THE RESPONSE OF DRUG INJECTION AND NEEDLE SHARING TO LOCAL COCAINE AND HEROIN PRICES, AIDS PREVALENCE, AND NEEDLE EXCHANGE PROGRAMS

Jeff DeSimone*

Abstract

This study examines the effects of local cocaine and heroin prices, AIDS prevalence rates, and needle exchange programs on drug injection and needle sharing by adult male arrestees in 24 large U.S. cities during 1989–97. Logit regressions indicate that cocaine and heroin prices are related negatively to own-drug injection, with the opposite true for personal income, and that these impacts are concentrated on non-sharing injection. Increases in previous year AIDS prevalence reduce injection by both sharers and non-sharers, leaving the proportion of injectors who share unchanged. The opening of a needle exchange program decreases injection by 20–34 percent and sharing by 12–23 percent.

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I. Introduction

The injection of illegal drugs such as heroin and cocaine is associated with substantial costs in terms of crime, criminal justice, and social services. Beyond these, injection drug use is a major public health problem. Drug injectors not only face considerably higher risks of infection with blood-borne diseases such as HIV and hepatitis B and C through the sharing of infected needles, but furthermore can transmit these diseases to needle-sharing or sexual partners or their children. National surveillance data from the Centers for Disease Control and Prevention (CDC) indicate that 42 percent of cumulative non-pediatric AIDS cases with identified exposure categories, and 45 percent of such cases reported in 2000, were attributed to injection drug use. Almost three-quarters of these cases stem from sharing needles, with the remainder resulting from unprotected sex with a drug injector. Moreover, 82 percent of cumulative pediatric cases with known exposure category, and 86 percent of such cases in 2000, involved transmission from a mother infected through drug injection or having sex with a drug injector to her fetus or newborn child (CDC 2001). In clinical settings, HIV has been recovered from syringes stored at room temperature for periods of up to 30 days (CDC 1997).

The number of injection drug users in the U.S. is unclear, which is not surprising since drug injection is both illegal and disproportionately engaged in by the marginalized of society. Estimates vary from 900,000 by the CDC to 1.64 million by the National Association of State Alcohol and Drug Abuse Directors, with an average of 2.8 average injections daily (Lurie et al. 1998). The Treatment Episode Data Set (TEDS) indicates that about 192,000 individuals who entered publicly funded facilities for drug treatment in 1999 administered the primary drug for which they sought treatment by injecting (SAMHSA 2001), but this omits injectors treated at private facilities and who fail to seek treatment. The 1999 National Household Survey on Drug

Abuse (NHSDA) estimates that 3,065,000 individuals have injected drugs at some point in their lifetimes (SAMHSA 2002), but this is likely an underestimate because of underreporting and because hardcore drug users are less likely than others to be sampled (Rhodes et al. 2000).

A rich source of information on drug injectors is data collected on arrestees in 24 large U.S. cities by the Drug Use Forecasting (DUF) program (which in 1998 expanded to 35 cities and became the Arrestee Drug Abuse Monitoring program) of the National Institute of Justice (U.S. Department of Justice, 1998). During the 1989–1997 period that I study, 8.9 percent of arrestees report injecting drugs in the past six months and 17.8 percent report previous drug injection. Past six month injection prevalence declined steadily during the period, from 16.2 percent to 8.3 percent for females and 12.0 percent to 6.7 percent for males, although male prevalence remained level since 1994 and males make up the majority of reported AIDS cases attributable to injection drug use (69 percent in 2000 and 73 percent cumulatively).

Hunt and Rhodes (2001) argue that it is reasonable to draw inferences about the general arrestee population from the DUF sample, while the Federal Bureau of Investigation (FBI) estimates that there were 11,921,800 arrests of males and 3,362,500 arrests of females in 1997 (FBI 1998). These figures imply that about 1,080,000 arrestees nationwide injected drugs during the previous six months in 1997. If the Rhodes et al. (2000) estimate that about one-half of hardcore cocaine and heroin users are arrested in a given year applies to drug injectors, then approximately 2,160,000 individuals injected drugs in the previous six months during 1997. Alternatively, Rhodes et al. (2000) estimate from DUF that in 1997, there were about 3,106,000 hardcore cocaine users and 917,000 hardcore heroin users in the U.S. Since 25 and 76 percent of those who meet the criteria for hardcore use of cocaine and heroin, respectively, report past six month drug injection, the Rhodes et al. (2000) projections suggest that in 1997 there were roughly 777,000 cocaine injectors and 697,000 heroin injectors among hardcore users, along with about 957,000 injectors who were not hardcore cocaine or heroin users, for a total of 2,431,000 drug injectors.

The main drugs injected are cocaine and heroin, with available evidence indicating, as the figures above imply, that cocaine injection is slightly more prevalent than heroin injection. In DUF, 75 percent of arrestees who previously injected drugs have injected cocaine and 69 percent have injected heroin, while 34 percent have injected amphetamines and 20 percent have injected other drugs. Similarly, the 1999 NHSDA estimates that 1,930,000 individuals have previously injected cocaine while 1,441,000 have previously injected heroin (SAMHSA 2002) and 1,076,000 have previously injected stimulants. In contrast, the 1999 TEDS shows that the primary drug for which treatment is sought is heroin for 81 percent of injectors, amphetamines for 11 percent and cocaine for only 6 percent. However, this distribution is misleading for two reasons. First, 19 percent of injection admissions involved injection of both cocaine and opiates.[1] Second, the fraction of users who seek treatment is much higher for heroin than cocaine. The preceding Rhodes et al. (2000) imputations, DUF urine test results, and reported past year use in the NHSDA suggest that hardcore, current, and overall use of cocaine is more prevalent than comparable heroin use by factors of three, five, and ten, respectively. Yet, there were 235,668 admissions for heroin treatment and only 228,206 for cocaine treatment in the 1999 TEDS. In sum, while cocaine use exceeds heroin use, heroin is far more likely to be injected than is cocaine. The 1999 TEDS (SAMHSA 2001) reports that 66.4 percent of those seeking heroin use treatment, but only 6.4 percent of those seeking cocaine use treatment, were injectors (though the heroin figure represents a decline from 77.1 percent in 1992).

[1] Heroin is the most rapidly acting and abused of the opiates, which also include codeine, morphine, and methadone. For instance, 1999 TEDS data indicate that the number of individuals seeking heroin treatment was ten times the number seeking treatment for other opiates combined.

This paper examines drug injection behavior among male DUF arrestees. In particular, it investigates how recent drug injection, current injection of cocaine and heroin, and needle sharing responds to three potential sets of incentives: economic factors, specifically personal income and local prices of cocaine and heroin; the perceived risk of infection with HIV, as represented by local AIDS prevalence rates; and the presence of a local needle exchange program (NEP).

Injected drugs are goods that are bought and sold in a market, albeit illegal, and thus should adhere to basic microeconomic principles regarding the impact of prices and income. The same is true for the needles used to inject drugs, which suggests relationships between economic factors and needle sharing that are in the opposite directions from those for purchased goods. Syringes are inexpensive, costing perhaps 25 cents each, when legally obtained from a pharmacy. However, as of April 2001, five states have laws requiring a prescription to purchase or possess syringes, and all but three states have drug paraphernalia laws that penalize syringe possession and distribution (Ruiz-Sierra 2001, Sidwell et al. 2001).[2] Underground market prices that drug users must pay for syringes are presumably much higher than those for legally obtained syringes (Sidwell et al. 2001), potentially to the extent that syringes make up a non-trivial component of the budget set for drug injectors. Many studies have examined the impact of prices and income on the use of cocaine and heroin, but most have not explicitly focused on drug injection, and none have investigated needle sharing.

Similarly, many studies have examined the impact of AIDS prevalence on behaviors that potentially expose individuals to AIDS, obtaining varying results. These studies assume, as I do here, that local AIDS rates reflect individual perceptions of HIV infection risks – an increase in

[2] The five states with prescription laws are California, Delaware, Illinois, Massachusetts, and New Jersey. The three states without paraphernalia laws are Alaska, Iowa, and South Carolina, although the former two have local ordinances that restrict syringe possession and sale.

the AIDS rate raises the probability of becoming infected with HIV – and use AIDS data because they are much more extensive than are data on HIV. Although this hypothesis has not yet been investigated for drug injection, the CDC statistics cited above suggest that perceived AIDS infection risk is even more relevant for the drug injection than the sexual behavior decision on which previous studies have focused.

Economic research has also yet to study NEPs, which provide sterile syringes in exchange for used ones in an effort to reduce the transmission of blood-borne infections among drug injectors. NEPs also offer a range of additional public health services, including referrals to substance abuse treatment, provision of needle-cleaning supplies, distribution of condoms, testing and counseling for HIV and hepatitis B and C, medical care, vaccination for hepatitis B, and screening for tuberculosis and sexually transmitted diseases. As of July 2002 there were 211 NEPs in 37 states, Washington DC and Puerto Rico (Purchase 2002). However, federal NEP funding has been banned since 1989, and many NEPs are illegal because of the aforementioned prescription and paraphernalia laws. Thus NEPs are funded either by permissive states and localities or privately, with some NEPs operating secretly and others in tacit agreement with local law enforcement personnel, and data on needle exchange programs are difficult to obtain. For instance, an October 1999 survey of NEPs administered by Beth Israel Medical Center and the North American Syringe Exchange Network (NASEN) was not completed by 16 percent of programs, and various additional programs asked that their responses be kept confidential. This survey revealed that NEPs operate at various sites such as health van stops, shooting galleries, sidewalk tables, cars, storefronts and health clinics; operate for 20 hours per week on average; and have a median budget of $38,000. The 107 responding programs exchanged a total of 19.4 million syringes (Singh et al. 2001). Although such detailed data are unavailable at the individual program or city level at regular time intervals, it seems logical that an examination of drug injection behavior should control for their presence to the extent possible.

This study analyzes DUF data on men arrested during 1989–1997. These data are fitting for a variety of reasons. Most importantly, information is available not only on recent drug injection but also on current use of cocaine and opiates and on needle sharing. In addition, arrestees represent a sizable fraction of drug injectors in the population, the sample size is large, and the sampling scheme makes it possible to merge precisely measured data on aggregate variables. The results show that, controlling for arrestee characteristics as well as indicators for arrest city and survey year, all of the explanatory variables on which the analysis focuses – personal income, prices, AIDS rates, and needle exchange programs – have significant impacts on drug injection behavior.

The paper proceeds as follows. Section II reviews the relevant literature from economics and other disciplines. Section III outlines the econometric procedures, namely binomial and multinomial logit models, employed and provides other details regarding the estimation methodology. Section IV discusses the data, section V presents the results of the analysis, and section VI concludes.

II. Literature Review

The previous literature relevant to this study can be divided into three categories. Two of these, addressing the effects of drug prices (and income) on drug use and of risks of infection with AIDS and other contagious diseases on risky behavior, have recently developed within economics. The third, which examines the impact of needle exchange programs on injection behavior, comes entirely from outside of economics.

Economic Factors. The only study in the economics literature that has explicitly examined the impact of prices and income on drug injection is Bretteville-Jensen (1999), who analyzed data from interviews of 1,834 heroin injectors near the NEP in Oslo, Norway. She estimates consumption elasticities of –0.36 for heroin price and 0.59 for income among male dealers, and –1.51 for heroin price and 0.51 for income among male non-dealers.

Many other studies have estimated price elasticities of cocaine and heroin use without regard to method by which the drug is administered. In monthly data aggregated over 15 poor nonwhite Detroit communities between November 1970 and July 1973, Silverman and Spruill (1977) estimate a long-run heroin consumption price elasticity of –0.25. Van Ours (1995) uses 1923–38 data from the Dutch monopoly of the Indonesian opium market to estimate short-run price elasticities of –0.7 for consumption and –0.4 for the number of users. Using state cross sections aggregated from the 1977–87 Monitoring the Future (MTF) surveys, DiNardo (1993) finds that past month cocaine participation by high school seniors is not related to the price of cocaine. Caulkins (1996) indirectly imputes population-wide participation price elasticity ranges of between –1.48 and –2.08 for cocaine and between –0.53 and –1.80 for heroin from estimates of the price elasticity of the percent of DUF arrestees testing positive for each drug. Saffer and Chaloupka (1999) estimate participation price elasticities of –0.28 for cocaine and –0.94 for heroin among 1988–91 NHSDA respondents. Chaloupka et al. (1999) estimate a past year participation cocaine price elasticity among MTF high school seniors of –0.88 in the pooled 1982 and 1989 MTF surveys but only –0.24 in the 1989 sample. Grossman and Chaloupka (1998) use a rational addiction framework to estimate short run past year participation cocaine price elasticities among 18 to 27 year-olds in the 1976–85 MTF panels and associated follow-ups of –0.42 when individual-specific fixed effects are included and –0.72 when they are omitted.

Controlling for fixed state effects in pooled 1990–97 NHSDA data, DeSimone and Farrelly (2003) estimate past year participation cocaine price elasticities of –0.36 for respondents aged 18–39.

AIDS rates. Several econometric studies have examined the effect of perceived infection risk on propensities to engage in risky behavior or protect against disease risks by regressing behavioral indicators against disease prevalence rates in the state or metropolitan area of residence, presuming that individuals perceive higher infection risks as local prevalence rates increase. Three studies estimate the response of sexual behavior to local AIDS rates. Ahituv et al. (1996) estimate a positive effect of state AIDS rates on the propensity of 1984–90 National Longitudinal Survey of Youth respondents to use condoms during sexual intercourse. Levine (2001) finds no relationship between lifetime or current sexual activity or current birth control use among 1991–97 Youth Risk Behavior Survey respondents and state AIDS rates other than a positive effect of AIDS rates on the probability that females have ever had sex. DeSimone (2002) shows that during 1989–95, promiscuous sexual behavior by male and single adult female arrestees in DUF decreases when metropolitan area AIDS rates rise.

Two additional studies examine similar relationships for protection against diseases other than AIDS. Mullahy (1999) estimates a positive relationship between the propensity of National Health Interview Survey respondents to obtain a flu shot before winter 1990–91 and the number of weeks during the preceding winter that a state was categorized by the CDC as having widespread flu activity. Philipson (1996) finds that increases in state measles prevalence reduce the age in months at which the first measles vaccination occurs in 1984–90 National Health Interview Survey data. All of the studies in this subsection except Mullahy (1999) combine several years of cross sectional data and include year and geographic area indicators, so that effects of disease prevalence variables are identified by temporal changes within geographic areas.

Needle Exchange Programs. The economics literature has yet to study needle exchange programs. Though health researchers have accumulated abundant evidence regarding the impact of individual NEPs on rates of injection and needle sharing, no study has estimated an aggregate or average impact across programs. Lurie and Reingold (1993) summarize the early evidence from a plethora of studies. They report no evidence that NEPs increase drug use among clients or in communities, some evidence that NEPs reduce the frequency of injecting, and abundant evidence that NEPs reduce needle sharing and increase needle cleaning.

Subsequent studies have produced similar results. Seven major government-funded reports concluded that NEPs reduce rates of HIV infection but do not increase drug use (Lurie and DeCarlo 1998, Ruiz-Sierra 2001). NEPs in New York City decreased risky injection behavior by up to 73 percent (Des Jarlais et al. 1996). Drug injectors attending NEPs in Oakland were 2.5 times more likely to stop sharing needles after six months than those not attending NEPs (Blumenthal et al. 2000). In four cities NEPs decreased the number of injections per syringe by 44–85 percent and significantly increased one-time use of syringes (Heimer et al. 1998). In San Francisco, daily injection frequency fell from 1.9 to 0.7 and the fraction of new injectors fell from 3 percent to 1 percent from 1986–92 (Watters et al. 1994). District of Columbia NEP participants reported drops of 50 percent in crack cocaine use, 29 percent in the number of drug injections, and 18 percent in heroin use (Drug Strategies 1999), and two-thirds in needle sharing (Klein et al. 1997), compared with the month prior to entering the program. In New York City injectors using NEPs were two-thirds less likely to become infected with HIV than other injectors (Des Jarlais et al. 1996). A worldwide survey of 81 cities (mostly in the

U.S.) found that HIV infection rates among injectors decreased 5.8 percent annually in the 29 cities with NEPs and increased 5.9 percent annually in the 52 cities without NEPs (Hurley 1997). In Hawaii, where the NEP is state-funded, HIV infection rates fell from 5 percent in 1989 to 1.1 percent in 1996 (Vogt et al. 1998).

In sum, previous studies from economics suggest that injected drugs might be normal goods with respect to income and prices of cocaine and heroin, which implies the opposite relationship for needle sharing if illegally obtained syringes are sufficiently costly. Similarly, existing economics evidence suggests a negative effect of local AIDS prevalence on both drug injection and needle sharing. Meanwhile, previous studies from outside economics indicate that the introduction of a needle exchange in the city of residence might reduce needle sharing while also reducing, or at least not increasing, rates of drug injection.

III. Data

This study examines annual DUF data on arrestees in 24 large U.S. cities from the 1989–1997 period.[3] Hunt and Rhodes (2001) provide a detailed description of the DUF data. In brief, DUF samples individuals arrested in the sample cities over the past 48 hours during one two-week period each quarter beginning in late 1987.[4] Arrestees are asked at the time of booking to provide information about their drug use through interviews and donations of urine specimens. Over 90 percent agree to be interviewed, and about 80 percent of those interviewed provide urine samples that are screened for various drugs. Although urinalysis data is only used in some of the

[3] These cities are Atlanta, Birmingham, Chicago, Cleveland, Dallas, Denver, Detroit, Fort Lauderdale, Houston, Indianapolis, Kansas City, Los Angeles, Miami, New Orleans, New York, Omaha, Philadelphia, Phoenix, Portland (OR), Saint Louis, San Antonio, San Diego, San Jose, and Washington D.C. All are sampled annually during 1989–97 except Atlanta, Denver, and Omaha, which were not sampled in 1989, Miami, which was not sampled in 1990, and Kansas City, which was not sampled after 1992.

[4] I use data from 1989–97 because many cities were not sampled until 1989, and the sampling frame was altered in 1998.

specifications here, only respondents who provided urine samples are included in the data available to researchers. I further restrict the sample to males aged 17–88.[5]

The DUF data are uniquely suited to this study for many reasons, the most obvious being that they contain information on recent drug injection, current use of cocaine and opiates, and recent needle sharing. Beyond that, arrestees are a natural group to study because they have much higher rates of drug injection than the population at large (which explains the existence of the DUF program). For instance, lifetime drug injection prevalence among male respondents aged 18 and above is 1.7 percent in the NHSDA (SAMHSA 1999) but 14.1 percent in DUF. More specifically, as alluded to earlier, Rhodes et al. (2000) estimate annual arrest rates of 44 and 51 percent for those who test positive for cocaine and heroin, respectively, and speculate that the analogous rate for hardcore users is even higher. Another advantage of the data set is that sampling is restricted to 24 large cities, meaning that estimates of "local" prices and AIDS prevalence rates that are matched to individual responses are more precise than when matching is done at the state level, and making it plausible that a simple indicator of local NEP presence (as explained below) could influence injection behavior. Moreover, the sample sizes are large, as the main drug injection and needle sharing samples contain 160,110 and 124,177 respondents, respectively.

[5] Separate adult and juvenile questionnaires are administered. Although the distinction is based on the typical legal categorization of adults as aged 18 and above, the juvenile sample contains some respondents aged 18–21, while the adult sample contains 358 individuals aged 16 and 69 individuals aged 15 or younger. I only analyze the adult sample because for juveniles, rates of past 6 month injection are quite low (around 0.5 percent) and personal income is not observed. Furthermore, I drop respondents aged 16 and younger because only one of them reports past 6 month injection, but retain 17 year olds because they comprise almost 2 percent of the original sample and have a non-negligible injection rate (0.9 percent), and their exclusion does not affect the results. Results for the adult female sample, which is roughly one-third the size of the male sample, are not shown because estimated effects are often insignificant. In particular, personal income and AIDS rate results are similar to, albeit slightly weaker than, those for males, while NEP program impacts are negative, but smaller than for males and generally insignificant. The main difference between male and female results regards drug prices: for females cocaine prices are mostly insignificant but positively affect heroin injection, while heroin prices have significant positive effects on all types of injection.

I create four binary indicators of drug injection behavior that serve as the primary dependent variables for the analysis. The first is a self-reported indicator of having injected a drug in the previous six months. This allows for the examination of drug injection without regard to the specific drugs injected. The next two binary dependent variables are proxies for current injection of cocaine and heroin, respectively. DUF does not ask respondents what drugs they currently inject, but does administer urine tests for cocaine and opiate use that detect drug use within the previous 72 hours. Thus respondents who report past six month injection, test positive for cocaine (opiates), and report previous cocaine (heroin) injection are considered current cocaine (heroin) injectors.

Presumably, past six month injection is underreported to some extent. Respondents apparently underreport cocaine and heroin consumption despite agreeing to submit to urine tests for these drugs that presumably reveal that they are lying. For instance, during the sample period, 41.7 percent of male DUF respondents test positive for cocaine, and 7.8 percent test positive for opiates, but only 20.6 percent report using cocaine (including crack) and 5.6 percent report using heroin in the past 72 hours. Rhodes et al. (2000) estimate that, taking into account false positive urine tests, 61 percent of DUF respondents truthfully report cocaine use and 73 percent truthfully report heroin use.

Beyond issues regarding self-reporting and test errors, the current cocaine and heroin indicators overestimate current injection of the specific drug, as respondents might have administered the drug in a different way in the past 72 hours but injected previously during the six month window. However, they might underestimate past six month injection of the specific drug, as someone who injected cocaine, for instance, more than three days previously but within the last six months will not be considered a current cocaine injector because the urine test for cocaine is negative. In addition, a substantial proportion of respondents are classified as current injectors of both drugs: 49 percent of current cocaine injectors are also current heroin injectors, while 63 percent of current heroin injectors are also current cocaine injectors.

The final binary dependent variable is an indicator of current needle sharing. Respondents are coded as sharing needles if they report ever having shared needles and sharing some or most of the time (as opposed to not sharing anymore) in response to a question asking how often they share. Two separate binary sharing models are estimated, one using the full sample and a second restricting the sample to those who report injecting in the previous six months.[6] These regressions include data extending only through 1995, since needle sharing questions were eliminated when the survey instrument was altered during that year.

A weakness of several of these binary dependent variables is that the not injecting or sharing categories sometimes include distinct outcomes that might have different relationships with injection or sharing. For current cocaine or heroin injection, non-injection includes both respondents who use cocaine or heroin without injecting and those who do not use the drug at all. One might expect that price increases and income decreases would reduce drug use, but it is unclear whether price and income changes will affect the method by which a drug is administered. Although AIDS increases and NEP presence are expected to reduce injection relative to all other outcomes, policy implications depend on whether those who would otherwise inject stop using the drug altogether or simply change modes of administrations. In the case of needle sharing, the full sample equation includes both injectors and non-injectors. Price hikes and income declines might decrease injection relative to non-injection while simultaneously

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[6] When equations for current sharing among injectors are estimated by ordinary least squares (OLS) or probit, corresponding models that control for sample selection using Heckman's two-step procedure, identified by the past year AIDS rate since it does not affect sharing, provide nearly identical estimates. In addition, current sharing results for samples restricted to those who have ever injected any drug and those who have ever injected cocaine or heroin are quite similar to the results for sharing among past 6 month injectors.

increasing sharing among injectors, rendering effects on sharing ambiguous. Moreover, AIDS increases and NEP presence might reduce injection propensities similarly for sharers and non-sharers, making effects on the proportion of injectors who share not indicative of the true policy effects of these variables.

I therefore also estimate three multinomial logit models for variants of the above dependent variables that have more than two categories. For the measures of current cocaine and heroin injection, I add a third outcome for respondents who test positive for the given drug but have not injected in the past six months. For needle sharing, I construct a dependent variable with four outcomes that comprise all combinations of the binary injection and sharing variables: injection with and without sharing and no injection with and without sharing. The third combination, no injection with sharing, seems illogical, but 992 respondents report current sharing despite not injecting in the past six months. I interpret this outcome as representative of injecting with shared needles in a period preceding the previous six months. Classifying these respondents as non-injectors does not affect the results for the comparisons of the other three outcomes. This model subsumes the binary dependent variable model in which current sharing is compared for past six month injectors.

Turning to the explanatory variables, data on past month income is collected by DUF, which includes separate questions asking respondents to report income obtained from legal and illegal sources. I construct a past month income variable by adding the responses to these two questions, and include the percentage of income that is illegal as an additional variable.[7] The remaining explanatory variables upon which I focus are city or metropolitan level measures from other sources that are matched with individual DUF responses.

[7] The income and price variables are converted to 1997 values using the CPI for all urban consumers.

Data on cocaine and heroin prices are collected by undercover drug agents, mostly from the DEA, and recorded by the DEA in their System to Retrieve Information from Drug Evidence (STRIDE). These prices are expected to be relatively accurate since an unreasonable price offer by an agent would invoke suspicion by the seller and thus endanger the agent.[8] Transaction sizes must be standardized because, as Caulkins and Padman (1993) and Rhodes et al. (1994) show, sizable quantity discounts exist for both drugs.

For each drug, I impute annual metropolitan area level prices from STRIDE data using the OLS regression

$$\log p = \alpha_0 + \alpha_1(\log \hat{r} + \log w) + m\alpha_2 + y\alpha_3 + (m \times y)\alpha_4 + \eta, \quad (1)$$

where p is the price; r is the purity, i.e. the pure drug weight divided by the total weight; $\log \hat{r}$ is the predicted value from a regression of log r on log w, m, y, and $m \times y$; w is the total gram weight of the purchase; m and y are vectors of indicators for metropolitan area and year, respectively; $m \times y$ is a complete set of interactions between metropolitan area and year; and η is the error term. For a particular area and year the predicted price is $\exp(\alpha_0 + \alpha_2^m + \alpha_3^y + \alpha_4^{m\times y})$, which represents the median price of one gram of 100 percent pure drug in the area and year with corresponding indicators set equal to one in the area and year vectors. I follow Caulkins (1994) in excluding outliers before estimating equation (1).[9] Although DUF samples cities (and in a few cases single counties) rather than entire metropolitan areas, drug prices correspond to the

[8] Horowitz (2001) argues that STRIDE cocaine and heroin price data are not representative of actual market prices, but Caulkins (2001) and Rhodes and Kling (2001) strongly disagree with the implication that drug policy would be better informed without the use of STRIDE data than with it.

[9] The number of STRIDE observations from 1989–97 that are in locations included in the calculation of DUF metropolitan area prices (see the next footnote) is 21,196 for cocaine and 7,458 for heroin. I omit 500 cocaine prices and 492 heroin prices with purity less than or equal to 0.001 percent or greater than 100 percent, since these values represent either data errors or purchases containing trivial amounts of the actual drug. I then estimate an initial price standardized by total weight as the predicted price from a regression of log price on log weight and year indicators, and for each year throw out observations with standardized prices that are less than one-eighth of or greater than eight times the mean price for that year. Finally, using observed values of price, weight and purity, I exclude observations with a price per pure gram of greater than $3,000 for cocaine and $20,000 for heroin. Only 171 cocaine prices and 284 heroin prices in STRIDE are eliminated by these latter two steps.

metropolitan area to include additional STRIDE price observations and thus improve the precision of the metropolitan area price measures.[10]

Annual AIDS counts per 1,000 residents are reported by the Centers for Disease Control and Prevention (CDC) on the CDC Wonder web site (http://wonder.cdc.gov/). In 1993 the CDC changed their definition of AIDS to encompass many cases that would not have previously been diagnosed as AIDS, but all data used here are based on the 1987 definition.[11] Again, AIDS rates correspond to the metropolitan area because that is the most comparable level to the city at which they are publicly available. I specify the one-year lagged AIDS rate in an attempt to capture the state of knowledge before the injection decision is made.

Optimal NEP control measures would incorporate information on program size, in terms of budgets, clients served or needles exchanged. However, NEP data are difficult to obtain because many are illegal and are thus forced to conceal their existence from local authorities. Hence the NEP variables employed here are simply indicators of whether a program exists in the city of residence. Data on the month and year in which the first program opened in eight of the sample cities (Chicago, Indianapolis, Los Angeles, New York, Philadelphia, Portland, San Diego, and San Jose) come from Lurie and Reingold (1993), which provides origination dates for the 37 programs that existed by the end of August 1993. Information on whether NEPs opened

[10] For metropolitan areas that do not have sufficient STRIDE observations to construct an annual price series, I combine data from metropolitan areas of similar population size that are in the same census division or region but do not include other DUF metropolitan areas. These areas are Indianapolis, Omaha, and San Jose (which simply is defined to also include data from Oakland and San Francisco) for both drugs and Birmingham, Kansas City, New Orleans, and Phoenix for heroin. In addition, heroin price data are combined for four pairs of cities with the same price assigned to each: Fort Lauderdale and Miami, Houston and San Antonio, Cleveland and Indianapolis, and Kansas City and Saint Louis.

[11] Rates provided in their semi-annual surveillance reports, however, do not adjust for the change in definition and thus give the misleading impression that AIDS rates jumped substantially in 1993.

during the sample period in remaining sample cities and beginning dates for such programs were graciously provided by Dave Purchase, chair of NASEN (Purchase 2002).[12]

All regressions also control for various personal characteristics of respondents. These include age and age squared; years of completed education and its square; indicators of black, Hispanic, and other race (mutually exclusive, with white omitted); indicators of being married, divorced, widowed, and living with a boyfriend or girlfriend; and indicators of working full and part-time.

For each binary dependent variable, several regressions are estimated that vary according to the permutation of the NEP variable that is included. The first model omits NEP information. The second includes a single indicator for whether a local NEP was open during the interview month. The third specification includes two separate NEP indicators corresponding to a program existing in the city for 0–6 months and more than 6 months, respectively. This allows for the possibility of a lagged effect, which might occur if injection and sharing behaviors take time to adjust or simply because the injection variable spans the previous six months. The fourth specification adds an indicator for the period 0–6 months before a NEP opened in the city, which should be insignificant if estimated NEP coefficients are indeed attributable to program effects as opposed to other unobserved spurious factors.[13]

Two notable characteristics of the sample suggest that further sensitivity analysis is warranted. First, almost one-fifth of sample respondents are arrested for drug possession, sales,

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[12] The considerable amount of time that was necessary for Mr. Purchase to obtain this information supports his conjecture that obtaining the relevant panel data on characteristics more detailed than the simple indicators used here would be all but impossible.

[13] The coding of the NEP variables is non-obvious for San Jose and Denver because of the proximity of San Francisco (41 miles) and Boulder (28 miles), respectively, and because the latter cities had NEP programs before the former. Considering that many NEP programs consist of mobile units that travel to various locations within a city, it seems unlikely that drug injectors would travel such distances to exchange needles, and thus I ignore the presence of these nearby programs. Results with the NEP variables recoded to include programs in San Francisco and Boulder, though, are quite similar, showing no difference for drug injection and slightly stronger NEP influence on needle sharing.

or use of a controlled substance, or report drug dealing as their primary source of income in the past month. These arrestees have past six month injection rates of 11.3 percent, as opposed to 7.2 percent for other arrestees, and their responses to prices, income, AIDS risk, and NEP availability might differ from those of other arrestees. Consequently, specification 5 omits these respondents from the sample. Second, the age range of respondents is quite wide, and injection behavior among prime-aged adults might differ from that of the younger and older cohorts. In particular, past six month injection rates are 1.7 percent among respondents aged 17–20, 9.3 percent among those aged 21–65, and 1.4 percent for those aged 66 and above. Thus, specification 6 restricts the sample to ages 21–65. These two specifications, referred to as the restricted sample specifications in the tables, are estimated only for the binomial logit equations.

IV. Methodology

The dependent variables that I examine are primarily binary indicators of drug injection or needle sharing. However, I also examine two categorical dependent variables with three and four outcomes, respectively. Since I use multinomial logit models to estimate these multiple outcome equations, I use logit models to estimate the binary dependent variable regressions in order to maintain consistency across specifications.[14] Consequently, when the categorical dependent variable y takes on $m \leq 4$ outcomes $k = 0, ..., m\text{-}1$, regressions are of the form

$$\Pr(y = k) = F(X\beta_k) = \frac{\exp(X\beta_k)}{1 + \exp(X\beta_1) + \exp(X\beta_2) + \exp(X\beta_3)}, \quad (2)$$

[14] The binomial logit results are quite similar to those obtained from probit and OLS regressions. The only notable differences are the significance of the AIDS prevalence variable, which is slightly lower using probit and somewhat lower using OLS, and the size of the needle exchange program marginal effects, which are larger using OLS and smaller using probit.

where $F(.)$ is the cumulative logistic distribution, X is the vector of explanatory variables, β_j are parameters to be estimated for $j > 0$, $\beta_0 = 0$ (so that effects on each outcome $j > 0$ are estimated relative to outcome 0), and $\exp(X\beta_j)$ terms are omitted for $j \geq m$. This simplifies to

$$\Pr(y=1) = F(X\beta_1) = \frac{\exp(X\beta_1)}{1+\exp(X\beta_1)} \tag{3}$$

for binomial logit models. All regressions are estimated by maximum likelihood.

Since the interpretation of logit coefficients is not particularly intuitive, the tables also report the marginal effects $\partial \Pr(y=k)/\partial X$, which equal $F(X\beta_k)[\beta_k - \sum_j \beta_j F(X\beta_j)]$ for outcomes j, $k > 0$. This reduces to $\beta_1 F(X\beta_1)[1 - F(X\beta_1)]$ for binomial logit marginal effects on the probability of injection or needle sharing. For simplicity I evaluate marginal effects at the means of the predicted $F(X\beta_k)$ values. Since these equal $\bar{y}_k$, the observed proportion of observations for which $y = k$, $\bar{y}_k$ is substituted for $F(X\beta_k)$ in the above expressions.

Unlike in the case of binomial logits, multinomial logit marginal effects are not a monotonic transformation of the coefficient vector, and more generally do not necessarily even have the same sign as the coefficient.[15] As a result, standard errors are reported for multinomial logit marginal effects, including those for the baseline outcome 0 for which the coefficient vector is normalized to zero, which are simply the negative of the sum of the other m-1 marginal effects. These standard errors are estimated using a bootstrap procedure with 100 replications for each model.

To further aid interpretation of the parameter estimates, elasticities are provided for continuous explanatory variables, while percentage changes in outcomes induced by changes

[15] For instance, NEPs might prompt many cocaine injectors to stop using cocaine altogether without affecting the propensity to use cocaine without injecting. In that case, NEPs would significantly increase non-injection use of cocaine relative to cocaine injection while having zero marginal effect on non-injection cocaine use.

from zero to one are provided for binary explanatory variables. Both are evaluated at the observed mean $\overline{X}$ and outcome proportion $\overline{y}_k$. Percentage changes equal marginal effects divided by $\overline{y}_k$, while elasticities equal marginal effects multiplied by $\overline{X}/\overline{y}_k$. For each regression the pseudo-R^2 statistic, equal to $1 - L_e/L_c$ where L_c and L_e and the log likelihoods from a constant-only and the estimated regression model, respectively, is also listed.

Finally, since the key explanatory variables in the analysis are measured at the metropolitan area level, all regressions include vectors of city and year indicators. This is clearly necessary in order to eliminate any spurious effects of time-invariant unobserved factors that are specific to metropolitan areas and correlated with injection or needle sharing. For instance, drug injection rates are more than 2.5 times higher in Los Angeles, in which an NEP opened in 1992, than in Omaha, which does not have a NEP, but the positive correlation between drug injection and NEP presence does not imply that NEPs increase injection. The relevant question is instead whether injection declined in Los Angeles relative to other metropolitan areas upon opening of its NEP. It is precisely this question that the analysis addresses with the inclusion of city and year indicators. Similarly, coefficients on AIDS prevalence and cocaine and heroin prices are identified by temporal changes of these variables within metropolitan areas.

V. Results

Table 1 provides summary statistics for the three regression samples. The drug injection sample is used for the three binary and two categorical drug injection regressions, the drug injectors for the regression of needle sharing among drug injectors, and the needle sharing for the remaining binary and categorical needle sharing regressions. Again, the sample size is smaller for the needle sharing than drug injection sample because needle sharing information is only collected through the middle of 1995. Eight percent of respondents report past month injection, but only 56 percent of these also test positively for cocaine and have previously injected cocaine, while 44 percent also test positively for opiates and have previously injected heroin. Thirty percent of drug injectors at least sometimes share the needles with which they inject, which represents 3.33 percent of the entire needle sharing sample.

Past month income averages about $1,300 for all respondents but almost $2,000 for injectors. The fact that the percentage of income that is illegally obtained is almost three times as high for injectors suggests that this is partially because injectors are disproportionately likely to be involved in drug dealing and other illegal activities besides injecting drugs. Cocaine and heroin prices per pure gram average $125 and $940, respectively, in the injection sample, and are slightly higher in the needle sharing samples, though it is unclear how many doses a pure gram represents. On average there is one AIDS case reported annually per 4,000 metropolitan area residents. Just over 30 percent of respondents live in a city that has an NEP. Comparing the means of the main explanatory variables for the needle sharing and drug injector samples anticipates some of the results from the analysis: drug injectors not only have higher incomes but face lower drug prices and AIDS rates. They are also more likely to live in a city with an NEP, which might indicate that NEPs endogenously open in areas where they have more potential clients and thus can have a larger public health impact. The lower panel shows means for the individual characteristics included in all the regressions.

Tables 2–6 each report results for one of the binomial logit drug injection and needle sharing equations. These tables report logit coefficient and standard errors, marginal effects (in bold), and elasticities (in italics) for each of the main explanatory variables. Results for individual characteristics in the regressions from column 3 of tables 2–6 are displayed in columns 1–5, respectively, of Appendix 1.

Table 2 gives past six month injection results. In column 1 with no NEP indicators included, the results for three of the four variables are highly significant in the expected directions, with only the heroin price insignificant.[16] The income elasticity of about 0.01 is quite small, and the cocaine elasticity of –0.21 is smaller than that estimated by previous studies of cocaine demand. The fact that both coefficients are highly significant despite their magnitudes indicates the importance of the large sample size and perhaps the added precision involved in the price construction and matching process because of the DUF sampling scheme. Both effects are quite stable across specifications 1–4. The combination of a significant cocaine price effect and a much smaller and insignificant heroin price effect has several possible explanations. The most straightforward is that cocaine injectors are price responsive but heroin injectors are not. Another possibility is that both are price responsive but there are few heroin injectors relative to cocaine injectors. Or, it could be simply that the heroin price, because of the data constraints outlined previously, is more afflicted by measurement error than is the cocaine price. The results in tables 3–4 will help to clarify these alternatives. The size of the AIDS effect falls when NEP indicators are added, implying that NEPs arise in areas with higher than average AIDS rates,

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[16] An alternative interpretation is that the negative AIDS coefficients primarily reflect sample selection: when AIDS rates rise, drug injectors die or become too ill to further inject drugs. However, the estimated AIDS prevalence effects are sufficiently large that sample selection must only play a minor role at most. The FBI (1998) reports that in 1997, for instance, the average annual arrest rate in cities with populations of 250,000 or more was 7.5 percent. Since about 80 percent of arrestees (in DUF and nationwide) are males, this corresponds to a male arrest rate of about 12 percent, meaning that in 1997, 120 of 1,000 male residents of DUF metropolitan areas were arrestees. Consider the impact of a unit increase in the lagged AIDS rate (1 case per 1,000 residents), which implies an increase of about 1.8 cases per 1,000 male residents since about 90 percent of those with AIDS are men. Under the unrealistically conservative assumptions that a) all AIDS cases represent arrestees who inject drugs and b) all AIDS sufferers are rendered unable to inject drugs within a year, the resulting reduction in drug injection is 1.8/120 = .015. Thus at most about 28 percent of the estimated effect in column 3 can be attributed to selection, which falls to 12 percent if the earlier-cited CDC statistic that 42 percent of non-pediatric AIDS cases involve drug injection is incorporated into the calculation.

consistent with the expectation that city officials are more likely to resort to NEPs when the public health situation is more dire and the potential program impact is larger.

NEPs appear to significantly reduce drug injection by 20–25 percent. It might seem dubious that a simple indicator of NEP presence, without controls for program size, could have such a large impact on injection. However, Lurie and Reingold (1993) report that some needle exchange programs appear to reach large proportions of the local drug injecting population, and the earlier cited result from Hurley (1997) regarding the impact of program presence on HIV infection rates implies large reductions in injection risk behavior. The timing of the impacts is plausible, with programs having significant effects immediately but taking at least six months to reach full effectiveness. However, this pattern could simply result from the time period encompassed by the drug injection variable.

Column 5 indicates that income and cocaine price effects are smaller, but early NEP effects are larger, for dealers and those arrested for drug-related offenses than for others, though the only change in result significance is for the latter variable. In column 6, effects of most variables are slightly larger when the sample is restricted to prime age males, though the conclusions are unaltered.

Table 3 shows results for current cocaine injection. Estimated effects are similar to those in table 2, except that the cocaine price elasticity is almost twice as large, and the decrease in injection attributable to NEP presence rises to 24 percent in the first six months and 30 percent thereafter. Columns 5 and 6 are similar to 1–4, except that once again income effects are larger and first six month NEP effects are smaller for non-drug offenders.

Table 4 displays results for current heroin injection. Income effects are only about half as large as for overall and cocaine injection and are insignificant. Price effects are symmetrical in significance to those for cocaine injection: the own-price is significant but the cross-price is not. In both equations, though, cocaine price elasticities are larger than heroin price elasticities. The price effects for cocaine and heroin injection suggests that the price effects for any injection in table 2 are explained by stronger responses of injection to cocaine prices than heroin prices coupled with more respondents injecting cocaine than heroin, though measurement error may play a role as well. AIDS effects are significant, but smaller in magnitude than for cocaine injection, further supporting the conjecture that heroin injection is less responsive to incentives than is cocaine injection. NEP effects are also smaller than for cocaine injection for the first six months but are similar beyond six months. The restricted sample estimates have the same relation to the main sample estimates as in tables 2 and 3.

Table 5 provides estimates of the full sample current needle sharing equation. Price and income effects have switched signs, as predicted, but are still significant. Both prices are significant at the 90 percent level and together prices are significant at the 95 percent level. AIDS rates do not appear to affect needle sharing, which is odd, since it is sharing that exposes injectors to HIV infection risk. NEPs have no effect in the first six months but ultimately reduce the propensity to share by 15 percent. Sharing by non-drug offenders does not respond to income or cocaine price changes, while dropping younger and older respondents has no effect.

Table 6 gives results for current needle sharing among past six month injectors. Results are similar to those in table 5 but income and price effects are larger. This makes sense: income increases should reduce sharing relative to injecting without sharing, but not necessarily relative to not injecting altogether, which is the dominant category in table 5 but is excluded from table 6. The same logic holds for drug price decreases. Although cocaine price effects no longer disappear for non-drug offenders, NEP effects do.

Tables 7 and 8 report multinomial logit results for the specification that uses the main sample and includes two NEP indicators, corresponding to column 3 in the earlier tables. Table 7 estimates current cocaine and heroin injection equations. In each case estimated elasticities for the current injection outcome are quite similar to the corresponding outcomes in tables 3 and 4. The advantage of the multinomial logit specification is that it shows whether reductions in current injection (or increases in the case of income) translate to increases in non-injection drug use or not injecting drugs altogether. Increases in income actually prompt cocaine users to substitute injection for other modes of administration, leaving overall use unchanged. In contrast, price increases lead to less non-injection drug use and thus to less use overall. Higher AIDS rates similarly reduce drug use, but surprisingly a large component of this effect for cocaine is a reduction in non-injection use. NEP openings seem to immediately decrease positive tests for both drugs that are not associated with injecting, but this effect diminishes after six months, particularly for cocaine use. The net effect long run effect of NEPs, though, is to increase the propensity to not use drugs by 4 percent for cocaine and 2 percent for heroin.

Table 8 displays the multinomial logit results for the four-outcome needle sharing equation, again for the specification from column 3 of the earlier tables. Comparing the marginal effects of "injecting with sharing" in column 2 and "injecting without sharing" in column 3 further explains the results of table 6. Income and prices do not significantly affect propensities to inject and share. However, since they do strongly affect propensities to inject without sharing in the opposite direction, they each ultimately have significant effects on the propensity to inject and share. For instance, an increase in income substantially raises the propensity to inject without sharing, while only marginally decreasing the propensity to share while injecting. The result is thus a significant decrease in the proportion of injectors who share.

The opposite is true of increases in either price. Similarly, NEPs significantly decrease the propensity to inject and share, but their effect on the proportion of injectors who share is not as significant because they also mildly decrease the propensity to inject by those who do not share. This also explains the lack of an AIDS prevalence effect on sharing in the earlier tables. In fact AIDS rates are strongly negatively related to propensities to inject and share. But because the effect of AIDS on injecting without sharing is of similar magnitude, so that the net effect is to significantly increase the likelihood of not injecting, AIDS appears to have no effect on sharing, particularly when only injectors are considered.

VI. Conclusion

This study shows that, controlling for arrestee characteristics as well as indicators for arrest city and survey year, personal income, prices of cocaine and heroin, AIDS rates, and needle exchange programs each have significant impacts on drug injection behavior. Logit results indicate that cocaine and heroin prices are related negatively to drug injection while the opposite holds for personal income, with the caveats that heroin prices affect only own-drug injection and that these impacts are concentrated on non-sharing injection. Increases in previous year AIDS prevalence reduce injection by both sharers and non-sharers, leaving the proportion of injectors who share unchanged. The opening of a needle exchange program reduces injection by 20–34 percent and sharing by 12–23 percent. Multinomial logit injection equations reveal that increases in all these factors except income reduce overall drug use propensities, for the most part by also reducing non-injection as well as injection drug use.

These results, and their associated policy implications, echo those of most previous related studies. Injected drugs appear to be normal goods with downward-sloping demands. If drug policy can raise drug prices, which is unclear, it could reduce both injection and non-injection drug use without increasing needle sharing. The AIDS prevalence results provide further support to the hypothesis that AIDS outbreaks are self-limiting to the extent that they will reduce behavior leading to AIDS and vice-versa (Ahituv et al. 1996). Thus drug policy makers must be aware that increases in drug injection and needle sharing are potential unintended consequences of programs that reduce AIDS incidence.

NEPs already have overwhelming support in the scientific community (Ruiz-Sierra 2001) and have been publicly endorsed by both former Secretary of Health and Human Services Donna E. Shalala and former Surgeon General David Satcher. Although I neither study NEP costs nor fully account for NEP benefits, other evidence on the costs of NEPs (Lurie and Reingold 1993, Singh et al. 2001) coupled with the large effects of NEPs on drug injection and needle sharing found here imply that NEPs might be a cost-effective way to reduce the transmission of HIV. This concurs with conclusions from Holtgrave et al. (1998), Lurie and DeCarlo (1998), and Lurie and Reingold (1993) that NEP costs per HIV infection prevented are far below the expected lifetime cost of treating an HIV-infected individual. The current federal government ban on NEP funding and state drug paraphernalia laws limiting syringe possession and distribution might therefore be ill-conceived. For instance, in October 1998, Congress barred the District of Columbia from funding NEPs and prohibited all federal NEP funding in the District. As a result, the original $220,000 District NEP closed (though it was soon replaced by a privately-funded program) despite a fiscal impact study estimating that failing to provide needle exchange would cost the District $8.3 million annually. In contrast, alternatives such as pharmacy syringe sales, physician prescription of syringes for drug injection, and automated needle exchange might reduce drug injection and needle sharing and thus rates of HIV infection.

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TABLE 1.—SUMMARY STATISTICS

Sample	Drug injection (*n* = 160,110)		Needle Sharing (*n* = 124,177)		Drug injectors (*n* = 10,475)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Dependent variables						
Injected drugs past 6 months	.0805	.2720	–	–	–	–
Positive cocaine test and injector	.0452	.2078	–	–	–	–
Positive opiates test and injector	.0353	.1845	–	–	–	–
Currently shares needles	–	–	.0333	.1795	.3000	.4583
Main explanatory variables						
Past month income (in $10,000s)	.1295	.3050	.1297	.3135	.1976	.4825
Cocaine price (in $100s)	1.257	.3640	1.298	.3828	1.238	.3683
Heroin price (in $1,000s)	.9397	.5442	1.050	.5549	.9399	.5013
Past year AIDS rate (per 1,000)	.2356	.1927	.2539	.2013	.2312	.1639
NEP in city	.3138	.4640	.2321	.4222	.3021	.4592
NEP in city 0–6 months	.0335	.1799	.0361	.1864	.0484	.2146
NEP in city more than 6 months	.2803	.4491	.1961	.3970	.2537	.4351
0–6 months before NEP opened	.0344	.1822	.0386	.1927	.0558	.2296
Other explanatory variables						
Age	29.73	9.360	29.51	9.229	34.03	8.002
Age squared	971.4	657.1	956.0	647.8	1222	579.6
Education	11.30	2.254	11.27	2.268	11.28	2.229
Education squared	132.8	46.35	132.1	46.49	132.1	45.89
Black	.5541	.4971	.5611	.4963	.3318	.4709
Hispanic	.1867	.3897	.1825	.3863	.2452	.4302
Other	.0161	.1259	.0144	.1194	.0071	.0838
Married	.1537	.3606	.1512	.3583	.1420	.3490
Cohabiting	.1508	.3578	.1353	.3420	.1328	.3394
Divorced	.1429	.3500	.1463	.3534	.2690	.4435
Widow	.0060	.0774	.0060	.0770	.0114	.1060
Works full time	.4069	.4912	.3931	.4884	.2394	.4268
Works part time	.1564	.3632	.1433	.3503	.1238	.3294
Percent income illegally obtained	.1031	.2750	.1036	.2754	.2749	.4008

The drug injection, needle sharing, and drug injector samples are used for the regressions in tables 2–4 and 7, tables 5 and 8, and table 6, respectively. The drug injection sample is larger than the needle sharing sample because the needle sharing question was eliminated from the survey upon redesign in mid-1995, while the drug injection sample extends through 1997. Drug injectors are needle sharing sample respondents who reported injecting drugs in the previous 6 months. The "positive cocaine test and injector" variable is an indicator that the respondent jointly tested positive for cocaine, injected drugs in the past 6 months, and injected cocaine previously. The "positive opiates test and injector" variable is the analogous indicator for testing positive for opiates and injecting heroin. 996 respondents reporting current needle sharing but no drug injection in the past 6 months are included in the needle sharing sample with the "currently shares needles" indicator coded 1 even though these respondents are excluded from the drug injector sample. Past month income and prices of cocaine and heroin are in 1997 dollars. The past year AIDS rate is the number of cases in the metropolitan area per 1,000 residents.

TABLE 2.—LOGIT EQUATIONS FOR PAST 6 MONTH DRUG INJECTION

Sample	Main (n = 160,110)				Restricted	
Specification	No NEP	1 NEP	2 NEP	Pre-NEP	Drug	Age
	(1)	(2)	(3)	(4)	(5)	(6)
Past month income (in $10,000s)	.0779[a]	.0764[a]	.0758[a]	.0757[a]	.1572[a]	.0763[a]
	(.0248)	(.0248)	(.0248)	(.0248)	(.0327)	(.0256)
	.0058	**.0057**	**.0056**	**.0056**	**.0116**	**.0056**
	.0093	*.0091*	*.0090*	*.0090*	*.0187*	*.0091*
Cocaine price (in $100s)	-.1824[a]	-.1855[a]	-.1777[a]	-.1817[a]	-.2135[a]	-.2002[a]
	(.0633)	(.0633)	(.0634)	(.0635)	(.0724)	(.0650)
	-.0135	**-.0137**	**-.0131**	**-.0134**	**-.0158**	**-.0148**
	-.2108	*-.2143*	*-.2053*	*-.2099*	*-.2467*	*-.2313*
Heroin price (in $1,000s)	-.0399	-.0441	-.0412	-.0450	-.0236	-.0449
	(.0396)	(.0396)	(.0396)	(.0397)	(.0446)	(.0404)
	-.0030	**-.0033**	**-.0030**	**.0033**	**-.0017**	**-.0033**
	-.0345	*-.0381*	*-.0356*	*-.0389*	*-.0204*	*-.0388*
Past year AIDS rate (cases per 1,000 residents)	-.9435[a]	-.7856[a]	-.7222[a]	-.7196[a]	-.6689[a]	-.7905[a]
	(.2094)	(.2115)	(.2137)	(.2137)	(.2456)	(.2166)
	-.0698	**-.0581**	**-.0534**	**-.0532**	**-.0495**	**-.0585**
	-.2044	*-.1702*	*-.1565*	*-.1559*	*-.1449*	*-.1713*
NEP in city		-.2179[a]				
		(.0381)				
		-.0161				
		-.2001				
NEP in city for 0–6 months			-.1218[b]	-.1447[b]	-.0623	-.1465[b]
			(.0587)	(.0609)	(.0669)	(.0603)
			-.0090	**-.0107**	**-.0046**	**-.0108**
			-.1119	*-.1330*	*-.0572*	*-.1342*
NEP in city for more than 6 months			-.2474[a]	-.2720[a]	-.2569[a]	-.2434[a]
			(.0406)	(.0442)	(.0469)	(.0415)
			-.0183	**-.0201**	**-.0190**	**-.0180**
			-.2274	*-.2498*	*-.2361*	*-.2237*
0–6 months before NEP opened in city				-.0799		
				(.0568)		
				-.0059		
				-.0733		
Pseudo R^2	.2118	.2122	.2122	.2064	.1904	.2123

The dependent variable is a binary indicator that the respondent injected drugs in the past 6 months. Columns 1–4 vary according to NEP variable specification as indicated. Restricted samples include only respondents not charged with drug-related offenses or reporting dealing as their primary income source in column 5 (n = 130,299) and those between 21 and 65 years old in column 6 (n = 131,665). Coefficients are from logit regressions. Parentheses contain standard errors. Marginal effects on the probability of drug injection are in bold. Italics represent elasticities for continuous variables and the percentage change in injection probability from a 0 to 1 change in NEP indicators. Marginal effects, elasticities, and percentage changes are evaluated at the observed means. Superscripts [c], [b], and [a] denote significance at the 90, 95, and 99 percent levels, respectively. The pseudo R^2 is $1 - L_e/L_c$, where L_e and L_c are the log likelihoods from estimated and constant-only models, respectively. All regressions include the variables listed in Appendix 1 as well as city indicators.

TABLE 3.—LOGIT EQUATIONS FOR JOINT INCIDENCE OF POSITIVE COCAINE TEST AND RECENT COCAINE INJECTION

Sample	Main (*n* = 160,110)				Restricted	
Specification	No NEP	1 NEP	2 NEP	Pre-NEP	Drug	Age
	(1)	(2)	(3)	(4)	(5)	(6)
Past month income	.0812[a]	.0793[a]	.0790[a]	.0789[a]	.1563[a]	.0777[a]
(in $10,000s)	(.0279)	(.0280)	(.0280)	(.0280)	(.0368)	(.0286)
	.0035	**.0034**	**.0034**	**.0034**	**.0068**	**.0034**
	.0100	*.0098*	*.0098*	*.0098*	*.0193*	*.0096*
Cocaine price	-.3102[a]	-.3204[a]	-.3157[a]	-.3215[a]	-.3053[a]	-.3076[a]
(in $100s)	(.0802)	(.0801)	(.0803)	(.0804)	(.0928)	(.0817)
	-.0134	**-.0138**	**-.0136**	**-.0139**	**-.0132**	**-.0132**
	-.3722	*-.3844*	*-.3788*	*-.3857*	*-.3663*	*-.3690*
Heroin price	-.0366	-.0473	-.0464	-.0520	-.0383	-.0611
(in $1,000s)	(.0476)	(.0476)	(.0476)	(.0478)	(.0542)	(.0484)
	-.0016	**-.0020**	**-.0020**	**-.0022**	**-.0017**	**-.0026**
	-.0328	*-.0424*	*-.0416*	*-.0467*	*-.0344*	*-.0548*
Past year AIDS rate	-1.182[a]	-.9936[a]	-.9574[a]	-.9622[a]	-.8951[a]	-.9498[a]
(cases per 1,000 residents)	(.2543)	(.2565)	(.2601)	(.2601)	(.3028)	(.2625)
	-.0511	**-.0429**	**-.0414**	**-.0416**	**-.0387**	**-.0410**
	-.2659	*-.2235*	*-.2154*	*-.2165*	*-.2014*	*-.2137*
NEP in city		-.3026[a]				
		(.0476)				
		-.0131				
		-.2896				
NEP in city for 0–6 months			-.2558[a]	-.2859[a]	-.1504[c]	-.2468[a]
			(.0727)	(.0757)	(.0830)	(.0738)
			-.0110	**-.0123**	**-.0065**	**-.0107**
			-.2432	*-.2719*	*-.1437*	*-.2365*
NEP in city for more than			-.3183[a]	-.3508[a]	-.3002[a]	-.3050[a]
6 months			(.0511)	(.0559)	(.0598)	(.0519)
			-.0137	**-.0152**	**-.0130**	**-.0132**
			-.3028	*-.3360*	*-.2874*	*-.2918*
0–6 months before NEP				-.0972		
opened in city				(.0677)		
				-.0041		
				-.0906		
Pseudo R^2	.1901	.1908	.1908	.1909	.1972	.1688

The dependent variable is a binary indicator that the respondent jointly tested positive for cocaine, injected drugs in the past 6 months, and injected cocaine previously. Columns 1–4 vary according to NEP variable specification as indicated. Restricted samples include only respondents not charged with drug-related offenses or reporting dealing as their primary income source in column 5 (*n* = 130,299) and those between 21 and 65 years old in column 6 (*n* = 131,665). Coefficients are from logit regressions. Parentheses contain standard errors. Marginal effects are in bold. Italics represent elasticities for continuous variables and percentage changes resulting from a 0 to 1 change in NEP indicators. Marginal effects, elasticities, and percentage changes are evaluated at the observed means. Superscripts [c], [b], and [a] denote significance at the 90, 95, and 99 percent levels, respectively. The pseudo R^2 is $1 - L_e/L_c$, where L_e and L_c are the log likelihoods from estimated and constant-only models, respectively. All regressions include the variables listed in Appendix 1 as well as city indicators.

TABLE 4.—LOGIT EQUATIONS FOR JOINT INCIDENCE OF POSITIVE OPIATE TEST AND RECENT HEROIN INJECTION

Sample	Main (n = 160,110)				Restricted	
Specification	No NEP	1 NEP	2 NEP	Pre-NEP	Drug	Age
	(1)	(2)	(3)	(4)	(5)	(6)
Past month income	.0465	.0443	.0432	.0433	.1094[b]	.0465
(in $10,000s)	(.0343)	(.0345)	(.0345)	(.0345)	(.0446)	(.0347)
	.0016	**.0015**	**.0015**	**.0015**	**.0037**	**.0016**
	.0059	*.0055*	*.0055*	*.0055*	*.0136*	*.0059*
Cocaine price	-.1242	-.1281	-.1142	-.1173	-.1658	-.0972
(in $100s)	(.0989)	(.0988)	(.0991)	(.0993)	(.1181)	(.1000)
	-.0042	**-.0044**	**-.0039**	**-.0040**	**-.0056**	**-.0033**
	-.1496	*-.1567*	*-.1389*	*-.1424*	*-.1995*	*-.1176*
Heroin price	-.2406[a]	-.2474[a]	-.2423[a]	-.2445[a]	-.1996[b]	-.2411[a]
(in $1,000s)	(.0728)	(.0728)	(.0728)	(.0730)	(.0847)	(.0734)
	-.0082	**-.0084**	**-.0082**	**-.0083**	**-.0068**	**-.0082**
	-.2184	*-.2246*	*-.2199*	*-.2219*	*-.1812*	*-.2189*
Past year AIDS rate	-.8541[a]	-.6351[b]	-.5491[c]	-.5456[c]	-.6053[c]	-.6026[b]
(cases per 1,000 residents)	(.2904)	(.2951)	(.2984)	(.2986)	(.3567)	(.3004)
	-.0291	**-.0216**	**-.0187**	**-.0186**	**-.0206**	**-.0205**
	-.1944	*-.1443*	*-.1249*	*-.1242*	*-.1376*	*-.1369*
NEP in city		-.2539[a]				
		(.0538)				
		-.0086				
		-.2438				
NEP in city for 0–6 months			-.1360[c]	-.1459[c]	-.0214	-.1403[c]
			(.0795)	(.0828)	(.0935)	(.0803)
			-.0046	**-.0050**	**-.0007**	**-.0048**
			-.1304	*-.1417*	*-.0198*	*-.1361*
NEP in city for more than			-.2949[a]	-.3059[a]	-.2591[a]	-.2945[a]
6 months			(.0577)	(.0632)	(.0687)	(.0583)
			-.0100	**-.0104**	**-.0088**	**-.0100**
			-.2835	*-.2948*	*-.2495*	*-.2835*
0–6 months before NEP				-.0322		
opened in city				(.0755)		
				-.0011		
				-.0312		
Pseudo R^2	.2354	.2359	.2360	.2360	.2460	.2132

The dependent variable is a binary indicator that the respondent jointly tested positive for opiates, injected drugs in the past 6 months, and injected heroin previously. Columns 1–4 vary according to NEP variable specification as indicated. Restricted samples include only respondents not charged with drug-related offenses or reporting dealing as their primary income source in column 5 (n = 130,299) and those between 21 and 65 years old in column 6 (n = 131,665). Coefficients are from logit regressions. Parentheses contain standard errors. Marginal effects are in bold. Italics represent elasticities for continuous variables and percentage changes resulting from a 0 to 1 change in NEP indicators. Marginal effects, elasticities, and percentage changes are evaluated at the observed means. Superscripts c, b, and a denote significance at the 90, 95, and 99 percent levels, respectively. The pseudo R^2 is $1 - L_e/L_c$, where L_e and L_c are the log likelihoods from estimated and constant-only models, respectively. All regressions include the variables listed in Appendix 1 as well as city indicators.

TABLE 5.—LOGIT EQUATIONS FOR CURRENT NEEDLE SHARING

Sample	Main (n = 124,177)				Restricted	
Specification	No NEP (1)	1 NEP (2)	2 NEP (3)	Pre-NEP (4)	Drug (5)	Age (6)
Past month income (in $10,000s)	-.0748[c] (.0438) **-.0024** *-.0094*	-.0761[c] (.0439) **-.0025** *-.0095*	-.0767[c] (.0439) **-.0025** *-.0096*	-.0768[c] (.0439) **-.0025** *-.0096*	-.0057 (.0516) **-.0002** *-.0007*	-.0733[c] (.0442) **-.0024** *-.0092*
Cocaine price (in $100s)	.2000[c] (.1078) **.0064** *.2510*	.1997[c] (.1075) **.0064** *.2507*	.2081[c] (.1077) **.0067** *.2612*	.2057[c] (.1082) **.0066** *.2582*	.1001 (.1204) **.0032** *.1256*	.1882[c] (.1093) **.0061** *.2362*
Heroin price (in $1,000s)	.1069[c] (.0571) **.0034** *.1085*	.1039[c] (.0570) **.0033** *.1055*	.1069[c] (.0570) **.0034** *.1085*	.1055[c] (.0573) **.0034** *.1071*	.1267[b] (.0629) **.0041** *.1286*	.1086[c] (.0576) **.0035** *.1102*
Past year AIDS rate (cases per 1,000 residents)	-.0622 (.3494) **-.0020** *-.0153*	-.0001 (.3517) **-.0000** *-.0000*	.0655 (.3567) **.0021** *.0161*	.0624 (.3570) **.0020** *.0153*	-.1294 (.4034) **-.0041** *-.0318*	.0292 (.3602) **.0009** *.0072*
NEP in city		-.1254[b] (.0623) **-.0040** *-.1212*				
NEP in city for 0–6 months			-.0538 (.0901) **-.0017** *-.0520*	-.0613 (.0953) **-.0020** *-.0593*	-.0702 (.1032) **-.0023** *-.0679*	-.0768 (.0919) **-.0025** *-.0742*
NEP in city for more than 6 months			-.1553[b] (.0682) **-.0050** *-.1501*	-.1628[b] (.0747) **-.0052** *-.1574*	-.1535[b] (.0774) **-.0049** *-.1484*	-.1472[b] (.0691) **-.0047** *-.1423*
0–6 months before NEP opened in city				-.0211 (.0867) **-.0007** *-.0204*		
Chi-square statistic for Wald test of joint price significance	7.33 [.0255]	7.15 [.0280]	7.63 [.0220]	7.31 [.0259]	4.96 [.0836]	6.86 [.0324]
Pseudo R^2	.1934	.1935	.1936	.1936	.1937	.1722

The dependent variable is a binary indicator that the respondent currently shares needles. Columns 1–4 vary according to NEP variable specification as indicated. Restricted samples include only respondents not charged with drug-related offenses or reporting dealing as their primary income source in column 5 (n = 101,347) and those between 21 and 65 years old in column 6 (n = 102,127). Coefficients are from logit regressions. Parentheses contain standard errors. Marginal effects are in bold. Italics represent elasticities for continuous variables and percentage changes resulting from a 0 to 1 change in NEP indicators. Marginal effects, elasticities, and percentage changes are evaluated at the observed means. P-values are in brackets. Superscripts [c], [b], and [a] denote significance at the 90, 95, and 99 percent levels, respectively. The pseudo R^2 is $1 - L_e/L_c$, where L_e and L_c are the log likelihoods from estimated and constant-only models, respectively. All regressions include the variables listed in Appendix 1 as well as city indicators.

TABLE 6.—LOGIT EQUATIONS FOR CURRENT NEEDLE SHARING AMONG DRUG INJECTORS

Sample	Main (n = 124,177)				Restricted	
Specification	No NEP (1)	1 NEP (2)	2 NEP (3)	Pre-NEP (4)	Drug (5)	Age (6)
Past month income (in $10,000s)	-.1489[a]	-.1517[a]	-.1522[a]	-.1522[a]	-.1013	-.1418[a]
	(.0538)	(.0541)	(.0541)	(.0541)	(.0631)	(.0544)
	-.0313	**-.0319**	**-.0320**	**-.0320**	**-.0213**	**-.0298**
	-.0206	*-.0210*	*-.0211*	*-.0211*	*-.0140*	*-.0196*
Cocaine price (in $100s)	.4123[a]	.4127[a]	.4175[a]	.4181[a]	.3323[b]	.4304[a]
	(.1487)	(.1486)	(.1488)	(.1491)	(.1692)	(.1510)
	.0866	**.0867**	**.0877**	**.0878**	**.0698**	**.0904**
	.3574	*.3578*	*.3619*	*.3624*	*.2881*	*.3731*
Heroin price (in $1,000s)	.1844[b]	.1741[b]	.1762[b]	.1769[b]	.1890[b]	.1870[b]
	(.0850)	(.0851)	(.0852)	(.0858)	(.0961)	(.0863)
	.0387	**.0366**	**.0370**	**.0372**	**.0397**	**.0393**
	.1213	*.1145*	*.1159*	*.1164*	*.1243*	*.1230*
Past year AIDS rate (cases per 1,000 residents)	-.7872	-.6055	-.5343	-.5336	-.9250	-.5238
	(.5542)	(.5623)	(.5753)	(.5754)	(.6683)	(.5819)
	-.1653	**-.1272**	**-.1122**	**-.1121**	**-.1943**	**-.1100**
	-.1274	*-.0980*	*-.0865*	*-.0864*	*-.1497*	*-.0848*
NEP in city		-.1646[b]				
		(.0831)				
		-.0346				
		-.1152				
NEP in city for 0–6 months			-.1177	-.1150	-.1996	-.1234
			(.1159)	(.1232)	(.1332)	(.1185)
			-.0247	**-.0242**	**-.0419**	**-.0259**
			-.0824	*-.0805*	*-.1397*	*-.0864*
NEP in city for more than 6 months			-.1874[b]	-.1847[c]	-.1654	-.1689[c]
			(.0920)	(.1014)	(.1063)	(.0935)
			-.0394	**-.0388**	**-.0347**	**-.0355**
			-.1312	*-.1293*	*-.1158*	*-.1182*
0–6 months before NEP opened in city				.0069		
				(.1100)		
				.0014		
				.0099		
Chi-square statistic for Wald test of joint price significance	13.35 [.0013]	12.81 [.0017]	13.05 [.0015]	12.88 [.0016]	8.40 [.0150]	13.74 [.0010]
Pseudo R^2	.0715	.0718	.0718	.0718	.0699	.0712

The dependent variable is a binary indicator that the respondent currently shares needles, with the sample restricted to those reporting drug injection in the past 6 months. Columns 1–4 vary according to NEP variable specification as indicated. Restricted samples include only respondents not charged with drug-related offenses or reporting dealing as their primary income source in column 5 (n = 101,347) and those between 21 and 65 years old in column 6 (n = 102,127). Coefficients are from logit regressions. Parentheses contain standard errors. Marginal effects are in bold. Italics represent elasticities for continuous variables and percentage changes resulting from a 0 to 1 change in NEP indicators. Marginal effects, elasticities, and percentage changes are evaluated at the observed means. P-values are in brackets. Superscripts [c], [b], and [a] denote significance at the 90, 95, and 99 percent levels, respectively. The pseudo R^2 is $1 - L_e/L_c$, where L_e and L_c are the log likelihoods from estimated and constant-only models, respectively. All regressions include the variables listed in Appendix 1 as well as city indicators.

TABLE 7.—MULTINOMIAL LOGIT EQUATIONS COMPARING JOINT INCIDENCE OF POSITIVE DRUG TEST AND RECENT DRUG INJECTION WITH NEGATIVE TEST AND POSITIVE TEST WITHOUT INJECTION

Drug	Cocaine			Heroin		
Test positive?	No	Yes	Yes	No	Yes	Yes
Inject this drug?	–	Yes	No	–	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Past month income	-.0503[c]	–	-.0919[a]	-.0459	–	-.0011
(in $10,000s)	(.0302)	–	(.0298)	(.0347)	–	(.0492)
	.0077	**.0029[b]**	**-.0106[b]**	**-.0033**	**.0015**	**.0018**
	(.0055)	**(.0013)**	**(.0053)**	**(.0020)**	**(.0011)**	**(.0015)**
	.0017	*.0083*	*-.0037*	*-.0005*	*.0055*	*.0053*
Cocaine price	.5186[a]	–	.1010	.1150	–	.0807
(in $100s)	(.0820)	–	(.0823)	(.0992)	–	(.1244)
	.1042[a]	**-.0154[a]**	**-.0888[a]**	**.0051**	**-.0039**	**-.0013**
	(.0092)	**(.0038)**	**(.0087)**	**(.0047)**	**(.0031)**	**(.0033)**
	.2246	*-.4281*	*-.3001*	*.0070*	*-.1388*	*-.0375*
Heroin price	.0743	–	.0123	.2505[a]	–	.0321
(in $1,000s)	(.0485)	–	(.0486)	(.0728)	–	(.0889)
	.0154[a]	**-.0022**	**-.0132[a]**	**.0169[a]**	**-.0082[a]**	**-.0087[a]**
	(.0051)	**(.0018)**	**(.0048)**	**(.0035)**	**(.0027)**	**(.0026)**
	.0248	*-.0457*	*-.0334*	*.0172*	*-.2183*	*-.1875*
Past year AIDS rate	1.173[a]	–	.6450[b]	.5731[c]	–	.2561
(cases per 1,000 residents)	(.2652)	–	(.2623)	(.2990)	–	(.3657)
	.1454[a]	**-.0418[a]**	**-.1036[a]**	**.0313[b]**	**-.0190[c]**	**-.0123**
	(.0232)	**(.0115)**	**(.0212)**	**(.0143)**	**(.0105)**	**(.0102)**
	.0588	*-.2179*	*-.0656*	*.0080*	*-.1268*	*-.0665*
NEP in city for 0–6 months	.3226[a]	–	.1869[b]	.1576[b]	–	-.1442
	(.0749)	–	(.0750)	(.0797)	–	(.1080)
	.0379[a]	**-.0117[a]**	**-.0263[a]**	**.0172[a]**	**-.0049**	**-.0123[a]**
	(.0084)	**(.0033)**	**(.0083)**	**(.0049)**	**(.0031)**	**(.0036)**
	.0650	*-.2588*	*-.0707*	*.0187*	*-.1388*	*-.2821*
NEP in city for more than	.3549[a]	–	.2924[a]	.3049[a]	–	.1697[b]
6 months	(.0524)	–	(.0525)	(.0578)	–	(.0745)
	.0229[a]	**-.0143[a]**	**-.0086**	**.0153[a]**	**-.0102[a]**	**-.0052[b]**
	(.0062)	**(.0021)**	**(.0060)**	**(.0030)**	**(.0023)**	**(.0021)**
	.0393	*-.3164*	*-.0231*	*.0166*	*-.2890*	*-.1193*
Sample proportion	.5829	.0452	.3718	.9211	.0353	.0436
Pseudo R^2		.1511			.1646	

The dependent variable is a categorical variable with three possible outcomes: testing negative for the specific drug; jointly testing positive for the specific drug, having injected drugs in the past 6 months, and having injected the specific drug previously; and testing positive for the specific drug but either not having injected drugs in the past 6 months or never having injected the specific drug previously. In the left (right) panel, the specific drug is cocaine (heroin). The sample size for both regressions is 160,110. Coefficients are from multinomial logit regressions, measured relative to the outcome indicating both a positive test and recent drug injection. Marginal effects are in bold. Parentheses contain standard errors, which are bootstrapped (using 100 replications) in the case of marginal effects. Italics represent elasticities for continuous variables and percentage changes resulting from a 0 to 1 change in NEP indicators. Marginal effects, elasticities, and percentage changes are evaluated at the observed means. Superscripts [c], [b], and [a] denote significance at the 90, 95, and 99 percent levels, respectively. The pseudo R^2 is $1 - L_e/L_c$, where L_e and L_c are the log likelihoods from estimated and constant-only models, respectively. All regressions include the variables listed in Appendix 1 as well as city indicators.

TABLE 8.—MULTINOMIAL LOGIT EQUATION COMPARING CURRENT NEEDLE SHARING WITH NO INJECTION, INJECTION WITHOUT SHARING, AND NO INJECTION AFTER PREVIOUS SHARING

Injection? Sharing?	No No (1)	Yes Yes (2)	Yes No (3)	No Yes (4)
Past month income	.0103	–	.1095[b]	-.2961[c]
(in $10,000s)	(.0462)	–	(.0481)	(.1647)
	-.0028	**-.0003**	**.0057[a]**	**-.0025**
	(.0026)	**(.0011)**	**(.0015)**	**(.0018)**
	-.0004	*-.0015*	*.0125*	*-.0405*
Cocaine price	-.0467	–	-.3563[b]	.4796[b]
(in $100s)	(.1271)	–	(.1426)	(.2366)
	.0117[b]	**.0015**	**-.0175[a]**	**.0043[a]**
	(.0057)	**(.0028)**	**(.0044)**	**(.0015)**
	.0138	*.0770*	*-.3850*	*.6977*
Heroin price	-.0911	–	-.1516[c]	-.0194
(in $1,000s)	(.0724)	–	(.0828)	(.1166)
	.0006	**.0023**	**-.0035**	**.0006**
	(.0038)	**(.0020)**	**(.0026)**	**(.0007)**
	.0007	*.0955*	*-.0623*	*.0788*
Past year AIDS rate	1.094[b]	–	.5731	2.411[a]
(cases per 1,000 residents)	(.4847)	–	(.5454)	(.7291)
	.0434[b]	**-.0265[b]**	**-.0279[c]**	**.0110[b]**
	(.0177)	**(.0118)**	**(.0154)**	**(.0046)**
	.0121	*-.2659*	*-.1201*	*.3491*
NEP in city for 0–6 months	.1243	–	.1062	.4205[c]
	(.1002)	–	(.1133)	(.2210)
	.0017	**-.0031**	**-.0010**	**.0024**
	(.0049)	**(.0025)**	**(.0038)**	**(.0016)**
	.0019	*-.1225*	*-.0169*	*.3000*
NEP in city for more than	.2304[a]	–	.1800[b]	.4884[a]
6 months	(.0785)	–	(.0882)	(.1643)
	.0061	**-.0057[a]**	**-.0026**	**.0021[c]**
	(.0040)	**(.0019)**	**(.0030)**	**(.0012)**
	.0067	*-.2253*	*-.0441*	*.2625*
Sample proportion	.9076	.0253	.0590	.0080
Pseudo R^2		.1844		

The dependent variable is a categorical variable with four possible outcomes: neither injecting drugs in the past 6 months nor recently sharing needles; both injecting drugs and sharing needles; injecting drugs but not sharing needles; and not injecting drugs but recently sharing needles. The sample size is 124,177. Coefficients are from a multinomial logit regression, measured relative to the outcome indicating both injection and sharing. Marginal effects are in bold. Parentheses contain standard errors, which are bootstrapped (using 100 replications) in the case of marginal effects. Italics represent elasticities for continuous variables and percentage changes resulting from a 0 to 1 change in NEP indicators. Marginal effects, elasticities, and percentage changes are evaluated at the observed means. Superscripts [c], [b], and [a] denote significance at the 90, 95, and 99 percent levels, respectively. The pseudo R^2 is $1 - L_e/L_c$, where L_e and L_c are the log likelihoods from estimated and constant-only models, respectively. All regressions include the variables listed in Appendix 1 as well as city indicators.

APPENDIX 1.—EFFECTS OF OTHER VARIABLES ON DRUG INJECTION AND NEEDLE SHARING

Variable	Past 6 month drug injection			Current needle sharing	
	By itself	and cocaine	and opiates	Full sample	Injectors
Table number	(2)	(3)	(4)	(5)	(6)
Age	.3946[a]	.4150[a]	.4218[a]	.4212[a]	.0962[a]
	(.0076)	(.0102)	(.0113)	(.0138)	(.0175)
Age squared	-.0046[a]	-.0048[a]	-.0046[a]	-.0050[a]	-.0011[a]
	(.0001)	(.0001)	(.0001)	(.0002)	(.0002)
Education	.3270[a]	.2481[a]	.3053[a]	.3124[a]	.0682
	(.0212)	(.0263)	(.0284)	(.0343)	(.0442)
Education squared	-.0169[a]	-.0127[a]	-.0148[a]	-.0172[a]	-.0051[b]
	(.0010)	(.0013)	(.0014)	(.0017)	(.0022)
Black	-1.366[a]	-.8341[a]	-.8842[a]	-1.234[a]	-.4116[a]
	(.0265)	(.0333)	(.0408)	(.0441)	(.0630)
Hispanic	-.2343[a]	.0851[b]	.5512[a]	-.1740[a]	.0769
	(.0299)	(.0391)	(.0418)	(.0476)	(.0620)
Other	-1.251[a]	-1.211[a]	-.9823[a]	-.8892[a]	-.1252
	(.1052)	(.1671)	(.1744)	(.1668)	(.2618)
Married	-.2429[a]	-.2811[a]	-.1196[a]	-.3585[a]	-.2360[a]
	(.0314)	(.0406)	(.0443)	(.0535)	(.0720)
Cohabiting	-.0054	-.1227[a]	.0143	-.0334	-.0241
	(.0308)	(.0406)	(.0454)	(.0524)	(.0700)
Divorced	.1719[a]	.1208[a]	.1506[a]	.0623	-.0600
	(.0273)	(.0341)	(.0389)	(.0441)	(.0575)
Widowed	.3617[a]	.3196[a]	.3354[b]	.0629	-.1952
	(.1020)	(.1217)	(.1318)	(.1722)	(.2169)
Works full-time	-.7046[a]	-.7279[a]	-.7072[a]	-.7821[a]	-.3294[a]
	(.0259)	(.0344)	(.0396)	(.0444)	(.0610)
Works part-time	-.1376[a]	-.1776[a]	-.0416	-.2490[a]	-.1924[a]
	(.0308)	(.0401)	(.0443)	(.0531)	(.0724)
Percent of income	1.680[a]	1.658[a]	1.642[a]	1.507[a]	.5315[a]
from illegal sources	(.0314)	(.0368)	(.0422)	(.0488)	(.0632)
1990	-.1765[a]	-.1357[b]	-.1581[b]	-.2625[a]	-.2357[b]
	(.0505)	(.0616)	(.0740)	(.0770)	(.1014)
1991	-.3945[a]	-.3216[a]	-.3521[a]	-.4380[a]	-.3363[a]
	(.0456)	(.0548)	(.0662)	(.0675)	(.0912)
1992	-.4720[a]	-.3550[a]	-.4090[a]	-.4837[a]	-.2245[b]
	(.0455)	(.0553)	(.0666)	(.0691)	(.0921)
1993	-.5325[a]	-.5139[a]	-.4377[a]	-.6310[a]	-.3037[a]
	(.0483)	(.0595)	(.0715)	(.0750)	(.1013)
1994	-.6064[a]	-.6593[a]	-.4450[a]	-.8006[a]	-.2365[c]
	(.0573)	(.0711)	(.0835)	(.0954)	(.1340)
1995	-.7104[a]	-.9454[a]	-.5879[a]	-.8752[a]	-.3724[a]
	(.0525)	(.0681)	(.0767)	(.0955)	(.1268)
1996	-.8211[a]	-1.043[a]	-.6782[a]	–	–
	(.0561)	(.0731)	(.0829)		
1997	-.8500[a]	-1.274[a]	-.7539[a]	–	–
	(.0615)	(.0824)	(.0924)		

The dependent variable in each column is the dependent variable from the table indicated by the column number. The regressions are those from column 3 of those tables and also include city indicators. Coefficients are from logit regressions. Parentheses contain standard errors. Superscripts [c], [b], and [a] denote significance at the 90, 95, and 99 percent levels, respectively.