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Beyond Dial-A-Cop:

A Randomized Test of Repeat Call Address Policing (RECAP)*

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Beyond Dial-A-Cop::

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Abstract

Calls for police service are highly concentrated by the addresses to which police are sent. An experimental crime prevention strategy was applied to a randomly selected half of the 452 most active commercial and residential addresses in one city. A special unit of four patrol officers and one sergeant was assigned to do nothing else but "problem-oriented" work on the experimental addresses for one year, although the unit voluntarily assumed additional duties of policy development affecting city-wide repeat calls (including the control addresses). The experiment featured a heterogeneous mix of the nature of the problems, of tactics employed, and the level of effort applied across experimental addresses. Pre-post differences in calls to 107 experimental commercial addresses showed no difference from the control group, while 119 experimental residential addresses showed a 15% greater reduction than at residential controls ($P < .05$) in the first 6 months, followed by a declining impact to 6% greater reduction for the full year. Other findings outside the randomized experiment suggest that the strategy is useful, but that more specific diagnostic and tactical tools are needed for broader success at crime prevention and call reduction. The experimental design suggests the difficulty of intervening in crime "hot spot" addresses, and the mixed results which problem-oriented strategies should anticipate when picking targets by objective criteria.

Can police prevent crime? Almost two centuries after Colquhoun (1806) first raised this question, criminologists can cite little evidence on point. The pessimism about patrol after the Kansas City Preventive Patrol Experiment (Kelling, et al, 1974) obscured the other strategies police have used historically to control criminogenic conditions, such as the regulation of alcohol and noise (Bacon, 1939; Reiss, 1984). The recent focus on police efforts to create artificially induced "community crime prevention" surveillance (Rosenbaum, 1986) is a possible exception, although that strategy shows little evidence of working.

The public health model of disease prevention features far more focused attempts to remove or alleviate specific causes of diseases. Examples include purifying water supplies to prevent typhoid, filling swamps to prevent malaria, and closing bathhouses to prevent AIDS. Yet criminology has done relatively little to develop analogies to the public health model (Clarke, 1983).

The potential value of the public-health model for crime prevention was reinforced by recent findings that the bulk of calls for police service are highly concentrated in a small number of apparently criminogenic addresses (Pierce, Spaar and Briggs, 1984; Sherman, Gartin and Buerger, 1989). The findings raise the twin questions of what causes such concentrations, and what police (or others) might do about them. This article reports an experiment addressing the latter of those questions, with the clear handicap of ignorance about the former.

The purpose of the experiment is to explore the effectiveness of a proactive police strategy, Repeat Call Address Policing (RECAP). The goal of the strategy is to solve problems causing repeat calls at addresses with the highest frequency rates of telephone calls for police service: "repeat offender" addresses, the "hot spot" locations of predatory crime, disorder, and persons in need of assistance.

The article begins by placing RECAP in the context of recent police strategy developments, especially problem-oriented policing. It then describes the experimental design and the nature and range of the police tactics actually implemented. The experimental results and conclusions address the future prospects for a crime prevention strategy focused upon repeat call analysis.

Changing Police Strategy

Over the past two decades, American police have become far more proactive. That is, more police work has been initiated by police themselves, rather than reacting to citizen demands for dealing with specific incidents (Reiss and Bordua, 1966; Black and Reiss, 1967; Reiss, 1971). The evidence for this claim abounds in press accounts of police crackdowns on localized street drug markets (Robinson, 1986; Kleiman, 1988), roadblocks checking for drunk drivers (Jacobs, 1988: 197-208), "sweeps" of juvenile gang members (Associated Press, 1988; Cockburn, 1988), "stings" of persons selling stolen goods (Marx, 1988), special units for surveillance on high rate repeat offenders (Martin and Sherman, 1986), and greater use of deceptive investigations against official corruption (Braithwaite, Geis and Fisse, 1987).

This appears to be a major change from the prevailing reactive mobilization of American police work through telephone calls, which Reiss (1972) described as a "dial-a-cop" system, in contrast to Europe's then-prevailing "wall-to-wall" cops system. The change has provoked great controversy (e.g., Levinger, ed., 1987), largely related to the fairness of target selection (Sherman, 1983). As Black (1973) suggests, the fear of discriminatory target selection has long been the basis of liberal opposition to proactive police mobilization. Yet he also points out that reactive mobilization can also be highly discriminatory, and cannot be subjected to the same accountability and scrutiny as a proactive system.

The controversy may be more intense than the actual changes to date can justify. Police reliance on the dial-a-cop system has always varied somewhat by neighborhood. A 1977 observation study in 60 neighborhoods, for example, found a range of 2.7 to 46.4 proactive patrol unit contacts with citizens per 100 hours of patrol time, with a mean of 20 (Smith, 1986: 318-327). Police were <u>least</u> likely to be proactive where the fear of discriminatory law enforcement has been greatest: in poorer, more segregated high crime neighborhoods (at least before the current wave of drug crackdowns).

Moreover, recent additions of proactive strategies have left the "dial-a-cop" system basically unchanged. Most of the new programs have been focused on the kinds of crime problems usually lacking a complainant ("victimless" crimes): drunk driving, fencing, bribery, and drug dealing. The kinds of problems generating calls to the police have remained almost entirely in the hands of reactive patrol car response.

Problem-Oriented Policing and Target Selection

The one exception to this pattern is the development of "problem-oriented" policing (Goldstein, 1979; Goldstein and Susmilch, 1982; McElroy, 1985; Eck and Spelman, 1987), which seeks to solve the kinds of problems reflected in repeated similar incidents being brought to police attention. Unlike its more fashionable (and more vague) competitor "community-oriented policing" (cf. Skolnick and Bayley, 1986), problem-oriented policing (POP) has the potential to create major reallocation of police resources. It could, for example, transform the majority of police tasks from reactive to proactive mobilization. By focusing on the <u>causes</u> of crime and disorder problems rather than the <u>symptoms</u>, it suggests the public health model of crime prevention. In all these ways, POP provides the clearest strategic vision yet suggested for going beyond dial-a-cop.

Yet one of the least developed aspects of POP is the central problem of proactive mobilization: the procedure for target selection. As a broad approach, it has encouraged many different dimensions for defining problems, from city-wide (Goldstein, 1979) problems to those highly specific in time and place (Eck and Spelman, 1987). The procedure for selecting problems has accordingly been highly subjective. In target selection for the Newport News POP demonstration, for example, "the single most-used source is personal experience" of the police officers (Eck and Spelman, 1987: 45).

This subjectivity has three possible consequences. One is to make POP vulnerable to attack on the grounds of discriminatory law enforcement. A second is to make the strategy less efficient, allowing it to ignore major consumers of police resources or major sources of bloodshed in favor of problems which constitute "pet peeves" of the police officers selecting them. The widespread empirical finding that police impose more effort in response to disrespectful citizens (Black, 1980; Smith and Visher, 1981) could apply to problems as well as to encounters.

A third possible consequence of subjective target selection could be selection bias in program evaluations. The Newport News evaluation (Eck and Spelman, 1987) suggests that POP is far more successful than other police strategies. Yet this may simply reflect the picking of easier to solve problems. The success may be due to the selection more than the strategy, just as the career success of Harvard graduates may be unrelated to the education they received. A more compelling test of both a Harvard education and a police strategy would have to deal with a more representative sample.

A non-subjective procedure for police target selection can be found in the growing computerization of police radio car dispatch records. Far more voluminous than official crime reports, calls for service data provide an excellent indicator of the distribution of emergency dial-a-cop services. This distribution in turn can provide an impartial criterion for target selection, or for setting priorities across possible targets. The computer enables police to rank order every address (or every phone number) in a city by the frequency of all calls, or of certain types of calls. Target selection can therefore be a wholly objective selection of the top addresses in the city, with a pre-fixed quota of targets based on available resources. This selection procedure, combined with police attempts to reduce the criminogenic conditions at the selected target locations, constitutes RECAP, or repeat call address policing.

Advantages of Repeat Call Address Targets

Target selection based on recent call histories of specific addresses has two ethically attractive features, at least in theory. Unlike selective incapacitation (Blumstein, et al, 1986), repeat call policing runs little risk of identifying false positives; proactive police efforts at a target address can be terminated immediately upon a reduction in calls for service. Second, the information for the selection criterion is generated in an entirely reactive manner, based on citizen-supplied information and complaints (Black, 1973) rather than government-selected informants. All the government does differently in selecting target problems with repeat call data is to take a longer time perspective than it does under incident-driven Dial-a-Cop.

RECAP also offers two methodological advantages over tests of problem-oriented policing based on subjectively selected targets. One is the replacement of haphazard sample selection bias with a systematic bias in favor of the most troublesome locations. The methodological advantage is that the bias is known, although the substantive disadvantage is that it stacks the deck against successful intervention. In the Harvard analogy, the RECAP selection bias admits to college only grade-school dropouts with dyslexia.

The other methodological advantage is the use of roughly comparable units of analysis (addresses), which allows for more rigorous experimental testing of the effectiveness of POP tactics. And for all its flaws, the experiment we report here is the first randomized experiment (to our knowledge) in any POP strategy, with the largest sample size of any single evaluation.

Theoretical Framework: Criminology of Place

Focusing POP on repeat call addresses also has an advantage in criminological theory. By dealing with a single unit of analysis, it allows POP to employ a consistent theoretical framework. While the pragmatic philosophy of POP prefers a wide range of theoretical approaches to police problems (Goldstein, 1979), it is just as pragmatic from a scientific perspective to assemble empirical findings in a way that develops the predictive power of a single theory.

The RECAP focus can both be guided by and inform the growing body of findings and theory about the concentrations of crime in micro-spatial environments. Drawing on aspects of "routine activities" (Cohen and Felson, 1979; Felson, 1988), "situational crime prevention" (Clarke, 1983) and "environmental criminology" (Brantingham and Brantingham, 1984), this line of research predicts the distribution of <u>crime</u> according to the distributions of <u>persons</u> and <u>activities</u> in time and space. The central unit of analysis for police requires a criminology of <u>place</u>, rather than the traditional criminology of persons, community, and larger social units (Sherman, Buerger, and Gartin, 1989).

The RECAP officers were trained in this perspective, at first implicitly, and then (by the end of the first six months) quite explicitly. The major conceptual tool was the "crime triangle" of motivated offenders, suitable victims and poor guardianship (Cohen and Felson, 1979). Take away any of the three elements, the officers were taught, and the likelihood of predatory crime will decline substantially. Although this framework may not have been suitable to all of the wide range of problems they confronted, they generally found it quite useful as a way to diagnose RECAP problems and develop possible solutions.

What the framework lacks is specific propositions comparable to public health theories. It lacks specific claims about the effectiveness of various kinds of guardianship with different kinds of "suitable" victims. It lacks propositions about the kinds of victims selected by criminals as most suitable in different circumstances. And it lacks propositions about the kinds of motivated offenders most likely to be involved in certain kinds of crimes in certain kinds of places. The framework provides little specific guidance on these questions, or on the more practical question of what kinds of tactics police can legally employ to alter the crime triangle in any specific place. But those are exactly the kinds of data which tests of the RECAP strategy can develop.

Experimental Design

The experiment began with a plan for picking 500 of the most active addresses in the city, and randomly assigning half to be subjected to problem-oriented policing. The first step was the researchers choosing (with the approval of the police chief and deputy chief) a sergeant and four patrol officers from among over twenty volunteers for the RECAP unit. The researchers and police then collaborated on fleshing out the design of the experiment beyond the conceptual plan of the grant proposal.

The design of the experiment began with computer sorting by address (on a personal computer, using DBASEIII) of all calls for service to which a police car had been dispatched by the Emergency Communications Center (ECC) over a one year period (December 15, 1985 to December 14, 1986). [Subsequent inspection of the data by day revealed that four separate six to seven day periods throughout the baseline year were blank, either from missing data on the ECC tapes or through copying problems onto our microcomputer]. All calls stripped from the weekly ECC tapes excluded administrative call codes, such as "out to lunch" or "transporting prisoner to headquarters". They included proactive police contacts, such as traffic stops and field interrogations, whenever the officers notified the dispatcher of such activity.

While the full year's data file was being built, we inspected a preliminary rank ordering of all addresses by call frequency. Two key findings emerged from that analysis: the top-ranked addresses were overwhelmingly commercial, and there were many classes of locations it would be inappropriate or dif~~ficult to deal with~~ to solve problems and reduce repeat calls. These locations included hospitals, police stations and city hall, to which cars were dispatched to deal with incidents that almost always occurred elsewhere. Schools were also excluded on the grounds that most schools were already assigned a full time police officer, and parks were excluded because of park police jurisdiction.

The question of intersections was considered at great length. The officers and researchers ultimately excluded intersections on practical grounds: the lack of continuity of population with which to work. In routine activities terms (Cohen and Felson, 1979), there was no consistent guardianship to build on. While intersections might be highly appropriate targets for some police strategies, such as intensified patrol presence (Sherman and Weisburd, 1988), they seemed inappropriate to the strategy planned for the RECAP experiment.

A large portion of the addresses were located in the major vice strip of the downtown area, which was scheduled for demolition during the experimental year. This block was excluded as well.

A final exclusion, at the request of the officers, was check-cashing establishments. Analysis of such establishments on the preliminary list of the top 2,000 addresses showed that most of their calls were for passing bad checks. The officers did not see how they could either reduce bad check passing in such settings, or find alternative responses not involving a police car dispatch.

The preliminary list also revealed many multiple listings of the same addresses by slightly different descriptions, which the computer treated as separate addresses for ranking purposes. Before the final list was prepared and randomized, the officers attempted to identify all the multiple listings that needed to be combined. They also agreed to visually inspect any locations which might be adjacent to or otherwise influenced by police interventions at another address on the list. The purpose of these inspections and the review of addresses from their knowledge was to insure independence of each address from all others, thus eliminating treatment contamination of the control group once it was chosen. This process was less than perfect, although generally successful.

The final decision before randomization was to block the sample of 500 addresses into half commercial and half residential. This decision was made on three grounds: the small proportion of residential addresses in the highest call frequency group of addresses, the RECAP unit's consensus that residential addresses and problems were very different from commercial places, and the objective of determining whether a RECAP strategy worked any better or differently at the two types of addresses. The police officers, using their existing knowledge of the city supplemented by a reverse telephone directory, labeled all of the top 2,000 addresses as either commercial or residential.

Randomization and Pairwise Deletions

Since we anticipated that some addresses might "die" (be closed down) during the course of the year, we decided to randomize in rank order of call frequency. A computerized random numbers generator supplied by Skamp Computer consultants was employed to assign the top 250 commercial addresses (after correction for multiple listings) into 125 experimentals and 125 controls, both listed in rank order of 1986 call frequency. The two rank order lists created 125 pairs, as distinct from random assignment within rank ordered pairs. The effect was little different, however, and allowed us to delete experimental-control address pairs without creating a sample selection bias on the dimension of call frequency. The same procedure was followed for residential addresses.

The original randomization procedures produced a slight imbalance in 1986 call totals between the two groups. After the pairwise deletions for contamination, the calls at the experimental addresses totaled 14,084 (X = 62) and 16,507 in the controls (X = 73). This imbalance was due to three factors: 1) we did not randomize within rank ordered pairs, so that steep declines from address to address could alter mean call totals within the first two or three addresses selected; 2) there were distant outliers in call volume at the very top of the distribution, compounding the effects of a truly random sequence, and 3) the actual distributions remaining after deletion of contaminated pairs were even more pronounced in this respect, apparently by chance. The first four addresses, for example, had 810, 686, 607, 479, and 471 calls in 1986, respectively. A random sequence that assigns the first three addresses to the control group would give that group a 2,103 call lead before even beginning to assign experimental cases at a much lower average level of call frequency per address.

The original randomization was modified with six pairwise deletions from the residential group and eighteen from the commercial group. The reasons for the deletions were all related to insufficient screening of the addresses for independence prior to the randomization. This, in turn, was related to the six month time frame originally planned for the experiment, and the officers' self-imposed pressure to begin "real police work"--despite the chief's encouragement to take as much time to analyze and plan as necessary. All of the officers had worked for many years in the areas in which they reviewed addresses for independence, so they were surprised to discover that several of the "independent" randomized addresses were different entrances to the same building. In some cases, this meant that the same building was assigned to the experimental and control group. Each case of this overlap required the deletion of two pairs, because both the control and experimental listings were matched to different addresses.

In other cases, a pair was deleted because our surveillance of the control address showed that it had been torn down; in still other cases it was because the business had been closed.

The final sample of residential addresses had a baseline period mean of 74 calls per address in the controls and 63 calls in the experimental group, with medians of 69 and 59, respectively. The commercial sample had a baseline mean of 72 calls per address in the controls, and 61 calls in the experimentals, with medians of 52 and 49, respectively. The lesser magnitude of difference in call volume for the medians than for the mean demonstrates the effects of a few outliers on total call volume differences.

The "baseline period" was retrospectively defined as the 1986 period comparable to the period after the (1987) "start date" for the experimental member of each pair, the day on which the RECAP officer assigned had his first contact with someone located at or responsible for the address. This definition of baseline period controls for the substantial effects of seasonality on call totals in the experimental site, as well as the fact that the large sample size and small number of officers prevented simultaneous application of the intervention at all experimental addresses.

We did not discover until a colleague's review of the data after the end of the experiment (Weisburd, 1988) that among the highest volume addresses, there is substantial instability in the year to year totals of calls to police. Of the 226 addresses in the control group subjected to no special police intervention, with a mean of 73 calls each in the baseline period, almost one third of them (67, or 30%) experienced 50% reductions or increases in total calls during the experimental period; 29 (13%) went up, and 38 (17%) went down. This unexpected instability greatly reduced the statistical power of the experimental design, and should be a key factor in designing police strategy for problems in general and repeat call addresses in particular.

A further limitation was a series of problems we discovered in the validity of the computerized call data (Sherman, Gartin and Buerger, 1989). Some addresses were merely pay telephone locations, at which events happening at different locations were being reported. Other addresses had their call histories fragmented into many other file descriptions not detected by the review of the top 2,000 addresses. Most maddening was the computer's "mirroring" effect, in which the exact same call record was entered twice and counted in all the totals presented here as completely independent calls. Our subsequent research on that problem suggests that mirrors can account for up to 15% of all dispatched calls for service (Buerger, 1989). Finally, we were unable to segregate the 1986 "closed call" (record only; no car sent) data from calls actually dispatched. The 1987 data are limited to the latter category.

For all these limitations, the design still offered substantial improvements over the quasi-experimental, single problem trend evaluations in earlier tests of POP. Most important was the control on "history" effects (Cook and Campbell, 1979). We were able to take into account the strong downward trend in all calls city-wide--which was caused partially by RECAP--as a rival theory of why calls went down substantially across all RECAP addresses. Both the control group and the large number of cases gave the design strong internal validity. The tradeoff was that the large number of cases greatly--yet realistically--limited the dosage of problem-solving the department could apply to any one target address.

Comparability of Experimentals and Controls

Despite the deletions, the experimental and control addresses showed generally similar distributions within both residential and commercial groups. In the commercials, for example, there were 11 office buildings in the controls and 13 in the experimental; 14 vs. 13 grocery stores, 7 versus 6 hotels, 31 vs. 28 retail stores (including 20 vs. 17 convenience stores), and 7 vs. 7 taverns. The largest differences were 4 vs. 16 restaurants, 4 vs. 11 social service providers, and 0 vs. 3 bus depots. These differences are all in the direction of making the experimental sample more difficult, although the control sample had the larger call totals.

When observed by researchers after the experimental year, the control and experimental commercial samples had comparable numbers of addresses open 24 hours (40 vs. 46), offering sales of alcohol (81 vs. 86) and location on a bus route (100 vs. 103). In the residential sample, these 1988 observations showed somewhat greater differences on pre-existing characteristics, such as 87 of the controls and 67 of the experimentals on a bus route, and 48 vs. 77 selling alcohol legally. Yet the mean numbers of units per residential building were almost identical (62 vs. 64).

These samples, like places in general, are so heterogeneous in the average population size and nature of activities that it is impossible to say whether they were different in any theoretically important way. We have too little theory of the criminology of place to know whether any one specific chance difference could have affected the results. From all apparent indications, the random assignment created a rough equivalence between the groups, making the comparisons in the findings below appropriate.

The RECAP Treatment

What did the RECAP officers actually do? It varied. It varied tremendously, across both addresses and officers. Just as we expected, an open-ended technology (problem-solving) stressing creativity produced a highly heterogeneous set of treatments. It also produced wide variations in the amount of attention each address received.

The officers were trained to follow the four steps developed in the

RECAP design (Sherman, 1985). First was the diagnosis of the problems causing repeat calls, using as many sources of information as possible: call histories, offense reports, interviews on site, interviews with responsible guardians as far away as corporate headquarters in other states. Second was development of an action plan, for discussion with other unit members and the approval of the unit commander. Third was implementation of the plan, either by police or by others responsible for the premises. Fourth was the weekly followup of call trends at the address, to see whether further efforts were required.

What the officers actually did was determined from several data sources. An on-site observer was present continuously throughout the experiment, taking detailed daily notes during the last seven months of the field test. The observer also produced the weekly computer printouts on the calls at each target address, and gave them to each officer. The principal investigator, who had guided the selection of the unit's officers, was in weekly contact with the unit commander. At the end of the first year, the department authorized the officers to take up to two months to write up a case study of all of their addresses (Buerger and Sherman, 1989). In that same time period, the officers filled out a questionnaire on each of their assigned target addresses and how they had handled the case.

City-Wide and Address-Specific Treatments

From all these sources, we know that the unit did two basic things. One was to work on the target addresses. The other was to follow the logic those addresses suggested to them: city-wide problem-solving. Unfortunately, the latter efforts undermined the power of the test of the former efforts.

Their diagnosis of many addresses often suggested city-wide solutions. Some of these were "load-shedding" in nature, such as car lockouts (which the city council had already debated eliminating in favor of private locksmiths). These accounted for 4.4% of the 1986 calls at experimental addresses, but 5.4% of all 1986 calls city-wide. Eliminating those responses in 1987 clearly helped achieve the reduction in total calls. RECAP also persuaded the department to stop responding to some 2,000 calls a year for no-pay driveoffs from gasoline stations. Most important, perhaps, RECAP prodded the department into better compliance with its policy for arresting misdemeanor domestic violence suspects, doubling the number of arrests after July of 1987.

In other cases, treatment spillover was unavoidable. Common ownership of multiple addresses in both the experimental and control samples made it impossible to restrict treatment to the experimental group. Convenience stores and apartment buildings were particularly prone to this limitation, as was the public housing authority. Meetings with guardians of experimental properties may (or may not) have affected their guardianship of control properties in unknown ways.

The vast majority of the RECAP unit's effort, however, was addressed to the target addresses. In the first half of the year, the most common effort was to empower resident property managers to deal with problem tenants more decisively. Officers provided documentation of repeat calls to specific apartments, which was an effective basis for obtaining otherwise hard-to-achieve evictions. But they were frustrated by landlord reluctance to evict troublemakers who paid their rent on time, especially suspected drug dealers. Landlords were often more interested in evicting less serious repeat call tenants who were also less punctual with their rent. Nonetheless, eviction and threat of eviction was a common tactic.

Another residential tactic was the "napalm letter" for domestic violence. One RECAP officer left a letter under the door of all his assigned apartment units with repeat domestic calls, advising them that they had been identified as repeat call problems and urging them to call the RECAP office immediately. The letter produced a flood of calls (mostly from women), followed by police advice to seek counseling or consult a shelter. Once the dialogue with domestic violence victims was initiated, some victims were reluctant to cut it off. Several repeatedly called their RECAP officers for assistance, thereby turning a proactive strategy into a reactive one.

Other residential tactics included raiding a drug dealer (producing the largest heroin seizure on record in the city), condemning apartments with no locks on the doors, and pressing landlords to control tenants with repeat noisy party complaints. In general, the landlords were very hard to reach, often unresponsive, and often told the resident managers not to cooperate with the RECAP plans. As the year progressed, RECAP used more coercive and less persuasive tactics with the landlords, such as threatening to have their certificates of occupancy revoked, and threatening to prosecute one of them for welfare fraud (for renting a mailbox for welfare checks to an out-of-state woman).

The commercial tactics were more heterogeneous than the residential. One officer persuaded the owner of a high crime parking garage to raise the monthly fee to finance fencing around the open perimeter of the garage.

Others organized an undercover investigation of drug dealing and serving of intoxicated persons in a high-crime tavern, leading to the suspension of the bar's liquor license--a first in the history of the department. (A second, even more violent tavern not in the experiment was closed the same way just after the end of the experimental year). A liquor store near a high-crime park and the number one robbery intersection in the city was reminded not to sell to intoxicated patrons--an attempt to reduce the supply of suitable victims. A gas station convenience store was threatened with loss of business license if it continued to ignore shoplifters and disorderly persons on the premises. A large discount store and the bus depot were both encouraged to hire off-duty officers to provide better guardianship to their large client populations.

Both residential and commercial samples were subjected to a wide range of levels of effort. Some addresses had as little as one contact during the entire year. Others had weekly contacts initiated by the citizens. One officer went so far as to drive one resident many miles out of the city to place her in residential treatment for alcoholism. Another officer obtained long-term commitment to a state mental hospital for a particularly disruptive homeless man afflicting several of the officer's addresses. Such efforts were enormously time-consuming, and probably not justified by the call volume at stake. But once they made contact with these problems, the officers could not ignore their human dimensions. Like other areas of police discretion, their followup work at each address was determined by their assessment of the seriousness of the problems and the amount of disrespect shown to the police (Black, 1971; Smith and Visher, 1981).

The researchers encouraged the officers to engage in a systematic triage, with some addresses put on the back burner, others checked on occasionally, and major effort invested wherever they thought they could achieve the greatest call reduction. This translated into their working the most on the cases with the greatest increases in calls over the previous year, which were identified for them in the weekly followup calls for service printouts. The questionnaires officers filled out on each of their addresses after the end of the experimental year showed that there was a moderate inverse relationship between their self assessment of level of effort and any reduction in calls ($R = .22$, $P < .05$).

In sum, the RECAP experiment was like a medical experiment in which all the patients had different diseases, with different levels of severity. The doctors gave the patients different pills, with different levels of dosage, at different intervals and for differing durations of treatment. The treatments were varied according to the emotional context of the interaction between the patients and doctors, and some of the pills were given to the control group. All of these factors limited the statistical power of the test. But they did not make it very different from many actual medical experiments (Pocock, 1983).

Nor did they make it very different from the major purpose of the experiment, which was to test the capacity of a police department to develop such a strategy focused on chronic addresses. Indeed, the test of the strategy depended upon the heterogeneity of the treatments. The hypothesis was merely that the strategy in general, rather than any specific tactics, could make a difference. An additional hope was that the officers would be able to prevent as many calls as they would have answered had they remained on patrol duty during the experimental year (about 1,000 per officer, or

4,000 calls).

Experimental Findings

Table 1 shows that the strategy did make a modest difference in total calls for the residential addresses, although not for the commercial addresses. But in its first developmental year, the strategy was apparently unable to pay for itself. Rather than preventing 4,000 calls, it was only able to prevent 475 within the experimental design. The unit should also be given credit, however, for load-shedding some 2,000 gas station no-pays annually, and for whatever number of repeat domestic violence calls were deterred by the doubling of domestic violence arrests after July of 1987 (cf. Sherman and Berk, 1984).

A more theoretically appropriate analysis examines the number of addresses at which calls were reduced or increased more than five percent, or the prevalence of call reductions rather than the frequency of calls. Table 1 shows no statistically significant differences using a six-celled chi-square test.

Tables 2 and 3 suggest that had the experiment stopped at six months, it would have produced a net gain of 597 calls prevented (or almost 1200 on an annualized basis), a difference in the residential sample not likely ($P < .01$) to be due to chance. The second six months of the experimental year showed no call reduction at the residential target addresses, and the commercial sample showed a statistically significant difference in favor of the control group in the prevalence of addresses with increases or decreases in calls. Figure 1 portrays the total call trends more clearly, with the last two quarters presented separately.

Several hypotheses might explain the dramatic change in effects after six months, which was replicated at our request by an independent statistician analyzing the same data set (Larntz, 1988). One is the change in command from one sergeant to another, with the style of the first sergeant more inclined towards persuasion and the style of the second more towards confrontation. It is hard to say whether the changed command style produced a change in the officers' approach, since the observers also changed at about the same time. (It is even possible that the change in observers influenced the change in effect, although all three writers, including the second observer, doubt it.)

A second hypothesis is that the novelty of confronting property guardians and troublemakers with their being "on the list" was beginning to wear off. A pattern of initial deterrence, with decaying or diminishing effect thereafter, is common to police crackdowns in general (Sherman, 1989). The early fear that police may do something terrible to you may wear off as you get used to the idea, and become more skeptical that they will actually do anything anyway.

A third possibility is that the officers themselves became burned out by working on the same addresses, confronting the same obstacles and frustrations. If chronic problems are generally not amenable to ready solutions, it may be better to rotate across many targets rather than to dig trenches around a few of them (Sherman, 1989). This was the basis for the officers' own recommendation at the end of the project that new targets be selected every three months, rather than once a year. They also recommended that the caseload be limited to 10 addresses per officer, rather than over 50. Fewer problem addresses for shorter periods of time was much more to their taste. In retrospect, four separate quarterly experiments (with different targets selected) might have been an even more powerful design than one large experiment, but the knowledge needed to make that judgment was not available in advance.

Year-long analysis of effects by specific call types are even more puzzling. The residential locations, relative to the control group, showed a 21% reduction in assault, a 12% reduction in disturbances, and a 15% reduction in calls related to drunkenness. [There was no difference in domestic calls, perhaps because of the city-wide policy change.] The commercial targets showed a 9% reduction in theft calls, and a 21% reduction in shoplifting calls at 7 stores participating in a special program for issuing their own citations. Yet residential burglaries were up 27% compared to the controls, and calls for commercial predatory crimes (criminal sexual conduct, robbery and kidnapping combined) were up 28% at the experimental addresses relative to controls. We have no speculation to offer that can account for these statistically significant results.

Finally, to whatever extent RECAP was successful in preventing calls for service at the residential locations, we must note two limitations on the assumption that crime was prevented. One limitation is that calls may have declined while crime stayed constant or increased. The message some people heard from RECAP officers may have been "don't call the police," rather than "don't cause trouble." Although police generally took great pains to distinguish those two messages, there are no guarantees in interpersonal communication.

A second limitation is potential displacement. While the extent of displacement may have been greatly exaggerated (Cornish and Clarke, 1988), it seems especially plausible in this case because of the evictions. A deterrent effect from such evictions could also be plausible, but we have no way of knowing. Limitations of funding and foresight prevented our tracking individual names of persons whose departure from an address was part of the action plan, to see if they showed up at other locations to cause trouble again. In two cases persons evicted from one RECAP address did move into another target address.

Yet locational strategies do not need to disprove displacement to be successful. Filling the swamps may not ultimately prevent malaria if the mosquitoes go elsewhere to breed. But it does prevent the disease in that place, for as long as the swamp stays filled. And the more swamps filled, perhaps the less malaria there will be in the long run.

Conclusions

These mixed results may be disappointing to those who expect revolutionary improvements in effectiveness to come from philosophical revolutions in police practice. They certainly suggest that merely focusing police attention on chronic problems cannot guarantee their solution. Like the first random assignment test of community crime prevention efforts (Pate, et al, 1986), the results of a test with objective target selection seem far more modest than the results of quasi-experiments using subjective target selection (e.g., Lindsay and McGillis, 1986; Schneider, 1986). When the most troublesome addresses in a city are intentionally selected as targets, perhaps a more appropriate goal would be "managing" rather than "solving" (Eck and Spelman, 1987) problems.

Nonetheless, the results are far better than those for many recent criminal justice innovations. First, the treatment was actually implemented, which is far from always the case with government programs in general (Pressman and Wildavsky, 1973) and community crime prevention in particular (Rosenbaum, 1986). Second, there was a clear, if deteriorating, effect on residential locations, reducing disorder and improving the quality of life--even without foot patrol (Wilson and Kelling, 1982). Third, even the generally unsuccessful commercial locations had some major isolated successes, such as the liquor license suspension of a bar implicated in hundreds of robberies and several recent murders.

It is important to stress that this experiment was conducted without any dry run or prior practice in the strategy or tactics. In terms of medical experiments testing new drugs, it skipped the first two usual stages of testing well persons for drug toxicity, and a small number of ill persons for effectiveness of the drug (Pocock, 1983). While both these stages had been previously accomplished in other cities (Goldstein and Susmilch, 1982; Eck and Spelman, 1987), they had not been part of the preparation in this city. On several occasions, the RECAP team suggested that we repeat the experiment to test their effectiveness at a higher level of skill. It is entirely possible, although by no means certain, that results might improve over time with different targets. The declining effectiveness over the first year shows that experience alone is no guarantee, and may be no substitute for initial enthusiasm.

Most important, the experiment lends modest support to a public health model of crime prevention. Without better understanding of the criminogenic conditions which produce high-crime places, it will continue to be difficult to diagnose and treat such problems effectively. But experienced patrol officers clearly appreciate the concept of treating causes, and police managers in Seattle, Kansas City, St. Paul, and Edmonton have already adopted some form of repeat call analysis based on the discovery of address-specific "hot spots." Future experiments might focus on specific tactics for treating specific types of problems (like domestic violence) or specific types of places (such as high-crime taverns). With an accumulation of findings from such tactical experiments, we may be able to advance both the theory and practice of locational crime prevention.

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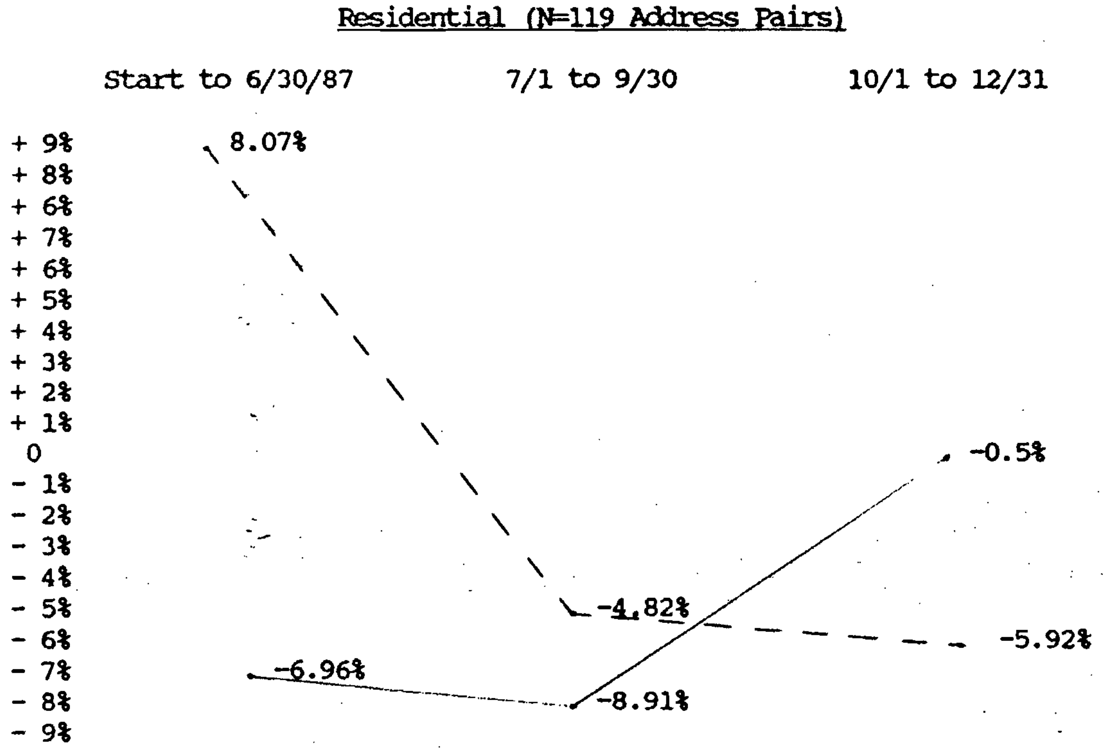
Percentage Changes in Call Totals from Comparable 1986 Period
to Three 1987 Periods by Experimental and Control Addresses



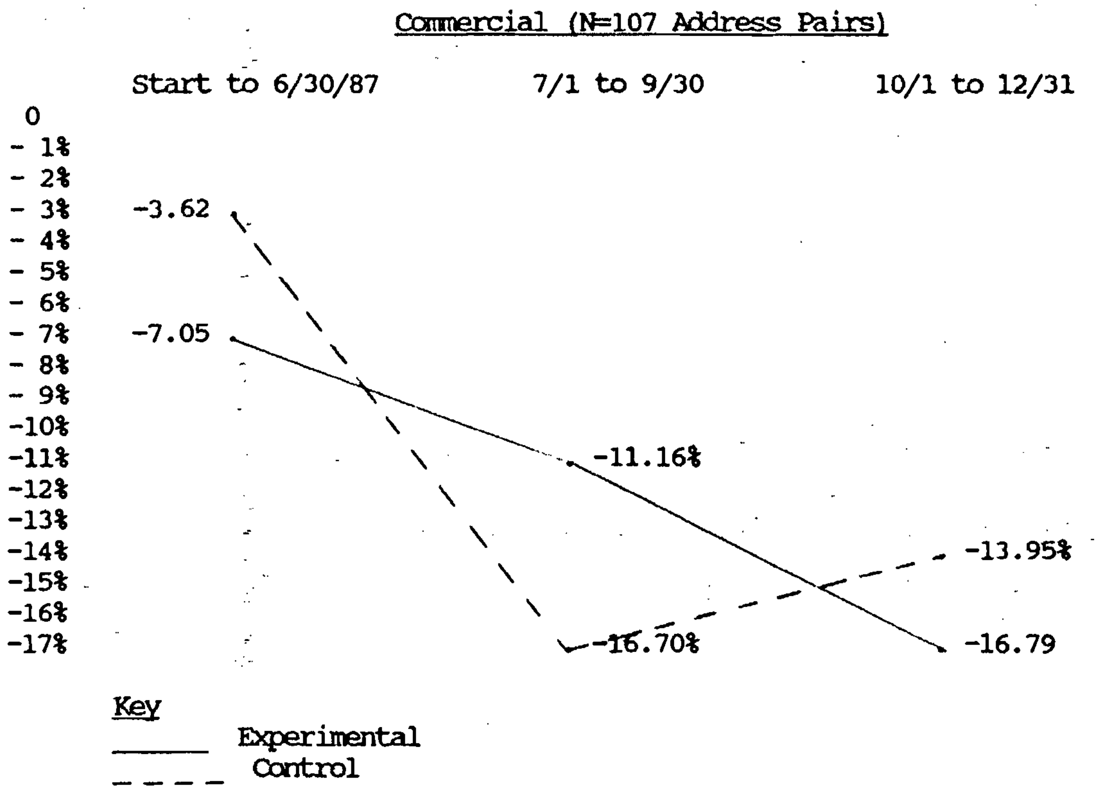


Table 1

Changes in Call Totals and Prevalence of Reduced and Increased Call Addresses From Comparable 1986 Period to 1987 Period From Start Date For Each Address to 12/31/87, By Experimental and Control Addresses

Residential (N=119 Address Pairs)

Treatment Group	Year 1986	1987	% Change	5% Down Addresses	5% Up Addresses
RECAP Targets	7,507 calls	7,056 calls	-6.01%	62 (57%)	44 (37%)
Controls	8,816 calls	8,825 calls	+0.10%	55 (46%)	49 (41%)

Net Experimental Reduction of 6.11%, or 458 calls X2=.83, df=2, P=.66

X2 = 7.84, df= 1, P = .005

Commercial (N = 107 Address Pairs)

Treatment Group	Year 1986	1987	% Change	5% Down Addressess	5% Up Addresses
RECAP Targets	6,577	5,856	-10.96%	71 (66%)	29 (27%)
Controls	7,691	6,868	-10.70%	66 (61%)	36 (34%)

X^2 = .012, df=1, P=.914 X2=1.27 , df=2, P=.53

Net Experimental Reduction of 0.26%, or 17 calls

Use these for odds ratios

Residential
OR=1.065
1.0193-1.1128

Commercial
OR=1.0029
.9559-1.0522

226 total pairs

Just use the Chi Square

Table 2

Changes in Call Totals and Prevalence of Reduced and Increased Call Addresses From Comparable 1986 Period to 1987, By Time Period and Treatment Group

Residential Sample (N=119 Address Pairs)

Start to 6/30

Treatment Group	Year 1986	1987	% Change	5% Down Addresses	5% Up Addresses
RECAP Targets	3,348 calls	3,115 calls	-6.96%	65 (55%)	46 (39%)
Controls	3,570 calls	3,858 calls	+8.07%	58 (49%)	50 (42%)

Net Experimental Reduction of 15.03%, or 503 calls X2=1.04,df =2, P=.6

X2 = 19.20, df=1, P = 0.000

7/1 to 12/31

Treatment Group	Year 1986	1987	% Change	5% Down Addresses	5% Up Addresses
RECAP Targets	4,159	3,941	-5.2%	64 (54%)	46 (39%)
Controls	5,246	4,967	-5.3%	59 (50%)	51 (43%)

X2 =0, df =1, P = .99 X2=.46, df = 2. P=.79

Net Experimental Increase of 0.1%, or 4 calls

Table 3

Changes in Call Totals and Prevalence of Reduced Call Addresses From Comparable 1986 Period to 1987, By Time Period and Treatment Group

Commercial Sample (N=107 Address Pairs)

Start Date to 6/30

Treatment Group	Year 1986	1987	% Change	5% Down Addresses	5% Up Addresses
RECAP Targets	2,750	2,556	-7.05%	64 (60%)	36 (34%)
Controls	3,067	2,956	-3.62%	59 (55%)	41 (38%)

Net Experimental Reduction of 3.43%, or 94 calls

X2 =.5, df=2, P=.77

X^2 =.892 , df=1, P = .345

7/1 to 12/31

Treatment Group	Year 1986	1987	% Change	5% Down Addresses	5% Up Addresses
RECAP Targets	3,827	3,300	-13.8%	57 (53%)	38 (36%)
Controls	4,624	3,912	-15.4%	72 (67%)	33 (31%)

X^2 = .332, df=1, P= .565

X2=9.24, df=2, P=.0099

sig diff in favor of ctrl grp

Net Experimental Increase of 1.6%, or 61 calls