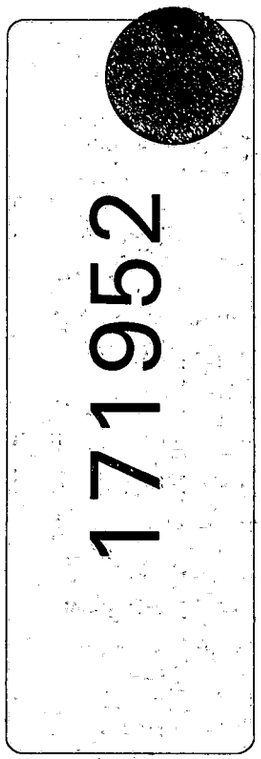

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OTHER DOCUMENTS PRODUCED FROM GRANT:

Lawrence Sherman

GRANTEE NAME: Kansas City (MO) Police Dept **TELEPHONE:** fax (202) 337-8324

ADDRESS: Contact: Lawrence Sherman, Crime Control Institute, 1063 Thomas Jefferson St, N.W., Washington, D.C. 20007

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RAIDING CRACK HOUSES:

THE KANSAS CITY EXPERIMENT*

A National Institute of Justice Research in Brief

by Lawrence W. Sherman

and

Dennis P. Rogan

In Collaboration With the Kansas City Police Department
Steven Bishop, Chief of Police

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The rise of crack in the late 1980s generated thousands of police raids on crack houses nationwide. These raids are both expensive and dangerous, but have been justified as a necessary response to flagrant defiance of the law. While few police chiefs expect raids to reduce the overall availability of drugs, many have hoped that raids would protect residential neighborhoods from the crime crack houses generate. The effectiveness of raids in accomplishing that objective, however, had never been tested.

Under a grant from the National Institute of Justice, the Kansas City (Mo.) Police Department and the Crime Control Institute tested the effects of crack house raids in a randomized, controlled experiment in 1991-92. Such experiments allow much stronger tests of cause and effect than other kinds of studies. This Research in Brief describes how the experiment was done and summarizes its major findings:

- Raids have little effect on crime and disorder on crack house blocks. Overall, the experiment's 98 raids prevented a total of only 35 reported crimes--about 1 crime for every three raids--and 85 calls for service--less than one call per raid.

- Even these small effects are extremely short-lived, disappearing after 12 days, and may be further reduced by displacement.

- Only 23 of the 98 raids in the experiment produced any arrests, but the effects of the raid were substantially the same regardless of whether arrests were made.

- Raids had more effect on calls for service in the winter than in warmer weather. The effects on offense reports, however, did not change with the seasons.

- 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.

o With 2 officers shot in the first 1,895 raids by Kansas City's Street Narcotics Unit, the estimated risk benefit ratio is 284 crimes prevented for every officer shot.

o Citizen calls to the police drug hotline actually went up after the raids, while the prevalence of such calls went down in the controls--probably a function of increased willingness to call.

RAIDING CRACK HOUSES

Like many cities other cities in the early 1980s, Kansas City (Mo.) concentrated drug enforcement efforts on mid-level dealers. Street sales were not a major enforcement priority until the late 1980s, when crack houses began to afflict literally thousands of residential blocks. Over an eighteen month period in 1991-92, for example, Kansas City citizens called an NIJ-funded drug hotline 8,874 times to report visible drug dealing at 4,837 separate locations, while Kansas City police officers identified 1900 locations--with only 15% overlap between the two lists (Sherman, Rogan and Beatty, 1993). The blocks identified in these reports suffered much higher levels of calls for police service about violent crime and disorder than other blocks.

This high prevalence of crack dealing on residential blocks created a great demand for police action against crack houses. In February, 1989, the Kansas City Police Department created the Street Narcotics Unit to spearhead the attack on street-level on drug sales. Annual arrests for narcotics selling quadrupled from 54 in 1988 to 222 in 1989, tripled to 687 in 1990, and tapered off somewhat to 554 in 1991 and 466 in 1992. About 4 out of every 5 drug sale arrests in that period was made by the Street Narcotics Unit (SNU). Most of those arrests were made in the course of raids, the annual number of which rose steadily with the 1990-91 expansion of SNU under special funding from the drug tax approved by Jackson County voters in a referendum.

Street Narcotics Unit Raids By Year

Year	Warrants Served
1989	182
1990	340
1991	403
1992	739

Most of these raids were conducted in the following fashion:

Making Buys. First, someone would attempt to buy drugs inside a crack house: an undercover police officer (in 35% of experimental cases) or a confidential informant, either working in the presence of (5%) or under the immediate supervision of the undercover officer (57%).[1] If the informant made the buy attempt, the officer would search the informant to insure no drugs were already on his or her person, then issue sequentially marked bills for buying the drugs. If the buy attempt was successful, the officer impounded the drugs and searched the informant to insure the marked money was gone. If all evidentiary requirements were satisfied, the SNU administrative officers would request a search warrant the next day. Once the warrant was signed by a judge, it could be served any time in the next ten days.

Preparing the Raid. A tactical team of seven uniformed officers and a sergeant was assigned the task of serving the warrant. This was always preceded by a roll call briefing on the location by the undercover officers. That was followed by the undercover sergeant driving with the tactical sergeant past the location, in order to insure the right address and door is targeted; a failure to do this in mid-1990 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 (the "confirmation buy"). The confirmation buy allows more positive identification of the seller if he or she is arrested

when the warrant is served. The tactical officers lie in wait a few blocks away until the confirmation buy is completed, so they can strike immediately upon its completion.

Serving Warrants. The service of the warrant then constitutes the "raid." It is a dramatic show of force. An unmarked van quickly drives to the front of the house. The side door opens, and the heavily armed squad runs out led by one officer holding a small metal battering ram. They scramble up to the front door, generally up some porch steps, and break the door down quickly; about 8% of all raids in 1991-92 preceded this step with a flash-bang explosive device. The team pours into the front room where the drug transactions generally take place. They order all present to lie on the floor, face down, where they are handcuffed. They check each room in the house and back doors for anyone who may have fled, supported by officers who went directly from the van to the back or side of the house. Despite this precaution, the observed seller has often left the scene by the time the raid occurs.

Once the house is secured, a thorough search for drugs and weapons is conducted. The process can take several hours, during which time drug buyers sometimes appear at the front door. They flee rapidly once they discover the raid in progress. If no drugs are found, some of the suspects may be released at the scene, but generally all will be brought to a police station for questioning and released within 12 hours. Suspects are also checked on the computer for any outstanding arrest warrants. Illegal weapons are seized regardless of whether arrests are made.

The raid is highly visible to the residents of the block on which it occurs, especially in the warmer months when the residents keep their windows open or spend their evenings out of doors. Raids almost always attract great interest, and sometimes receive applause and cheering. How visible raids are to the frequent customers of the crack house is less clear, except for those who find it closed for business during and immediately after the raid. How

many of those customers live close by, how many live far away, and how many will hear word of the raid are all important but unanswered questions.

HYPOTHESIS AND GOALS

Hypothesis. Based on the high visibility of the raids, it seems reasonable to hypothesize that they can produce a short-term micro-deterrent effect on block-level crime and disorder. There is a large enough audience aware of the raid to perceive that enforcement "heat" is on the block, and that offenses might be more likely to result in detection and apprehension, for offenses to reasonably decline. Because arrests are often not made, and pretrial release is speedy even when they are, this is not likely to be an incapacitation effect. If the crack house does not reopen immediately, that can be interpreted as part of the micro-deterrence. By extension, so can any reduction in disorder attendant to the absence of customers due to the crack house closing.

Goals. This hypothesis is central to the entire purpose of the Street Narcotics Unit (SNU), which assumed that raids would be focused on blocks with measureable public order problems. According to former Kansas City Police Chief Larry Joiner, who created SNU, its purpose was not to raise the retail price of drugs, or even to disrupt the drug market substantially (Kleiman, 1992). Given the speedy release of the arrestees on bail, the purpose of the unit was not even "to put bad guys in jail." Rather, the purpose of the unit was to improve public order in residential neighborhoods, making the city more liveable than it would have been if crack houses were allowed to operate unchallenged.

Target Selection. Mounting that challenge was hindered, however, by the high volume of crack houses relative to SNU's capacity. The fact that not all crack houses could be raided, and that not all drug purchase attempts are successful, put great discretion in the hands of the undercover officers. In

order to guide that discretion to make best use of scarce SNU resources, the chief asked the research team to develop a system for rationalizing the undercover officers' selection of targets, directing it to the crack houses causing the worst public order problems. The research team developed an elaborate data system for setting priorities among attempted buy locations, linking drug hotline reports, calls for service, arrest and offense reports. But the priority list, produced each week, encountered great resistance from the SNU undercover officers.

This reaction reveals the contradictions embedded in public expectations of police work. Regardless of the public pressure on the department to close the most noxious crack houses, the undercover officers were under deaprtmental pressure to make successful buys. The most efficient way for them to do that was to follow their informants to the most likely places they knew. The result was often buys targeting blocks which had no indications of disorder or crime in the police data base. In a 1989 sample of 129 blocks with raids, 23 blocks had no calls at all in the 30 days prior to the raid. From a measurement perspective, there was nothing to go down on these blocks as an indication of micro-deterrence. The researchers jokingly suggested the undercover officers were acting like the drunk who looks for his lost wallet under the streetlight where it is easier to see than in the dark around the bushes where he actually lost the wallet.

Raiding discreet crack houses which caused no public order problems was outside the apparent SNU priority. But SNU was judged, both internally and in the public eye, primarily on the number of arrests it made, and not by any before-after count of public order problems on the blocks where raids took place. In order to keep the arrest count up, there was little alternative to following the least difficult path to a successful undercover buy. Only the federally-funded experiment provided sufficient rationale for reducing the arrest count in order to focus on blocks with measureable problems of crime and disorder.

EXPERIMENTAL DESIGN

MEASURING RESULTS

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), broken down as violent crime, property crime, and disorder.

In general, calls for service are best interpreted as a measure of the quality of public order, while offense reports are a better measure of serious crime. While some advisors were concerned that these indicators could rise if the block residents gained confidence in police as a result of the raid, the consensus was that this was unlikely. Prior to the experiment, police argued that residents of chronic poverty areas already use police services so heavily that raids were unlikely to alter the probability that a crime or disorder problem would be reported to police.

A thirty day followup period was chosen for reasons of both theory and practicality. Given the theoretical perspective, we expected any micro-deterrent effects to be immediately observable. But we did not expect to find effects lasting much longer than thirty days. In any case, 30 days was the limit approved by the Board of Police Commissioners for the control group and the delay of any further action on the raided blocks. A longer time period was thought to be unfair to residents of blocks affected, whereas 30 days was not inconsistent with the general problem of delay and priorities in raiding crack houses once they had been reported.

SAMPLE

In order to give the raids a fair test, the design required all eligible cases to be drawn from blocks with at least 5 calls for police service in the 30 days preceding the undercover "buy." This minimum was needed to allow raids a chance to reduce calls for service. Assuming a sample size of 100 cases per treatment group, the 5-call criterion achieved a 90% chance of

detecting a 20% reduction in calls as being statistically significant at the .05 probability level (one-tailed test). The sample was also restricted to the inside of residences: single-family houses (which comprised 68% of the sample), multi-family 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 which cases were screened and randomized without actual implementation in October of 1991, 207 eligible cases were assigned to the experiment from November 4, 1991 to May 20, 1992. The early rate of caseflow was quite slow, until an early 1992 reorganization of the weekly list of eligible blocks (alphabetical by street name rather than rank order of seriousness, as measured by call frequency) made it easier for informants to find eligible cases.

The 207 eligible cases came out of a "pipeline" of all 1,421 attempted buys made by undercover SNU officers during the experimental time period. These attempted buys were reported on a short form designed just for the experiment. No reporting had been required for unsuccessful buy attempts prior to the dry run, and there was some slippage in timely reporting. Some buy attempts no doubt went unreported, especially when the site manager was not present. But since the site manager was present for 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. Of the attempted buys excluded from the experiment, 839 (60%) were simply unsuccessful buys. Another 345 were completed but excluded, of which 154 had fewer than 5 calls on the block during the 30 days prior to the most recent weekly list of

eligible cases. Others were excluded for reasons of insufficient evidence (19), or a 30 day hold restriction from prior SNU action (26). Another 46 buys were excluded as "confirmation" of existing warrnts just prior to raids to improve the evidentiary basis for arrests. Thus 71% of the excluded buys were for reasons of experimental eligibility set by researchers in advance, and not due to police discretion at any level. Other cases were excluded for reasons of seriousness, such as special requests from the chief's office (33), reported PCP dealing (14) and abuse of undercover officers (16). Another federally funded program in a small area required exclusion of 31 other buys.

RANDOM ASSIGNMENT

Eligibility screening and random assignment were performed on-site each morning by a research site manager working in collaboration with the SNU administrative officer. If the buy reports provided a sufficient evidentiary basis, the SNU administrative officer would prepare the paperwork to request a search warrant. During the experiment, however, the SNU administrator would first submit these approved cases to the research manager. The research manager would check for the 5 or more calls for service criterion. He would also check the "30 day hold list" to make sure that the block had not been contaminated by a raid or was not serving as a control block already. He then open the sequentially numbered envelope which contained the words "raid" or "no action."

If the envelope said "raid," a warrant application was filed. If the envelope said "no action," then 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 followup period. It was maintained both for no action cases, and raid cases once the raid had occurred. It was distributed each week to all SNU officers so that no SNU action, raids or otherwise, would take place on those blocks. No attempt was made to keep

other units from taking action on those blocks, however, since only the SNU actions were being tested.

All but seven of the 207 cases received the randomly assigned treatment (97% protocol compliance). All seven of those misassignments were randomly assigned to receive raids but could not, for various reasons. Thus 103 cases were successfully assigned to no action, while 104 cases were assigned to raids and 98 actually received them, with one of those receiving a double dose of police storming into the building. There were no differences between the experimental and 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 no action). There were no differences in the source of information leading to the buy, nor in the method used to complete the buy.

RESULTS

Calls for Service Table 1 shows the effects of crack house raids on block-level calls for police service throughout the entire experiment. Its most important finding is how misleading a no-control group evaluation design would have been. Absent the control group, the treatment effects appear quite impressive, ranging from a 10% decline in disorder calls to a 39% decline in calls about property offenses.

[Table 1 about here]

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

The type of call we expected to be deterred the most was disorder,

consisting of calls about noise, parking problems, disturbances, public intoxication, and the like. These comprised over half of all calls, and were thought to be most sensitive to the operation of a crack house on the block. Yet they only showed a net reduction of 5% relative to the control group. The most impressive reduction is for the problem usually thought to be hardest to control, the 14% net reduction in calls about violent crime, although this result was probably chance. The 13% net reduction in calls about property crime was much less likely a chance result.

[Table 2 about here]

Offense Reports. Table 2 shows the effects of raids on block-level offense reports during the entire experiment. These effects are in the same direction and generally of greater magnitude than the effects on calls, 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% on the raided blocks, the only exception to the consistent net reductions. Offense reports are least likely to be taken for disorder problems (Black, 1980: 70-73), leaving too small a base rate for meaningful comparison with the call data.

Seasonal Differences. Other analyses show a strong period or seasonal difference in the effects of crack house raids on block-level calls for service. In the experiment's first 87 cases, there were two clearly non-chance effects: a net reduction in total calls of 18% and a 17% net reduction in calls about disorder. A 28% net decrease in violence and 2% net increase in calls about property crime were both more likely to be chance results. This pattern of effects is closer to the overall offense report findings than to the overall calls for service findings. The latter indicator shows virtually no effects of raids for the last 120 cases in the experiment [data not displayed], with almost identical reductions in experimental and control blocks in calls about all offenses, violent offenses, and disorder (but perversely, a 22% net reduction in property crimes on the raided blocks).

Figure 1 graphically depicts the differences in overall effects between the two periods.

[Figure 1 about here]

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 may be connected somehow to the higher base rates of offenses in the warmer weather.[2] While the magnitude of the before-after change increases in the second period, it does so about equally for both experimental and control groups. Thus the net percentage reduction in total offenses is 7% in the first period and 11% in the second, with a 24% net reduction in violent crimes in the first period and a 25% reduction in the second [data not displayed].

[Figure 2 about here]

Short-Lived Effects. For both measures, however, the micro-deterrent effects are concentrated in the immediate aftermath of the raid and decay very quickly. Figure 3 shows the most impressive graph of the micro-deterrent effect over time, using the total offense report data for the full experimental period. The figure plots cumulative before-after differences for the indicated number of days before and after at each point, starting with the difference between the day before and the day after, then between two days before and two days after, to the difference between 30 days before and 30 days after. While 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 calls for service data show a similar trend, also with the effects disappearing by day 12, but with much greater narrowing of the differences by day 2 [data not displayed].

[Figure 3 about here]

Raids With Arrests. The hypothesis that arrests during raids enhance the micro-deterrent effect receives no consistent support. Due to the small number of raids with arrests, the analysis is limited to the calls for service

data as the only source of adequate statistical power. The analysis shows that the net reduction in total calls on raided blocks was even higher in the 81 cases when no arrest was made than in the 23 cases when they were: 10% reduction in the no-raid blocks compared to 14% in raid-with-arrest blocks and 19% in raid-with-no-arrest blocks [data not displayed]. Similar results were obtained for the various categories of calls.

Calls to the Drug Hotline. The 474-DRUG hotline number that generated the target address lists showed the raids caused an increase in calls alleging drug dealing activity. For the experimental blocks, 67% of them had a 474-DRUG call in the 30 days prior to the raid, but 94% had a call in the 30 days after. For the control blocks, the comparable percentage with such calls dropped from 69% to 59%. This result probably does reflect changing public perceptions of the efficacy of calling the police about the 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.

IMPLICATIONS

These findings contribute to the growing body of evidence about the effects of police activity on crime (Sherman, 1990; 1992a). Consistent with most of that literature, it shows that micro-deterrent effects are possible, modest and brief. This experiment shows briefer effects than most. But other studies 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 evaluations lacking randomly chosen control cases, and emphasize the importance of doing classical experiments in policing.

The mystery of the period difference in calls for service effects, not matched by a similar difference in offense report effects, has several post-hoc explanations. The most plausible seems to be that in cold weather, crack

houses are a proportionately 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.

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 (Petersilia and Reuter, 1992; Reuter, 1993), there is an apparently major problem of harm caused by crack houses on residential blocks. This problem has only grown in magnitude since the Street Narcotics Unit was created, at least as measured by the number of unique addresses identified by the citizens' hotline. Whether it would have grown at an even greater rate without crack house raids is impossible to say.

What we can say is 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 <u>some</u> raids might be achieved with far fewer raids at much less cost, with little loss in micro-deterrent benefits to the complaining blocks.

Some efforts have been made to enhance the effects of raids, such as closing down crack houses under city building and health codes or seizing them under federal law. But the low property values and high abandonment rate in poor neighborhoods limits these alternatives; many boarded-up, padlocked houses without utilities have fallen prey to crack dealers. Absent an effective police response, some neighbors have been driven to arson as a form of "capital punishment" of crack houses, with the attendant dangers of such a strategy for innocent bystanders (Wilkerson, 1988). Such vigilantism suggests the hidden potential costs of not raiding crack houses, regardless of the micro-deterrent benefits.

More cost-effective alternatives may include directed patrol presence on crack house blocks, with the explicit goal of harm reduction. For the same

person-hours of police time, a greater deterrent effect could be achieved from intermittent and ongoing uniformed presence (Sherman, 1990) than from the more intensive but short-lived crack house raid. But this remains a hypothesis for another experiment to test.

Whether these findings apply to other cities is also a question for further research. As NIJ's recent domestic violence experiments have shown, what works in one city may not work in another--and vice versa (Sherman, 1992b). But in the absence of further research, the Kansas City Crack House Raid Experiment should make us search even harder for alternative ways to control crack houses.

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This Research in Brief summarizes a report entitled "The Micro-Deterrent Effects of Police Raids on Crack Houses: A Randomized, Controlled Experiment," to be published in _____. The authors of that report were

LAWRENCE W. SHERMAN

DENNIS P. ROGAN

University of Maryland

with

TIMOTHY EDWARDS

RACHEL WHIPPLE

DENNIS SHREVE

DANIEL WITCHER

WILLIAM TRIMBLE

and

THE STREET NARCOTICS UNIT

Kansas City, Mo., Police Department

ROBERT VELKE

MARK BLUMBERG

ANNE BEATTY

CAROL A. BRIDGEFORTH

Crime Control Institute

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J. Reiss, Jr., Stephen D. Mastrofski and Michael Maltz.

Please direct correspondence about this research to Lawrence W. Sherman, Crime Control Institute, 1063 Thomas Jefferson St., N.W., Washington, D.C. 20007, fax # 202-337-8324.

TABLE 1

IMPACT OF NARCOTIC RAIDS ON REPORTED CALLS FOR SERVICE
30 DAY EVALUATION PERIOD BEFORE AND AFTER TREATMENT
N = 207 RAIDS N = 104 CONTROL N = 103
11/04/91 - 05/28/92

Call Type	Pre Experimental Calls on Block			Post Experimental Calls on Block			Mean Difference	Negative Binomial
	Score	Mean	S.E.	Score	Mean	S.E.	% Change	Coef./p*
All Calls For Service								
Raid	1059	10.18	10.38	865	8.32	9.29	-0.18	-1.156
No Raid	1037	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)

* 1-tailed test, Negative Binomial Regression.

TABLE 2

IMPACT OF NARCOTIC RAIDS ON REPORTED OFFENSES
30 DAY EVALUATION PERIOD BEFORE AND AFTER TREATMENT
N = 207 RAIDS = 104 CONTROL = 103
11/04/91 - 05/28/92

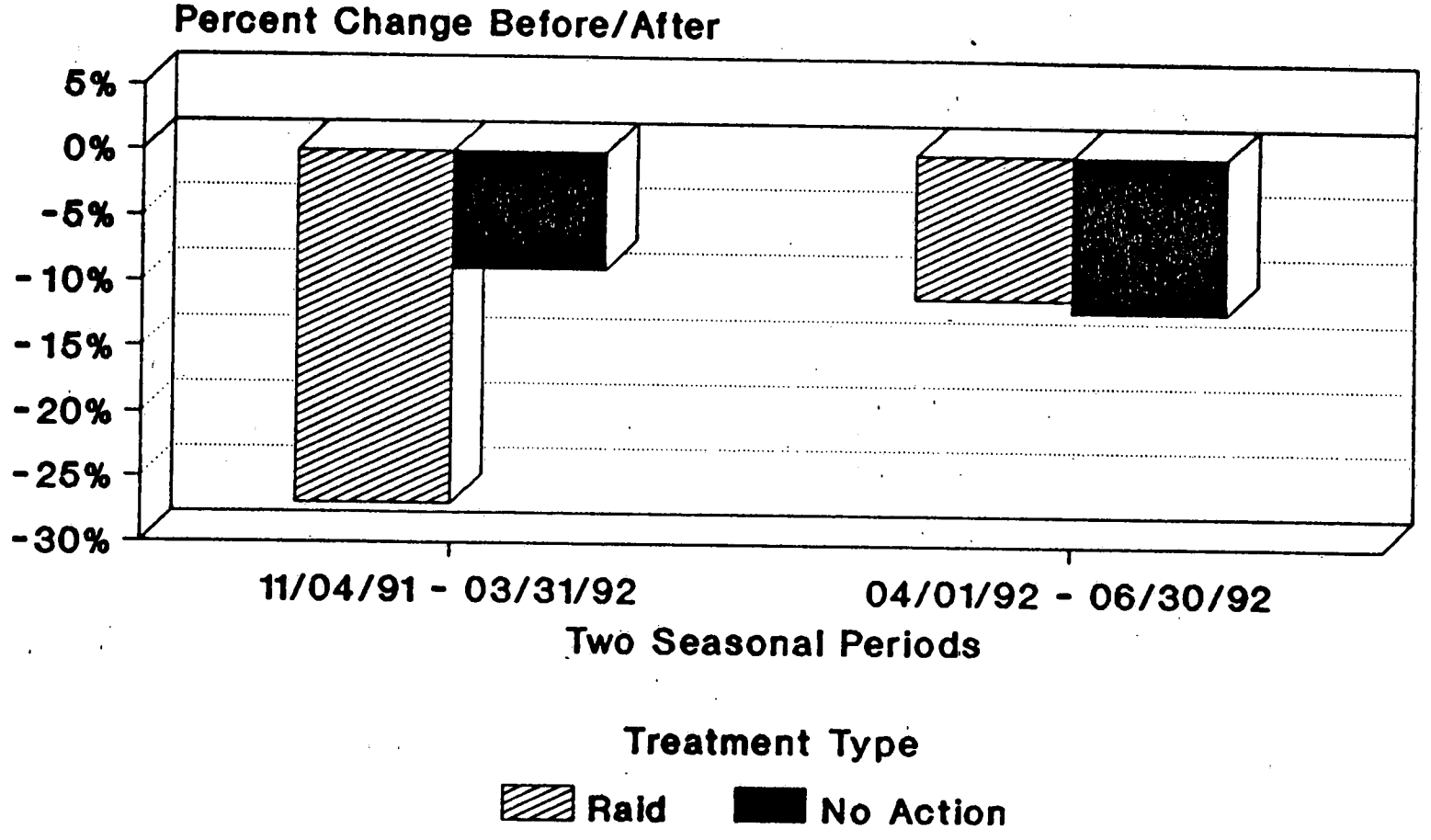
Call Type	Pre Experimental Calls on Block Score	Mean	S.E.	Post Experimental Calls on Block Score	Mean	S.E.	Mean Difference % Change	Negative Binomial Coef./p*
All Offenses								
Raid	248	2.38	2.86	188	1.81	2.78	-0.24	-1.420
No Raid	212	2.06	1.90	190	1.84	2.03	-0.10	(p=.15)
Violent Offenses								
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	*****	n/a
No Raid	0	0.00	0.00	0	0.00	0.00	*****	n/a

* 1-Tailed Test, Negative Binomial Regression

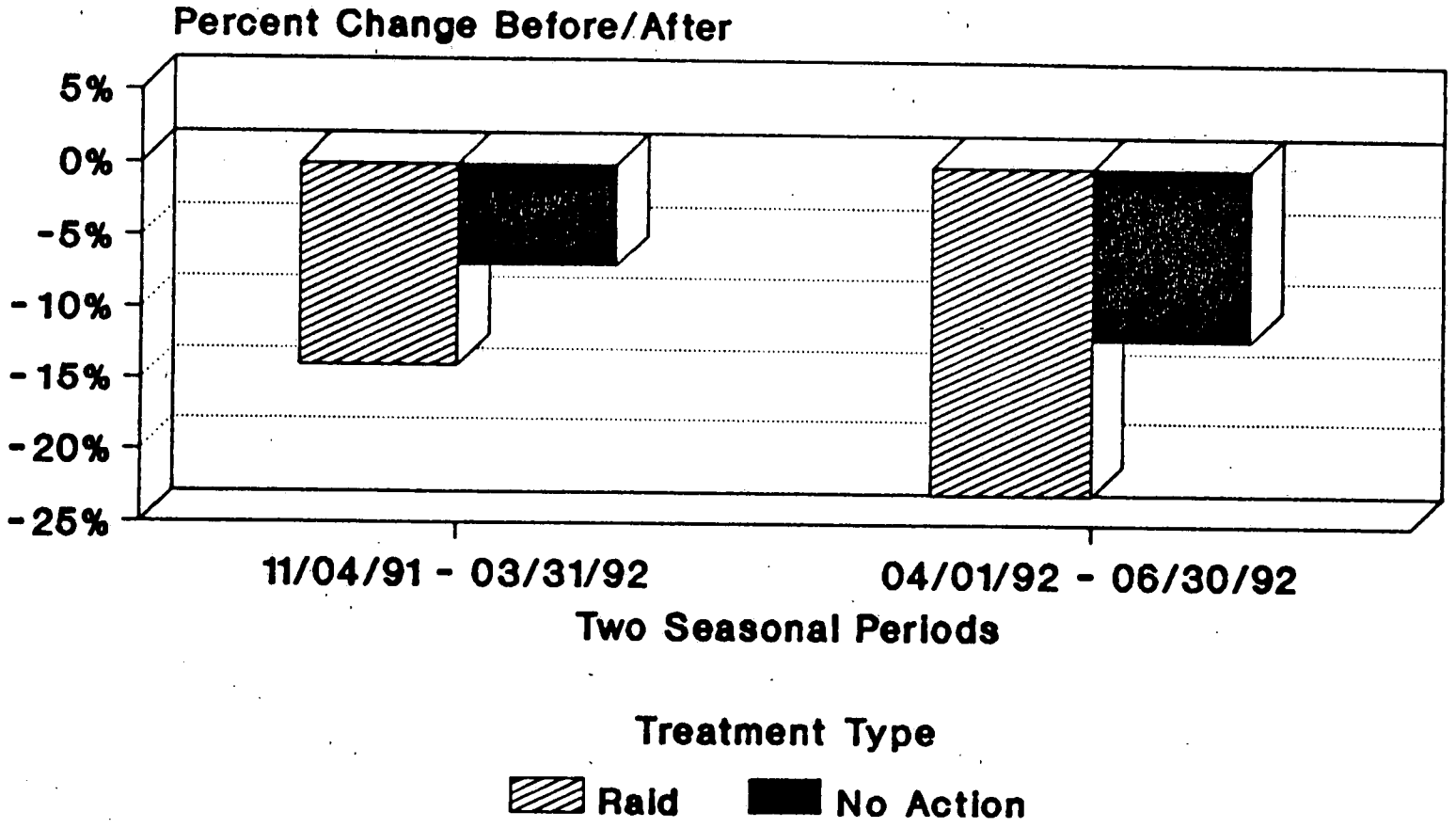
1. "Unwitting" informants who did not know they were buying drugs for police officers were used in 6 of the experimental cases. 2 cases had the method of buy unknown. These percentages do not differ significantly from the distribution of buy methods for the non-experimental cases during the same time 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 were also sold during that period, notably PCP, but the vast majority of drug-dealing locations sold crack. According to the citizens reporting locations to the drug hotline during the experimental period and its followup, 66% of the 1,231 reports in which drug type was specified (73% of all 1,872 calls in the period 11/4/91 to 6/30/92) said the drug sold at the location was crack.

2. 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 in April through June.

Seasonal Impact of Narcotic Raids on Calls for Service For All Crimes (11/04/91-03/31/92) (04/01/92-06/30/92)

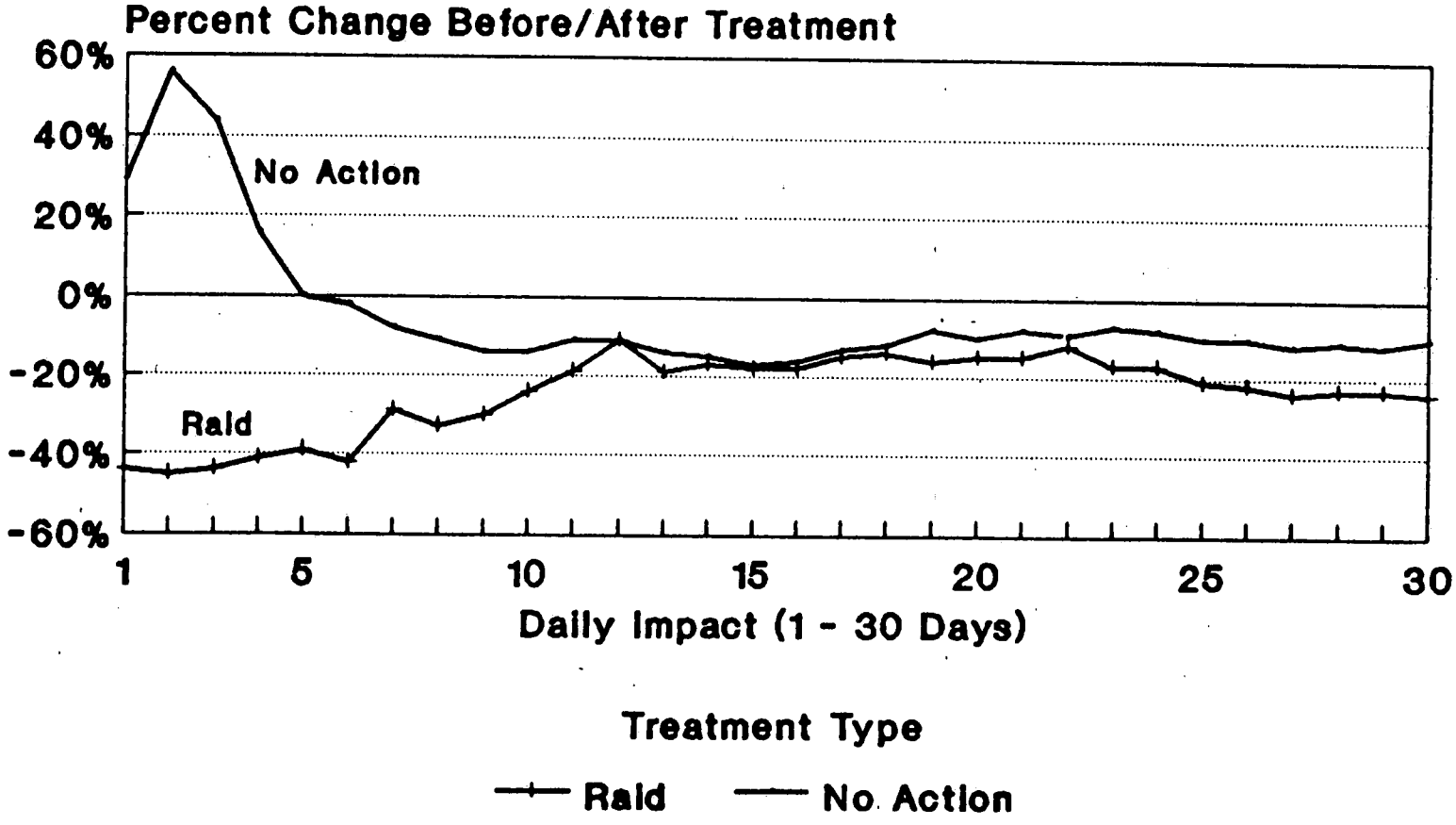


Seasonal Impact of Narcotic Raids on Offenses For All Crimes (11/04/91-03/31/92) (04/01/92-06/30/92)



Police Department
Experiment Offense Report Data

Daily Impact of Narcotic Raids On Offenses All Crime Types 30 Days Before and After Treatment



Police Department
Computerized Offense Reports