tested Arrests are Required

Estimation further requires that offenders must have at least two tested arrests during the observation period. These provide the opportunity for detecting changes in arrest rates associated with detected drug use patterns. Even though the observation period is reasonably long (6.5 years), it is short relative to individual arrest rates that average one arrest every 4 to 9 years for relatively rare personal violence offenses. The requirement of at least two arrests of any type will tend to bias the observations toward shorter intervals and contribute to an upward bias in the estimated arrest rates.³⁰ Expanding the likelihood function to explicitly include the conditioning probability of two arrests controls this bias.

Excluding Data from the Original Sampling Window

The observed lengths of the interarrest intervals may be further distorted by the original sampling design that limited the sample to offenders who have an arrest sometime during the sampling window from July 1, 1985 to June 30, 1986. While the sampling decision was not based on offense type or drug test results, the requirement of having an arrest during this 1 year period will potentially distort the length of interarrest intervals that either end or begin with an arrest in that window. In a Poisson process, the potential distortions from the window arrest are easily handled by excluding the sampling window from the observation

³⁰ Two arrests in 6.5 years is associated with a minimum observed arrest rate of .3 total arrests annually.

period. Arrests during that period are ignored entirely, and the one-year window period is excluded from any interarrest intervals.

Individual Covariates of Offending

The final adjustment explicitly includes a limited set of covariates characterizing the offenders at the time of the tested arrest. Some of these are stable over time—race and sex—and others may vary. The latter include a simple time trend associated with year of arrest, the arrestee's work or school status, and whether the arrestee lives with a spouse (legal or common law). The covariates also include an explicit estimate of the probability of completing a drug test as a control for possible selection biases that result from unmeasured traits of offenders that influence both the likelihood of being tested and the seriousness and rates of offending of arrestees.³¹

4. RESULTS

The impact of drug use on offending is estimated from the ratio R of individual arrest rates when offenders use and do not use drugs. This ratio is detrended by the change over time observed when offenders test clean of drugs on two arrests. In each contrast the null hypothesis of no difference in rates amounts to testing the ratio R for statistically significant departures from a value of 1.0. These departures may be in the direction of aggravating effects, $R > 1$, or inhibiting effects, $R < 1$. A standard F-test for comparing hazard rates tests the statistical significance of differences departures from the null effect.

³¹ See Technical Appendix for further discussion of selection bias problem.

Exhibit 13 reports the estimated effects of drug use on offending rates. The reported values are multiplier effects on arrest rates associated with episodic drug use, withdrawal effects when offenders go from using to not using drugs, and chronic effects of continued drug use.

The dominant effects are:

- broad *inhibiting* effects of heroin and cocaine use on most types of offending,
- *aggravating* effects on predatory offending (robbery and burglary) during *withdrawal* from cocaine use (primarily in crack form), and
- both short- and long-term *aggravating* effects of PCP use on most types of offending, including personal violence.

These results—based on illicit drug use in real-world settings and actual dose levels—are especially noteworthy because they confirm findings previously observed only in artificial experimental settings (Fagan, 1990; Miczek and Thompson, 1983).

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**Exhibit 1. Contrasts between Offending Levels by Drug Users and Non-Users:
Variations by Offense Type and Drug Type**

Offense Type	Drug Type		
	Opiates/Heroin	Cocaine	PCP
Personal Violence		-	+
Predatory	-		+
Property/Theft	+		+
Drug Offenses	+	+	+
Public Order/Vice		-	-

NOTE: The contrast between offending levels relies on the ratio of arrest rates by drug users to arrest rates by nonusers. Positive and negative signs indicate the direction of statistically significant contrasts (+ if user rates exceed nonuser rates, - if user rates fall below nonuser rates. No entry indicates that the ratio of user-to-nonuser rates does not differ significantly from 1.0. The shaded areas are generally consistent with prior results of differences in offending by drug users and non-users of heroin.

Exhibit 2. Changes in Offending Levels by Drug Use Status: Variations by Offense Type and Drug Type

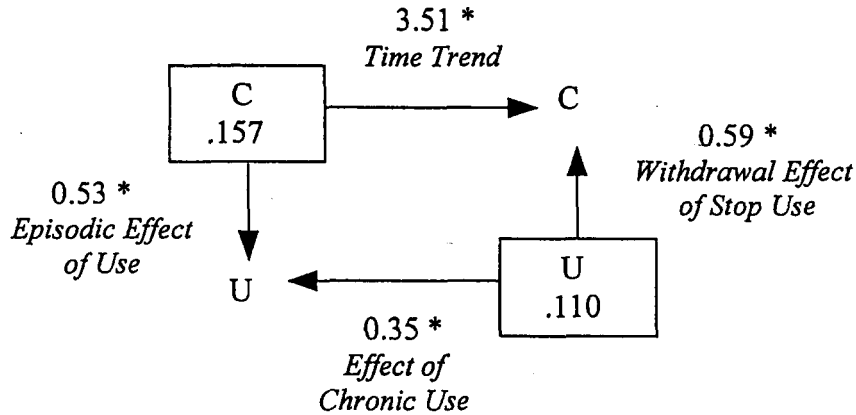
Offense Type	Drug Type					
	Opiates/Heroin		Cocaine		PCP	
Personal Violence	-	-	-	ns	ns	+
		-		ns		+
Predatory	ns	ns	ns	+	+	ns
		-		+		+
Property/Theft	ns	ns	-	ns	-	ns
		ns		ns		ns
Drug Offenses	ns	ns	-	ns	ns	-
		-		ns		+
Public Order/Vice	ns	ns	ns	ns	-	ns
		-		ns		+

NOTE: The contrast between offending levels relies on the ratio of arrest rates when the same sample of offenders use, U_i , and do not use (test clean for) drugs, C_i . The three entries in each cell are the contrasts between: U_i / C_{i-1} when use follows non-use, U_i / U_{i-1} when use follows use, and C_i / U_{i-1} when non-use follows use.

$\begin{matrix} U_i / C_{i-1} & C_i / U_{i-1} \\ U_i / U_{i-1} \end{matrix}$
--

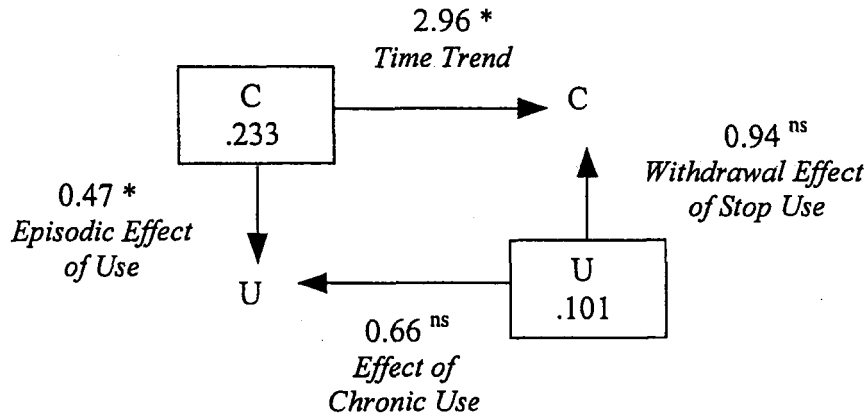
Positive and negative signs indicate the direction of statistically significant contrasts (+ if the rate at arrest i exceeds the rate at arrest $i-1$; - if the rate on arrest i falls below the rate on arrest $i-1$). Entries of ns indicate that the ratio of rates does not differ significantly from 1.0.

Exhibit 3. Transitory Effects of Opiate Use on Arrest Rates for Personal Violence



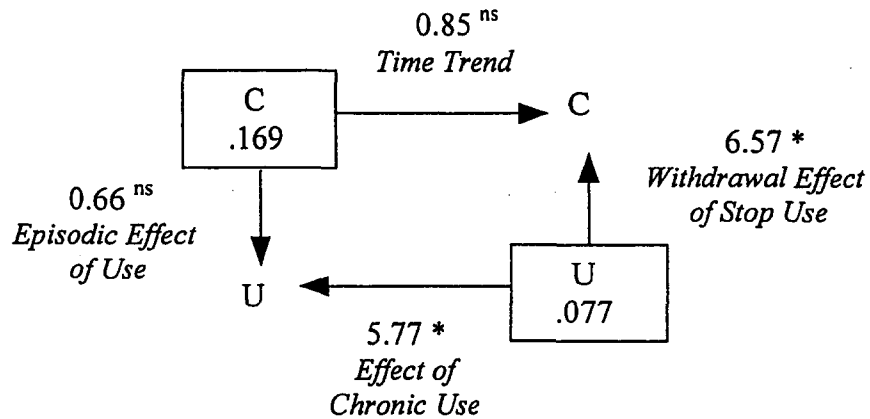
NOTE: Numbers in the boxes represent the overall mean annual arrest rate per offender when not using and when using drugs. The numbers accompanying each arrow are the multiplier effects of changing drug use status on the mean arrest rate of individual offenders, after controlling for enduring traits of the offenders and broad time trends in offending. U indicates drug use near the time of an offense and C indicates that the offender was not using—i.e., was “clean”—near the time of the offense. An asterisk indicates that the observed ratio of rates is substantially different from 1.0 (at the .00001 level in a two-tail approximation of a standard F-test for comparing hazard rates).

Exhibit 4. Transitory Effects of Cocaine Use on Arrest Rates for Personal Violence Offenses (Assaultive Crimes)



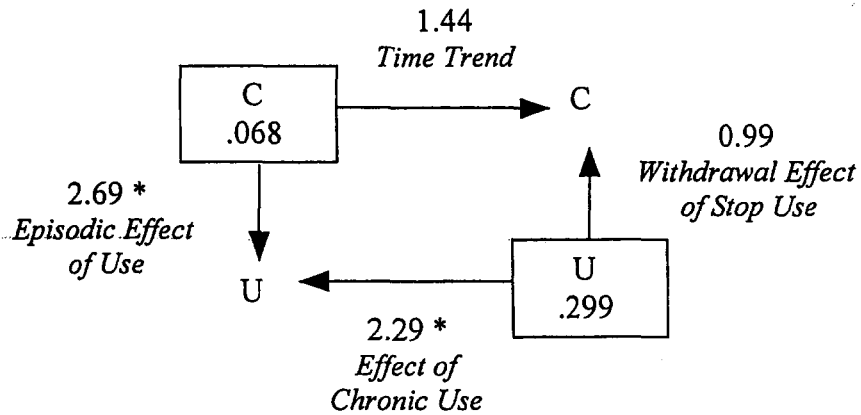
NOTE: Numbers in the boxes represent the overall mean annual arrest rate per offender when not using and when using drugs. The numbers accompanying each arrow are the multiplier effects of changing drug use status on the mean of individual offender arrest rates, after controlling for enduring traits of the offenders and broad time trends in offending. U indicates drug use near the time of an offense and C indicates that the offender was not using—i.e., was “clean”—near the time of the offense. An asterisk indicates that the observed ratio of rates is substantially different from 1.0 (at the .00001 level in a two-tail approximation of a standard F-test for comparing hazard rates).

Exhibit 5. Transitory Effects of Cocaine Use on Arrest Rates for Predatory Offenses (Robbery and Burglary)



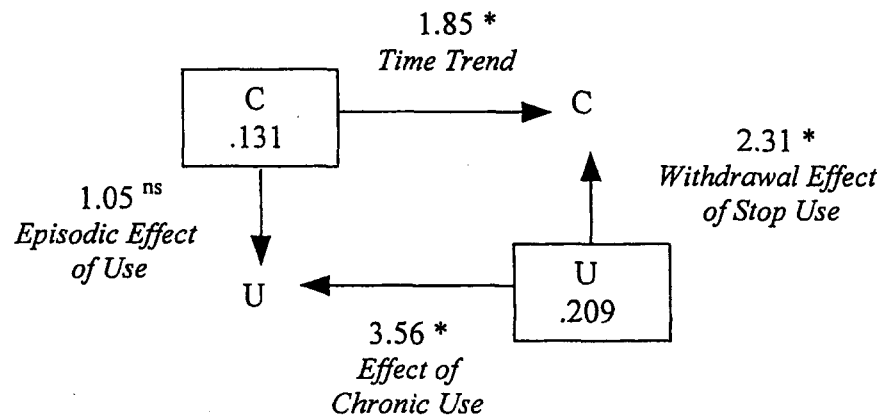
NOTE: Numbers in the boxes represent the overall mean annual arrest rate per offender when not using and when using drugs. The numbers accompanying each arrow are the multiplier effects of changing drug use status on the mean of individual offender arrest rates, after controlling for enduring traits of the offenders and broad time trends in offending. U indicates drug use near the time of an offense and C indicates that the offender was not using—i.e., was “clean”—near the time of the offense. An asterisk indicates that the observed ratio of rates is substantially different from 1.0 (at the .00001 level in a two-tail approximation of a standard F-test for comparing hazard rates).

Exhibit 6. Transitory Effects of PCP Use on Arrest Rates for Predatory Offenses



NOTE: Numbers in the boxes represent the overall mean annual arrest rate per offender when not using and when using drugs. The numbers accompanying each arrow are the multiplier effects of changing drug use status on the mean of individual offender arrest rates, after controlling for enduring traits of the offenders and broad time trends in offending. U indicates drug use near the time of an offense and C indicates that the offender was not using—i.e., was “clean”—near the time of the offense. An asterisk indicates that the observed ratio of rates is substantially different from 1.0 (at the .00001 level in a two-tail approximation of a standard F-test for comparing hazard rates).

Exhibit 7. Transitory Effects of PCP Use on Arrest Rates for Personal Violence



NOTE: Numbers in the boxes represent the overall mean annual arrest rate per offender when not using and when using drugs. The numbers accompanying each arrow are the multiplier effects of changing drug use status on the mean of individual offender arrest rates, after controlling for enduring traits of the offenders and broad time trends in offending. U indicates drug use near the time of an offense and C indicates that the offender was not using—i.e., was “clean”—near the time of the offense. An asterisk indicates that the observed ratio of rates is substantially different from 1.0 (at the .00001 level in a two-tail approximation of a standard F-test for comparing hazard rates).

Exhibit 8. Types of Offending and Illicit Drugs Included in Analysis

	Category	Included Items
Offense Types: ^a	Personal Violence	Murder, Manslaughter, Rape, Aggravated Assault, Simple Assault
	Predatory Offending	Robbery, Burglary
	Property/Theft	Larceny, Motor Vehicle Theft (including joy riding and theft from vehicle), Fraud, Embezzlement, Forgery, Stolen Property, Burglary Tools
	Drug Offenses	Possession, Manufacture, and Sales of Illicit Drugs
	Public Order/Vice	Commercial Sex (Prostitution), Gambling, Liquor Law Violations, Public Order, Trespassing, and other nuisance offenses
Illicit Drugs: ^b	Heroin Cocaine PCP	

^a Individual arrests may include more than one charge. All the charged offense types are used to characterize an arrest.

^b The test for drug use is an EMIT immunoassay urine screen. The screen is administered while arrestees are being processed for arraignment following an arrest, and is typically completed well within 24 hours of the arrest.

Exhibit 9. Sample Sizes for Each Drug Class

Mixture of Drug Use Types in Arrest Pairs ^a	Heroin	Cocaine	PCP	Polydrugs
<i>CC</i>	98	45	147	86
<i>CU</i>	25	37	14	31
<i>UC</i>	19 ^b	26	20	27
<i>UU</i>	59	93	20	57

^a *U* indicates arrests when the offender used the specified drug type and *C* indicates arrests when the offender tests clean for the specified type. So, for example, an arrest pair of type *CU* under heroin involves no heroin use on the first arrest in the pair but use on the second arrest in the pair.

^b The maximum likelihood procedure jointly estimates *C* and *U* as competing rates. In this formulation, observations of time intervals to a *C* arrest also provide information about the time to a *U* arrest—time to *U* > time to *C*. Thus, information from a larger number of *C* arrests augments the information from a smaller number of *U* arrests.

Exhibit 10. Sample Sizes for Each Offense Class

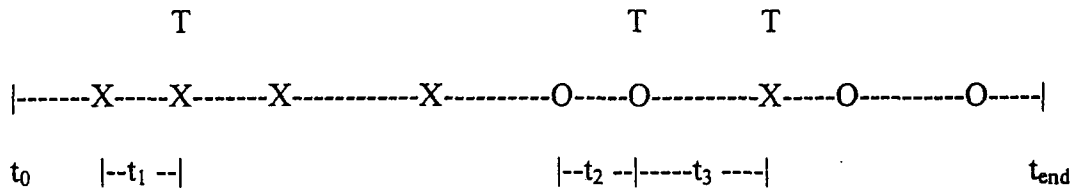
Prevalence of Targeted Offense Types	Personal Violence	Predatory	Property / Theft	Drugs	Public Order / Vice
1 st Arrest	16 ^a	13	46	99	65
2 nd Arrest	19	13	44	94	68

^a The maximum likelihood procedure jointly estimates rates for the targeted offense type (e.g., personal violence) and its complement (e.g., not personal violence) as competing rates. In this competing rates formulation, observations of time intervals to arrest for a complement offense also provide information about the time to the targeted offense—time to target > time to complement. Thus, information from the usually larger number of complement arrests augments the information from the smaller number of target arrests.

Exhibit 11. Demographic Attributes of Estimation Sample of Offenders (n=201)

Attribute	First Arrest in Tested Pair	Second Arrest in Tested Pair
Race and Sex: Percent Black Males	31.3	na
Percent White Males	10.4	na
Percent Black Females	33.8	na
Percent White Females	24.4	na
Age at Arrest: Percent 16 to 20	10.0	4.5
Percent 21 to 25	28.9	30.8
Percent 26 to 30	27.4	23.9
Percent 31 to 35	21.9	23.9
Percent 36 or older	11.9	16.9
Mean Age at Arrest	28.5	29.7
Number of Prior Arrests: Under 3	25.4	12.9
3 to 5	25.8	22.4
6 to 9	28.9	28.9
10 or more	19.9	35.8
Mean Number of Prior Arrests	6.69	8.75
Employed at Time of Arrest	44.4	37.3
Lives with Spouse (Married or Common Law)	8.0	5.5

Exhibit 12. Schematic of an Offender's Arrest History



where:

- t_0 = later of start of drug testing at arrest (March 1984)
- t_{end} = end of data collection (August 1990)
- X = violent arrest
- O = nonviolent arrest
- T = arrest is accompanied by a drug test
- t_1 = interval to 1st tested arrest (violent in this example)
- t_2 = interval to 2nd tested arrest (nonviolent in this example)
- t_3 = interval to last tested arrest (violent in this example)

Exhibit 13. Multiplier Effects of Changes in Drug Use on Individual Arrest Rates: Episodic, Withdrawal, and Chronic Effects by Offense and Drug Type^a

Offense and Drug Type	Episodic Use ^b $C \rightarrow U$	Withdrawal Effect ^c $U \rightarrow C$	Chronic Use ^d $U \rightarrow U$
Personal Violence:			
Heroin	.5287 * ^e	.5877 *	.3525 *
Cocaine	.4709 *	.9379	.6603
PCP	1.0484	2.3122 *	3.5638 *
Predatory:			
Heroin	.6872	1.1448	.0000 *
Cocaine	.6603	6.5712 *	5.7658 *
PCP	2.6874 *	.9865	2.2862 *
Property/Theft:			
Heroin	.7515	1.1687	.7385
Cocaine	.4400 *	1.1254	.9254
PCP	.6409 *	.7045	1.2259
Drugs:			
Heroin	.9007	.7693	.4786 *
Cocaine	.5984 *	1.1325	.8887
PCP	.6709	.5706 *	1.8546 *
Public Order/Vice:			
Heroin	1.2370	.6542	.4573 *
Cocaine	.6999	1.2439	.8158
PCP	.4637 *	1.0208	1.5739 *

^a Changes in individual arrests rates are reflected in the ratio of rates, V_i^D / V_{i-1}^D for drug use status, D , on a pair of arrests. Ratio values > 1.0 reflect aggravating multiplier effects associated with increasing arrest rates between the $i-1$ and i arrests in a tested pair. Ratios < 1.0 reflect inhibiting multiplier effects associated with decreasing arrest rates. All reported ratios are detrended relative to the change in rates on a pair of clean arrests.

^b The episodic effect of drug use is reflected in the rate change as offenders go from not using (i.e., testing "clean") drugs on one arrest to using drugs on a later arrest, $D = CU$. The magnitude of this effect is estimated from the detrended ratio, $(V_i^{CU} / V_{i-1}^{CU}) / (V_i^{CC} / V_{i-1}^{CC})$.

^c The withdrawal effect of drug use is reflected in the rate change as offenders go from using drugs to not using drugs on a tested pair of arrests, $D = UC$. The magnitude of this effect is estimated from the detrended ratio, $(V_i^{UC} / V_{i-1}^{UC}) / (V_i^{CC} / V_{i-1}^{CC})$.

^d The effect of chronic drug use is reflected in the rate change associated with sustained drug use on both arrests in a tested pair. This effect is estimated from the detrended ratio, $(V_i^{UU} / V_{i-1}^{UU}) / (V_i^{CC} / V_{i-1}^{CC})$.

^e Multiplier effects followed by an asterisk are significantly different from 1.0 at the .00001 level in a 2-tailed F-test (403,403 df) applied to a ratio of exponential hazard rates (Lawless, 1982). The test invokes an unusually high significance level to accommodate multiple tests performed on the same underlying data and the approximation of treating the trend effect in the denominator as a fixed parameter rather than a random variable.

Distinguishing Between Effects of Criminality and Drug Use on Violent Offending

TECHNICAL APPENDIX

I. MEASUREMENT ERRORS IN DETECTING DRUG USE

Two distinct sources of measurement errors are possible in detecting drug use: (1) proximity in time between occurrence of the violent incident and administration of the drug test following arrest, and (2) errors in the drug screen itself, particularly false negative errors of failing to detect some actual drug users.

The timing concern in item (1) potentially introduces errors of two types. On the one hand, delays between the incident and the drug test may lead to failures to detect actual drug use. On the other hand, drug use that is detected may actually occur prior to or after the violent incident. For slower metabolizing drugs that remain detectable for days, or even weeks, actual drug consumption may precede the violent incident by similarly long intervals. These errors in distinguishing between "cleans" and "users" will reduce the differences in observed outcomes for the two groups, and so will understate the impact of drug consumption on levels of violent offending.

Errors from source (2) will similarly understate the role of drugs. The EMIT drug assay is calibrated to be highly "specific" (i.e., highly accurate in identifying non-users as "clean"). It, however, has a lower "sensitivity" level (i.e., accuracy in identifying actual drug users), and so will incorrectly identify some "users" as "clean." These false negatives will reduce the differences observed between "users" and "cleans" and contribute to underestimates of the effect of drug use on violence.

Proximity in Time between Violent Incident and Drug Test

A variety of factors operate to minimize concerns about delays between the violent incidents and drug tests. First, research on police success in effecting arrests has shown consistently that successful clearance of offenses by arrest is rare, and those arrests that do occur are most likely to occur within the first 24 hours following the offense. Indeed, the original responding patrol officers are responsible for most arrests and these typically occur at the scene of the offense (e.g., Greenwood, 1970; Greenwood and Petersilia, 1977; Spelman and Brown, 1984).

Drug trafficking is one offense for which a more substantial delay to arrest might be expected. Arrests for trafficking offenses sometimes follow longer-term investigations involving undercover drug buys before arrests are made. Arrests for selected offenses involving theft of property may be similarly delayed by undercover fencing operations by the police. The violent offenses, which are of primary concern in the current research, are more typically cleared by arrest soon after the offense. Nevertheless, whatever arrest delays do occur will contribute to errors in classifying offenders as drug users or not at the time of the offense.

A second concern arising from the timing of offenses and drug tests is length of the detection window for tested drugs. Cocaine has the shortest detection window among the five drugs tested in the current analysis.¹ It can generally still be detected in EMIT urine tests up to 48 hours after ingesting the drug. Metabolites from the other tested drugs remain in urine even longer. Thus, it is very unlikely that the timing of drug tests—which typically occur within 24 hours of the violent incident—will contribute to failures to detect drug consumption that occurred at the time of the violent incident.

The EMIT tests at arrest, however, may detect drug use that occurs after the violent incident, or drug use that precedes the violent incident by enough time that the drug use may not be a direct precipitating factor in the incident. The closer the arrest is to the actual occurrence of the offense, the less likely it is that drug consumption follows the offense, and so the first source of false drug positives is not expected to be large. The second source of false drug positives, however, persists as some "cleans" at the time of the criminal incident are incorrectly classified as "users."

Whatever the origins, classification errors of "users" and "cleans" will reduce the difference in offending rates estimated for offenders who use and do not use drugs at the time of the offense, and will tend to understate the influence of drug use in violent incidents. Thus, the direction of bias from these classification errors is toward finding no effect, and any effects that are detected are likely to be larger than estimated.

Failure to Detect Drug Use

The EMIT immunoassay test has been calibrated specifically to minimize false positive results (i.e., non-users who test positive). With very few false positive errors found among individuals who are classified as drug users, the estimated offending levels by drug users will be unbiased.

False negative results are more common: the test fails to detect about 20% of actual drug users. Nevertheless the urine tests provide a markedly more reliable indicator of actual drug use by offenders than has traditionally been available from self reports (Wish, 1987; Magura, et. al, 1988; Mieczkowski, 1990). In a comparison of three immunoassay techniques to self reports, Mieczkowski, et. al, (1990) report that only 25% of arrestees who test positive for cocaine on an EMIT urine test self report that they used cocaine in the previous 48 hours. A similarly high non-reporting rate is also found for two other assays, including a relatively new assay that is intended to be sensitive to drug use at much lower threshold levels than have been traditionally used in EMIT urine tests.²

¹ The tested drugs are heroin, methadone, cocaine (crystal "crack" or powder), PCP, and amphetamines.

² Assuming complete accuracy for gas chromatography/mass spectrometry (GC/MS) confirming tests, the data in Table 1 of Fenton, et. al (1980) are consistent with "specificity" levels (i.e., the fraction of true negatives who test negative) in excess of 98% for EMIT immunoassays for methadone, opiates, barbiturates, and propoxyphene (PCP), and "sensitivity" levels (i.e., the fraction of true positives who test positive) of about 80% for the same drugs in 137 postmortem urine samples. Similar results are reported more recently in Visher (1991: Table 3) for three assay tests, including EMIT, applied to a criminal justice sample of 2,668 California parolees. These results are reproduced in

As a result, some individuals who are classified as "clean" in the analysis will actually be drug users. If drug use stimulates violence, these individuals will inflate the estimated levels of violent incidents that are found among the measured "clean" offenders, and thereby will reduce the contrast in violence levels between "users" and "cleans" and understate the magnitude of the effect of drug use on violence.³

With independently available information on the magnitude of the false negative problem, it is possible to recalibrate the results and remove the bias from the estimated violence levels. For example, assume that:

- p_t = proportion of total population who are "clean" of (i.e., not using) drugs at t ;
- $1 - p_t$ = proportion of drug "users" at t ;
- ρ_C = proportion of true "users" who test clean;
- ρ_U = proportion of true "cleans" who test as using;
- k_U = rate of violent offenses among true "users";
- k_C = rate of violent offenses among true "cleans".

The desired effect of drug use on violence is given by:

$$R = k_U / k_C$$

If there are no detection errors, then the rate of violent incidents within each subgroup yields:

(..continued)
the following table:

Drug/ Drug Class	EMIT		TDX		RIA	
	Specificity* (%)	Sensitivity** (%)	Specificity (%)	Sensitivity (%)	Specificity (%)	Sensitivity (%)
Cocaine	97.5	77.2	97.9	74.5	95.9	82.5
Opiates	97.8	82.1	98.3	82.5	98.2	85.3
Marijuana	97.9	71.0	99.1	59.2	98.8	62.9
PCP	99.8	78.6	98.4	93.2	98.7	94.2
Amphetamines	99.2	97.6	99.2	95.3	99.9	87.1

* Specificity = Identified Non-Users / Actual Non-Users

** Sensitivity = Identified Users / Actual Users

³ A similar bias occurs among "cleans" if drug use inhibits violence. In that case, drug users who are mistakenly included among "cleans" will reduce the estimated level of violent incidents by "clean" offenders. Again the bias shifts violence levels by "cleans" closer to those of "users", thus underestimating the effect of drug use.

$$R = (V_t^{CU} / V_{t-1}^{CU}) / (V_t^{CC} / V_{t-1}^{CC}) = (k_U / k_C) / (k_C / k_C) = k_U / k_C$$

When there are detection errors, with some "users" testing clean and some "cleans" testing as users, the measured rate of violent offenses in each subgroup is a weighted average of k_U and k_C :

$$\begin{aligned} V_t^{CU} &= \frac{(1-p_t)(1-\rho_C)k_U + p_t\rho_U k_C}{(1-p_t)(1-\rho_C) + p_t\rho_U} \\ V_{t-1}^{CU} &= \frac{p_{t-1}(1-\rho_U)k_C + (1-p_{t-1})\rho_C k_U}{p_{t-1}(1-\rho_U) + (1-p_{t-1})\rho_C} \\ V_t^{CC} &= \frac{p_t(1-\rho_U)k_C + (1-p_t)\rho_C k_U}{p_t(1-\rho_U) + (1-p_t)\rho_C} \\ V_{t-1}^{CC} &= \frac{p_{t-1}(1-\rho_U)k_C + (1-p_{t-1})\rho_C k_U}{p_{t-1}(1-\rho_U) + (1-p_{t-1})\rho_C} \end{aligned} \tag{A1}$$

and

$$R = (V_t^{CU} / V_{t-1}^{CU}) / (V_t^{CC} / V_{t-1}^{CC})$$

becomes by substitution,

$$\frac{(R/K)p_t(1-\rho_U) - p_t\rho_U}{(1-p_t)(1-\rho_C) - (R/K)(1-p_t)\rho_C} = \frac{k_U}{k_C} \tag{A2}$$

where

$$K = \frac{p_t(1-\rho_U) + (1-p_t)\rho_C}{(1-p_t)(1-\rho_C) + p_t\rho_U}$$

Equation (A2) can be evaluated to obtain the desired unbiased estimate of the effect of drug use on offending, k_U / k_C .⁴ As detection errors decline (i.e., ρ_U, ρ_C approach 0), the estimated drug effect, R , approaches its unbiased value of k_U / k_C .

⁴ Note, that the proportion of "cleans" estimated from the offender sample is biased by the presence of detection errors to be:

$$\hat{p}_t = p_t(1-\rho_U) + (1-p_t)\rho_C$$

The adjustment for detection errors should rely on the estimate of the true proportion of "cleans" obtained from:

$$p_t = (\hat{p}_t - \rho_C) / (1 - \rho_C - \rho_U)$$

Summary

Measurement errors in detecting drug use will tend to understate the impact of drug consumption on offending rates. This means that estimates that **do find a significant influence** of drug use on offending provide even more compelling evidence that such an effect does in fact exist. A failure to find effects, however, may reflect measurement error bias.

One solution to measurement error problems is to seek more precise measures of drug use that more accurately identify the time when the drug is actually consumed. It is also possible, however, to recalibrate the original biased results using estimates of the extent of measurement errors in the data. By utilizing a plausible range of estimates of the extent of measurement errors in combination with the relationship in equation (A2), it is possible to do sensitivity analyses that explore bounds on the influence of measurement errors in the results.

Table A1 provides an example of the impact of measurement error on the results reported here. The illustration adjusts the effects estimated from the data by an assumed 20% error rate of true "users" who test as clean and a 7.5% error rate in of true "cleans" who test as users. The 20% false negative errors ("users" who test clean) are compatible with reported sensitivity levels of the EMIT urine screen (see note 2). The rate of 7.5% false positive errors ("cleans" who test as using) is several times larger than reported specificity levels of the EMIT test. This allows for additional errors of commission arising from the long detection window of tested drugs. The results after adjusting for measurement error largely support the unadjusted estimates. In every case the unadjusted multiplier effects estimated directly from the data understate the magnitude of the adjusted effect, and the estimated ratio is always closer to a "no effect" value of 1.0. Furthermore, in all but two cases the adjusted and unadjusted ratios agree on significance. In the two cases of disagreement (for public order/vice arrest rates) the unadjusted rates are not far from significance.

II. MAXIMUM LIKELIHOOD ESTIMATION OF INDIVIDUAL OFFENDING RATES

Estimating the impact of drug use on individual offending rests fundamentally on calibrating the magnitude of changes in the annual offending rate associated with changes in individual consumption of illicit drugs. Since it is generally not possible to observe and measure actual offending behavior, the analysis relies on the subset of offending that is detected through arrest. Furthermore, there are no experimental controls regulating the dose, timing, or even the types of illicit drugs that are consumed, and no experimental controls for factors other than drugs that influence individual offending. Instead the data derive from the actual drug consumption patterns of a sample of offenders facing the real day to day challenges of a large urban environment. This makes estimation of the effects much more difficult than in a controlled experiment, but the results are likely to be more relevant to the actual drug consumption and offending experiences that confront offenders on a daily basis.

The main challenge for estimation is to isolate the effect of drug use on offending from the intervening arrest and drug consumption processes that generate the data. We accomplish this through a probability model that explicitly represents the influence of these processes in the

observed data. The model is applied to data on the arrest histories—including results from urine drug screens administered at the time of arrest—for a sample of adults arrested in Washington, DC from July 1, 1985 to June 30, 1986. Applying the model to data on the length of interarrest intervals provides the basis for maximum likelihood estimates of the changes in individual arrest rates associated with drug use status near the time of the offense. The remainder of this technical appendix describes the probability model and the associated maximum likelihood estimation of individual arrest rates when offenders are using and not using various types of drugs.

A Probability Model Linking Drug Use and Offending

The fundamental observations are changes in the length of interarrest intervals associated with drug use status at the time of arrest. Estimation requires that offenders must have at least two tested arrests (henceforth referred to as the "tested pair") revealing both drug-use status and charged offenses type on each arrest in a pair.

It is vitally important that the estimates address potential sources of bias in the observed lengths of interarrest intervals for an offender. Two factors of note are: (1) a general sampling bias toward shorter intervals—and hence toward higher arrest rates—among offenders who qualify for analysis because they have at least two arrests in the observation period, and (2) a "squeezing" effect toward shorter intervals on the second arrest in a tested pair due to constraints on the total length of the observation period. The first bias arises from the sampling conditions and distinguishes the offenders included in the analysis from offenders generally. Without appropriate adjustments, the sampled data will understate interval lengths, and overstate associated arrest rates of offenders generally. The second bias applies differentially to the first and second arrest in tested pairs for each sampled offender, biasing second intervals downward and hence overstating arrest rates on the second arrest in a tested pair.

Characterizing Interarrest Intervals between Tested Arrests

Maximum likelihood estimation corrects for the above sources of bias in the estimated arrest rates by explicitly incorporating the processes that produce the observed interval data. The likelihood function derives from a basic model of individual offending characterized by the following assumptions:

1. Offenders are assumed to be at risk of offending over the entire observation period.⁵

⁵ The model of offending makes a number of simplifying assumptions about offending "careers". The estimated offending rates make no attempt to distinguish between periods of active offending and inactive periods when the offending rate may drop to trace levels at or near zero. Instead, the estimated rates combine the frequency of crimes committed during active periods together with an intermittency rate of moving between active and inactive periods during the observation period. This characterization of intermittent offending (introduced in alternative forms in Barnett, et al, 1989; Nagin and Land, 1993) is an extension of earlier, more restrictive assumptions of an active career bounded by a single initiation point (usually occurring during the teenage years), and continuing to another single point of permanent termination from offending (Blumstein, et al, 1986; Blumstein and Cohen, 1987).

Assuming a Poisson process in which active days occur at rate π , the observed active days are expected to be distributed uniformly over the observation period. *Conditional on being active*, an offender is assumed to commit crimes at Poisson rate λ_C , and crime days will be distributed uniformly over the active days. Observed crimes, then,

2. Arrest rates vary by offense type for violent and non-violent arrests ($j = V, N$), drug-use status at arrest ($k = CC, CU, UC, UU$), and the order of arrests in tested pairs ($n = 1, 2$).
3. Individual offenders are arrested for violent (V) and non-violent offenses (N) at competing Poisson rates λ_{nV_k} and λ_{nN_k} , respectively.^{6,7}
4. Arrests result in urine drug screens at a single constant probability per arrest, Q , resulting in competing Poisson rates $\mu_{nV_k} = Q \lambda_{nV_k}$ and $\mu_{nN_k} = Q \lambda_{nN_k}$ for *tested* violent and non-violent arrests, respectively; and $m_{nk} = (1 - Q)(\lambda_{nV_k} + \lambda_{nN_k})$ for *untested* arrests.⁸
5. Ignoring the particular offense types of outcomes in a Poisson competing rates process, individual *tested* arrests occur at the combined rate, $(\mu_{nV_k} + \mu_{nN_k})$, and the probability of an interarrest interval exactly x in length is,

$$f_{X_{nk}}(x) = (\mu_{nV_k} + \mu_{nN_k}) \exp[-(\mu_{nV_k} + \mu_{nN_k})x].$$
6. When a tested arrest occurs, the probability that the arrest is violent is given by $\mu_{nV_k} / (\mu_{nV_k} + \mu_{nN_k})$ and the probability the arrest is non-violent by $\mu_{nN_k} / (\mu_{nV_k} + \mu_{nN_k})$.

We begin by ignoring, for the moment, the biasing conditioning events described above and the possibility of intervening *untested* arrests. In estimating arrest rates for *violent* and *non-violent tested* arrests we focus initially on the *time-back* interval from a tested arrest to the immediately preceding *tested* arrest. Poisson processes have the unique property that the time back from one arrest to the immediately preceding arrest ("backward recurrence time" in a renewal process), the time forward from one arrest to the next arrest ("forward recurrence time"), and the ordinary unconditional time interval between arrests all share the same

(..continued)

will occur at an unconditional, reduced Poisson rate, $\pi\lambda_C$, during the observation period. We make no attempt to separately estimate the parameters π and λ_C , and instead base our estimates on the reduced offending rate, λ_R .

⁶ A competing rates process is a particular form of a renewal process in which the stochastic process generating violent arrests runs in parallel with the process generating non-violent arrests. As soon as an arrest of either type occurs, both processes stop and begin again ("renew"). The observed arrest type is said to have "beaten" the other type, yielding information on the exact length of the time interval to the winning type, say x , as well as information that the interval to the losing type is longer than x .

⁷ The empirical analysis is restricted to the *arrest* process, which may be represented as a "thinned" sample from an underlying *crime* process. The Poisson arrest rate is derived from the product of the underlying Poisson *offending rate*, λ_R , and a constant *probability of arrest per crime*, q_A , such that $\lambda_{nj_k} = \lambda_{R_{nj_k}} * q_{A_{nj_k}}$ (for $n = 1^{\text{st}}$ or 2^{nd} arrest in a tested pair; $j = V$ or N offense types; $k =$ drug use status $CC, CU, UC, \text{ or } UU$). The estimates developed here will be *arrest rates* with no attempt to separately identify the offending rate and probability of arrest that contribute to individual arrest rates.

⁸ We make no distinction between violent and non-violent arrests among untested arrests.

probability density function. For $n =$ the order of arrests in a tested pair ($n = 1, 2$) and $k =$ drug-use status in a tested pair ($k = CC, CU, UC, UU$), let X_{nVk} and X_{nNk} be random variables of the time-back intervals from a violent or non-violent tested arrest, respectively, to a preceding tested arrest of any type. The resulting probability density function (pdf) of observing a time-back interval of length x from a tested violent arrest back to a preceding tested arrest of any type is,

$$\begin{aligned} f(X_{nVk} = x) &= P \begin{bmatrix} \text{tested} \\ \text{arrest} \\ \text{at } x \end{bmatrix} * P \begin{bmatrix} \text{arrest} \\ \text{at } x \text{ is} \\ \text{violent} \end{bmatrix} \\ &= (\mu_{nVk} + \mu_{nNk}) \exp[-(\mu_{nVk} + \mu_{nNk})x] * \frac{\mu_{nVk}}{\mu_{nVk} + \mu_{nNk}} \\ &= \mu_{nVk} \exp[-(\mu_{nVk} + \mu_{nNk})x] \end{aligned} \tag{A3}$$

Likewise, the same pdf for time back from a tested non-violent arrest to a preceding tested arrest of any type is given by,

$$f(X_{nNk} = x) = \mu_{nNk} \exp[-(\mu_{nVk} + \mu_{nNk})x] \tag{A4}$$

In a tested pair of arrests, both the timing of the two arrests and the offense types of the arrests are independent. And so, continuing to ignore the conditioning events and any intervening untested arrests, the joint pdf of time-back intervals for a tested pair of arrests is given by a simple product of equations (A3) and (A4):

$$f_{X_{1ik}, X_{2jk}}(x_1, x_2) = \mu_{1ik} \mu_{2jk} \exp[-(\mu_{1Vk} + \mu_{1Nk})x_1] \exp[-(\mu_{2Vk} + \mu_{2Nk})x_2] \tag{A5}$$

where:

- subscripts $n = 1$ and 2 indicate the first and second arrests, respectively, in a tested pair,
- subscript i and j are the offense types (Violent or Non-violent) of the first and second arrest, respectively, in a tested pair, and
- subscript k is the drug-use class for a tested pair (i.e., CC, CU, UC , or UU on the first and second arrests, respectively).

Incorporating Possibility of Intervening Untested Arrests

We now include the possibility that the immediately preceding arrest in a time-back interval may be either tested or untested. Define two new random variables, A_{1ik} and A_{2jk} , representing

the time back to the most recent *untested* arrest that occurs before each arrest in a tested pair.⁹ In this event, the time-back intervals to the arrests that immediately precede the two arrests in a tested pair are simply,

$$T_{nik} = \text{Min}[A_{nik}, X_{nik}]$$

$n = 1^{\text{st}}$ or 2^{nd} arrest in tested pair
 $i = V$ or N offense type of tested pair
 $k =$ drug use status at tested arrests

(A6)

We find the desired joint pdf, $f_{T_{1ik}, T_{2jk}}(t_1, t_2)$ by first finding,

$$P[T_{1ik} \leq t_1, T_{2jk} \leq t_2] = \int_0^\infty \int_0^\infty P[T_{1ik} \leq t_1, T_{2jk} \leq t_2 | X_{1ik} = x_1, X_{2jk} = x_2] * f_{X_{1ik}, X_{2jk}}(x_1, x_2) dx_1 dx_2$$

(A7)

where, $f_{X_{1ik}, X_{2jk}}(x_1, x_2)$ is defined above in (A5) and,

$$P[T_{1ik} \leq t_1, T_{2jk} \leq t_2 | X_{1ik} = x_1, X_{2jk} = x_2] =$$

1 if $x_1 \leq t_1$ and $x_2 \leq t_2$
 $a_1 > t_1$ and $a_2 > t_2$
 $(1 - e^{-m_{2k} t_2})$ if $x_1 \leq t_1$ and $x_2 > t_2$
 $a_1 > t_1$ and $a_2 \leq t_2$
 $(1 - e^{-m_{1k} t_1})$ if $x_1 > t_1$ and $x_2 \leq t_2$
 $a_1 \leq t_1$ and $a_2 > t_2$
 $(1 - e^{-m_{1k} t_1})(1 - e^{-m_{2k} t_2})$ if $x_1 > t_1$ and $x_2 > t_2$
 $a_1 \leq t_1$ and $a_2 \leq t_2$

(A8)

By substitution of (A8) into (A7),

⁹ The prior *untested* arrest need not immediately precede the tested arrest.

$$\begin{aligned}
P[T_{1ik} \leq t_1, T_{2jk} \leq t_2] &= \int_0^{t_1} \int_0^{t_2} f_{X_{1ik}, X_{2jk}}(x_1, x_2) dx_2 dx_1 \\
&+ \int_0^{t_1} \int_{t_2}^{T-x_1} f_{X_{1ik}, X_{2jk}}(x_1, x_2) (1 - e^{-m_{2k} t_2}) dx_2 dx_1 \\
&+ \int_0^{t_2} \int_{t_1}^{T-x_2} f_{X_{1ik}, X_{2jk}}(x_1, x_2) (1 - e^{-m_{1k} t_1}) dx_1 dx_2 \\
&+ \int_{t_1}^{T-t_2} \int_{t_2}^{T-x_1} f_{X_{1ik}, X_{2jk}}(x_1, x_2) (1 - e^{-m_{1k} t_1}) (1 - e^{-m_{2k} t_2}) dx_2 dx_1
\end{aligned} \tag{A9}$$

where T is the total time during which offenders may incur tested arrests.¹⁰

After substituting (A5) for $f_{X_{1ik}, X_{2jk}}(x_1, x_2)$ in (A9), we can solve each of the four double integrals found in (A9). Since we are only interested in $P[T_{1ik} \leq t_1, T_{2jk} \leq t_2]$ in order to obtain the joint pdf of time-back intervals,

$$f_{T_{1ik}, T_{2jk}}(t_1, t_2) = \frac{\delta^2 P[T_{1ik} \leq t_1, T_{2jk} \leq t_2]}{\delta t_1 \delta t_2} \tag{A10}$$

we can drop all terms in the solution to (A9) that do not contain *both* t_1 and t_2 , and we are left with,

$$\begin{aligned}
P^o [T_{1ik} \leq t_1, T_{2jk} \leq t_2] &= \frac{\mu_{1ik} * \mu_{2jk}}{\mu_{1k} \mu_{2k}} * \{ \exp[-(\mu_{1k} + m_{1k}) t_1 - (\mu_{2k} + m_{2k}) t_2] \\
&- \frac{\mu_{2k}}{\mu_{2k} - \mu_{1k}} \exp[-\mu_{1k} T - m_{1k} t_1 - (\mu_{2k} + m_{2k} - \mu_{1k}) t_2] \\
&- \frac{\mu_{1k}}{\mu_{1k} - \mu_{2k}} \exp[-\mu_{2k} T - m_{2k} t_2 - (\mu_{1k} + m_{1k} - \mu_{2k}) t_1] \}
\end{aligned} \tag{A11}$$

where,

¹⁰ Adult arrestees in Washington, D.C. were subject to urine testing at arrest from the start of a policy of EMIT drug testing in March 1984 to the end of data collection in August 1990 for a total $T = 78$ months. Almost all (92%) of the arrestees who had at least two tested arrests had turned age 18 by the start of the testing program, and thus were eligible for testing for the entire observation period, T . The remaining 8% of arrestees were observed for ranges from 53 to 76 months, with an average of 65.3 months. These observation periods are sufficiently long that we make the reasonable assumption of a single common observation period, $T = 78$ months, for all arrestees in the analysis.

$$\mu_{1k} = \mu_{1Vk} + \mu_{1Nk}$$

$$\mu_{2k} = \mu_{2Vk} + \mu_{2Nk}$$

$i, j =$ offense type V or N on 1st and 2nd tested arrest

Taking derivatives of (A11) with respect to both t_1 and t_2 as in (A10), we have:

$$\begin{aligned} f_{T_{1ik}, T_{2jk}}(t_1, t_2) &= \frac{\mu_{1ik}}{\mu_{1k}} * \frac{\mu_{2jk}}{\mu_{2k}} * \{(\mu_{1k} + m_{1k})(\mu_{2k} + m_{2k}) \exp[-(\mu_{1k} + m_{1k})t_1 - (\mu_{2k} + m_{2k})t_2] \\ &\quad - \frac{\mu_{2k}}{\mu_{2k} - \mu_{1k}} m_{1k} (\mu_{2k} + m_{2k} - \mu_{1k}) \exp[-\mu_{1k}T - m_{1k}t_1 - (\mu_{2k} + m_{2k} - \mu_{1k})t_2] \\ &\quad - \frac{\mu_{1k}}{\mu_{1k} - \mu_{2k}} m_{2k} (\mu_{1k} + m_{1k} - \mu_{2k}) \exp[-\mu_{2k}T - m_{2k}t_2 - (\mu_{1k} + m_{1k} - \mu_{2k})t_1]\} \end{aligned} \quad (A13)$$

where μ_{1k} and μ_{2k} are defined above.

Adjusting for Bias Arising from Conditioning Events

Equation (A5) provides the joint pdf of time-back intervals for any pair of tested arrests where the first tested arrest is for offense type i and the second tested arrest is for offense type j . This pdf is *unconditional*, applying to all such tested pairs no matter when they occur. The data we will use, however, include only time-back intervals for those offenders who have at least two tested arrests within a constrained observation period. This constraint will tend to bias the observations toward shorter intervals.

The data are thus generated by a *conditional* process, and the expression $f_{X_{1ik}, X_{2jk}}(x_1, x_2)$ should be replaced by the *conditional* pdf,

$$f_{X_{1ik}, X_{2jk} | X_{1ik} + X_{2jk} \leq T}(x_1, x_2) = \frac{f_{X_{1ik}, X_{2jk}}(x_1, x_2)}{P[X_{1ik} + X_{2jk} \leq T]} \quad (A14)$$

in the derivation of (A13). For any particular tested pair of type i on the first arrest and type j on the second arrest, the conditional probability that both arrests occur within an observation period of length T is,

$$\begin{aligned}
P[X_{1ik} + X_{2jk} \leq T] &= \int_0^T \int_0^{T-x_1} f_{X_{1ik} X_{2jk}}(x_1, x_2) dx_2 dx_1 \\
&= \mu_{1ik} \mu_{2jk} \int_0^T \int_0^{T-x_1} \exp[-(\mu_{1Vk} + \mu_{1Nk})x_1] \exp[-(\mu_{2Vk} + \mu_{2Nk})x_2] dx_2 dx_1 \\
&= \frac{\mu_{1ik}}{\mu_{1Vk} + \mu_{1Nk}} * \frac{\mu_{2jk}}{\mu_{2Vk} + \mu_{2Nk}} * \left[1 - \frac{\mu_{1k}}{\mu_{1k} - \mu_{2k}} e^{-\mu_{2k}T} - \frac{\mu_{2k}}{\mu_{2k} - \mu_{1k}} e^{-\mu_{1k}T} \right]
\end{aligned} \tag{A15}$$

where,

$$\mu_{1k} = \mu_{1Vk} + \mu_{1Nk}$$

$$\mu_{2k} = \mu_{2Vk} + \mu_{2Nk}$$

When the conditioning probability in (A15) is applied to any arbitrary tested pair of arrests, regardless of the offense types on those arrests, (A15) becomes,

$$\begin{aligned}
P[X_{1k} + X_{2k} \leq T] &= \sum_{i=V,N} \sum_{j=V,N} \frac{\mu_{1ik}}{\mu_{1Vk} + \mu_{1Nk}} * \frac{\mu_{2jk}}{\mu_{2Vk} + \mu_{2Nk}} \\
&\quad * \left[1 - \frac{\mu_{1k}}{\mu_{1k} - \mu_{2k}} e^{-\mu_{2k}T} - \frac{\mu_{2k}}{\mu_{2k} - \mu_{1k}} e^{-\mu_{1k}T} \right] \\
&= 1 - \frac{\mu_{1k}}{\mu_{1k} - \mu_{2k}} e^{-\mu_{2k}T} - \frac{\mu_{2k}}{\mu_{2k} - \mu_{1k}} e^{-\mu_{1k}T}
\end{aligned} \tag{A17}$$

Since (A17) does not vary across arrestees -- it depends only on the total observation time, T , and arrest rates, $\mu_{nk} = (\mu_{nVk} + \mu_{nNk})$, which are assumed to be homogeneous across offenders who share a common drug-use status k -- we can represent this probability by a constant,

$$P[T_{1k} \leq t_1, T_{2k} \leq t_2] = \mathbf{K} = 1 - \frac{\mu_{1k}}{\mu_{1k} - \mu_{2k}} e^{-\mu_{2k}T} - \frac{\mu_{2k}}{\mu_{2k} - \mu_{1k}} e^{-\mu_{1k}T} \tag{A18}$$

Final Probability of Observed Time-Back Intervals in Tested Pairs of Arrests

Substituting (A17) and (A5) into (A14), the desired *conditional* pdf of observed interarrest intervals when arrestees have at least two tested arrests is,

$$f_{X_{1ik}, X_{2jk} | X_{1ik} + X_{2jk} \leq T}(x_1, x_2) = \frac{1}{\mathbf{K}} \mu_{1ik} \mu_{2jk} \exp[-(\mu_{1Vk} + \mu_{1Nk})x_1] \exp[-(\mu_{2Vk} + \mu_{2Nk})x_2] \tag{A20}$$

Equation (A20) is then substituted for $f_{X_{1ik}, X_{2jk}}(x_1, x_2)$ in the derivation of (A13) to yield the final conditional probability of an observed pair of time-back intervals for offenders who have at least two tested arrests in observation period T . This probability for observed tested pairs of arrests of offense type i on the first arrest and type j on the second arrest is as follows:

$$\begin{aligned}
 f_{T_{1ik}, T_{2jk} | X_{1ik}, X_{2jk} \leq T}(t_1, t_2) &= \frac{1}{K} * \frac{\mu_{1ik}}{\mu_{1k}} * \frac{\mu_{2jk}}{\mu_{2k}} \\
 &\quad * \{ (\mu_{1k} + m_{1k})(\mu_{2k} + m_{2k}) \exp[-(\mu_{1k} + m_{1k})t_1 - (\mu_{2k} + m_{2k})t_2] \\
 &\quad - \frac{\mu_{2k}}{\mu_{2k} - \mu_{1k}} m_{1k} (\mu_{2k} + m_{2k} - \mu_{1k}) \exp[-\mu_{1k}T - m_{1k}t_1 - (\mu_{2k} + m_{2k} - \mu_{1k})t_2] \\
 &\quad - \frac{\mu_{1k}}{\mu_{1k} - \mu_{2k}} m_{2k} (\mu_{1k} + m_{1k} - \mu_{2k}) \exp[-\mu_{2k}T - m_{2k}t_2 - (\mu_{1k} + m_{1k} - \mu_{2k})t_1] \}
 \end{aligned} \tag{A21}$$

Maximum Likelihood Estimation of Arrest Rates

The final conditional probability of any pair of time-back intervals in (A21) is used in forming the likelihood function for the time-back intervals that are actually observed in the data. Within any drug-use class k , this likelihood function, L_k , is as follows:

$$\begin{aligned}
 L_k &= \frac{1}{K^{N_k}} * \left(\frac{\mu_{1Vk}}{\mu_{1k}} \right)^{n_{ok} + n_{pk}} * \left(\frac{\mu_{1Nk}}{\mu_{1k}} \right)^{n_{qk} + n_{rk}} * \left(\frac{\mu_{2Vk}}{\mu_{2k}} \right)^{n_{ok} + n_{qk}} * \left(\frac{\mu_{2Nk}}{\mu_{2k}} \right)^{n_{pk} + n_{rk}} * e^{-m_{1k}T_1 - m_{2k}T_2} \\
 &\quad * \prod_{y=1}^{N_k} \{ (\mu_{1k} + m_{1k})(\mu_{2k} + m_{2k}) e^{-\mu_{1k}t_{1y} - \mu_{2k}t_{2y}} \\
 &\quad - \frac{\mu_{2k} m_{1k} (\mu_{2k} + m_{2k} - \mu_{1k})}{\mu_{2k} - \mu_{1k}} * e^{-\mu_{1k}T - (\mu_{2k} - \mu_{1k})t_{2y}} \\
 &\quad - \frac{\mu_{1k} m_{2k} (\mu_{1k} + m_{1k} - \mu_{2k})}{\mu_{1k} - \mu_{2k}} * e^{-\mu_{2k}T - (\mu_{1k} - \mu_{2k})t_{1y}} \}
 \end{aligned} \tag{A22}$$

where

- y = a tested pair of arrests in drug class k
- n_{ok} = number of VV tested pairs
- n_{pk} = number of VN tested pairs
- n_{qk} = number of NV tested pairs
- n_{rk} = number of NN tested pairs
- $N_k = n_{ok} + n_{pk} + n_{qk} + n_{rk}$

which after algebraic manipulation becomes,

$$L_k = \frac{\lambda_{1Vk}^{n_{ok}+n_{pk}} * \lambda_{1Nk}^{n_{ok}+n_{rk}} * \lambda_{2Vk}^{n_{ok}+n_{qk}} * \lambda_{2Nk}^{n_{pk}+n_{rk}}}{[\lambda_{2k} - \lambda_{1k} - \lambda_{2k} e^{-\lambda_{1k} Q T} + \lambda_{1k} e^{-\lambda_{2k} Q T}]^{N_k}} * e^{-\lambda_{1k} \Pi_1 - \lambda_{2k} \Pi_2} * \prod_{y=1}^{N_k} [(\lambda_{2k} - \lambda_{1k}) - (1-Q)(\lambda_{2k} - \lambda_{1k} Q) e^{-\lambda_{1k} Q(T-t_{1y}-t_{2y})} + (1-Q)(\lambda_{1k} - \lambda_{2k} Q) e^{-\lambda_{2k} Q(T-t_{1y}-t_{2y})}] \quad (A24)$$

where

$$\begin{aligned} \mu_{1k} &= \lambda_{1k} Q = (\lambda_{1Vk} + \lambda_{1Nk}) Q \\ \mu_{2k} &= \lambda_{2k} Q = (\lambda_{2Vk} + \lambda_{2Nk}) Q \\ m_{1k} &= \lambda_{1k} (1-Q) = (\lambda_{2Vk} + \lambda_{2Nk}) (1-Q) \\ \mu_{nk} + m_{nk} &= \lambda_{nk} Q + \lambda_{nk} (1-Q) = \lambda_{nk} \quad (n = 1,2) \\ \Pi_1 &= \sum_{y=1}^{N_k} t_{1y}, \quad \Pi_2 = \sum_{y=1}^{N_k} t_{2y} \end{aligned}$$

Taking the natural logarithm of L_k , the log-likelihood function is,

$$\begin{aligned} LL_k &= (n_{ok} + n_{pk}) \ln \lambda_{1Vk} + (n_{ok} + n_{rk}) \ln \lambda_{1Nk} + (n_{ok} + n_{qk}) \ln \lambda_{2Vk} + (n_{pk} + n_{rk}) \ln \lambda_{2Nk} \\ &\quad - N_k \ln (\lambda_{2k} - \lambda_{1k} - \lambda_{2k} e^{-\lambda_{1k} Q T} - \lambda_{1k} e^{-\lambda_{2k} Q T}) - \lambda_{1k} \Pi_1 - \lambda_{2k} \Pi_2 \\ &\quad + \sum_{y=1}^{N_k} \ln (\lambda_{2k} - \lambda_{1k} - (1-Q)(\lambda_{2k} - \lambda_{1k} Q) e^{-\lambda_{1k} Q(T-t_{1y}-t_{2y})} + (1-Q)(\lambda_{1k} - \lambda_{2k} Q) e^{-\lambda_{2k} Q(T-t_{1y}-t_{2y})}) \end{aligned} \quad (A26)$$

With the expression for the log-likelihood in (A26), we are now in a position to estimate the desired arrest rates from observed data on time-back intervals for the subgroup of offenders in drug use class k . LL_k relies on observed data on each pair of time-back intervals t_{1y} and t_{2y} , and their sums Π_1 and Π_2 , as well as the number of tested pairs in each combination of offense types, n_{ok} , n_{pk} , n_{qk} , and n_{rk} (for pairs VV , VN , NV , and NN , respectively). The data can be used with maximum likelihood procedures available in GAUSS software to estimate the four arrest rate parameters for violent (V) and non-violent (N) offenses on the 1st and 2nd arrests in tested pairs, λ_{1Vk} , λ_{1Nk} , λ_{2Vk} , λ_{2Nk} for each drug class k . The probability that an arrest is tested for drug use, Q , can be estimated exogenously from data on the relative frequencies of tested and untested arrests in the arrest history data. Alternatively, the likelihood functions for the four drug use classes can be combined to jointly estimate all the desired rates simultaneously from the product of the individual likelihood functions,

$$L = L^{CC} L^{CU} L^{UC} L^{UU}$$

and the corresponding sum of the log-likelihoods,

$$LL = LL^{CC} + LL^{CU} + LL^{UC} + LL^{UU}$$

Adjustment to Exclude the Sampling Window

The final likelihood function in (A24) and (A26) includes explicit conditioning for the requirement that offenders must have two tested arrests during the observation period. The estimation sample is further constrained to include only offenders who have at least one arrest of any type during the original sampling window from July 1, 1985 to June 30, 1986. The fact that an arrest must occur during the specific period will potentially distort the length of intervals that either end or begin with an arrest in the sampling window. In a Poisson process, the potential distortions from the window arrest are easily handled by excluding the sampling window from the observation period. Arrests during that period are ignored entirely, and the one-year window period is excluded from any interarrest intervals.

Individual Covariates

Pairwise comparisons of arrest rates within the same sample of offenders control for the influence of enduring traits of offenders. These contrasts, however, do not control for time varying attributes that may differentially affect individual offending levels of sample members when they use and do not use drugs. The covariates also provide some control for differences in sample composition between offenders who are clean on both tested arrests (CC) and offenders who use drugs on at least one tested arrest (CU, UC, and UU). These cross-sample controls are important in the detrended estimates of drug use effects.

To accommodate the influences of covariates, the specification of the arrest rate parameters can be further generalized to become:

$$\lambda = \prod_j \gamma_j^{Z_j} * (\beta_1^{X_1} \beta_2^{X_2} \dots \beta_p^{X_p})$$

for p covariates and the Z_j representing 16 dummy variables for the combination of two crime types ($l = V$ or N), four drug use classes ($k = CC, CU, UC, UU$), and two tested arrests ($i = 1, 2$). The individual β_i represents the multiplier effect from each one-unit change in covariate X_i on the base arrest rate γ_j .

The new rate parameters, including the covariate effects, are estimated by substitution into the existing likelihood function. So, for example, the arrest rate parameter for violent rates on the 1st arrest by offenders who are clean on the 1st arrest and using on the 2nd arrest, $\lambda_{V,CU}$, becomes:

$$\begin{aligned} \lambda_{V,CU} &= \gamma_{V,CU}^1 \prod_j \gamma_j^0 * (\beta_1^{X_1} \beta_2^{X_2} \dots \beta_p^{X_p}) \\ &= \gamma_{V,CU} * (\beta_1^{X_1} \beta_2^{X_2} \dots \beta_p^{X_p}) \end{aligned}$$

in the likelihood function and,

$$\ln \lambda_{V,CU} = \ln \gamma_{V,CU} + \sum_i \ln \beta_i * X_i$$

in the log-likelihood. Similar substitutions to replace all of the arrest rate parameters in the combined likelihood function will produce simultaneous estimates of all sixteen arrest rates along with the effects of any included covariates X_1, X_2, \dots, X_p .

Controls for Selection Bias Arising from the Drug Testing Process

In addition to personal attributes of the offender, the covariates include an estimate of the probability that an offender is tested for drugs on an arrest. Our primary concern is selection bias that obscures actual differences between offending rates when offenders use and do not use illicit drugs. These biases arise from unmeasured traits of offenders that influence both the risk of being tested at arrest and offending levels.

The strategy of restricting the analysis to pairwise comparisons for the same sample of offenders controls for time stationary enduring traits of offenders that influence their offending rates. But suppose offenders continue to differ on unmeasured, time varying traits that increase their rates of offending when they are "clean." Suppose further that being tested for drugs is a selection process influenced by many of the same unmeasured traits, and that "cleans" must pose a greater offending risk before they are tested. Without satisfactory controls for the unmeasured traits, offending rates may appear equal when offenders use and do not use drugs. But in fact, testing clean may be associated with higher risks of offending on unmeasured dimensions so that the contrasted rates are no longer associated with "otherwise equal" offenders. In this scenario, real differentials in offending when clean and when using drugs might be seriously underestimated. Including an estimate of the testing probability provides one means for controlling for otherwise unmeasured differences in offending risk associated with drug use status within the tested sample.

The testing probability is estimated independently of arrest rates using a logit model applied to data from the full stratified sample of 1,365 adults arrested in Washington, DC sometime between July 1, 1985 and June 30, 1986. Table A2 presents the estimated coefficients, β , and their transformation to a multiplier effect, $\exp(\beta)$. Values of the transformed coefficients > 1.0 increase the probability of being tested, and values < 1.0 decrease the risk of a drug test. Charged offense and location where arrest is processed are the main determinants of whether a drug test is completed. Tests are much more likely when the individual is charged with crimes against persons and vice (including prostitution) and when the arrest is processed through the central booking and lockup.

Estimated Effects of Drug Use

We assess the influence of drug use on violence levels by examining the changes in arrest rates as offenders use and do not use drugs. Changes in arrest rates within a drug use class k ($k = CC, CU, UC, UU$) are calibrated by using the ratio,

$$R^k = \frac{\hat{\lambda}_i^k}{\hat{\lambda}_{i-1}^k} \frac{\lambda_{i-1}^k}{\lambda_i^k} = \frac{\hat{\lambda}_i^k}{\hat{\lambda}_{i-1}^k} (1) = \frac{\hat{\lambda}_i^k}{\hat{\lambda}_{i-1}^k}$$

of the estimated rate at arrest i to the estimated rate for the same offenders at arrest $i-1$. The null hypothesis of no difference in rates amounts to testing the ratio R^k for statistically significant departures from a true value of $\lambda_{i-1}^k / \lambda_i^k = 1.0$. These departures may be in the direction of aggravating effects, $R^k > 1$, or inhibiting effects, $R^k < 1$, of drug use on arrest rates. For exponential hazard rates, the ratio R^k is an F statistic ($2n+1, 2n+1$ df) (Lawless, 1982).

A detrended change ratio is obtained by dividing each ratio by the change observed on a pair of clean arrests,

$$\tilde{R}^k = \frac{\hat{R}^k}{\hat{R}^{CC}}$$

This is evaluated as an approximate F statistic by treating the denominator \hat{R}^{CC} as if it is a fixed parameter and not an estimate of a random variable. \tilde{R}^k is tested against a null value of 1.0. Values $\tilde{R}^k > 1.0$ indicate increases in the arrest rate that are larger than trend, and $\tilde{R}^k < 1.0$ are increases smaller than trend. Similar statistics are formed for each drug use class, k , and evaluated for five offense types (V = personal violence, predatory, property/theft, drugs, and public order/vice) and three drug types (heroin, cocaine, and PCP). Because the tested sample statistic is only approximately correct, and the test includes multiple comparisons on the same data, we use a stringent threshold of .000001 to evaluate significance in a two-tail test.

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Table A1. Multiplier Effects of Changes in Drug Use on Individual Arrest Rates: Sensitivity to Measurement Errors in Detecting Drug Use ^a

Offense and Drug Type	Episodic Use ^b		Withdrawal Effect ^c	
	$C \rightarrow U$		$U \rightarrow C$	
Personal Violence:				
Heroin	.5287 * ^d	(.4249 *) ^c	.5877 *	(.4810 *)
Cocaine	.4709 *	(.3086 *)	.9379	(.8993)
PCP	1.0484	(1.0833)	2.3122 *	(6.5861 *)
Predatory:				
Heroin	.6872	(.6059 *)	1.1448	(1.1975)
Cocaine	.6603	(.5009 *)	6.5712 *	(14.5353 *)
PCP	2.6874 *	(4.1826 *)	.9865	(.9793)
Property/Theft:				
Heroin	.7515	(.6827)	1.1687	(1.2309)
Cocaine	.4400 *	(.2811 *)	1.1254	(1.2058)
PCP	.6409 *	(.3955 *)	.7045	(.5992)
Drugs:				
Heroin	.9007	(.8692)	.7693	(.7021)
Cocaine	.5984 *	(.4330 *)	1.1325	(1.2175)
PCP	.6709	(.4453 *)	.5706 *	(.4488 *)
Public Order/Vice:				
Heroin	1.2370	(1.3364)	.6542	(.5607 *)
Cocaine	.6999	(.5473 *)	1.2439	(1.4031)
PCP	.4637 *	(.1056 *)	1.0208	(1.0326)

^a Changes in individual arrests rates are reflected in the ratio of rates, V_i^D / V_{i-1}^D for drug use status, D , on a pair of arrests. Ratio values > 1.0 reflect aggravating multiplier effects associated with increasing arrest rates between the $i-1$ and i arrests in a tested pair. Ratios < 1.0 reflect inhibiting multiplier effects associated with decreasing arrest rates. All reported ratios are detrended relative to the change in rates on a pair of clean arrests.

^b The episodic effect of drug use is reflected in the rate change as offenders go from not using (i.e., testing "clean") drugs on one arrest to using drugs on a later arrest, $D = CU$. The magnitude of this effect is estimated from the detrended ratio, $(V_i^{CU} / V_{i-1}^{CU}) / (V_i^{CC} / V_{i-1}^{CC})$.

^c The withdrawal effect of drug use is reflected in the rate change as offenders go from using drugs to not using drugs on a tested pair of arrests, $D = UC$. The magnitude of this effect is estimated from the detrended ratio, $(V_i^{UC} / V_{i-1}^{UC}) / (V_i^{CC} / V_{i-1}^{CC})$.

^d Multiplier effects followed by an asterisk are significantly different from 1.0 at the .00001 level in a 2-tailed F-test (403,403 df) applied to a ratio of exponential hazard rates (Lawless, 1982). The test invokes an unusually high significance level to accommodate multiple tests performed on the same underlying data and the approximation of treating the trend effect in the denominator as a fixed parameter rather than a random variable.

^e The first number in each table entry is the drug use multiplier effect obtained directly from the data (also in Exhibit 12). The number in parentheses illustrates the impact of errors in detecting drug use. In the example presented here, the multiplier effect is adjusted to accommodate a 7.5% error rate of true "cleans" who test as users and a 20% error rate of true "users" who test as clean.

**Exhibit A2. Determinants of the Probability of Being Tested for Drugs at Arrest:
Logit Model Estiamates (n=2,983 Arrests, 57.7% Tested)**

Variable	Coefficient β	Standard Error	p-value	Multiplier Effect, $\exp(\beta)$
Intercept	- .5263	.2700	.0512	
Race	- .0191	.0830	.8183	.9811
Sex	- .1421	.0872	.1031	.8675
Age Category	.0087	.0357	.8074	1.0088
Stable	- .0700	.0716	.3281	.9324
Prior Arrests	- .0147	.0074	.0464	.9854
Persons Offense	.5237	.1658	.0016	1.6885 *
Property Offense	- .1155	.1180	.3276	.8909
Assault Offense	.0256	.1618	.8744	1.0259
Vice Offense	.5281	.1189	.0001	1.6956 *
Other Offense	- .5453	.1082	.0001	.5797 *
Citation Arrest	-1.1520	.2611	.0001	.3160 *
Lock-Up	1.0061	.2358	.0001	2.7350 *
Weekend	- .0897	.1105	.4169	.9142
Summer	.1520	.0812	.0613	1.1642
Trend (Months)	.0031	.0026	.2378	1.0031

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