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THE EMERGENCE OF CRACK COCAINE AND THE RISE IN URBAN CRIME RATES

Jeff Grogger and Michael Willis*

Abstract—Despite widespread popular accounts that link crack cocaine to inner-city decay, little systematic research has analyzed how the emergence of crack affected urban crime. We study this question using FBI crime rates for 27 metropolitan areas and two sources of information on when crack first appeared in those cities. Using methods designed to control for unobserved differences among metropolitan areas, we find that the introduction of crack had substantial effects on crime. In the absence of crack cocaine, the 1991 peak in urban crime rates would have been approximately 10% lower, remaining below the previous peak levels of the early 1980s.

I. Introduction

CRACK cocaine, a simple derivative of powdered cocaine hydrochloride, appears to have been first developed in the early 1980s (Kozel, 1997), spreading through cities across the country over the course of the decade. By popular account, the arrival of crack cocaine led to increases in central-city crime and accelerated trends toward general urban decay. Popular and ethnographic reports link crack to gang violence, high murder rates, urban unemployment, poverty, and family disruption (Shenon, 1986; Chiles, 1986; Wolff, 1988; Johnson et al. 1990).

Despite widespread interest in the topic, however, there has been rather limited research on the link between crack and crime. Much of what is known is based on fairly small and geographically limited samples. Dembo et al. (1990), Inciardi (1990), and Inciardi and Pottieger (1994) analyze various Florida samples of people with differing degrees of involvement in crime or drugs, and Fagan and Chin (1990) study a sample of drug users from northern Manhattan. All find that crack users and dealers commit a substantial amount of crime. Inciardi (1990) further finds that people with greater involvement in the crack trade tend to commit more crime. Fagan and Chin (1990) find that people who become crack sellers likely would be violent even in the absence of crack, although, at the same time, they report that such individuals are more likely to become more violent rather than less violent after they start using drugs.

Blumstein (1995) displays time-series plots of nationwide homicide data disaggregated by age and race. He notes that juvenile homicides show sharp upturns in the mid-1980s, particularly for blacks. He speculates that this increase in youth murders is related to the crack trade, although he provides no formal test of this hypothesis. Cork (1997) attempts to formalize the link between youth homicide and crack, asking whether, on a city-by-city basis, the rise in youth murders post-dates the emergence of crack cocaine.

In this paper, we provide a systematic analysis of the link between crack and crime, and we estimate specifically how the emergence of crack affected crime rates in urban areas. To pose our research question in terms of a counterfactual, we seek to estimate what the urban crime rate would have been if crack had not appeared during the mid-1980s.

For the period 1979 to 1991, we relate FBI reported crime rates from 27 metropolitan areas to information on the year in which crack arrived in each of those areas. Our arrival dates are drawn from two independent sources, one of which is a survey of police chiefs. In 1991, we mailed out a survey in which we asked a number of questions about how crack cocaine had affected each respondent's city. One question asked when the police department first became aware of the existence of crack.

Our second source is drawn from data on emergency-room admissions collected by National Institute on Drug Abuse (NIDA). In the NIDA data, hospitals report the type of drug present in each drug-related emergency room incident. They also report the manner in which the drug was administered. Because cocaine was rarely smoked prior to the introduction of crack (Chitwood, 1985; Siegel, 1985), we interpret a sharp increase in the fraction of cocaine-related admissions involving smoked cocaine as evidence of the arrival of crack.

Using the arrival dates from these two sources, we seek to determine how urban crime rates rose after the emergence of crack. We employ a difference-in-differences strategy that accounts for many of the unobservables that may be confounded with the spread of crack across different metropolitan areas (MAs). Our approach relies on the simple observation that crack markets are largely limited to the central city (Fagan & Chin, 1990; Johnson et al., 1990). Thus, we estimate the effect of crack's arrival on urban crime within MAs, implicitly comparing changes in central-city crime rates to changes in suburban crime rates in the same metropolitan area. In effect, our MA-fixed effects

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estimator uses the suburbs in a given area as a control for the central city.

This approach controls for two potentially important sources of omitted variables bias. First, it controls for economy-wide trends in factors that may be correlated with both the emergence of crack and the level of crime. For example, the average wage paid to low-skill workers fell during our sample period. Because low-skill workers' participation in crime is responsive to wages (Grogger, 1998), failing to control for such trends could cause us to exaggerate the effects of the arrival of crack.

The MA-fixed effects approach also controls for another type of omitted variable bias: If MAs differ in their susceptibility to crack, then crack may not have spread randomly across the country, but rather may have arrived first in more-susceptible areas. For example, MAs may differ in their climate of law enforcement, their local labor markets, or the structure of their existing illegal drug markets. If susceptibility to crack (which is likely to be determined by factors that are unobservable to the analyst), is correlated with local crime rates, then estimators that fail to account for it may be biased. However, if the factors that determine susceptibility do not vary over our sample period, then the MA-fixed effects estimator controls for them and solves the potential omitted-variables problem.

In section II, we provide a conceptual framework to motivate the link between crack and crime. We then describe our data in section III, discussing how we estimate the dates at which crack arrived in each of the cities in our sample. Section IV formalizes our analytical approach. The results are presented in section V, along with some tests for the adequacy of the MA-fixed effects estimator. Section VI concludes.

II. A Conceptual Framework

In this section, we provide some conceptual underpinnings for the hypothesized link between crack cocaine and urban crime. To do this, we first characterize crack as a technological innovation in the market for cocaine intoxication. We then briefly discuss the role played by crime in illegal markets. The results of this discussion are some predictions about the effect of the arrival of crack on urban crime.

Although crack is simple to manufacture, it has important effects on how the user ingests the active cocaine alkaloid and how the user experiences cocaine intoxication. Crack cocaine, a derivative of powdered cocaine, is made by dissolving cocaine powder in water, adding baking soda, and boiling the mixture until a solid base separates from the solution. This process does not change the chemical composition of the active cocaine alkaloid, but it does change the manner by which it may be ingested. Once converted into crack, the cocaine can be smoked, which allows the cocaine molecules to concentrate in the brain much more rapidly than is possible by taking powdered cocaine intranasally (U.S. Sentencing Commission, 1995). Because the euphoric effects of cocaine have more to do with the speed at which the alkaloid concentrates in the brain than with the level of the drug in the bloodstream (Schuster, 1986; Jones, 1985), crack is the more intoxicating form of the drug. Because crack causes greater cocaine intoxication than powder on a molecule-for-molecule basis, the process of synthesizing crack from powdered cocaine reduces the unit cost of cocaine intoxication, shifting the supply curve to the right and expanding the market.

An expansion of the market for cocaine intoxication, in turn, may lead to an increase in violence. Because the cocaine trade is illegal, crack dealers lack the property rights enjoyed by participants in conventional markets. As a result, explicit contracts are impossible to make in such markets, and implicit contracts are impossible to enforce. Disputes with suppliers, employees, and other "firms" cannot be taken to the courts as can be done in legal markets. Nor can a dealer's market share be expanded by buying out his competition, because the target of the buyout could renege after taking payment. As noted by many researchers, violence—or even the threat of violence—may be an effective if costly means of enforcing agreements, expanding one's operations, and protecting one's profits in an environment in which legal protections are unavailable (Goldstein, 1985; Fagan & Chin, 1990; Johnson et al., 1990). Thus, as the market for cocaine intoxication expands, we would expect violent crime to rise.

Property crime may rise as well. First, property crime may be complementary to drug dealing as a means of earning an (illegal) income. Levitt and Venkatesh (1998) show that, although workers in the illegal drug trade may do better than they would in the job market, they nevertheless are not particularly well compensated. Conventional property crime may give them a means of earning extra income that, at the same time, provides them with the flexibility they need to pursue the drug trade. If so, then an expansion of the drug market, which presumably increases the number of drug dealers, in turn could lead to increases in property crime.

Furthermore, the way crack is marketed may have increased the demand for cocaine. Pieces of crack that are large enough for one euphoric experience are sometimes sold for as little as $10. In contrast, powdered cocaine is conventionally sold in larger quantities with unit prices that are many times higher (U.S. Sentencing Commission, 1995). If small, low-priced units of crack draw into the cocaine market customers who were previously shut out by high unit prices, then the synthesis of crack may have stimulated demand. If cocaine consumers commit property crimes to support their habits, then the arrival of crack could lead to an increase in property crime.

III. Data

To estimate the effect of the arrival of crack using the difference-in-differences approach discussed above, we require two types of data: data on the date that crack cocaine appeared in each urban area, and data on the crime rate for

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TABLE 1.—CRACK INTRODUCTION DATES

Metropolitan Area	Police Survey (1)	DAWN Data (2)	Period of Uncertainty (3)
Atlanta, GA	1981	1984	1981–1983
Baltimore, MD	1988	1986	1986–1987
Boston, MA	n/a	1986	—
Buffalo, NY	1986	1987	1986
Chicago, IL	1988	1984	1984–1987
Cincinnati, OH	1988	n/a	—
Cleveland, OH	1988	1983	1983–1987
Columbus, OH	1986	n/a	—
Dallas/Fort Worth, TX	1986	1987	1986
Denver, CO	1986	1986	—
Detroit, MI	1986	1984	1984–1985
Indianapolis, IN	1988	1986	1986–1987
Kansas City, MO	1982	1983	1982
Los Angeles, CA	1984	1984	—
Miami, FL	n/a	1984	—
Milwaukee, WI	1991	n/a	—
New Orleans, LA	1986	1987	1986
New York, NY	1985	1986	1985
Newark, NJ	1985	1985	—
Norfolk, VA	1988	1985	1985–1987
Philadelphia, PA	1985	1985	—
Pittsburgh, PA	1987	n/a	—
Saint Louis, MO	1986	1987	1986
San Diego, CA	1984	1984	—
San Francisco, CA	1985	1985	—
Tampa, FL	1985	n/a	—
Washington, D.C.	1986	1986	—

both the central-city and suburban parts of each of a number of metropolitan areas. We have assembled this information for 27 metropolitan areas for the period 1979 to 1991. These MAs, listed in table 1, were chosen because, within them, demographic data from the Current Population Survey (CPS) can be disaggregated by central-city and suburban areas. We use demographic control variables constructed from these data in some of the analyses discussed below.

The final year of the sample period is the year in which the U.S. crime rate reached its peak. Because our objective is to explain how crack contributed to rising crime rates, we end the sample period in that year.[1] The starting date was chosen so that the sample period would extend for roughly equal time periods before and after the mid-1980s, when crack was rapidly emerging in major cities across the country.

A. *Crack Cocaine Arrival Dates*

The dates used to determine the arrival of crack cocaine in a metropolitan area were measured in two ways: from police data and from emergency-room admissions data.

Police data: In 1991, we surveyed various police departments about the effect that crack had on their cities. The surveys were addressed to each city's chief of police and included a question about the date when they had first encountered crack. Out of 27 surveys mailed out, we received responses from 25 police departments. A few departments provided responses to both the original survey and to a follow-up questionnaire. In one such case, we were given two different dates; fortunately, our results are largely the same regardless of which is used in the analysis. Arrival dates from our police survey are listed in the first column of table 1. The reported introduction dates range from 1981 in Atlanta to 1991 in Milwaukee. The modal introduction year is 1986.

Emergency-room admissions data: We also inferred arrival dates from the data on hospital emergency-room (ER) admissions that are published by NIDA as part of the Drug Awareness Warning Network (DAWN). DAWN reports the number of drug-related ER admissions in a given metropolitan area by the type of drug involved in the event. Among cocaine-related ER events, it also reports how the drug had been administered. This detail is informative because powdered cocaine cannot be smoked, and free-base cocaine concocted with volatile solvents—the only other form of the drug that can be smoked—is relatively rare (Chitwood, 1985; Siegel, 1985). Thus, during the early 1980s, the fraction of cocaine-related ER incidents attributable to cocaine smoking averaged approximately 2% of the total. Beginning in the mid-1980s, this fraction rose substantially, in some MAs within a single year. When the fraction of ER incidents attributable to cocaine smoking jumped from one year to the next, we took that as evidence of the arrival of crack.

In many cases, the year in which the jump occurred was obvious. Other cases, however, were more ambiguous. Because we were already familiar with the arrival dates reported in the police surveys, we adopted an arm's-length approach to the DAWN data by asking the members of a class of seventeen first-year PhD students to inspect time-series plots of the ER data and to date the year in which the jump occurred. We use their modal response as the arrival date. As shown in the second column of table 1, arrival dates based on ER admissions range from 1983 to 1987. Like the data from the police survey, this measure also yields a modal arrival date of 1986.

Data from Congressional testimony: For a few of the largest cities, additional information is available from Congressional hearings. The testimony of police officials places the arrival dates at 1985 for New York, late 1985 or early 1986 for Detroit, 1986 for Dallas, and 1988 for Chicago (Holliday, 1986; Gilliam, 1986; Vines, 1990; Ramsey, 1990). This testimony accords closely with the information from the police survey.

Dealing with the discrepancies between arrival dates: In seven of the twenty cases for which data are available from both sources, the police survey and DAWN provide the same arrival date. In six other cases, the discrepancy is only one year; in the remaining cases, the discrepancy ranges from two to five years. Where the dates differ, it is clear that at least one is measured with error. Nevertheless, it is difficult

[1] Grogger (1999) discusses the decline in crime since 1991.

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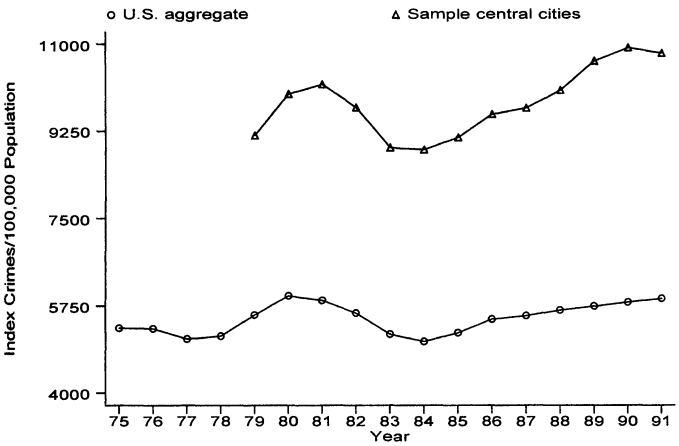
FIGURE 1.—CRIME RATE FOR U.S. AND SAMPLE MAS



to rationalize choosing either of the arrival dates as superior to the other. In cities with existing drug problems, police may have had early information on the arrival of a new drug. In cities in which police attention was focused elsewhere, crack may have been observed first in hospital emergency rooms. Therefore, we combine information from both sources in constructing the variables to be used in our regression models.

Our approach is to disaggregate the 1979–1991 time period for each MA into as many as three time segments. We label the period (strictly) prior to the earlier of the two arrival dates as *before crack* and the period (weakly) after the latter arrival date as *after crack.* When the two sources disagree, we label the intervening period as *uncertain.* All metropolitan areas have before-crack and after-crack segments, and the thirteen metropolitan areas with differing reports have the intermediate uncertain segment as well. Seven metropolitan areas have estimates from only one of the sources, and therefore have no period of uncertainty. The third column of table 1 displays the period of uncertainty for each MA.

B. *Crime Rates*

The number of reported offenses for the "index" crime categories—murder, rape, robbery, aggravated assault, burglary, larceny theft, and auto theft—are published for U.S. MAs by the FBI in its annual *Uniform Crime Reports,* subdivided by central-cities and suburbs. We analyze annual crime rates, coded as crimes per 100,000 residents.

Figure 1 displays a time-series plot of the average crime rate from the central cities of our 27 sample MAs. Also plotted is the nationwide crime rate from 1975 to 1991. Because urban areas have more crime (Glaeser & Sacerdote, 1999), the crime rate for the sample cities is higher than that for the United States as a whole. Aside from the level differences, however, both series exhibit similar trends, peaking first in the early 1980s, falling for a few years, then rising again for the duration of the sample period.

TABLE 2.—DESCRIPTIVE STATISTICS

	Observations	Mean
Violent Crime Rates		
Murder		
Central-city	347	25.89
		(14.18)
Suburbs	347	5.18
		(3.89)
Rape		
Central-city	334	93.42
		(38.97)
Suburbs	334	24.65
		(11.20)
Aggravated Assault		
Central-city	347	747.60
		(423.20)
Suburbs	347	263.57
		(157.63)
Robbery		
Central-city	347	846.04
		(424.61)
Suburbs	347	149.96
		(124.75)
Property Crime Rates		
Burglary		
Central-city	347	2347.69
		(810.68)
Suburbs	347	1148.01
		(486.17)
Larceny Theft		
Central-city	347	4543.80
		(1400.92)
Suburbs	347	2861.24
		(858.10)
Auto Theft		
Central-city	347	1439.86
		(875.79)
Suburbs	347	468.26
		(265.11)

Notes: There are 347 observations because crime rates were not published for Buffalo in 1981, Miami in 1988, or Tampa in 1988 and 1990. Additionally, rape figures were not published for Chicago after 1985. We exclude Chicago in the rape regressions, thus leaving 334 observations. Standard errors are reported in parentheses.

Descriptive statistics by crime category are presented in table 2.[2] Again, central cities appear to be more crime-ridden than suburbs. For instance, the mean murder rate in central cities is five times greater than that in the suburbs, the mean rape rate is nearly four times greater in central cities than in the suburbs, the mean robbery rate in the central cities is more than five times greater than that in the suburbs, and the mean aggravated assault rate is approximately three times greater in the central cities. The disparity between mean central-city and suburban property crimes rates (burglary, larceny theft, and auto theft) is less dramatic, although the central-city and suburban crime rates still differ by a factor of two or three.

[2] With 27 MAs and a thirteen-year sample period, one would expect a sample size of 351, but the actual sample sizes are lower due to missing data. FBI data are unavailable for Buffalo in 1981, Miami in 1988, and Tampa in 1988 and 1990. Rape data for Chicago were not published after 1985. For this reason, we exclude Chicago from the rape tabulations altogether.

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TABLE 3.—MEAN CRIME RATES FOR CENTRAL CITIES AND SUBURBS, BEFORE AND AFTER THE INTRODUCTION OF CRACK

	Murder			Rape			Robbery			Aggravated Assault		
	Central-city (1)	Suburbs (2)	Difference (1)−(2)	Central-city (1)	Suburbs (2)	Difference (1)−(2)	Central-city (1)	Suburbs (2)	Difference (1)−(2)	Central-city (1)	Suburbs (2)	Difference (1)−(2)
Before (B)	23.5	5.1	18.4	87.8	23.3	64.5	792.23	139.9	652.3	569.1	236.1	333.0
	(0.8)	(0.8)	(1.1)	(2.2)	(2.2)	(3.2)	(24.4)	(24.4)	(34.5)	(22.8)	(22.8)	(32.3)
After (A)	28.3	5.4	22.9	95.4	26.4	68.9	914.0	169.8	744.2	948.3	307.2	641.1
	(0.9)	(0.85)	(1.2)	(2.4)	(2.4)	(3.4)	(25.6)	(25.6)	(36.2)	(24.0)	(24.0)	(33.9)
Change (A)−(B)	4.7	0.3	4.4	7.6	3.1	4.5	121.8	29.9	91.9	379.9	71.1	308.1
	(1.2)	(1.2)	(1.7)	(3.3)	(3.3)	(4.6)	(35.4)	(35.4)	(50.0)	(33.1)	(33.1)	(46.1)

	Burglary			Larceny Theft			Auto Theft		
	Central-city (1)	Suburbs (2)	Difference (1)−(2)	Central-city (1)	Suburbs (2)	Difference (1)−(2)	Central-city (1)	Suburbs (2)	Difference (1)−(2)
Before (B)	2401.4	1233.3	1168.1	4373.7	2868.7	1505.0	1136.5	393.7	742.8
	(51.6)	(51.6)	(73.0)	(89.3)	(89.3)	(126.2)	(46.9)	(46.9)	(66.3)
After (A)	2287.6	1078.13	1209.5	4808.7	2895.8	1912.9	1783.6	563.6	1216.3
	(54.3)	(54.3)	(76.8)	(93.8)	(93.8)	(132.7)	(49.4)	(49.4)	(69.8)
Change (A)−(B)	−113.8	−155.2	41.4	435.0	27.1	407.9	647.2	173.6	473.6
	(74.9)	(74.9)	(106.0)	(129.5)	(129.5)	(183.2)	(68.1)	(68.1)	(96.2)

Table 3 presents the crime data in a manner that sheds some light on the effects of the arrival of crack cocaine. For each of the seven index crime rates, we average the data separately for central cities and suburbs, each before and after the arrival of crack. Within each of the seven panels of table 3, central-city data are presented in the first column and suburban data are presented in the second. The third column gives the difference in crime rates between the central cities and the suburbs. The first row of the table presents data from the before-crack period, and the second presents the after-crack data. The third row reports the mean change before and after the arrival of crack. The entry in the third column of the third row of each panel gives the difference between the cities and the suburbs in the before-and-after change in crime rates. Under our maintained assumption that crack had no effect on suburban crime rates, this is the so-called difference-in-difference estimator (Ashenfelter, 1978) of the effect of the arrival of crack cocaine on central-city crime rates.

For most of the index crimes, these estimates suggest that the arrival of crack had sizeable and significant positive effects. The difference-in-difference estimate for murder indicates that the arrival of crack caused murder rates to rise by 4.4 per 100,000 population, an amount equal to 18.7% of the before-crack murder rate in the central cities. The only statistically insignificant effects are for rape and burglary, although the robbery effect has a t-statistic of only 1.83.

From table 3, one can also see how valuable it is to use the suburbs as controls for the central cities. The simple before-and-after estimate of the effect of the arrival of crack is given by the mean change over time in central-city crime rates, reported in the third row of the first column in each panel. With the exception of burglary (which actually fell over the sample period in both the central cities and suburbs), the simple before-and-after estimates are larger than the difference-in-difference estimates. For some crimes, such as murder and larceny theft, the difference is small. For others, however, the difference between the two estimators is sizeable. The before-and-after estimate of the effect of crack on aggravated assault is 379.9; the difference-in-difference estimate is 308.1 (19% smaller). For auto thefts, the before-and-after estimate is 647.2, whereas the difference-in-difference estimate is 473.6 (30% smaller). For these two crimes, failing to account for economy-wide trends—as summarized by the changes in suburban crime rates—would lead to substantially larger estimates of the effects of the arrival of crack.

As instructive as these simple difference-in-differences estimates may be, however, they nevertheless are restrictive in some potentially important ways. First, they implicitly impose the assumption that the true model is linear in levels, which in turn implies that the arrival of crack had additive effects on urban crime rates. If, instead, it had proportionate effects, than a log-linear specification would be appropriate. The distinction may be particularly important because the crime rates vary so much between the central city "treatment" areas and the suburban "control" areas. It would be desirable to test between these two alternatives and to allow for a more general specification if need be.

It also would be desirable to test whether the MA-fixed effects in the model provide adequate controls for omitted-variable bias. Although the conventional difference-in-differences estimator allows us to control only for time-invariant unobservables that vary between metropolitan areas, the structure of our data allows us to generalize the model somewhat and to test for unobservables that vary within MAs over time as well. Moving to a regression framework also allows us to include covariates in the model,

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which provides another, admittedly limited, way to test for omitted variable bias.

IV. Estimation

By using the suburbs as controls for the central city within the same MA, we provide an estimator that accounts for economy-wide trends in crime that are independent of the arrival of crack, and time-invariant, MA-specific unobservables that capture differences in susceptibility to crack between metropolitan areas. Our regression model can be written as

$$Y_{ijt} = A_{ijt}\gamma + U_{ijt}\beta + \alpha_c + \mu_i + \delta_t + \epsilon_{ijt}$$
$$i = 1, \ldots, N; j = c, s; t = 1, \ldots, T. \quad (1)$$

The dependent variable Y_{ijt} denotes the crime rate in the j_{th} component of MA i in year t. Each MA has two components: a central city ($j = c$) and a suburban component ($j = s$). The variable A_{ijt} is the dummy variable that captures the arrival of crack cocaine. For the central city of the i_{th} MA, $A_{ict} = 1$ during the after-crack period (that is, during and after the latter of the two arrival dates given in table 1). $A_{ict} = 0$ in all previous years. The variable U_{ijt} captures the period of uncertainty. For the central city of the i_{th} MA, $U_{ict} = 1$ during the period of uncertainty given in the third column of table 1, and $U_{ict} = 0$ in all other years. For cities with no period of uncertainty, $U_{ict} = 0$ in all years, and $A_{ijt} = 1$ for all years including and after the (single) arrival date.[3] For the suburban observations from each MA, $A_{ist} = U_{ist} = 0$, reflecting the identifying assumption that the effects of the arrival of crack are limited to the central cities.

The α_c term is a central-city effect that captures the substantial difference between central-city and suburban crime rates. The μ_i terms are MA-fixed effects that are common to both the urban and suburban areas in the MA. These terms account for time-invariant determinants of crime that vary among the MAs in our sample. If μ_i were correlated with A_{ijt} in equation (1), then estimators that failed to include the MA-fixed effects would yield inconsistent estimates of the effect of the arrival of crack.

The δ_t terms are economy-wide period effects and account for trends that affect all areas of the country similarly, such as the decline in the mean wages of low-skill workers. Because crack spread across the country in the latter part of our sample period, a model that failed to account for such economy-wide trends would confound those trends with the effects of crack's arrival.

The terms γ and β are parameters to be estimated. In particular, γ gives the effect of the arrival of crack cocaine on urban crime rates. The ϵ_{ijt} terms are idiosyncratic error terms that vary by MA, year, and between the central city and suburb of each MA. The sample has N MAs and T time periods.[4]

Equation (1) can be estimated by regressing the dependent variable on the after-crack variable A_{ijt}, the period-of-uncertainty variable U_{ijt}, a central-city dummy, MA dummies, and year dummies. This regression will yield consistent estimates of the effects of the arrival of crack provided three conditions are satisfied:

1. The functional form of the model must be correctly specified.
2. The disturbance term ϵ_{ijt} must be uncorrelated with A_{ijt}.
3. The assumption that the effects of crack were limited to the central cities must be satisfied.

Because we have no strong priors regarding the correct functional form of the model, we present both linear and log-linear specifications. We also provide estimates from the Box-Cox transformation, where the dependent variable is given by

$$Y_{ijt}^{(\lambda)} = \frac{Y_{ijt}^{\lambda} - 1}{\gamma}.$$

The parameter λ is estimated by a grid search. As is well known, $\lambda = 1$ corresponds to a linear specification, whereas $\lambda = 0$ corresponds to a log-linear specification. Tests about λ thus aid in selecting the proper functional form.

The second assumption needed to obtain consistent estimates is that A_{ijt} be uncorrelated with ϵ_{ijt}. This assumption could be violated if the factors that determine an MA's susceptibility to crack are not time-invariant but rather change over time. If, for example, state-level judicial rulings or local labor-market shocks both affect an MA's susceptibility and are correlated with its crime rate, then the MA-fixed effects approach could yield inconsistent estimate of the effects of crack's arrival. We provide two types of tests for the importance of such time-varying unobservables, one based on a generalization of the MA-fixed effects model and one based on the inclusion of a limited set of time-varying covariates.

The final assumption required for the estimates to be consistent is that the effects of crack are confined to the central cities. If, instead, crack markets spill over into the suburbs, then the suburbs would constitute a contaminated comparison group, because they would also be affected by the arrival of crack. This contamination would bias the regression estimates toward zero, leading us to understate the effect of the arrival of crack.

[3] For example, in Atlanta, $U_{ict} = 1$ in 1981, 1982, and 1983, and 0 in all other years; $A_{ict} = 1$ in all years from 1984 to 1991 inclusive, and 0 in all other years.

[4] Strictly speaking, the missing-data problems discussed in footnote 2 indicate that we have an unbalanced rather than balanced panel. Treating the unbalanced nature of the data explicitly would only complicate the notation, however, while adding no new insights.

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TABLE 4.—REGRESSION ESTIMATES OF THE EFFECT OF THE INTRODUCTION OF CRACK ON URBAN CRIME RATES

Crime: Variable	Murder (1)	Rape (2)	Robbery (3)	Aggravated Assault (4)	Burglary (5)	Larceny Theft (6)	Auto Theft (7)
			Panel A: Linear Specification				
After	3.5	2.4	67.7	257.3	9.9	328.2	385.8
	(1.1)	(3.3)	(30.7)	(29.6)	(59.8)	(93.9)	(62.4)
Uncertain	2.4	14.5	25.5	142.5	156.2	−65.0	250.2
	(1.7)	(5.2)	(48.2)	(46.6)	(93.9)	(147.6)	(98.0)
R^2	0.75	0.77	0.81	0.76	0.80	0.81	0.74
			Panel B: Log Specification				
After	0.068	−0.117	−0.024	0.189	0.076	0.073	0.140
	(0.054)	(0.048)	(0.047)	(0.046)	(0.032)	(0.023)	(0.041)
Uncertain	0.122	0.199	0.104	0.285	0.143	−0.004	0.114
	(0.085)	(0.077)	(0.074)	(0.073)	(0.050)	(0.036)	(0.065)
R^2	0.881	0.847	0.916	0.836	0.837	0.843	0.873
			Panel C: Box-Cox Specification				
After	0.114	−0.139	−0.002	0.210	0.221	0.178	0.048
	(0.068)	(0.066)	(0.081)	(0.050)	(0.111)	(0.055)	(0.019)
	[0.090]	[−0.102]	[−0.001]	[0.194]	[0.063]	[0.074]	[0.105]
Uncertain	0.173	0.283	0.182	0.308	0.471	−0.014	0.042
	(0.107)	(0.105)	(0.128)	(0.079)	(0.175)	(0.086)	(0.030)
	[0.136]	[0.208]	[0.105]	[0.285]	[0.134]	[−0.006]	[0.093]
λ	0.109	0.082	0.097	0.013	0.172	0.108	−0.118
$\lambda = 0$	28.9	7.5	22.9	0.3	15.0	4.7	15.1
$\lambda = 1$	1056.9	612.2	1097.1	715.9	294.4	249.5	1006.4

Notes: In addition to the variables shown, all models include a central-city dummy and a full set of year dummies. Standard errors are in parentheses. Figures in square brackets are mean logarithmic derivatives. Sample sizes are 668 for rape, 694 for all others. See note for table 2.

V. Results

A. Basic Estimates

Panel A of table 4 presents estimates from a linear-regression specification that includes the after-crack variable, the period-of-uncertainty variable, a central-city dummy, MA dummies, and year dummies as regressors. As one would expect, the estimates are similar to the simple difference-in-difference estimates shown in table 3.[5] All estimates of the effect of crack's arrival are positive, although the coefficient for rape is insignificant. Some of the estimates are fairly sizeable as well. The estimated effect of the arrival of crack on aggravated assault amounts to 45% of the pre-crack central-city mean from table 3.

In all cases but one, the period-of-uncertainty coefficients are positive as well. This suggests that, at least on average, the earlier arrival date indeed signals the emergence of crack, triggering an increase in crime. The one negative coefficient, in the model for larceny theft, is both numerically small and statistically insignificant.

Results from the log-linear specification are presented in panel B of table 4. The only difference between the models reported in panel B and those reported in panel A is the functional form of the dependent variable. Whereas the dependent variables for panel A were the various crime rates, the dependent variables for panel B are the logarithms of the various crime rates. Although we note many similarities in the results between the two specifications, we find some important differences as well.

The estimated effect of the arrival of crack on the murder rate remains positive, but is now insignificant. The rape and robbery coefficients are negative rather than positive, and significant in the case of rape. However, we are reluctant to conclude that the arrival of crack reduced rape and robbery. In both cases, the period-of-uncertainty coefficient is positive and sizeable. It is significant in the model for rape. To the extent that the period-of-uncertainty coefficient may reflect an effect that should be attributed to the arrival of crack, the conclusion that crack reduced these two types of crimes would be ill considered.

Aggravated assault is the most prevalent form of violent crime, and the estimates indicate that the arrival of crack increased aggravated assault by approximately 19%. The coefficient is significant as well, with a t-statistic of 4.1. Although this effect is only about half the proportionate effect implied by the estimates from the linear model, it is nevertheless quite substantial.

The log-linear specification reveals the effect of crack's arrival on burglary to be larger than did the linear specification. Whereas the linear estimates showed crack to have a miniscule effect on burglary, the log-linear estimates reveal a significant effect of roughly 7.6%. The effect of the arrival of crack on larceny is similar, at about 7.3%, whereas its effect on auto theft appears larger, at about 14%.

Because the functional form of the model affects the results, and since we have no a priori basis for selecting one functional form over the other, it is particularly helpful to

[5] Indeed, they differ only because the regression model incorporates more-general period effects than the difference-in-difference table.

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have the Box-Cox estimates to aid in discriminating between them. (The Box-Cox estimates are shown in panel C of table 4.) In addition to presenting the regression coefficient and its standard error, we also report in brackets the mean logarithmic derivative of the dependent variable with respect to the after-crack variable and the period-of-uncertainty variable. The logarithmic derivative is directly comparable to the regression coefficients from the log-linear models and so aids in comparing the results across the two sets of specifications. The logarithmic derivative is a function of the regression coefficients and the transformation parameter λ, which is also reported in the table. Because λ is estimated by a line search, there is no standard error to report. Rather, we report likelihood-ratio statistics corresponding to tests for log-linearity ($\lambda = 0$) and linearity ($\lambda = 1$). Both test statistics are asymptotically distributed as chi-square with one degree of freedom under the null.

The tests for linearity are soundly rejected. With the exception of the aggravated-assault regression, the tests for log-linearity reject as well. Despite the formal rejection of log-linearity, however, the Box-Cox estimates yield mean derivatives that are generally similar to the log-linear regression coefficients. The murder effect from the Box-Cox specification is a bit larger and a bit closer to statistical significance than is the corresponding log-linear estimate, whereas the rape and robbery coefficients are slightly less negative. In general, however, differences between the two specifications are small. The estimates from the Box-Cox specifications indicate that the arrival of crack led to a 9% increase in murder, a 19% increase in aggravated assault, a 6.3% increase in burglary, a 7.4% increase in larceny, and a 10.5% increase in auto theft.

B. *Implications for Crime Trends*

These estimates can be used to answer the counterfactual posed in the introduction: How would crime trends have changed during the late 1980s and early 1990s if crack had never appeared? It would be desirable to use the Box-Cox estimates for this purpose, because the other models are rejected by the likelihood-ratio tests. What we require, however, are predictions of the expectation of the crime rate, and, whereas such predictions are easy to construct based on the log-linear model (under a normality assumption on error term in the regression), they are at best difficult to construct from the Box-Cox transformation. Therefore, we use the log-linear specifications reported in panel B of table 4. Because these estimates are similar to those from the Box-Cox specifications, we suspect that they are adequate for our purpose.

To answer the counterfactual, we form predictions from our regression models in which we set the value of the after-crack and period-of-uncertainty variables to zero for all observations. To eliminate sample composition effects, we restrict attention to the 23 MAs for which we have crime data for every year. This exercise suggests that the emergence of crack had substantial effects on urban crime rates. For example, the 1991 crime rate in the central cities of our sample MAs was 10,819 per 100,000 residents. In the absence of crack, our models predict that the crime rate would have been only 9,694 (10% lower).

It is instructive to compare the 1991 peak in the crime rate (10,819) with the previous peak (10,192), which occurred in our sample MAs in 1981.[6] Our models predict that, in the absence of crack, the 1991 peak would have been approximately 5% lower than the 1981 crime rate, rather than nearly 6% higher. In other words, crack was the reason that crime rose above its previous high during the late 1980s and early 1990s.

C. *Some Tests for Omitted-Variables Bias*

Of course, these simulations and the estimates from which they are derived are based on some assumptions. One of these assumptions is that, conditional on an MA-fixed effect, the error term in the regression model is uncorrelated with the date that crack first emerged in the MA. Although the MA-fixed effects approach controls for unobservable time-invariant characteristics of MAs that may affect their susceptibility to crack and hence the arrival date of crack, the approach provides no controls for unobservable determinants of susceptibility that vary over time. Therefore, if time-varying factors such as local labor-market shocks or state-level judicial rulings affect the MAs susceptibility, then our estimates may be inconsistent.

In this section, we present results from two approaches to deal with this potential omitted-variable bias. In the first, we generalize the regression model to allow for MA effects that vary over time. In the second, we add to the MA-fixed effects model some additional regressors that vary both over time and between the urban and suburban components of each MA.

MA-by-year-fixed effects: Our first approach for dealing with potentially omitted time-varying factors is to generalize the error structure of the regression model. We fit models of the form

$$Y_{ijt} = A_{ijt}\gamma + U_{ijt}\beta + \alpha_c + \mu_{it} + \epsilon_{ijt}$$
$$i = 1, \ldots, N; j = c, s; t = 1, \ldots, T. \qquad (2)$$

Equation (2) generalizes equation (1) by adding time-varying MA effects, or MA-by-year-effects, denoted by μ_{it}. Our data structure allows this generalization because each treatment observation is paired with a corresponding control observation; that is, each MA has both an urban observation and a suburban observation in each time period. Thus, it is possible to remove a common, MA-specific period effect and still estimate the effect of the arrival of crack. Moreover, we can use a likelihood-ratio test to test the fit of the

[6] The national crime rate peaked one year earlier, in 1980.

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TABLE 5.—FURTHER BOX-COX REGRESSION ESTIMATES OF THE EFFECT OF THE INTRODUCTION OF CRACK ON URBAN CRIME RATES

Crime: Variable	Murder (1)	Rape (2)	Robbery (3)	Aggravated Assault (4)	Burglary (5)	Larceny Theft (6)	Auto Theft (7)
			Panel A: Models with MA-by-year-fixed effects				
After	0.149	−0.129	0.033	0.232	0.480	0.584	0.076
	(0.090)	(0.096)	(0.115)	(0.064)	(0.234)	(0.188)	(0.026)
	[0.117]	[−0.087]	[0.017]	[0.228]	[0.082]	[0.092]	[0.142]
Uncertain	0.349	0.564	0.441	0.493	0.936	0.071	0.070
	(0.166)	(0.182)	(0.212)	(0.118)	(0.434)	(0.347)	(0.047)
	[0.275]	[0.379]	[0.235]	[0.485]	[0.160]	[0.011]	[0.130]
λ	0.109	0.107	0.113	0.003	0.242	0.228	−0.095
Likelihood-ratio statistic for restrictions implied by MA-fixed effects model	177.6	163.2	206.6	142.0	245.8	208.0	353.4
Degrees of freedom for test	308	302	308	308	308	308	308
			Panel B: MA-fixed effects models with additional regressors				
After	0.238	−0.136	0.084	0.252	0.237	0.155	0.051
	(0.068)	(0.066)	(0.094)	(0.053)	(0.097)	(0.049)	(0.019)
	[0.154]	[−0.100]	[0.038]	[0.214]	[0.075]	[0.071]	[0.109]
Uncertain	0.049	0.271	0.057	0.284	0.319	−0.008	0.032
	(0.108)	(0.106)	(0.150)	(0.083)	(0.152)	(0.077)	(0.030)
	[0.032]	[0.199]	[0.025]	[0.242]	[0.101]	[−0.004]	[0.069]
Percent black	0.043	0.002	0.041	0.012	0.023	−0.003	0.182
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.063)
Percent male, 16–29 years old	0.032	0.016	0.066	0.013	0.066	0.031	0.016
	(0.012)	(0.012)	(0.017)	(0.010)	(0.018)	(0.009)	(0.003)
λ	0.200	0.082	0.141	0.027	0.158	0.095	−0.117

Notes: In addition to the variables shown, all models include a central-city dummy and a full set of year dummies. Standard errors are in parentheses. Figures in square brackets are mean logarithmic derivatives. Sample sizes are 668 for rape and 694 for all others. See note for table 2.

restrictive MA-fixed effects model against the more general MA-by-year-fixed effects alternative. The test statistic is distributed asymptotically as chi-square with degrees of freedom equal to the difference in the number of parameters between the two models.

Box-Cox estimates from the model that includes MA-by-year-fixed effects are reported in panel A of table 5. Although the coefficient estimates differ from those based on the simpler MA-fixed effects model, the transformation parameters also vary in such a way as to leave the mean logarithmic derivatives essentially unchanged. Although the rape coefficient has become slightly less negative and the robbery coefficient has become positive, neither of these changes is at all significant. In the main, the logarithmic derivatives from the more general MA-by-year-fixed effects model are very similar to those from the more restrictive MA-fixed effects model.

Moreover, the likelihood-ratio tests generally fail to reject the more restrictive model in favor of the more general one. Indeed, in six cases out of seven, the test statistic is smaller than its expected value. The one exception is auto theft, in which the MA-fixed effects model is rejected in favor of the MA-by-year-fixed effects model. Despite this rejection, however, the estimated effect of crack's arrival is very similar to the estimate that results from the more restrictive model. By way of summarizing these results, we conclude that these estimates show little evidence of omitted-variable bias.

MA-fixed effects models with additional regressors: As a further check for omitted-variable bias, we sought to fit MA-fixed effects models to which we added a limited number of additional regressors. Because the MA-by-year-fixed effects model above already accounts for any regressor that varies only between MAs and over time, this approach will shed new light on the problem only if we add variables that vary not only between MAs and over time, but also between the urban and suburban components of the MA at a given point in time. Unfortunately, annual data on metropolitan areas that are disaggregated by their urban and suburban components are difficult to come by.

One potential source of such data is the Current Population Survey (CPS), which satisfies the minimal requirements of providing annual data that can be disaggregated by MA and by urbanicity within MA. In many respects, however, the CPS is less than ideal for our purposes. The survey is designed to provide estimates representative of the entire United States and the larger states. Only in the two largest MAs are the CPS samples representative of the area; even in these large MAs, however, the urban and suburban components of the sample may not be representative of those separate parts of the city.

Furthermore, the CPS sampling procedure changed at various times over our sample period, so the representativeness of the MA-specific subsamples changed over time as well (Zimmerman & Robison, 1996). The result is that MA- and urbanicity-specific variables constructed from the CPS

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are likely to be measured with substantial error. Although the effects of measurement error in the case of a single regressor are well known, it is unclear how adding a number of mismeasured covariates from the CPS would affect our estimates of the effect of crack's arrival.

Nevertheless, it would be desirable to assess whether our results are robust at least to the introduction of a few variables that are widely known to influence crime rates. For this reason, we reestimate equation (1), this time adding the fraction of blacks and the fraction of males aged 16 to 29 in the local population. These variables are widely regarded as important predictors of crime (Blumstein et al., 1986).

The results are presented in panel B of table 5. The additional variables are significant in most cases and generally have the expected sign. However, adding these variables to the model has little effect on the results. The murder effect rises somewhat and becomes significant, but otherwise the mean logarithmic derivatives are similar to their corresponding values from panel C of table 4. Thus, the estimated effect of the emergence of crack is robust to the addition of these two variables known to predict crime. Although this hardly constitutes a comprehensive test, it provides us (in the absence of better data) with a bit more confidence in the validity of our estimates.

C. *Estimates Based on Alternative Arrival Dates*

We also reestimated our models using alternative arrival dates. In one set of estimates, we used only the earlier arrival date to date the arrival of crack; in another, we used only the latter. In yet another, we used only the DAWN-based arrival dates. In the final set of estimates, we used only the arrival dates from the police survey. Although pooling the arrival date information in the manner described above has the virtue of utilizing all available information, we would be concerned if our results proved sensitive to alternative measures of the arrival dates.

When the after-crack variable was based on only the earlier of the two arrival dates, the estimated effects of the arrival of crack generally were slightly larger than those reported above. When the after-crack variable was based only on the latter measure, the estimated effects were slightly smaller. These changes are consistent with the estimates reported in tables 4 and 5. When the after-crack variable is constructed from the earlier arrival date, the period of uncertainty is pooled in with the post-arrival observations. Because the period-of-uncertainty coefficients are mostly positive, this increases the estimated effect of the arrival of crack.[7] When the after-crack variable is based only on the latter arrival date, the estimated effect of crack falls, for the same reason. In any case, the effects on the estimates were small, and the significance of the results was not affected by changing the measures of arrival date. Similarly, estimates based on the DAWN measure alone and the police survey measure alone were similar to those reported above.

[7] The period-of-uncertainty coefficient is negative in the model for larceny theft. In that one case, the estimated effect of crack is slightly smaller when the after-crack variable is constructed solely from the earlier arrival date.

VI. Conclusions

Using two different sources of information to date the emergence of crack in 27 metropolitan areas across the U.S., our analysis shows that the arrival of crack cocaine led crime to rise substantially in the late 1980s and early 1990s. The most prevalent form of violence—aggravated assault—rose significantly. We argue that such an increase is the predictable result of an expanding illegal market. Property crimes rose as well, which could result either from complementarity between drug dealing and conventional property crime or from an increased demand for drugs. Regardless of the precise mechanism that underlies the link between crack and crime, we estimate that the effect of the emergence of crack was substantial. Our results indicate that, in the absence of crack cocaine, the crime rate in 1991 would have remained below its previous peak in the early 1980s.

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