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ABSTRACT

The number of prisoners incarcerated on drug-related offenses rose fifteen-fold between 1980 and 2000. This paper provides the first systematic empirical analysis of the implications of that dramatic shift in public policy. We show that the increase in drug prisoners led to reductions in expected time served for other crimes, especially for less serious offenses. Reductions in time served, however, increased other crimes by no more than a few percent. Moreover, incarcerating drug offenders is found to be almost as effective in reducing violent and property crime as locking up other types of offenders. We estimate that cocaine prices are 10-15 percent higher today as a consequence of increases in drug punishment since 1985. Based on previous estimates of the price elasticity of demand for cocaine, this implies a reduction in cocaine consumed of as much as 20 percent.

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Public policy towards drug offenders has undergone a dramatic transformation in the United States over the last two decades. In 1980, approximately 24,000 inmates in U.S. prisons (state and federal combined) had drug crimes as their most serious offense. Twenty years later, that number is estimated to be near 400,000. Drug offenders now make up over 30 percent of all inmates in state and federal prisons, compared to less than eight percent in 1980. The enormous increase in incarcerated drug offenders has come at a time when U.S. drug use, as measured by self-report surveys, has been steadily falling (Reinarman and Levine 1997).

There is a large body of literature analyzing the causes and consequences of the increased criminal justice response to the drug trade (for example, (Boaz 1990, Duke and Gross 1993, Rasmussen and Benson 1994, Donziger 1996, Nadelman 1997). Most observers agree the use and sale of illegal drugs impose externalities including community disruption, crime, or the spread of AIDS, making lower drug consumption a socially beneficial objective. With few exceptions, however, analysts have been highly critical of government policies, including the rising drug-offender prison population. Even former proponents of the approach, including ex-Drug Czar William McCaffrey and the Republican Governor of New York, George Pataki, have recently expressed reservations about the continued emphasis on law enforcement in combatting the drug problem (Alter 2001, Perez-Pena, 2001).

In spite of all the attention devoted to the question of drug policies, however, there has been remarkably little rigorous empirical analysis of the impact of these policies. Benson and Rasmussen (1991), using a cross-section of Florida counties, find that counties with high rates of drug arrests have lower clearance rates for property crime. One interpretation of this result is that diversion of police resources to drug offenses reduces the ability to fight other crimes.

Miron (1999) demonstrates that homicide rates are positively related to drug and alcohol prohibition policies. On the other hand, DeSimone (forthcoming) finds a negative relationship between cocaine prices and crime rates, suggesting that drug enforcement might reduce crime at the margin. With respect to the impact of criminal justice sanctions on drug consumption, both Desimone (1998) and Chaloupka, Grossman, and Tauras (1999) show a negative, but relatively weak, relationship between the severity of statutory sanctions for drug use and higher drug consumption. Farrelly et al. (2000) identifies a negative relationship between the certainty and severity of marijuana punishment and marijuana usage

We are unaware of any empirical studies addressing a number of other fundamental issues in the evaluation of criminal justice policies towards drug offenders: the extent to which the imprisonment of drug prisoners has crowded-out other types of offenders from prisons, the relative crime reduction achieved from incarcerating drug offenders vis-a-vis other criminals, or even the relationship between drug punishment and drug prices.

In this paper, we provide a first attempt at a systematic analysis of those questions. We begin by examining whether imprisoning a greater number of drug criminals reduces the time offenders serve for other crimes due to prison capacity constraints. We find substantial (but not complete) crowd-out occurs. While the increased incarceration of drug prisoners has resulted in other inmates being released sooner, the implied increase in other crimes through this channel is small: only a 1-3 percent increase in property and violent crime since 1980 as a consequence of allocating a greater share of scarce prison cells to drug offenders. Changes in drug punishment might also affect the general crime level through other avenues if, for instance, drug offenses and other crimes are either substitutes or complements, or changes in drug punishment affects the level of violence associated with establishing and maintaining property rights to illegal drug distribution. Empirically, however, we find that the reduction in violent and property crime associated with adding one additional drug prisoner is almost as great as the reduction in crime when a violent or property offender is sentenced to prison.[1]

We then explore the impact of drug punishment on cocaine prices. Using city-level panel data on cocaine prices derived from street buys conducted by undercover agents, we find that harsher punishments for drug offenders are associated with substantial increases in the price of drugs. The combined increase in the certainty and severity of drug punishment between 1985 and 1996 is estimated to have raised the street price of cocaine 12-14 percent. These findings suggest that the current drug policy imposes a substantial cost on drug suppliers. Existing estimates of the long-run price elasticity of demand for hard drugs (i.e., cocaine, heroin, and opium) typically range between -1 and -1.8 (Van Ours 1995, Saffer and Chaloupka 1995, Grossman and Chaloupka 1998, Chaloupka, Grossman, and Tauras 1999, Liu 1999). Thus, the price increase associated with increased punishment of drug offenders between 1985 and 1996 is estimated to have reduced cocaine consumption by almost 20 percent.

There are a number of important limitations to our analysis that must be noted. First, the data we use, although the best available, are nonetheless of questionable quality.[2] Second, while we consider a range of outcome variables, there are other consequences of drug policy that we

[1] This implies either that drug offenders are committing other crimes at rates comparable to those of criminals arrested for non-drug crimes, or that the resulting reduction in the drug trade from incarcerating drug dealers indirectly reduces violent and property crime.
[2] For a highly critical discussion of the state of available data on drug consumption and prices see National Research Council (2001).

do not examine, e.g. the effect of anti-drug efforts on municipal or state budgets. Third, our analysis is limited by the fact that we have no power to test the efficacy of the current drug regime versus alternative approaches such as increased availability of drug treatment facilities. While we find that the criminal justice approach to fighting drugs has been at least somewhate successful, it is possible that other approaches would have been more cost-effective in achieving the same goals. Fourth, much of the analysis that we conduct is correlational in nature, despite the fact that earlier research highlights the importance of endogeneity in the criminal justice system (Fisher and Nagin 1978, Levitt 1996, Levitt 1997). In most of our applications, however, it is possible to sign the likely biases in our estimates that result from relying on correlations.

The remainder of the paper is structured as follows. Section II briefly overviews the economics of drug prohibition. Section III presents estimates of the crowd-out effect of drug prisoners on inmates convicted for other crimes. Section IV analyzes the link between the scale of imprisonment, expected punishments for drug and non-drug offenders, and property and violent crime. Section V assesses the relationship between drug punishment and drug prices. Section VI concludes.

II. The Economics of Fighting Drugs and American Drug Public Policy

The dominant economic feature differentiating the market for illicit drugs from other products is the strict prohibition on their distribution and use, enforced by criminal punishments. Figure 1 demonstrates the profound shift in United States policy towards drugs in the last two decades. The figure presents national time series data for the number of adult drug-related arrests and new commitments of drug offenders to state prisons. For purposes of comparison, arrests for Index (violent and property) crimes and new commitments to state prisons for these crimes are also shown,[3] as is the total number of prisoners whose most serious offenses is drug related. In all cases, the time series are indexed with the 1980 value normalized to 100 in order to facilitate comparisons. The most striking result in the figure is that new drug commitments to state prisons increased more than ten-fold in less than a decade (to a rate of approximately 100,000 per year). The increase in drug-related prison commitments far outpaced the increases in drug arrests, although that number itself more than tripled during the time period to over 1.5 million annually. While not shown in the figure, the trend in the number of drug prisoners (i.e. the stock of those incarcerated, as opposed to the flow) is virtually identical to that of new drug commitments, implying that none of the increase in the number of drug prisoners is a consequence of longer prison terms conditional on being sentenced to prison. Non-drug arrests have been essentially flat over this time period, whereas non-drug commitments to prison approximately doubled. Thus, while less pronounced than in the drug case, the trend toward greater use of prisons for punishing other crimes is also apparent.

The prohibition of illegal drugs stands in stark contrast to the Pigouvian tax approach used to internalize the externalities associated with similar products such as tobacco and alcohol. Rasmussen and Benson (1994) and Miron and Zwiebel (1995) analyze the economics of prohibited goods. At a fixed price, the supply of a prohibited good is likely to be lower than if that good were freely traded. Avoiding detection introduces inefficiency into the production and distribution of goods. For instance, under the current regime, manufacturing of cocaine is done

3 We present these particular data series because it is possible to gather consistent data over the entire period. Including federal prisons does not alter the picture, but data are not available in all years.

at an inefficiently small scale in operations hidden in remote areas of South America; the product is then smuggled into the United States at high cost; and the final steps in producing and packaging crack cocaine are carried out in the retail distributor's kitchen.[4] Furthermore, the street price of the good must be elevated to compensate distributors for both the risk of punishment and the non-trivial fraction of illegal goods that are confiscated by authorities.[5] The Drug Enforcement Agency (DEA 1996) estimated global cocaine production in 1996 to be 700 metric tons. The amount of cocaine seized worldwide in 1996 exceeded 200 metric tons (or about 30% of the total produced), with US Federal law enforcement agencies accounting for a majority of the seizures. On the demand side, at any given price, the quantity consumed of an illegal drug is likely to be lower than if that drug were legal, due both to stigma effects and the risk of punishment a user faces.[6] Thus, the basic prediction of the standard economic model is that when a good is prohibited, the quantity consumed will be unambiguously lower, but the impact on price is indeterminate.

As demonstrated in Figure 1 above, the punishment for drug-related offenses in the United States over the last two decades increased dramatically. Given that punishment is

4 Although Miron (2000) argues that the regulatory compliance costs for legally produced goods is substantial – perhaps even as great as these detection avoidance costs for prohibited goods.

5 It is often argued by critics of government drug policy that the supply of labor to the drug market is quite elastic (e.g. Moore 1990, Rasmussen and Benson 1994), i.e. when one drug dealer is incarcerated, another person simply takes his place with little disruption to the drug-selling operation. Even if this is true, however, one would expect increasing drug punishments to induce an inward shift of the supply curve since drug sellers must be compensated for the increased risk of punishment. Levitt and Venkatesh (2000) present empirical evidence that wages of street-level sellers rise when the risks associated with drug dealing rise.

6 Offsetting these factors would be any increased desirability of consuming the illegal good due to its "forbidden fruit" nature.

overwhelmingly directed at drug traffickers as opposed to drug users,[7] one would expect to have seen rising drug prices and falling drug quantities as punishment rose. In practice, however, this was not the case. Figure 2 presents yearly estimates of cocaine prices and quantities in the United States. Prices in the figure are estimates from Rhodes and Kling (1997) which uses information from the DEA's STRIDE database of undercover drug purchases. We report two different sets of estimates on aggregate U.S. cocaine consumption over time, one from RAND researchers (Everingham et al. 1995) and the other from Rhodes et al. (1998).[8] Cocaine prices fell sharply through most of the 1980's, and have remained essentially flat since that time. Consumption of cocaine rose sharply as the price fell in the 1980's. Depending on which estimate of consumption one relies upon, cocaine use either stayed level since the late 1980's, or fell sharply in the late 1980's before leveling off. The pattern of falling prices and rising consumption in the 1980's is precisely the opposite of what would be expected based on the incarceration patterns. Rather than an inward shift in supply, these data appear consistent with a dramatic outward shift in supply, perhaps do to increased efficiency and sophistication on the part of drug cartels, smugglers, and retailers. Combining the information in Figures 1 and 2, it is easy to understand the broad criticism that has been leveled against the "War on Drugs,"

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7 According to Bureau of Justice Statistics (1995), although almost three-quarters of drug-related *arrests* are for possession rather than trafficking, two-thirds of drug-related *convictions* are for trafficking. A higher fraction of those charged with trafficking are sentenced to jail or prison, and mean maximum sentences of traffickers are three times longer. Thus, as of 1993, our calculations suggest that 86 percent of all drug-related time incarcerated was on trafficking charges. To the extent that some of those convicted of possession were in fact drug dealers caught with only small amounts of drugs, the true fraction is likely to be even higher.

8 Cocaine consumption estimates are based on estimated prevalence of use from household survey data, combined with estimates of expenditures per user and the price of cocaine. See the original sources for greater detail on the methodologies. For our purposes, it is important to note that the estimated quantities are based in part on estimated cocaine prices, potentially inducing a mechanical negative relationship between price and quantity.

although it is unclear whether prices may have fallen even further and consumption increased even more had increased punishment not been put into place.

Further reinforcing criticism of increasing punishments for drug offenders is the fact that the empirical relationship between prohibition and elevated violence is well documented (Miron 1999). Peak murder rates in the United States in the Twentieth century correspond to the early 1930's, when Prohibition was in place, and the early 1990's when the crack epidemic was in full force. The primary reason for the link between violence and prohibition appears to be the absence of legally enforceable property rights in such markets. While it seems clear that a policy of prohibition fosters violence, it is less obvious whether crime will rise or fall when punishment increases for an already prohibited good. In the short run, greater enforcement can lead to rising violence by disrupting the existing allocation of property rights. If a drug lord who controls an area is removed, competition (often in the form of violence) among those attempting to establish dominance is likely to ensue. On the other hand, the willingness to engage in violence should be linked to the possible profits associated with illegal drug distribution. To the extent that increased drug enforcement reduces the demand for illicit drugs, profits will fall, and in the long-run, violence should also decrease.

The impact of increased punishment on the number of crimes committed by drug *users* to raise money to support their drug use is also indeterminate. If the demand for drugs was perfectly inelastic, than an increase in the price of drugs would likely lead to more crime among addicts attempting to support their habit. If, however, drug consumption falls in response to increased enforcement, this need not be the case. In practice the demand for hard drugs appears to be relatively elastic in the long run (e.g. Van Ours 1995, Grossman and Chaloupka 1999).

An alternative channel through which drug enforcement can affect violence is by crowding-out criminal justice resources for fighting other types of crime. At least in the short run, such an argument appears plausible. Virtually every state prison system in the country is at or near capacity and has been for at least a decade (Selke 1993). Increasing capacity through the construction of new prisons takes many years to complete. There are, however, a number of other margins along which a prison system can accommodate increases in inmates in the near term, including overcrowding existing prison facilities, housing prisoners in jails (which are typically reserved for individuals awaiting trial or those with sentences of less than a year), using private prison providers, or contracting with other states that have excess prison capacity. With the exception of prison overcrowding, which is endemic, use of the other strategies has been relatively limited in scope. Diversion of police, prosecutor, and court resources away from other crimes towards drug offenses are also possible.

III. Does Incarceration of Drug Offenders Crowd-Out Punishment for Other Criminals?

The empirical analysis of this paper begins with an examination of whether incarceration of drug offenders reduces the number of prison cells available to punish those committing violent and property crime. In order to examine the question of crowd-out, we utilize data from the National Crime Reporting Program (NCRP) administered by Bureau of Justice Statistics. NCRP provides two separate individual-level data files for participating states: one data set contains all prison commitments, the other has all prison releases. Data collection began in 1983 with 29 states, and has continued annually, with the number of participating states rising to 37 in 1996, the most recent year available. A number of states report incomplete or clearly flawed data, especially in the earlier years; we are thus forced to eliminate 152 state-year pairs of data from our analysis, or 29% of state-year observations for the time period 1983-1996. Among the variables included for those being committed to prison are whether they are being newly committed (as opposed to being re-committed due to a parole revocation), the most serious offense among the current set of charges, and the maximum time to be served.[9] The data set on prison releases includes those variables, as well as actual time served. The top panel of Table 1 presents summary statistics for the NCRP data used in this analysis. Drug offenders represent over twenty percent of all new commitments. The median percent of time served ranges from 30 to 46 percent of the maximum sentence handed down. Contrary to public perception regarding mandatory minimum sentencing of drug offenders, the fraction of time served by such criminals in state prisons is actually the lowest of all crime categories included.

In our empirical analysis, we include state-fixed effects to capture any omitted state characteristics that remain constant over time, as well as year dummies to absorb annual nationwide fluctuations. Column 3 of Table 1 presents standard deviations of the variables after these state and year indicators are removed. For the NCRP data, between 30 and 60 percent of the overall variation remains. For variables like the percent Black population in a state or state per capita income, the fraction of variation that remains is much smaller.

We estimate the crowd-out effect of drug imprisonments using the following specification:

$$\textit{Median \% of time served}_{st} = \beta * \textit{DrugShare}_{st} + X_{st}'\Gamma + \lambda_s + \theta_t + \varepsilon_{st} \quad (1)$$

9 The data also include limited individual-level characteristics, such as age, gender, and race of the offender, although we do not make use of this information in the current paper since we focus only on aggregated data. No information on prior criminal activity is available in the data.

where *s* corresponds to states and *t* to years. The dependent variable in equation (1) is the median time served by the cohort *released* in year *t* who most serious conviction was for a particular crime.[10] Equation (1) is estimated separately for each crime category. Our measure of the extent of drug incarceration is the percent of all new prison commitments that are on drug charges for the cohort *committed* in year *t*. Thus, equation (1) relates the composition of the current batch of criminals sent to prison to the sentences actually served by those being released. Assuming that the number of new commitments for drugs does not affect the number of new commitments for other crimes and the prison system is in a steady state, the coefficient $ has a straightforward interpretation: the degree of crowd-out of the sentences of previously convicted offenders.[11] A coefficient of -1 implies one-for-one crowd-out; a coefficient of zero corresponds to no crowd-out. Other covariates (represented by the vector *X* in the equation) in the specifications are the unemployment rate, per capita income, the Black share of the population, per capita malt liquor consumption, and the "effective" abortion rate (Donohue and Levitt 2001).[12] All of these covariates are available annually at the state level. State-fixed effects and year dummies are included in all regressions.

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10 Because of the structure of the data, there is no way to effectively compute median time served for the cohort of prisoners entering in a given year.

11 If an increase in new commitments for drugs causes fewer new commitments for other crimes (for instance, due to congestion in courts or policing), then our estimates understate the total degree of crowd out. Empirically, we find no evidence that drug commitments crowd out the number of new prison commitments for other offenses. An alternative way of specifying the model is to use new drug commitment rates per capita, perhaps controlling for the rate of new non-drug commitments. We obtain similar results when that alternative specification is employed, but opt for the model presented because of the ease of interpretation of the coefficients.

12 The "effective" abortion rate is the weighted average abortion rate for individuals born in a state, with weights determined by the national average fraction of arrests for violent crime by age group.

Empirical results from the estimation of equation (1) are presented in Table 2 for the FBI index I crimes, as well as fraud and drug offenses. One clear pattern that emerges from the table is that the degree of crowding-out attributable to drug commitments is inversely correlated with the severity of the crime. For the most serious offenses, murder and forcible rape, the point estimates are small and not statistically significant. For the other violent crimes, robbery and aggravated assault, the point estimates are roughly -.35. Fraud carries a similar coefficient. For property crimes, the extent of crowd-out is much larger: ranging from -.53 to -.93. Higher rates of new drug commitments reduce the share of the original sentences served by earlier drug offenders to the same extent as they reduce the share served by property crime offenders. Taking a weighted average across crime categories, with weights proportional to the fraction of releases by offense type in our sample, the overall crowd-out estimate is –.53 in the relatively short time horizon implied by an analysis with year- and state-fixed effects included. Thus, on average for every two new drug prisoners sent to prison, one represents a real increase in the prison population and the other displaces an existing prisoner who is released early.[13]

The last two rows of Table 2 present coefficients on the drug commitments variable from alternative specifications. Excluding the other covariates in the regression has little impact on the coefficients. In the bottom row of the table, we use the actual median time served in months as the dependent variable (as opposed to the percent of the maximum sentence served). Coefficients from this last row are not readily comparable to those of the other specifications because of the different units of analysis. With the exception of rape, all of the other coefficients

[13] The crowd out effects we identify are not limited only to new drug commitments. Empirically, increases in the number of offenders sent to prison for other crimes also reduces time served by existing prisoners, as would be expected. The magnitude of the crowd-out effect for non-drug commitments in some cases appears to be smaller than for drug commitments.

on the drug-share variable are negative. A ten percentage point increase in the share of new commitments on drug charges reduces the median time served for murder by 7.6 months. For other crimes, the corresponding reduction is two to six months. Because the more serious crimes have longer sentences, the percentage reduction in time served is smaller for the violent offenses, consistent with the main findings of the table.

The crowd-out related reduction in expected time served may increase non-drug crime due to lessened deterrence and decreased incapacitation, as argued by critics of current policy. Back of the envelope calculations, however, suggest that the magnitude of this effect is small. The fraction of new prison commitments on drug-related charges in the United States rose from roughly 10 percent in 1985 to 30 percent in 1996. Based on the coefficients in Table 2, this implies an approximate reduction in time served of 10-12 percent for property crimes, 7 percent for robbery and aggravated assault, and 4 percent for murder. Previous estimates of the elasticity of crime with respect to punishment typically range from -0.10 to -0.30 (Marvell and Moody 1994, Levitt 1996, Levitt 1997, Donohue and Siegelman 1998). Thus, the predicted increase in property crime as a consequence of lower punishments due to drug-offender crowding out is 1-3.6 percent. For robbery and aggravated assault, the projected increase is 0.7 to 2.1 percent; for murder the number is only half as large. Relative to the observed fluctuations in crime in recent years – murder rates in the United States fell 46 percent between 1991 and 1999 – the potential impact of crowd-out appears small.

The other coefficients in Table 2 are also of potential interest. When unemployment rates are high, the fraction of the sentence that is served falls slightly: a one percentage point increase in the unemployment rate lowers median time served by about 1 percent for most crime categories. Higher state income, however, is associated with serving a shorter fraction of the sentence. Increases in the share of the state population that is black and per capita malt liquor sales correlate with a higher fraction of the sentence being served.

IV. How effective is incarcerating drug offenders in reducing violent and property crime?

These simple crowd-out estimates are unlikely to reflect the full impact of drug enforcement efforts on other types of crime for a range of reasons. First, to the extent that drug and non-drug offenses are substitutes (complements) for one another, the sharp increase in the "price" of drug-related crime due to increased punishments would lead to an increase (decrease) in non-drug crime. There is some evidence (Thornberry et al. 1994 and Levitt and Venkatesh 2001) that drug and non-drug crimes may be complements to one another, implying that rising drug penalties reduce other types of crime as well.[14] Second, regardless of the first point, those imprisoned for drug offenses also engage in violent and property crime.[15] Thus, holding constant the number of other inmates, an increase in the number of drug prisoners is likely to lower violent and property crimes through incapacitation effects. Finally, as noted in the preceding section, punishing drug offenders may change the incentives for engaging in property-rights-related violence or criminal activities by drug users.

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[14] It has also been argued that large-scale incarceration of drug offenders has resulted in a greater number of individuals being part of the drug trade at some point in time (because many potential sellers are incarcerated and thus are temporarily unable to sell drugs). If this is true, and one-time exposure to the drug trade increases long-term non-drug criminal activity, then high rates of drug punishment may lead to increased non-drug crime.

[15] Beck and Shipley (1989) analyze three year re-arrest rates among state prisoners released in 1983. Roughly half of the released prisoners whose most serious offense was drug-related are re-arrested within three years. Of those re-arrested, one-fourth are arrested for a violent crime and half are arrested for property offenses. Conversely, for the group of felons who had been incarcerated for either property or violent offenses, one in six is re-arrested on drug charges within three years of release. Note also that this study analyzes data from the mid-1980's, when drug arrests were only half the current level and before the peak of crack-related gang violence. If the study were redone today, the overlap between drug and non-drug offenders would likely be even greater.

The discussion above suggests that *a priori* we are uncertain how incarcerating drug offenders will affect violent or property crime rates. To explore this issue more formally, we estimate a model with the following structure:

$$\ln(\text{crime}_{s,t}) = \underset{c}{\mathbf{j}} *_c \ln(\text{prisoners}_{s,t-1})*(\text{prisoner share}_{cs,t-1}) + X_{st}'\,' + l_s + q_t + ,_{st} \qquad (2)$$

where *s* indexes states, *t* corresponds to years, and *c* represents different types of crimes (violent, property, drug, or other). The dependent variable is the per capita crime rate. [16] The specification presented in equation (2) mirrors that used in previous research, e.g. Marvell and Moody (1994), Levitt (1996), except that those papers focus exclusively on aggregate prison populations, and thus implicitly restrict the * coefficients to be equal across those sentenced for committing different crimes. In contrast, we allow prisoners convicted of violent crime to have a differential impact on crime than those convicted of drug offenses, property offenses, or other offenses.[17] Both the number of prisoners and the share of prisoners by each type of crime are based on the stock of prisoners in a given state at a particulare point in time. If the coefficient $*_{drug}$ is smaller in absolute value than the other *'s, then this would imply that states in which prison population growth has been more heavily concentrated among drug offenders have experienced smaller declines in crime than did states where prison population growth has been less concentrated among drug offenders. If the marginal criminal sentenced to prison had an identical impact on crime across the four prisoner categories (drug, violent, property, and other), each of the * coefficients would be identical, and equal to the elasticity estimated using

15 Holding constant the expected punishment per crime, if crime rises, then the prison population will also rise roughly in proportion to the increase in crime, perhaps with a lag due to delays in prosecution and sentencing. Using the once-lagged value of the prison population avoids this mechanical, reverse-causality driven relationship.

[17] Offenses classified as "other" are made up primarily of weapons offenses, receiving stolen property, fraud, and public order offenses.

aggregate prison populations.[18] The same covariates used earlier are also included in this specification. In order to minimize endogeneity, the prison variable is once lagged.

Table 3 presents separate results using violent and property crime rates as the dependent variable. In columns 1 and 3, baseline estimates using aggregate prison populations are presented. Columns 2 and 4 disaggregate by the crime category for which a prisoner is sentenced. The results in columns 1 and 3 are consistent with estimates obtained in previous research utilizing similar specifications, although the standard errors in Table 3 are much larger than in previous estimates because our sample size is smaller.[19] The elasticity of crime with respect to the size of the prison population is roughly -.09 for violent crime and -.17 for property crime. to -.15. Allowing the coefficients to vary by the most serious offense committed by the prisoner yields point estimates on drug offenders that are greater than those for property crime, but smaller than the coefficients associated with violent crime or "other" offenses.

In no case is the difference between the coefficient on drug prisoners and the coefficient on any other type of prisoner statistically significant at the .05 level. Thus, one cannot reject that, on the margin, an increase in the prison population as a consequence of more drug offenders has the same impact on property and violent crime as increases in other types of prisoners. This result is

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[18] Alternatively, one could simply estimate the elasticity of crime with respect to prisoners of different types and calculate the marginal impact evaluated at the mean. We obtain very similar results when doing so.

The prison share variables are calculated by the authors using information from NCRP on the number of prisoners sentenced and released by crime type.

[19] Levitt (1996) finds substantially larger elasticities when using prison overcrowding litigation as an instrument for the size of the prison population. Our estimates in columns 1 and 3 also become more negative when instrumenting in this fashion. Unfortunately, the prison overcrowding litigation instruments have little power to separately identify the impact of prison size by the type of crime committed, i.e. the decline in prisoners in response to the litigation is roughly proportional across crimes. Thus, this instrumenting strategy does not aid in addressing the question of whether incarcerating drug offenders has a differential impact than imprisoning other offenders.

consistent with optimizing behavior on the part of the criminal justice system in allocating scarce prison cells across offenders of different types.

V. The impact of drug incarceration on drug prices

One of the primary goals of the increased punishment of drug offenders is to decrease the consumption of illegal drugs. As noted earlier, roughly 85 percent of the drug-related punishments are for the distribution of drugs, as opposed to possession or consumption. Thus, one would expect that the primary impact of the dramatic increase in drug incarceration would be to shift the supply curve inward, leading to an increased street-price of drugs and decreased usage. Although the aggregate time-series evidence presented in Figure 2 is not consistent with that prediction, it is possible that the increase in drug punishment has indeed had the predicted effect, but that other factors such as technological advances in drug production and distribution have offset the incarceration-driven supply shift. If that is the case, then absent the increase in punishment for drugs, the street-price of drugs might be much lower and quantity of drugs consumed much higher than currently observed..

Since 1981, the DEA has recorded the details of undercover drug purchases in the System to Retrieve Information from Drug Evidence (STRIDE) data set. Included in the data set is information on the price paid, quantity purchased, location of the sale, and purity of the drug. In a typical year, more than 2,000 cocaine/crack cocaine purchases are included in the data set. For a large city such as Chicago, more than 150 individual cocaine/crack cocaine buys of 5 ounces or less are reported. Using the STRIDE data, Rhodes and Kling (1997) construct an annual, city-level time series on cocaine prices for 31 large U.S. cities. These price indexes serve as our measure of city-level cocaine prices.

We estimate a reduced-form relationship between drug prices and criminal justice variables as follows:

$$P_{ct} = \sum_{o} (\beta_o Certainty_{cto} + \gamma_o Severity_{cto}) + X_{ct}'\delta + \varepsilon_{ct} \qquad (3)$$

where *c* indexes cities, *t* corresponds to years, and *o* represents offense categories (i.e. drug, violent, or property). The dependent variable is city-level cocaine prices. *Certainty* is the number of arrests per capita for the relevant offense category. *Severity* is the fraction of arrests for a given offense category which result in the criminal being sentenced to prison. The same covariates included in the earlier tables are also included here, as are city-level fixed effects and year indicators. Although our price series has city-level variation, the right-hand-side variables in the regression vary only at the state level. The reported standard errors have been corrected to take into account the state-level clustering of the data.

The two criminal justice measures in the regression are clearly imperfect. The arrest rate per capita is affected not only by the certainty of punishment, but also by the level of criminal activity in a city. For a fixed likelihood of apprehension, the greater the underlying amount of crime, the higher is the arrest rate per capita. To the extent that drug prices respond to the scale of criminal activity, the interpretation of these arrest rate coefficients may be misleading. For instance, if changes in drug prices within a city over time are primarily driven by city-level demand shifts (which would not be absorbed in either city-fixed effects or national-level year dummies) and the probability of punishment remains constant over time, then one would expect a positive relationship between the arrest rate variable and drug prices that is not causally related to drug punishment. On the other hand, if cities systematically increase arrest rates in response to falling drug prices, the β_d coefficient could yield a spurious negative relationship between drug prices and arrests, even if the true relationship is positive.

The proxy for severity of punishment – the fraction of arrests that result in prison terms – would appear to be less sensitive to such biases than the arrests per capita variable. The primary weakness of our severity measure is that it focuses solely on whether an individual is sent to prison, ignoring the expected time served. We use this variable because of the absence of well-defined measures of expected time served. It is worth noting, however, that when we adjust this variable to incorporate expected time served using proxies we construct from NCRP data, the results obtained are similar.

The empirical results from the estimation of equation 3 are presented in Table 4. Column 1 includes the proxy for certainty of punishment (as well as the other covariates included in the earlier tables), but not the proxy for severity. Column 2 does the reverse. Column 3 includes both measures together. The coefficient on per capita drug arrests is strongly positive and highly statistically significant in both the first and third columns. A one-standard deviation increase in the rate of drug arrests (implying an additional 280 drug arrests per 100,000 residents annually) is associated with an 18 percent rise in the street-price of cocaine. The coefficient on commitment rates per drug arrest is also positive (our proxy for severity), but not statistically significant. Nonetheless, the point estimate implies that a one-standard deviation increase in this variable (making the probability of going to prison conditional on a drug arrest 8 percentage points higher), is associated with cocaine prices that are 4.5 to 9 percent higher. Comparing our sample average values of drug arrest rates per capita in 1985 and 1996 (.0046 versus .0060), the increase in arrests would imply a cocaine price rise of roughly 10 percent. A similar calculation for commitments to prison conditional on being arrested (which rose from .026 to .061) suggests a 2-4 percent increase in cocaine prices. The combined impact of changes in drug policy between 1985 and 1996 is thus estimated to have raised cocaine prices 12-14 percent.

Previous estimates of the long-run price elasticity of hard drugs are typically between -1.0 and -1.8 (Van Ours 1995, Saffer and Chaloupka 1995, Grossman and Chaloupka 1998, Liu 1999). Using the mid-point of that range, we estimate that the increase in price associated with rising drug punishments between 1985 and 1996 lowered the quantity of cocaine consumed by almost 20 percent.[20]

The coefficients on the violent and property crime punishment proxies are mixed. Harsher punishments for violent crimes are generally also associated with higher cocaine prices, although the magnitude of the effects are smaller than for drug punishments, and in one case the sign reverses. The point estimates on property crime sanctions are uniformly negative, although never statistically significant. One (highly speculative) interpretation of these results is that drug selling and violence are complements, whereas drug selling and property crime are substitutes. Tougher enforcement of property crime leads more criminals to find drug selling attractive, shifting out the supply of drug sellers and lowering the price.

VI. Conclusion

Despite its public policy importance, there have been no previous attempts to analyze empirically the impact that the unprecedented increase in drug-related imprisonment has had on

[20] This estimate is consistent with the findings of Farrelly et al. (2000), who estimate an elasticity of marijuana usage with respect to marijuana punishments equal to -.3.

crime rates or drug markets. Our results suggest that imprisoning drug offenders leads to the earlier release of prison inmates, especially those convicted of relatively minor property offenses or, not surprisingly, drug offenses. The rise in drug incarceration does not, however, appear to have increased other crimes. On the margin, the reduction in violent and property crime associated with locking up a drug offender is not statistically different than for other types of criminals. In addition, increases in both the certainty and severity of drug punishment are associated with higher drug prices, and presumably, lower drug consumption.

Whether or not the enormous investment in incarcerating drug offenders has been cost-effective depends critically on the social valuation of reduced crime and drug use. For typical values of the costs of crime to victims, even the most generous estimates of the crime reduction attributable to prison (Levitt 1996) suggest that current levels of incarceration are excessive. The cost-benefit calculation for drug offenders, however, might be somewhat more favorable since their incarceration not only lowers crime, but also drug consumption. If the estimates in our paper are true, then incarceration of 400,000 drug prisoners at an annual cost of roughly $10 billion reduces cocaine consumption by perhaps 20 percent. Harwood, Fountain, and Livermore (1998) estimate that illegal drug usage in 1992 was associated with $12.1 billion in health care costs and $17.5 billion in lost productivity.[21] The extent to which a reduction in drug usage at the margin affects these social costs is not known. If the relationship between social costs and drug usage is linear, however, then expenditures on drug incarceration almost pays for itself through reductions in health care costs and lost productivity due to illegal drug use, based on our estimates, even ignoring any crime reductions. Of course, it must be stressed that this sort of

[21] The original estimates of Harwood, Fountain, and Livermore (1998) have been adjusted to year 2000 dollars using the consumer price index.

cost-benefit calculation is highly speculative and many other relevant potential costs and benefits are not included. Furthermore, it is possible that there are more cost-effective ways of reducing drug usage than incarceration (e.g. treatment, information campaigns), which are not explored in this paper.

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Data Appendix:

Data

The National Corrections Reporting Program (NCRP) provides the majority of the data for the analysis in this paper. For each year between 1983 and 1996, the NCRP supplies two separate data sets: admission data, covering those inmates who were admitted to prison that year, and release data, covering those who were released from prison that year. There is no way to reliably link individual prisoners across years. The NCRP relies on official state prison records of the 30 to 35 states that participate each year. The states participating vary year to year, though about twenty-five appear in at least ten of the fourteen years the study covers.

The NCRP admission data provides demographic information on each admitted prisoner, as well as information on the offense for which the inmate was convicted and the length of his sentence. The NCRP release data provides all of the information that the admissions data does, in addition to information on the length of time served by the defendant.

The NCRP data was supplemented by a range of state-level covariates used in Donohue and Levitt (2001). All of these variables are from easily available government sources, except per capita malt beverage consumption, which is from the annual publication The Brewers Almanac. All of these covariates are readily available from the authors.

Constructing the variables

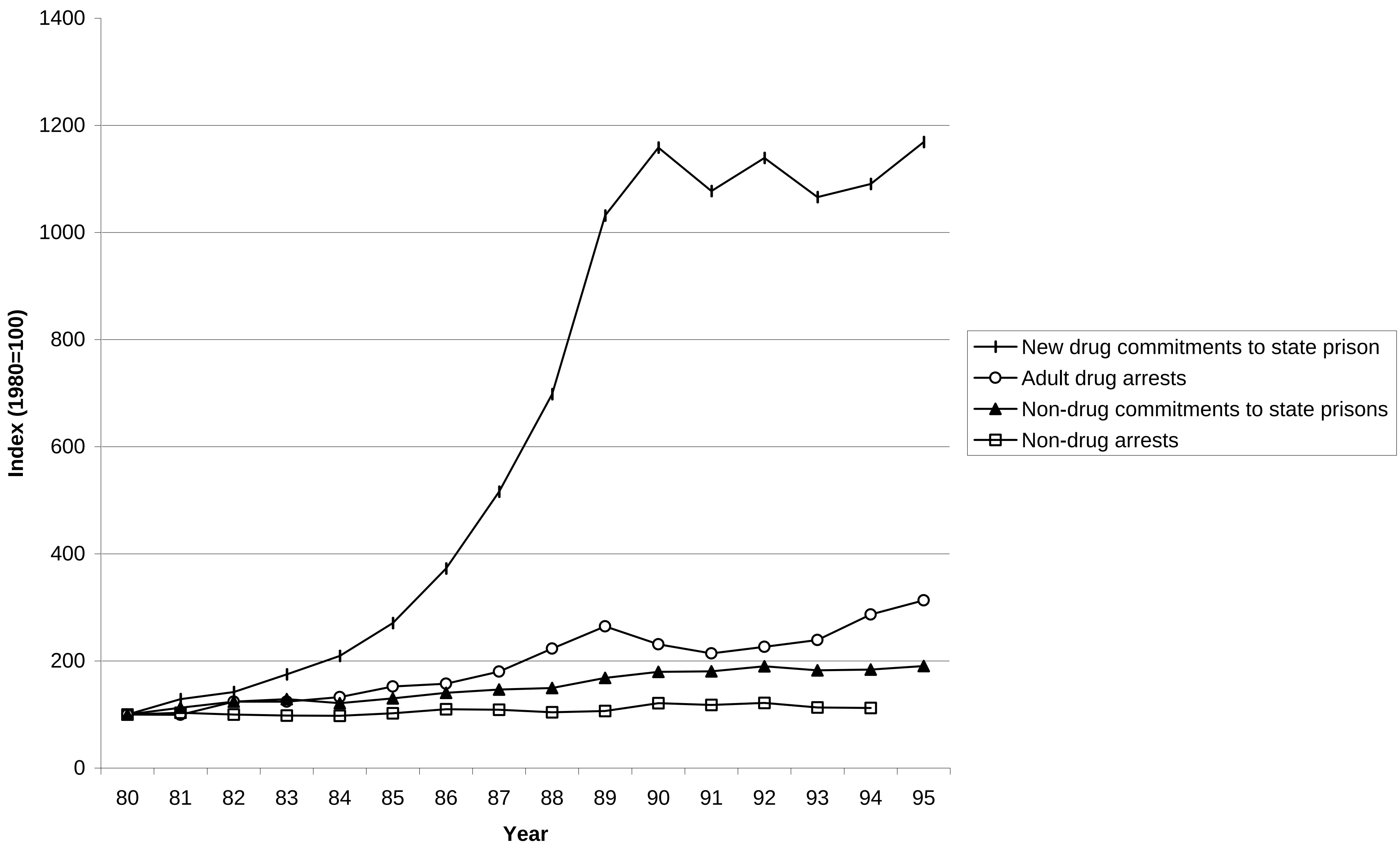
Our analysis uses variation at the state-year level while the NCRP provides data at the individual-inmate level. For each state-year, the NCRP data was used to generate the medians (and in some cases means) of 1) sentence length; 2) time served in prison; 3) percent of sentence actually served using only those inmates convicted of drug offenses. These three calculations were repeated for those inmates convicted of murder, rape, armed robbery, aggravated assault, burglary, auto theft, larceny, fraud, and driving while under the influence of alcohol. An inmate was excluded from the calculation of the summary statistics if he was admitted to prison for a parole violation (instead of directly from a court conviction), or if he/she had missing values for any of the variables required in the calculation. Additionally, the NCRP data was used to calculate the drug share of all new admissions for each state-year, as well as the share of new admissions attributable to each of the other offenses listed above.

Several decisions made in constructing the variables warrant further discussion. First, the percent-of-sentence-served variable was calculated by taking the mean (median) of the individual percent-of-time-served values, instead of dividing the mean (median) time-served in prison by the mean (median) sentence length. Second, some of the values for the sentence-length variable were changed in order to generate meaningful summary statistics. Those given life sentences were assigned a 50 year sentence for the purpose of calculating mean and median sentences (although this assumption only affects calculation of the means, which are used sparingly in our analysis). In addition, some values for the sentence-length variable were improbably large (such as 1200 months for a burglary conviction). Such values were also changed to 50 years. Third, the time-served variable records only the time served after the inmate was sentenced, and does not include the time he may have served before trial, which is credited toward his sentence. Though the NCRP does in fact provide information on the amount of time served before trial, that information is missing for many observations in the data so we do not use it.

Sampling Rules

The NCRP data quality varies from state to state and from year to year. State i in year t was included in the analysis only if it met a number of criteria. First, state i must have had at least 100 new (i.e. not parole violations) admissions in year t. Second, state i must have at least 100 new admissions in ten of the fourteen years the NCRP data covers. Third, state i must have a mean (median) value for the percent-of-time-served variable that is less than one in year t. Fifth, Hawaii and Missouri were excluded from the analysis due to very poor data quality. Finally, four state-years were eliminated because their drug-share-of-total-admissions variables were vastly incongruent with the values of the same variable in surrounding years.

Figure I: Arrests and State Prison Committments by Crime Type



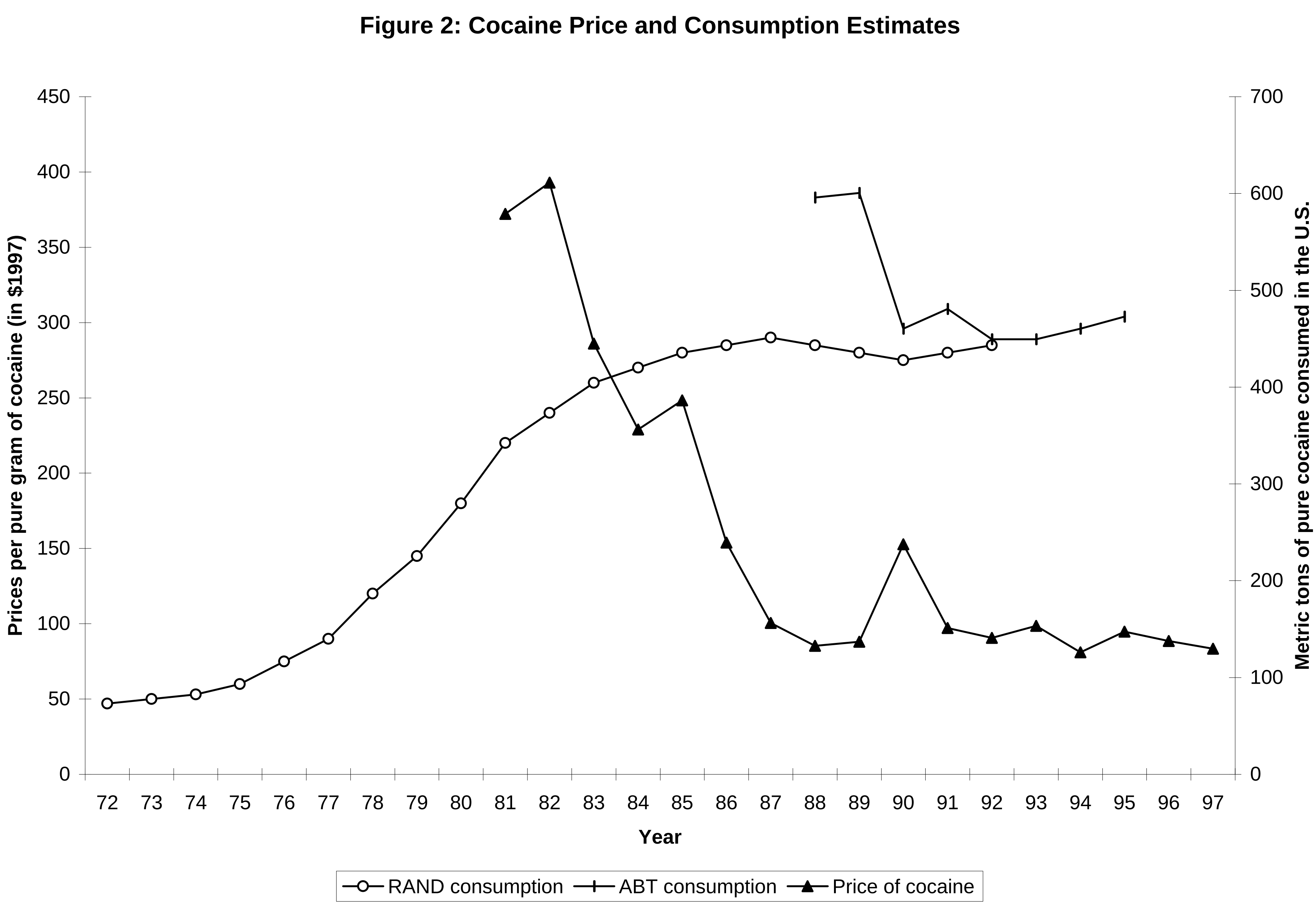


Figure 2: Cocaine Price and Consumption Estimates
Prices per pure gram of cocaine (in $1997)
Metric tons of pure cocaine consumed in the U.S.
450
400
350
300
250
200
150
100
50
0
700
600
500
400
300
200
100
0
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
Year
RAND consumption
ABT consumption
Price of cocaine

Table I: Summary statistics

Variable	Mean	Standard deviation: Overall	Removing state-fixed effects	Removing state- and year-fixed effects
Percent of new state prison admissions for:				
drug offenses	.220	.104	.062	.038
violent offenses	.225	.180	.160	.159
property offenses	.229	.143	.125	.121
other offenses	.341	.196	.166	.163
Median percent of sentence served				
robbery	.353	.140	.072	.070
auto theft	.462	.197	.104	.100
rape	.422	.151	.096	.085
burglary	.335	.141	.069	.068
aggravated assault	.372	.125	.069	.067
Fraud	.333	.150	.090	.070
Larceny	.351	.149	.090	.089
Murder	.324	.126	.093	.088
drug offenses	.307	.141	.068	.066
Unemployment rate	.060	.020	.013	.009
Black share of population	.125	.122	.004	.003
Per capita income (in 1998 dollars)	22,183	3,683	1,362	519
Per capita malt liquor purchases	23.39	4.39	1.17	1.08
Cocaine prices (in 1998 dollars per pure gram)	105.7	59.4	50.0	31.1
Per capita drug arrests	.0039	.0028	.0009	.0007
Per capita violent arrests	.0022	.0014	.0004	.0004
Per capita property arrests	.0077	.0025	.0009	.0008
Commitment rate for drug offenders	.063	.084	.076	.073
Commitment rate for violent offenders	.119	.130	.114	.113
Commitment rate for property offenders	.033	.038	.0330	.0323
Per capita violent crime	.0061	.0039	.0011	.0009
Per capita property crime	.0464	.0123	.0038	.0034
Per capita prison population	.0028	.0020	.0009	.0006

Notes: All variables correspond to state-year pairs, except cocaine prices which vary by city and year. Data cover the period 1983-1996. Data on prison admissions and time served are computed based on National Crime Reporting Program (NCRP) data. Other criminal justice variables are from Federal Bureau of Investigation Uniform Crime Reports. The remaining variables are from a variety of readily available sources and were collected by Donohue and Levitt (2001). Commitment rates are new prison commitments per arrest. Columns 3 and 4 report the standard deviation in variables after state-fixed effects and state-fixed effects and year dummies are included.

Table II: The Effect of Drug Admissions on Median Percent of Sentence Served across Offense Categories

	Median percent of maximum sentence actually served by offense type								
Variable	Murder	Rape	Robbery	Agg. assault	Fraud	Burglary	Larceny	Auto	Drug offenses
Percent of new prison admissions for drug offenses	-.226 (.143)	.018 (.131)	-.405 (.113)	-.353 (.111)	-.311 (.126)	-.657 (.117)	-.536 (.137)	-1.026 (.201)	-.601 (.109)
Unemployment rate	-.38 (.69)	-.44 (.64)	-1.08 (.56)	-1.37 (.54)	-.44 (.61)	-1.01 (.58)	-.23 (.66)	-1.73 (1.03)	-.77 (.52)
Percent Black	11.2 (1.8)	4.9 (1.7)	5.8 (1.4)	5.2 (1.4)	2.7 (1.6)	1.7 (1.5)	-0.3 (1.7)	1.0 (2.5)	8.0 (1.7)
Ln (per capita state income)	-.75 (.35)	.00 (.31)	-.14 (.28)	-.73 (.26)	-.78 (.30)	-.27 (.28)	-.55 (.33)	-.50 (.46)	-.94 (.28)
Per capita malt liquor purchases	.030 (.009)	.014 (.008)	.008 (.007)	.016 (.007)	.014 (.008)	.009 (.007)	.011 (.009)	.026 (.012)	.008 (.007)
Effective abortion rate (*100)	.023 (.025)	.016 (.021)	.038 (.016)	.003 (.018)	-.021 (.020)	.012 (.017)	-.023 (.022)	.021 (.030)	-.018 (.020)
Observations	304	307	319	315	316	320	316	196	300
Adj. R-Squared	.560	.718	.777	.734	.737	.756	.698	.807	.806
Coefficient on drug-share when other covariates are excluded	-.239 (.129)	.052 (.125)	-.352 (.110)	-.280 (.108)	-.236 (.120)	-.558 (.111)	-.430 (.129)	-.876 (.192)	-.604 (.112)
Coefficient on drug-share when median time served is the dependent variable	-75.9 (39.2)	148.1 (40.0)	-34.4 (15.84)	-31.7 (7.5)	-21.7 (3.9)	-32.5 (5.9)	-28.6 (4.4)	--57.0 (25.9)	-35.8 (4.1)

Notes: The dependent variable is the median percentage of maximum sentence that is actually served for the named offense type, among state prisoners released in the current year. If new commitments of drug offenders crowd out time served by other offenders, then the coefficients in the top row would be expected to be negative. State and year fixed effects are included in all specifications. The effective abortion rate is the weighted average of past abortion rates in a state, with weights based on the age distribution of the criminal population (see Donohue and Levitt 2001). Standard errors in parentheses. All regressions are weighted by the number of valid observations on released prisoners by state and year in the NCRP data set. The bottom row of the table reports coefficients on the drug-share of new prison admissions variable when actual time served in months is the dependent variable.

Table III: The Impact on Violent and Property Crime of Imprisoning Drug Offenders versus Those Committing Other Types of Crimes

	Dependent Variable			
Variable	Logged per capita violent crime		Logged per capita property crime	
Logged per capita prison population	-.086 (.090)	-----	-.165 (.111)	-----
Logged per capita prison population interacted with:				
Drug share of prisoners	-----	-.067 (.097)	-----	-.160 (.109)
Violent crime share of prisoners	-----	-.098 (.096)	-----	-.186 (.108)
Property crime share of prisoners	-----	-.057 (.098)	-----	-.156 (.108)
Other crime share of prisoners	-----	-.094 (.100)	-----	-.183 (.111)
Observations	388	388	388	388
Other covariates included?	Yes	Yes	Yes	Yes
R-Squared	.978	.980	.959	.960

Notes: The dependent variable is the logged per capita crime rate for the named crime category. In columns two and four, the logged per capiat prison population is interacted with the share of prisoners sentenced for different types of crimes. If the reduction in crime is the same per prisoner across the different offense categories, the coefficients in columns two and four will all be identical. In all cases, the prison variables are once-lagged to minimize endogeneity. All regressions include the full set of covariates included elsewhere in the paper, as well as state- and year-fixed effects. Standard errors in parentheses. All regressions are weighted by number of valid NCRP observations that year.

Table IV: The Effect of Certainty and Severity of Punishment on Cocaine Prices

	Coefficients (Standard errors in parentheses)		
Variable	(1)	(2)	(3)
Certainty of punishment:			
Per capita drug-offense arrests	6795 (1309)	---	8139 (1546)
Per capita violent-crime arrests	-1132 (4179)	---	7002 (3875)
Per capita property-crime arrests	-5790 (6024)	---	-10017 (6650)
Severity of Punishment:			
Commitment rate for drug-offense arrests	---	56.7 (49.8)	110.1 (63.5)
Commitment rate for violent-crime arrests	---	34.2 (18.4)	18.8 (12.5)
Commitment rate for property-crime arrests	---	-105.3 (128.4)	-172.1 (99.0)
Observations	256	245	245
R-squared	.907	.886	.904

Notes: The dependent variable, cocaine prices, are city-year averages obtained from cocaine purchases of five ounces or less made by undercover Drug Enforcement Agency officials. All regressions include controls for the full set of covariates included elsewhere in the table, as well as city and year fixed effects. Standard errors have been corrected to account for the fact that the right-hand-side variables of interest vary only at the state-level. All regressions are weighted by number of valid NCRP observations that year.