

DETERRENT EFFECTS OF POLICE RAIDS ON CRACK HOUSES: A RANDOMIZED, CONTROLLED EXPERIMENT*

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We tested the block-level deterrent effects on crime of uniformed police raids of crack houses. Court-authorized raids were legally possible on 207 blocks with at least five calls for police service in the preceding 30 days. Raids were assigned randomly to 104 locations and were conducted at 98 of those sites; the other 109 were left alone. Experimental blocks, in relation to controls, showed reductions in both calls for service and offense reports,

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but effects were quite small and decayed in two weeks. Raids in which arrests were made (23 of 104 assigned) had no consistently different impact from raids in which no arrests were made. Raids had more effect on calls for service in the winter than in the spring, but we found little seasonal or period difference in effects of raids on offense reports. Alternative police methods may be far more cost-effective than raids in "harm reduction" for crack houses.

What can police do about crack houses? As the crack epidemic expanded in the late 1980s, many cities adopted a policy of intensive drug enforcement, emphasizing undercover purchases of narcotics at crack houses and raids to seize evidence and crack dealers. These raids are conducted thousands of times a year nationwide, at great public expense and risk of injury to police officers. Like most crime control strategies, however, they have not been evaluated carefully for their costs and benefits.

In this article we report a randomized controlled trial of the effects of crack house raids on crime on the block, conducted in collaboration with the Kansas City (MO) Police Department. We address both the substantive issues in local crime deterrence and the methodological issues in subjecting police methods to randomized tests. Although the results are not encouraging in regard to police ability to reduce crack-related crime, the experiment shows once again the feasibility of subjecting new police procedures to the same rigorous evaluation methods as new medical procedures (Reiss and Roth 1993).

CRACK HOUSES AND CRIME ON THE BLOCK

Of all the hot spots of crime on the public agenda in the past decade, crack houses seemed to top the list. Open-air drug markets actually may have supplied more crack cocaine, but the image of a residential block infested by indoor crack trade has caused despair and even arson (Wilkerson 1988). Although "hot spot" street addresses (Sherman, Gartin, and Buerger 1989) may produce most of the crime in a city, certain types of street addresses seem to raise crime levels on their surrounding blocks and beyond. Like taverns (Frisbie et al. 1978: 229; Roncek and Maier 1991), schools (Roncek and Faggiani 1985), and fast-food restaurants (Brantingham and Brantingham 1982), crack houses appear to generate crime problems in their immediate micro-environments.

Although Detroit crack dealers view crack houses as less violent than open-air crack markets (Mieczkowski 1992), the crack industry appears to be generally far more violent than the distribution systems for earlier illegal drugs such as heroin. This

violence comes from several sources: self-selection and social selection of violent persons; use of weapons to intimidate workers, competitors, and neighborhood citizens; and violence among crack-selling groups (Johnson et al. 1990:35-39). The dominant role of teenage operatives also increases the violence because these operatives are heavily armed (Johnson et al. 1990:36) and reputedly enjoy gratuitous violence (Mieczkowski 1992:161).

Even if the crack trade lacked these dramatic features, a crack house should generate at least as much trouble as some taverns do: both businesses give rise to parking problems, romantic disputes, robbery of intoxicated or high patrons, prostitution, noise, loitering, and auto thefts by patrons unwilling to walk home. These events may occur out on the sidewalk, street, or alley as easily as inside a crack house. The hypothesized role of the crack house in their etiology is based on the routine activities associated with such commerce, and requires no assumptions at all about the pharmacological role of crack in crime causation (Reiss and Roth 1993:194).

The micro-environmental crime problems of crack houses may be more difficult to measure than those of taverns or other legitimate facilities because crack house locations appear to be far less stable. Over an 18-month period in 1991-1993, for example, citizens reported 4,837 unique drug-dealing locations in 8,874 calls to the Kansas City (MO) Police Department's 24-hour anonymous drug hotline. Seventy percent (3,400) of those locations, however, received only one complaint. This suggests that the drug dealing did not persist long enough to generate repeat complaints (in sharp contrast to the distribution of local calls at and around taverns, which shows a high prevalence of repeat cases). Another interpretation of this finding, however, is that no crack house ever existed at many of the locations and that the caller misinterpreted the observed activity.

Measurement error in locating crack houses may be a substantial problem, despite journalistic claims that "everyone knows" where crack is being sold in inner cities (Raspberry 1990). To test this hypothesis in August and September of 1990, we interviewed 24 police officers on patrol in the three inner-city patrol divisions in Kansas City. These divisions contained 90 percent of the drug-dealing locations identified in the citywide citizens' drug hotline. Half of the officers were selected at random, and half for their reputed

expertise and interest in the crack problem.¹ Their combined responses yielded 226 unique locations, but with very little agreement: within one sector, for example, one officer identified three addresses, while another identified 25. Only five of the 100 locations named in one division were named by more than one officer (5%), as were seven of 71 (10%) in the second division and six of 47 (13%) in the third.

The police officers' list showed even less agreement with the addresses reported on the citizens' hotline. The hotline registered 939 citizen calls about 517 unique addresses and intersections in the one month before and the one month during the interviews of officers. When we combined these locations with the police list, we found a total of 706 unique locations, but only 37 (5%) of the addresses on the total list appeared on both citizen and police lists. Although some of this disparity may be caused by minor discrepancies in street addresses, the 423 uniquely identified census blocks contained only 57 blocks that appeared on both citizen and police lists, or 13 percent of the total list of blocks.²

Police and citizens thus appear to be reporting different drug-dealing locations, not only by address but also by the block on which the problem reportedly is occurring. These results were largely confirmed by an 18-month analysis of drug-dealing locations reported by citizens to the hotline and by police officers in monthly surveys conducted by the project sergeant. Of the 5,848 unique addresses identified as drug-dealing locations, only 889 (15%) were named by both police and citizen sources.

Nonetheless, both citizen and police lists are accurate predictors of higher than average levels of reported crime on the block. Over the two-month period we observed in 1990, the 6,953 census blocks in Kansas City generated 68,568 calls for police service. The 423 blocks identified by the combined police and citizen lists of drug-dealing locations constituted only 6 percent of all

¹ Each of the three divisions is divided into four sectors. We used a quasi-random number generator to determine the sequence of the sectors, and each evening the interviewer asked the sector sergeant to select an officer from the appropriate sector. The interviewer then drove around the sector with the officer, asking him to point out *currently active* crack-dealing locations and confirming their exact street addresses or street corner intersection. After completing that officer's list of crack houses, the interviewer asked the officer to transfer her to the car of another officer working in the sector, who knew a great deal about crack houses. On all but one occasion that transfer occurred, and again the interview was conducted during the drive around the sector. In the single exception, the officer had another passenger, so he simply wrote down a list of addresses he knew. The interviews were conducted from September 5 to October 5, 1990.

² Because of programming limitations, blocks could not be identified if they had no calls for police service during the study period. This situation eliminated 21 percent of the citizen addresses and 19 percent of the police addresses from the block-level analysis.

blocks, but produced 26.5 percent of all calls (18,230) and 21.5 percent of all calls about violent crime (589 of 2,734). The blocks on the police list (with a mean of 58 calls per block) were in the top 2 percent of blocks for all calls citywide; the blocks on the citizen list (with a mean of 47 calls) were in the top 3 percent.

The most accurate predictor of high-crime blocks, however, was inclusion on both citizen and police lists, with a mean of 100 calls per block in the two-month period. This frequency placed these blocks in the top 0.5 percent of all blocks citywide. These data include all kinds of drug-dealing locations. However, as in at least one other midwestern city dominated by detached, single-family houses (Mieczkowski 1992), most of the locations in Kansas City were indoors, even in black neighborhoods (contrary to Blumstein 1993:4). In a four-month sample of citizens' hotline calls in 1990, 52 percent of the 1,072 locations were in houses, 23 percent were in apartments, 5 percent were in businesses, and only 6 percent were identified as "street." Thus most of the drug-dealing locations were on residential blocks, and the disorder and crime associated with drugs were all the more noxious to local residents.

These data help to explain the great number of demands placed on the police chief in 1988-1989 to do something about the perceived spread of crack houses (Larry Joiner, personal communication, May 15, 1989). Regardless of how they are identified, blocks where drugs are sold have three to four times higher than average rates of police problems and reported violence. The higher rates may be either a direct consequence of drug dealing, a cause (Skogan 1990; Wilson and Kelling 1982), or the result of other factors that cause both drug dealing and crime. Yet these distinctions mattered little to the political reality in Kansas City on February 17, 1989, when the Street Narcotics Unit was created to raid crack houses.

RAIDING CRACK HOUSES

It is tempting to interpret crack house raids as the leading edge of a proactive enforcement crackdown on drugs in the late 1980s. As Blumstein (1993:3) demonstrates, drug enforcement nationwide skyrocketed in the late 1980s; drug arrest rates for nonwhites more than doubled from 700 per 100,000 in 1985 to 1,500 per 100,000 by 1989, the peak year for total drug arrests in the nation's history. In Kansas City, total drug-related arrested person-events (as distinct from charges) more than tripled from 1,110 in 1988 to 3,806 in 1989. Originally we assumed that this increase to had resulted from a shift in drug enforcement strategy, from targeting a few

mid-level dealers to many retail-level street sellers, as recommended by many experts (Kleiman 1992; Kleiman and Smith 1990; Skolnick 1989). The data show otherwise, however.

Most of the increased drug enforcement in Kansas City was produced not by proactive units, but by patrol officers in the course of their predominantly reactive work. The Street Narcotics Unit (SNU), which ranged from 20 to 40 officers, made only 17 percent of all narcotics arrests in 1989, 31 percent in 1990, 19 percent in 1991, and 12 percent in 1992. Other special units made only negligible numbers of drug arrests. Traffic stops and frisks that coincidentally found drugs apparently produced most of the possession arrests, which in turn dominated all drug arrests. The increase in total drug enforcement seemingly was more a reactive indicator of the prevalence of drug possession than a result of targeted, proactive drug enforcement.

On the other hand, the crack house raids spearheaded a crack-down on drug sales; arrests for sales quadrupled from 54 in 1988 to 222 in 1989, tripled again to 687 in 1990, and declined to 554 in 1991 and 466 in 1992. About four of every five drug sale arrests in that period were made by SNU, mostly in the following fashion:

First, someone attempted to buy drugs inside a crack house: an undercover police officer (in 35% of experimental cases), or a confidential informant working in the presence or under the immediate supervision of the undercover officer.³ If the informant attempted to buy the drugs, the officer searched the informant to ensure that no drugs were already on his or her person, and then issued sequentially marked bills for buying the drugs. If the "buy" attempt was successful, the officer impounded the drugs and searched the informant to ensure that the marked money was gone. If all evidentiary requirements were satisfied, the SNU administrative officers requested a search warrant the next day. Once the warrant was signed by a judge, it could be served at any time in the next 10 days.

A tactical team of seven uniformed officers and a sergeant was assigned the task of serving the warrant. This action was always

³ "Unwitting" informants who did not know they were buying drugs for police officers were used in six of the experimental cases. In two cases the method of buy was unknown. These percentages do not differ significantly from the distribution of buy methods for the nonexperimental cases during the same period. Again, the most common location is a single-family house. Of 1,421 attempted narcotics buys reported during and after the experimental period, 1,391 (98%) were in residential locations, and 68% of those (941) were at single-family houses. Other drugs, notably PCP, also were sold during that period, but the great majority of drug-dealing locations sold crack. According to the citizens reporting locations to the drug hotline during the experimental period and its follow-up, 66 percent of the 1,231 reports in which drug type was specified (73% of all 1,872 complaints in the period from November 4, 1991 to June 30, 1992) said the drug sold at the location was crack.

preceded by a roll call briefing on the location, given by the undercover officers. Then the undercover sergeant drove past the location with the tactical sergeant to ensure that the right address and door were targeted; in mid-1990 a failure to do this resulted in a raid on the wrong house, accompanied by much negative publicity and a substantial tightening of procedures. Finally, a second undercover narcotics purchase was sometimes made at the same location. This purchase, the "confirmation buy," allowed more positive identification of the seller if he or she was arrested when the warrant was served. The tactical officers lay in wait a few blocks away until the confirmation buy was completed, so they could strike immediately after its completion.

The service of the warrant then constituted the "raid." This was a dramatic show of force: an unmarked van drove quickly to the front of the house, the side door opened, and the heavily armed squad ran out, led by one officer holding a small metal battering ram. They scrambled up to the front door, generally up some porch steps, and quickly broke down the door; in about 8 percent of all raids in 1991-1992, this step was preceded with a flash-bang explosive device. The team poured into the front room, where the drug transactions generally took place. Police ordered all persons present to lie face down on the floor, where they were handcuffed. The police checked the back doors and each room in the house for anyone who might have fled, supported by officers who went directly from the van to the back or the side of the house. (Despite this precaution, the previously observed seller often had left the scene by the time the raid was made.)

Once the house was secured, a thorough search for drugs and weapons was conducted. The process could take several hours, during which time drug buyers sometimes appeared at the front door. They fled rapidly when they discovered the raid in progress. If no drugs were found, some of the suspects might be released at the scene, but generally everyone was brought to a police station for questioning and released within 12 hours. Suspects also were checked on the computer for any outstanding warrants. Illegal weapons were seized regardless of whether arrests were made.

The raids were highly visible to the residents of the block on which they occurred, especially in the warmer months, when the residents keep their windows open or spend their evenings outdoors. Raids almost always attracted great interest, and sometimes drew applause and cheers. How visible the raids were to the frequent customers of the crack house is less clear, except for those who found it closed for business during and immediately after the raid. How many of those customers lived nearby, how many lived

far away, and how many heard about the raid are all important but unanswered questions.

HYPOTHESIS AND GOALS

On the basis of the high visibility of the raids, it seems reasonable to hypothesize that raids *can produce a short-term general deterrent effect on block-level crime and disorder*. The audience aware of the raid is large enough to perceive that enforcement "heat" is on the block, and that offenses might be more likely to result in detection and apprehension; thus offenses might reasonably decline. Because arrests often are not made, and because pretrial release is speedy even when they occur, an incapacitation effect is not likely. If the crack house does not reopen immediately, but its former operators are not in jail, that fact can be interpreted as part of a deterrent effect. By extension, so might any reduction in disorder accompanying the absence of customers due to the closing of the crack house. The fact of a recent raid may increase the audience's uncertainty about the likelihood of additional police actions or presence on the block (Sherman 1990:11-14). Until that uncertainty wears off, behavior that might attract police attention should decline.

This hypothesis is central to the entire purpose of the Street Narcotics Unit, which assumed that raids would be focused on blocks with measurable problems in public order. According to the chief who created SNU, the purpose was not to raise the retail price of the drugs or even to substantially disrupt the drug market (Kleiman 1992). In view of arrestees' speedy release on bail, the purpose of the unit was not even "to put bad guys in jail." Rather, the purpose of SNU was to improve public order in residential neighborhoods, making the city more livable than it would have been if crack houses were allowed to operate unchallenged (Skolnick, 1989).

THE EXPERIMENTAL DESIGN

From November 1991 through May 1992, the Kansas City Police Department SNU implemented a randomized controlled trial in raiding crack houses; to our knowledge, this was the first randomized test of police raids ever conducted. The research design was focused on the outcome measures, a sample of eligible cases drawn from a continuous pipeline of undercover investigations, a procedure for screening and random assignment, and an analytic plan made in advance and applied continuously as the experiment progressed.

OUTCOME CRITERION MEASURES

The experimental design focused on the indicators of public order and safety as the test of the effectiveness of raids. These indicators were limited to offense reports and calls for service on the street block (both sides of the street, intersection to intersection), both in the aggregate and when disaggregated as violent crime, property crime, and disorder.

In general, calls for service are interpreted most appropriately as a measure of the quality of public order, while offense reports are a more accurate measure of serious crime. The offense reports are partially (but not entirely) a subset of the calls for service. Most offense reports are generated in response to a call; others are generated by police-initiated contacts. The main reason for considering calls and offense reports separately is that most calls do not result in offense reports; rather, they provide ephemeral indications of quality-of-life problems, about which no one is willing to invest time in cooperating with police (Sherman et al. 1989). Someone may call anonymously about a fight in progress, for example, but the fighters leave when police approach and no witnesses come forward to tell police what happened. The event is coded "unfounded" and no offense report is made, but the quality of residential life has been harmed nonetheless.

Offense reports may capture a narrower and deeper domain of events, serious enough to move some citizens to provide evidence. Offense data have the virtue of indicating events for which firm enough evidence exists to make them part of the official count of crimes. This is the case despite the socially organized filtering out of some events (Black 1980:ch. 3)

The interpretation of these indicators lacks a firm consensus in police research. Many readers might argue that block residents may call police differentially on the basis of confidence in police as a result of the raid, or in relation to other factors that change over time. Kansas City police thought that possibility unlikely, and selected these criteria as the basis for the evaluation. Before the experiment, police argued that residents of chronic poverty areas already used police services heavily; thus raids were unlikely to alter the probability that a crime or disorder problem would be reported to police. We were less certain, however, about calls to the drug hotline; these calls were connected to drug raids so directly that they could be a highly reactive measure. Thus we rejected such calls as a possible outcome measure.

We decided on a 30-day follow-up period for both theoretical and practical reasons. From the theoretical perspective, we expected any deterrent effects to be immediately observable, but not

to last over the long term. The Board of Police Commissioners approved the 30-day limit for the control group as consistent with the general problem of delay in raiding crack houses once they had been reported, but viewed a longer period as unfair to residents of blocks affected.

THE SAMPLE

To avoid bias towards the null hypothesis, the design required all eligible cases to be drawn from blocks with at least five calls for police service in the 30 days preceding the undercover buy. We chose this criterion after creating numerous statistical power curves for detecting different effect sizes with different levels of baseline calls for service. With an assumed sample size of 100 cases per treatment group, the five-call criterion achieved a 90 percent chance of detecting a 20 percent reduction in calls as statistically significant at the .05 probability level (one-tailed test).

The sample was also restricted to the inside of residences: single-family houses (which made up 68% of the sample), multifamily houses (4%), or apartments (28%). All cases were to be based on buys that would be fully eligible for a search warrant. This judgment was to be made by SNU administrative officers before random assignment took place. Further exclusions before random assignment were allowed for all PCP dealers, other high-priority cases, and successful buys that produced abuse of the undercover officers by the drug sellers (such as gun pointing, threats, or even physical injury).

After a dry run in October 1991, in which cases were screened and randomized without actual implementation, 207 eligible cases were assigned to the experiment from November 4, 1991 to May 20, 1992. The case flow was quite slow until early 1992, when a reorganization of the weekly list of eligible blocks (alphabetically by street name rather than in rank order of seriousness, as measured by call frequency) made it easier for informants to find eligible cases.

Pipeline. The 207 eligible cases came from a pipeline of all 1,421 attempted buys made by undercover SNU officers during the experimental period. These attempted buys were reported on a short form designed for the experiment. No reporting had been required for unsuccessful buy attempts before the dry run, and reports sometimes were late. Some buy attempts no doubt went unreported, especially when the research manager was not present. Yet because the manager was present on most evenings, the undercover officers were constantly reminded to file their attempted buy reports.

The filed reports show that 1,391 of the attempts were made at residential locations (an eligibility requirement, as stated above). Of the attempted buys excluded from the experiment, 839 (60%) were simply unsuccessful. Another 345 were completed but were excluded; 154 of these simply had fewer than five calls on the block during the 30 days preceding the most recent weekly list of eligible cases. Others were excluded because of insufficient evidence (19) or because the SNU had taken action within the preceding 30-day baseline period (26). We excluded another 46 buys as "confirmation" of existing warrants just before raids to improve the evidentiary basis for arrests. Thus 71 percent of these buys were excluded for reasons of experimental eligibility set by researchers in advance, and not because of police discretion at any level. Other cases were excluded because of seriousness, such as special requests from the chief's office (33), reported PCP dealing (14), abuse of undercover officers (4), or unknown reasons (12). Another federally funded program in a small area caused the exclusion of 31 other buys.

RANDOM ASSIGNMENT

Eligibility screening and random assignment were performed on-site each morning by a research site manager for all but one week of the experiment. For the first half of the experiment, the manager was a university professor on 50 percent release time. For the second half, this professor concentrated on obtaining the attempted buy reports and on a second experiment during the evening hours; the daytime site manager was a police sergeant (experienced in experimental methods) from another unit located many miles away. The police research manager had experience in a randomized diversion experiment conducted a decade earlier, was culturally independent of the SNU environment, and identified strongly with the goals of the research project, for which the grant paid his full salary during that period.

Both research managers worked in collaboration with the SNU administrative officer. That officer's job was to review the successful buy reports left the previous evening by the undercover officers. If the buy reports provided a sufficient evidentiary basis, the SNU administrative officer prepared the paperwork to request a search warrant. During the experiment, however, the SNU administrator first submitted these approved cases to the research site manager. In turn, the research manager checked for the criterion of five or more calls for service. He also checked the 30-day hold list to make sure the block had not been contaminated by a raid or was not already serving as a control block. He then opened a sequentially

numbered envelope, which contained either the statement "raid" or "no action." The statements were arranged in sequence by a computerized quasi-random number generator program and were sealed in the numbered envelopes off-site in Washington. If the statement in the envelope was "raid," a warrant application was filed. If it was "no action," the buy report was filed and the block was placed on the 30-day hold list, which was updated and republished each day.

The purpose of the 30-day hold list was to prevent contamination by SNU activity of blocks already in a follow-up period. It was maintained for "no action" cases, for "raid" cases after the raid had occurred, and for other SNU enforcement actions outside the experiment. It was distributed each week to all SNU officers so that no SNU action—experimental raids or otherwise—would take place on those blocks. No attempt was made, however, to keep other units from taking action on those blocks, because only the SNU actions were being tested. Actions by other units were defined as part of the general background level of policing in the city, which would be applied at random to both experimental and control blocks.

All but seven of the 207 cases received the randomly assigned treatment as assigned (97% compliance with protocol). All seven of those misassignments were assigned randomly to receive raids, but six could not do so and one received two raids.⁴ In one case the buy had been followed immediately by a "bust," and no warrant could be sought. In three other cases, the buys had taken place in apartment hallways, and no warrant could be issued for the inside of a specific apartment. A fifth case was completely unpreventable because the warrant was issued and the squad was dispatched, whereupon police found the apartment abandoned and padlocked by another city agency. In the two remaining cases, the apartment units were not described fully enough in the warrants to enable the tactical squad to knock down the right door. In one of these cases the squad returned the next night and knocked down the right door, but this was almost a double dose of the raid treatment.

Thus 103 cases were assigned successfully to no action; 104 cases were assigned to raids and 98 actually received them, and one of the 98 received a double dose of police visibly rushing into the building. The random assignment succeeded in making the groups roughly comparable; for example, there were no differences between

⁴ Four of the seven misassignments occurred in the one week when the site manager, with the advance approval of the first author (principal investigator), opened envelopes by telephone from the ASC conference without reading the buy reports himself. This procedure was unexpectedly risky because the regular SNU administrative officer was unavailable, and the officer screening the cases was not experienced enough to recognize evidentiary flaws.

the experimental and the control (treatment as assigned) groups in the distribution of dwelling types (70 single-family houses in each group, 28 apartment buildings in the raid group, and 30 in the "no action" group). There were no differences in the source of information leading to the buy, nor in the method used to complete the buy. Undercover officers made the buy themselves in 33 of the experimental cases and 39 of the controls; informants made the buys alone in 57 of the experimental cases and 60 of the controls.

THE ANALYTIC PLAN

The analysis presented here follows the "intention-to-treat" (Pocock 1983:182) or "analyze as you randomize" principle, in which cases are compared according to the treatments randomly assigned to them rather than the treatments they actually received. The standard justification for this approach is that the inferential benefits of randomization for approximating baseline comparability of the groups (in all respects except the treatment) surpass the benefits of uniform treatment within groups. This principle has been challenged when taken too far, as in surgical experiments in which half of the patients assigned to surgery do not receive it (Weinstein and Levin 1989). Yet as long as treatment "crossover" remains a very small percentage of the sample, as in this study, the principle remains valid. Moreover, analysis of the treatments as actually delivered shows results virtually identical to those presented here.

The comparisons are made with a slight disjunction in time between experimental cases and controls. For purposes of observing the 30 days before and after, the control group uses the day of the undercover buy as the benchmark or intervention point. The experimental group, however, uses the day when the raid was actually performed; this could be any time within 10 days after the warrant was issued. We decided on this procedure in advance of the experiment in order to manage the 30-day hold periods. The decision was justified by the impossibility of serving all warrants with an equal lag time after the warrant was issued, given the highly variable levels of workload.

The mean time from the buy to the raids was 6.83 days, with a median of seven days. This lag was generally so short that the comparability of the treatment and the control groups was unlikely to be affected. The lag was long enough, however, to reduce the statistical power of the experiment by leaving some blocks with fewer than five calls in the 30 days before the raid, as distinct from the 30 days before the buy (which was the basis for the eligibility screen).

Because we analyzed the data as the experiment progressed, we discovered strong seasonal differences in the effects of raids on

calls for service. We did not expect seasonality on theoretical grounds, but we present the differences for their empirical importance.

We also analyze the data according to whether the raids resulted in arrests. This was not part of the original plan because it could not be controlled in random assignment, but when we presented the main effects of the randomized design to the Street Narcotics Unit officers, they asked us to examine the differences between raids with and without arrests. In keeping with the deterrence hypothesis, the officers predicted that raids with arrests would have stronger deterrent effects than raids without arrests. Their hypothesis reflected their frustration in being unable to make more arrests: only 23 of the 104 randomly assigned (and 98 actual) raids produced one or more arrests. The SNU officers blamed this rate on the requirements of the experiment, which they said would have shown larger deterrent effects if more raids had yielded arrests.

This analysis can examine the interaction of the raid treatment with arrests, but it cannot assess the casual link between arrests and crime because of the lack of randomization of arrests. The factors that produce arrests in a raid may also influence the level of measurable crime and disorder on the block.⁵

RESULTS

Table 1 shows the effects of crack hose raids on block-level calls for police service throughout the experiment. The most important finding in this table is the consistent downward trend across all categories of *control* cases, with reductions ranging from 5 percent to 25 percent. This pattern suggests that buys may tend to occur in peak periods of public order problems on the blocks with crack houses and that some regression to the mean occurs thereafter. The pattern also shows how misleading a quasi-experimental evaluation design with no control group would be. Absent the control group, the treatment effects appear much more impressive, ranging from a 10 percent decline in disorder calls to a 39 percent decline in calls about property offenses.

⁵ Before the experiment, the research team had proposed random assignment of arrests to individuals in raids where arrests were possible, but the idea was rejected as politically objectionable and statistically impractical with so few arrests.

Table 1. Impact of Narcotic Raids on Reported Calls for Service, 30-Day Evaluation Period Before and After Treatment, November 4, 1991-May 28, 1992^a

Call Type	Pre-Experimental Calls on Block			Postexperimental Calls on Block			Mean Difference	Negative Binomial
	Score	Mean	S.E.	Score	Mean	S.E.	% Change	Coef.p ^b
All Calls for Service								
Raid	1,059	10.18	10.38	865	8.32	9.29	-0.18	-1.156
No raid	1,037	10.07	7.61	929	9.02	9.02	-0.10	(P=.06)
Violent Calls for Service								
Raid	103	0.99	1.32	79	0.76	0.98	-0.23	-0.438
No raid	89	0.86	1.00	81	0.79	1.12	-0.09	(P=.33)
Property Calls for Service								
Raid	115	1.11	1.74	70	0.67	1.39	-0.39	-1.208
No raid	99	0.96	1.04	74	0.72	0.96	-0.25	(P=.11)
Disorder Calls for Service								
Raid	644	6.19	6.76	581	5.59	6.67	-0.10	-1.191
No raid	649	6.30	5.86	616	5.98	6.79	-0.05	(P=.12)

^a Based on 104 experimentals and 103 controls. N = 207.^b One-tailed test, negative binomial regression.

Taking the control group into account, however, we find quite modest deterrent effects of the raids on calls for police service. Across all offense types, the net reduction is only 8 percent, computed by subtracting the percentage decline in the control group from the percentage decline in the experimental group. Although this reduction is 80 percent greater than would have occurred with no action, it is still a net reduction of only 85 calls—not much return on an investment of 30 to 40 officers for most of their time for seven months. This outcome works out to less than one call deterred per raid.

We tested statistical significance of these differences in criminal event counts using negative binomial regression, after tests for fit of the actual distribution (Cameron and Trivedi 1986; King 1988). The results are generally unlikely (88% to 94%) to be due to chance, except for the violence calls. The standard deviation exceeded the mean in every period for every measure, however, so we must attend to both Type I and Type II error. Another way to view the significance of results is to observe that four out of five independent measures showed crime reductions associated with the raids associated with the raids (Tables 1 and 2, excluding the “total” analyses); for this result, the binomial probability of a chance result was .15.

We expected disorder to be the type of call deterred most; this measure consists of calls about noise, parking problems, disturbances, public intoxication, and the like. They accounted for more than half of all calls, and were thought to be most sensitive to the operation of a crack house on the block. Yet they showed a net reduction of only 5 percent in relation to the control group. The most impressive reduction is the 14 percent net decrease in calls about violent crime, the problem usually thought to be hardest to control, although this result was probably due to chance. The 13 percent net reduction in the more numerous calls about property crimes was much less likely to be a chance result.

Table 2 shows the effects of raids on block-level offense reports during the entire experiment. These effects are in the same direction as the effects on calls and generally are of greater magnitude, but are based on much lower base rates of events and are far more likely to be due to chance. The overall net reduction was 14%, led by a 24%, net reduction in violent offenses. Property offenses showed a slight net increase of 3 percent on the raided blocks, the

Table 2. Impact of Narcotic Raids on Reported Offenses, 30-Day Evaluation Period Before and After Treatment, November 4, 1991-May 28, 1992^a

Call Type	Pre-Experimental Calls on Block			Postexperimental Calls on Block			Mean Difference	Negative Binomial
	Score	Mean	S.E.	Score	Mean	S.E.	% Change	Coef.p ^b
All Offenses								
Raid	248	2.38	2.86	188	1.81	2.78	-0.24	-1.420
No raid	212	2.06	1.90	190	1.84	2.03	-0.10	(P=.15)
Violent Offenses								
Raid	121	1.16	1.39	88	0.85	1.22	-0.27	-0.535
No raid	96	0.93	1.30	93	0.90	1.55	-0.03	(P=.59)
Property Offenses								
Raid	105	1.01	1.33	90	0.87	1.61	-0.14	-0.626
No raid	109	1.06	1.20	90	0.87	1.12	-0.17	(P=.53)
Disorder Offenses								
Raid	0	0.00	0.00	0	0.00	0.00	—	NA
No raid	0	0.00	0.00	0	0.00	0.00	—	NA

^a Based on 104 experimentals and 103 controls. N = 207.

^b One-tailed test, negative binomial regression.

only exception to the otherwise consistent net reductions across offenses and calls. Offense reports are least likely to be taken for disorder problems (Black 1980:70-73): too small a base rate is left for meaningful comparison with the call data.⁶

Table 3 reveals a strong period or seasonal difference in the effects of crack house raids on block-level calls for service. In the first 87 cases of the experiment we found two clearly nonchance effects: an 18 percent net reduction in total calls and a 17 percent net reduction in calls about disorder. Both the 28 percent net decrease in violence and the 2 percent net increase in calls about property crime were more likely to be chance results. This pattern of effects is closer to the overall offense report findings than to the overall findings on calls for service. The latter indicator shows virtually no effects of raids for the last 120 cases in the experiment (data not displayed): reductions are almost identical in experimental and in control blocks for calls about all offenses, violent offenses, and disorder (but the net reduction in property crimes on the raided block was 22%). Figure 1 depicts the differences in overall effects between the two periods.

⁶ As we feared, the 474-DRUG hotline number apparently provided a highly reactive measure: it showed an increase in prevalence (but not frequency) of calls alleging drug dealing on blocks assigned to be raided. For the experimental blocks (treatment as assigned), 67 percent had a 474-DRUG call in the 30 days before the raid but 94 percent had a call in the 30 days after. For the control blocks, the comparable proportion with such calls fell from 69 percent to 59 percent. This result probably reflects a change in public perceptions of the efficacy of calling the police about a specific problem in which police had shown enough interest to conduct a raid. It seems unlikely that raids would actually cause an increase in drug dealing, although anything is possible, and open-air markets can spring up in the vacuum created by raids. We found little difference in frequency of 474-DRUG calls, as distinct from prevalence, between treatment and control groups: 75 to 212 in 103 experimental cases, and 72 to 193 in 104 controls.

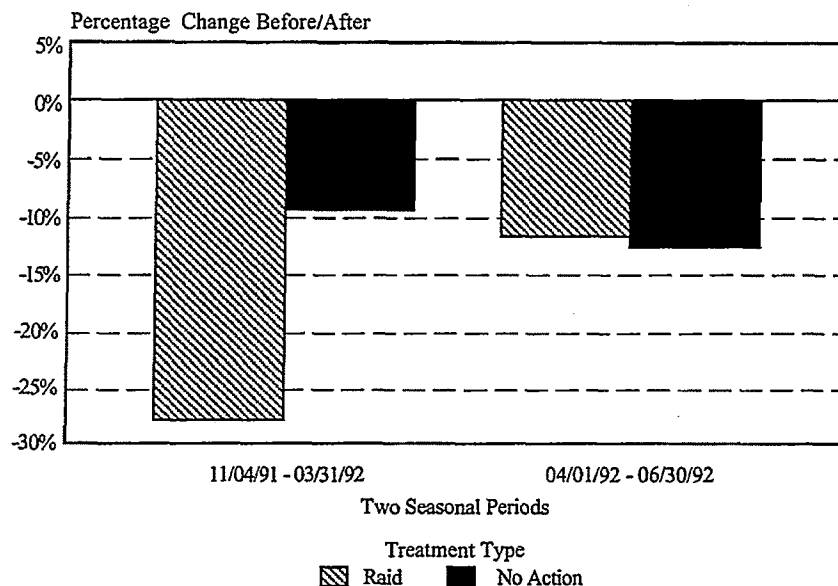
Table 3. Winter Impact of Narcotic Raids on Reported Calls for Service, 30-Day Evaluation Period Before and After Treatment, November 4, 1991-March 31, 1992^a

Call Type	Pre-Experimental Calls on Block			Postexperimental Calls on Block			Mean Difference	Negative Binomial
	Score	Mean	S.E.	Score	Mean	S.E.	% Change	Coef.p ^b
All Calls for Service								
Raid	453	10.79	12.75	329	7.83	10.36	-0.27	-2.354
No raid	487	10.82	7.33	445	9.89	7.63	-0.09	(P=.00)
Violent Calls for Service								
Raid	46	1.10	1.52	33	0.79	1.06	-0.28	-0.674
No raid	36	0.80	0.83	36	0.80	1.07	0.00	(P=.25)
Property Calls for Service								
Raid	47	1.12	2.16	39	0.93	1.92	-0.17	-0.853
No raid	48	1.07	1.12	39	0.87	0.98	-0.19	(P=.20)
Disorder Calls for Service								
Raid	271	6.45	7.13	205	4.88	6.41	-0.24	-1.785
No raid	302	6.71	5.44	281	6.24	5.70	-0.07	(P=.04)

^a Based on 42 experimentals and 45 controls. N = 87.

^b One-tailed test, negative binomial regression.

Figure 1. Seasonal Impact of Narcotic Raids on Calls for Service for All Crimes, November 4, 1991 to March 31, 1992 and April 1, 1992 to June 30, 1992

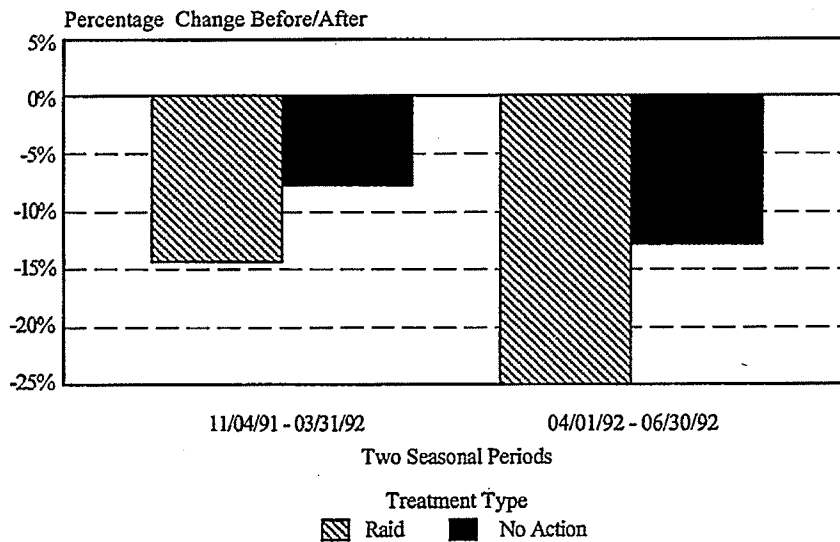


Kansas City (Mo) Police Department Calls for Service Data

Figure 2 shows that the period difference in the effects of raids does not affect the offense report data. If anything, the effects are *stronger* for offense reports, not weaker, in the second period than in the first, although this finding may be connected somehow to the higher base rates of offenses in warmer weather.⁷ The magnitude of the before-after change increases in the second period, but it does so about equally for the experimental and the control group. Thus the net reduction in total offenses is 7 percent in the first period and 11 percent in the second, with a 24 percent net reduction in violent crimes in the first period and a 25 percent reduction in the second (data not displayed).

⁷ Base rates for all offenses in the 30-day "before" period rose from a mean of 1.35 per experimental block and 1.19 per control block from November through March to 2.05 per block for experimentals and 1.77 for controls from April through June.

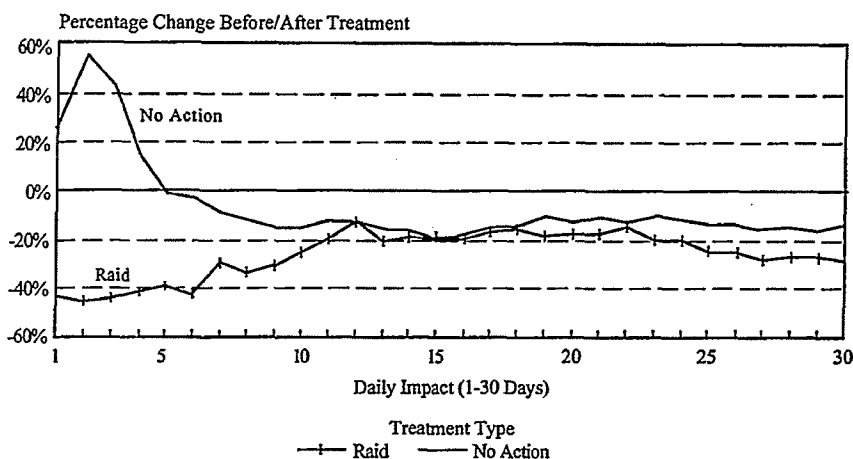
Figure 2. Seasonal Impact of Narcotic Raids on Offenses for All Crimes, November 4, 1991 to March 31, 1992 and April 1, 1992 to June 30, 1992



Kansas City (Mo) Police Department, Offense Reports

For both measures, however, the deterrent effects are concentrated in the immediate aftermath of the raid and decay very quickly. Figure 3 is the most impressive depiction of the deterrent effect over time, based on the total offense report data for the full experimental period. The figure plots the cumulative before-after differences for the indicated number of days before and after at each point on the Y axis, starting with the difference between the day before and the day after, then between two days before and two days after, and so on to the difference between 30 days before and 30 days after. Although the immediate effect is substantial and continues to grow for the first two days, it begins to decline thereafter and disappears by Day 12. The data on calls for service show a similar trend: again, the effects disappear by Day 12, but with a much greater narrowing of the differences by Day 2 (data not displayed).

Figure 3. Daily Impact of Narcotic Raids on Offenses, All Crime Types, 30 Days Before and After Treatment



Kansas City (Mo) Police Department Offense Reports

We find no consistent support for the hypothesis that arrests during raids enhance the deterrent effect. Because of the small number of raids with arrests, the analysis is limited to the data on calls for service as the only source of adequate statistical power. The analysis shows that the net reduction in total calls on raided blocks was even greater in the 81 cases when no arrest was made than in the 23 cases when they were made: a 10 percent reduction in the no-raid blocks, compared with 14 percent in raid-with-arrest blocks and 19 percent in raid-with-no-arrest blocks (data not displayed). We obtained similar results for the various categories of calls.

DISCUSSION

Taken as a whole, these results suggest that crack house raids in fact produce some deterrence of block-level crime and disorder, but with four important qualifications. First, these effects are quite modest, even within the limited 30-day follow-up period. Second, the effects decay very quickly, and virtually disappear by the twelfth day after the raid. Third, the effect on disorder found in the winter disappeared in the spring. Finally, even these modest results on the block may be mitigated by displacement.

The modesty of the deterrence is perhaps the most important qualifier. Although the overall effects on offense reports are more powerful than the effects on calls, they are still small in relation to the investment of resources. The net reduction of 29 violent crimes, and of 35 crimes of all types, is less than 0.3 crimes deterred per

raid. With a 40-officer squad producing at most 800 raids a year, the full labor cost per raid (including training, undercover buys, and preparation) is about 2.6 officer-weeks, or 8.7 officer-weeks per crime prevented. This cost-benefit ratio may be much lower than for other forms of police activity. Given the shooting of 2 officers (one undercover and one tactical) in the process of generating the 1,895 raids conducted by SNU from 1989 through early 1993, the risk-benefit ratio is one officer shot for every 284 crimes prevented.

The rapid decay compounds the modesty of the effects, raising substantial questions about a major strategy in the "war" on drugs. Initial decay of deterrence is a common pattern (Sherman 1990), but 12 days is unusually rapid. Although raids clearly satisfy a demand that police "do something," they seem to provide afflicted blocks with nothing more than a brief respite from crack houses. Like aspirin for arthritis, the painkiller does nothing to remedy the underlying condition.

The mystery of the period difference in effects on calls for service, which is not matched by a similar difference in effects on offense reports, has several post hoc explanations. The most plausible is that in cold weather, crack houses are proportionately a much larger source of trouble on residential blocks in poor neighborhoods than they are in warm weather. In warm weather, more people are outside for reasons unconnected to crack retailing—people who are causing, detecting, and calling police about troubles also unconnected to crack houses. This theory, however, is weakened by the lack of a major increase in total calls for service per block in the second period over the first; on average, the total rises from about 10 to 11 calls per block per 30-day period. That evidence does not eliminate the hypothesis, however, because there could be a substitution effect: troubles related to crack houses (such as parking or even indoor fights) become less noticeable in warmer weather, thus producing fewer calls, but calls about trouble from other sources take their place.

The difference also could be a period effect unrelated to seasonality but connected to some substantive change in the content or perception of police raids. This explanation is just as plausible in theory as is seasonality, but it lacks a specific candidate. After extensive discussions with police and month-by-month inspection of the data, we can discern no substantive change in the content or the sanctioning consequences of the raids. Such a change may have occurred, but we have no idea what it may have been. The one change we have identified is the increased emphasis on making buys in blocks with five or more calls for service in the last 30 days. These became a higher proportion of all buys made in the

second period, when the research team pressed police for a more rapid case flow.

A third explanation—that the difference occurred by chance—seems unlikely. Chance differences in the effects of raids would be more likely to be spread out over the experimental period. Nonetheless, we cannot eliminate that possibility.

Another issue of interpretation is the lack of enhanced deterrence when raids produced arrests. This could be interpreted as negative evidence for deterrence because more severe sanctions failed to make any consistent difference. This interpretation, however, can be rejected on several grounds. The most powerful rebuttal is the lack of random assignment, which renders the causes of the arrest unknown and possibly connected to the outcome measure. Another argument is that deterrence may depend more on certainty of sanction than on severity. The same theory that predicts initial deterrence followed by decay of effects—exactly the pattern found here—rests on the alteration of the audience's perceptions of levels of sanction certainty, not severity (Sherman 1990:11-14). Finally, from a neighborhood audience perspective, the fact that suspects are taken to police stations whether or not formal arrests are made means that this is a distinction without a difference. From the crack house operators' perspective, the short-term disruptions due to raids with and without arrests may hardly appear to be different.

Both the presence of the period difference and the absence of an arrest-no arrest difference highlight the major limitation of this experiment. The design offers no data on audience perceptions or any other causal process; thus a "black box" is left between the input of police raids and the output of crime and disorder. This gap leaves unanswered a wide range of questions such as the scope of the relevant audience, the communication of the threat, and whether the communication produced a "simple" deterrent effect (Zimring and Hawkins 1973:75) or something more complex such as shame and embarrassment (Tittle 1977; Williams and Hawkins 1986) or a multivariate model of expected utility (Grasmick, Bursick, and Arneklev 1993). It may even be preferable to interpret the results as "incapacitation" of the crack house as a criminogenic place (and not of any specific individuals) because its operators were temporarily deterred from a particular block, and possibly displaced to other locations. Conversely, local market demand may have been satisfied quickly by open-air dealers who took the place of the arrested

indoor dealers, operating in a fashion less visible to police. Unfortunately the measurement problems in addressing all of these issues are enormous, and remain a major challenge to the advancement of deterrence theory.

The absence of displacement data, however, makes the experiment no less valuable from the deterrent perspective. Even if the drug retailers opened up shop in the next block the day after the raid, the raid still had the hypothetical effect of stopping the crack house on the raided block. The more troubling fact is that the deterrent effect is so short-lived; thus the search for displacement effects virtually becomes moot. If displacement is to matter, there must first be a deterrent effect that matters. Many people would argue that a mere 12-day effect does not qualify.

These findings contribute to the growing body of evidence about the effects of police activity on crime, especially in hot spots. Consistent with most of that literature, it shows that police deterrent effects are possible, modest, and brief. This experiment shows briefer effects than most, but the more commonly reported quasi-experimental designs may overstate the duration of such effects because they lack control groups. The present finding of consistent reductions in outcome measures for the control groups should emphasize the limitations of designs lacking randomized controls, and should dispose researchers to place greater emphasis on the results of statistically powerful, randomized experiments.

The implications of these findings for drug control policy are less clear. Even if we shift from a "zero tolerance" punishment strategy to a more realistic "harm reduction" strategy (Reuter 1993), crack houses on residential blocks apparently cause major harm. This problem has only increased since the Street Narcotics Unit was created, at least as measured by the number of unique addresses identified by the citizens' hotline. It is impossible to say whether it would have grown even more rapidly without crack house raids.

We can say, however, that the benefits in the number of crimes prevented by each raid are so small that other uses of police resources may be far more cost-effective. The same hypothesized city-wide symbolic moral condemnation (or general deterrent) effect of conducting *some* raids might be achieved with far fewer raids at much less cost, with little loss in deterrent benefits to the complaining blocks.

Some efforts have been made to use "problem solving" instead of raids—for example, by closing down crack houses under city building and health codes or seizing them under federal law. Yet the low property values and the high abandonment rate in poor

neighborhoods limit these alternatives; many boarded-up, padlocked houses without utilities fall prey to crack dealers. Absent an effective police response, some neighbors are driven to arson as a form of "capital punishment" of crack houses, with the attendant dangers of this strategy for innocent bystanders (Wilkerson 1988). Such vigilantism suggests the hidden potential costs of not raiding crack houses, regardless of the small deterrent benefits.

More cost-effective alternatives may include directed patrol presence on blocks containing crack houses, with the explicit goal of harm reduction. For the same person-hours of police time, a greater deterrent effect could be achieved through intermittent, ongoing uniformed presence (Koper 1995; Sherman and Weisburd 1995) than through a more intensive but short-lived crack house raid. This remains another hypothesis to be tested, a commodity of which there is no shortage.

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