The Crime Displacement Hypothesis: An Empirical Examination

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It has been suggested that crime prevention programs comprising deterrence and/or target-hardening components are futile, since they fail to deal with primary criminogenic factors. According to this position, efforts to suppress criminality will result in compensating behavior on the part of those committed to criminal life styles. Crime displacement, this adjustment to circumvent preventive measures, can occur along three fundamental dimensions—the spatial, temporal, or qualitative. Irrespective of which mode of adaptation is adopted in a given situation, displacement ideally manifests itself in the stabilization of crime rates. The displacement hypothesis was subjected in this study to empirical analysis through the evaluation of a residential burglary prevention program that involved the marking of household property. Three forms of displacement were examined: the possibility of a shift in criminal activity from the homes of program participants to those of nonparticipants, a shift in crime from residences to businesses, and the theft of unmarked rather than marked merchandise. Statistically significant indicators are found for the first form of displacement only; however, for the latter two types of displacement, changes in the patterns of burglary did occur in the direction consistent with the hypothesis.

Until recently, evaluations of crime prevention programs have shared the underlying assumption that the scope of criminal behavior is restricted to the terms set by the evaluative study. Such assessments have generally focused only on the form of violation the program was designed to prevent, with little consideration given to the possibility of a concomitant change in the frequency with which other offenses are committed in the area studied. Also, the urban areas under study have been treated as if self-contained, as if offenders are not able to cross the boundaries of the targeted areas for the duration of the study.

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The alteration of criminal activity as a consequence of preventive efforts is referred to as crime displacement. An ostensibly effective prevention program may actually lead to changes in criminal behavior so that offenders can circumvent the preventive measures instituted. Offenders may relocate the site of their activities; they may select different targets within the original site; they may alter the tactics used or the time of their violations; or they may even engage in different forms of criminality.[1]

The crime displacement hypothesis is consistent with a hydraulic model of criminal behavior, whereby the offender is viewed as responding compulsively to adverse biological, psychological, economic, and/or social conditions. The manifestations of this response can be deflected but not eliminated by ad hoc measures.[2] It is interesting to note that displacement is also explicable under a rationalistic, utility maximization framework, as the offender can be seen to be responding to objective changes in the system of costs and benefits.[3]

A third possible explanation for displacement, in this case more appropriately termed spurious displacement, may be derived from the criminal justice system's resistance to reductions in crime rates resulting from successful prevention programs. As some persons and groups have a vested interest in stabilizing crime rates, discretionary powers may be used to counteract reductions in certain offenses by more stringent enforcement of others.[4] In fact, dramatic reductions in certain offenses may result in appeals for criminalization of behaviors not formerly sanctioned.[5] This type of stabilization yields what has been referred to as a statutory displacement effect.[6]

The concern here is the genuine alteration of offenders' behavior, rather than the effects of adjustment mechanisms operating in criminal justice systems. A previous review of the literature revealed that displacement has been a neglected aspect of evaluative research; the few studies of displacement that do exist are flawed by numerous methodological problems.[7] Nevertheless, it should be noted that all five forms of displace-

1. Thomas A. Reppetto, "Crime Prevention and the Displacement Phenomenon," *Crime & Delinquency,* April 1976, pp. 166-77.

2. George B. Void, *Theoretical Criminology,* 2d ed. (New York: Oxford University Press, 1979), pp. 9-10.

3. Jan Palmer, "Economic Analyses of the Deterrent Effect of Punishment: A Review," *Journal of Research in Crime and Delinquency,* January 1977, pp. 4-21.

4. Edwin M. Schur, *Radical Nonintervention: Rethinking the Delinquency Problem* (Englewood Cliffs, N.J.: Prentice-Hall, 1973), p. 131.

5. Kai T. Erikson, *Wayward Puritans* (New York: John Wiley, 1966).

6. Planning and Management Consulting Corporation, *Crime Incidence and Displacement Model* (Santa Barbara, Calif.: Planning and Management Corp., 1974).

7. Thomas Gabor, "Crime Displacement: The Literature and Strategies for its Investigation," *Crime andlet Justice,* vol. 6, no. 2 (1978), pp. 100-07.

ment have been observed. By shedding light on the extent of offenders' mobility and flexibility (in committing a variety of offenses and employing different modus operandi), displacement studies can aid in efforts to assess the usefulness of focused, localized prevention programs.

Peter Lejins has identified three types of prevention programs. The first, punitive prevention, involves the deterrence of potential offenders through the threat of punishment and prevention through incapacitation. The second type, corrective prevention, is the elimination of criminogenic psychosocial conditions. Finally, a third type of preventive activity, mechanical prevention, aims to reduce criminal opportunity through "target hardening" and increased surveillance levels in the community.[8]

THE STUDY

Selected for the displacement study was the property-marking theft prevention program called Operation-Identification (O-I). Program participants engrave an identification number on movable property (usually household property, although commercial establishments occasionally are involved), and decals are placed on the front and rear doors of the participant's residence (or place of business) as a warning to prospective intruders. Proponents of O-I claim that its objective, burglary prevention, can be attained because of burglars' reluctance to handle material that is readily identifiable. Possession of marked property[9] increases the likelihood of a burglar's apprehension and conviction and makes the disposal or conversion of such property more difficult.

The program is both punitive, in its threat of apprehension and conviction, and also a form of mechanical prevention, in its reduction of criminal opportunity. Since it can be hypothesized that displacement effects are more likely to occur as a result of punitive or mechanical, as opposed to corrective, measures, the O-I program was particularly appropriate for the present study. Evaluations of O-I programs show contradictory findings, although many of these studies lack any type of controls.[10] Clearly, program effectiveness, in its conventional sense, is a precondition for displacement.

The objective of the study was to determine whether displacement occurred as a consequence of an O-I program implemented in Nepean, a township located south of Ottawa, Ontario. The program began in March 1975 and was monitored through August 31, 1976. One of the nine police

8. Peter Lejins, "The Field of Prevention," in *Delinquency Prevention: Theory and Practice,* William E. Amos and Charles F. Wellford, eds. (Englewood Cliffs, N.j.: Prentice-Hall, 1967), pp. 1-21.

9. N. B. Heller et al., *Phase 1—Evaluation of Operation Identification* (St. Louis, Mo.: Institute for Public Program Analysis, 1975).

10. Ibid.

patrol zones in Nepean was selected as the site for the study. A police patrol zone was selected rather than a political jurisdiction or census tract because this permitted an assessment of police behavior in the area during the monitoring period. Changes in enforcement activities could produce a confounding influence on the crime rate, possibly obscuring or exacerbating the effects of O-I. Furthermore, natural geographic boundaries virtually separated this zone from the rest of the township. Thus, it could be assumed more safely than in other areas that program effects would be contained within the zone and the influences of adjacent communities would be minimized.

The extent of program membership in the zone studied also favored its selection. By the end of the study, 171 of 1,093 (15.6 percent) occupied dwellings were enrolled, while only 1 of the 625 operating businesses in the area was protected by the program. These conditions were conducive to the examination of at least two forms of displacement: first, the possible displacement of burglaries from the dwellings of participants to those of nonparticipants (here referred to as spatial displacement) following the implementation of O-I; and second, target displacement, or the possibility that, because of the relatively high enrollment of households in the program, residences in general would become less desirable as burglary targets, whereas businesses, which did not participate, would become more attractive.

A third form of displacement studied was tactical displacement, or the change among offenders in their modus operandi in response to the O-I program. Since marking property does not directly prevent its theft, the tactics discussed here do not refer to the methods used by the offender to secure the property but to the means used in circumventing the preventive measure—which, in this case, involved avoiding marked merchandise. Displacement could be indicated by the increased theft of unmarked items.

The two other forms of displacement identified in the literature were not studied. Temporal displacement was not applicable since the coverage offered by O-I is continuous. Nor was the possibility of displacement from burglary to other offenses studied, because data on numerous types of offenses, specific to the zone studied, were unavailable.

The data used in this study comprised all cases of breaking, entering, and theft in the designated zone reported to the Nepean Police Department from 1970 through 1976. Rates of breaking and entering were computed in terms of occupied units and were adjusted for seasonal and random effects, where applicable.[11] Each apartment, private house, and commercial establishment was considered as one unit. Premises, rather than

11. John A. Neter and William Wasserman, *Fundamental Statistics for Business and Economics* (Boston: Allyn and Bacon, 1966).

population, were used as the base for computing the burglary rates because of the frequent observation that offenses tend to cluster in areas according to the availability of criminal opportunity presented by dwellings and businesses, rather than according to area population size.[12]

Various procedures have been employed in past studies to probe for displacement. A critical review of these methods has been undertaken elsewhere by the author.[13] In this study, the possibility of spatial and target displacement was examined through time series analysis, a procedure that enabled assessment of the immediacy, duration, and possible changing character of program effects. Quarterly observations of burglaries were made for all residences and businesses from March 1973, two years before the inception of O-I, through August 1976, eighteen months after implementation. In studying spatial displacement, an attempt was made to ascertain whether residential program participants were becoming significantly less vulnerable to burglary than they had been before O-I and whether unprotected dwellings were becoming significantly more vulnerable as a result of the program. In probing for target displacement, burglary among all residences was compared with burglary among all businesses. If the former exhibited a significant reduction in burglary with the onset of the program and the latter an increase, displacement could be posited. In the study of tactical displacement, items taken during burglaries were examined before and after the program was initiated. In this case, observations were annual rather than quarterly because of the small number of residential burglaries for which items taken were known. This situation precluded the use of inferential statistical techniques.

In probing for spatial and target displacements, two forms of analysis were used to ascertain the effects of O-I. First, the pre- and postprogram changes in overall burglary rates were examined through Burr's formula for the comparison of trend slopes.[14] Then, to gauge more precisely the dynamics of the suggested displacements, the observed and expected burglary rates were compared at quarterly intervals during the period after the program was begun. The expected rates with which the actual rates were compared were calculated through bivariate, linear regression analysis.[15]

For the study of spatial displacement, residences were divided into two cohorts: participants and nonparticipants. Membership was determined by a dwelling's status at the final point of observation in the study (August 1976). All residences were monitored for victimizations over the three and one-half years of observation, and the two cohorts were com-

12. Peter A. Engstad, "Environmental Opportunities and the Ecology of Crime," in *Crime in Canadian Society,* Robert A. Silverman and James J. Teevan, Jr., eds. (Toronto, Canada: Butterworth, 1975), pp. 193-211.

13. Gabor, "Crime Displacement."

14. Irving W. Burr, *Applied Statistical Methods* (New York: Academic Press, 1970).

15. Neter and Wasserman, *Fundamental Statistics for Business and Economics.*

pared. This ex post facto method of selecting units and retracing groups' activities over time is referred to as retrospective cohort analysis.[16] Changes in burglary rates among the two cohorts, following the onset of O-I, could be evidence of displacement if participants' rates decreased and nonparticipants' rates increased (taking into consideration the general trend in burglary rates).

The first procedure undertaken in investigating dwelling-to-dwelling displacement was ascertaining O-I's effects on residences in general. Disregarding the phenomenon of displacement, an effective program would reduce the overall burglary rate for residences. Displacement would be most apparent with a stabilization of this overall rate. However, this was not a prerequisite for a finding of program effectiveness or displacement, because other factors operating independently of the program could have accounted for a change or stabilization in overall burglary rates. The purpose of the trend analyses was to gauge such contemporaneous effects.

After the overall effects of O-I on residences were determined, dwellings that remained unprotected by the program were compared with protected premises through a difference-of-means (t) test. Unprotected dwellings, at each point of observation, were those that remained nonparticipant cohort members over the course of the program as well as participant cohort members not yet participating at a given point of observation. Since apparent differences might be due to prior differences in general vulnerability rather than to O-I, the two cohorts were observed for differences in victimization before the program.

The cohorts were also examined individually for clues as to program efficacy and displacement. Examination of the participant cohort to judge the effects of differing points of entrance into the program extended to (1) the victimization of all units before the onset of the program; (2) burglaries among units that had not yet joined O-I at the point of observation; and (3) burglaries among this latter group after joining O-I. Comparisons of (1) with (2) and of (2) with (3) were seen as a way of demonstrating program effects. The results of such comparisons were likely to be more reliable than results obtained from comparisons of different cohorts, because of the possible nonequivalence of the cohorts in terms of initial vulnerability. Such nonequivalence could be reflected in a disproportionate enrollment in the program of the more prudent members of the community.

Finally, the nonparticipant cohort was examined both before and after O-I's onset. It would be expected that as the number of program participants increased, the more likely it would be that members of the nonparticipant group would become targets of burglary. Members of this cohort would be expected to experience an accelerating increase in burglary as a result of the program.

16. Brian MacMahon and Thomas F. Pugh, *Epidemiology: Principles and Methods* (Boston: Little, Brown, 1970).

Target displacement, the suggested shifting of offenders' activity from residences to businesses because of nonparticipation in O-I among commercial establishments, was studied in a similar fashion. First, changes in the overall community burglary rate (residences and businesses) that were attributable to O-I were examined. Residences and businesses were then observed separately for O-I's effects. Theoretically, if displacement did occur, one would expect a stabilization of the overall rate, with a decrease in the residential rate and a concomitant increase in the commercial rate.

Tactical displacement as a result of the program, meaning a change in items taken during burglaries from those marked to those unmarked, could conceivably occur in two ways. First, a burglar, having entered a home and having realized that numerous articles are protected, might select unmarked property. Second, if the offender were aware of such a program, he might shift his attention to other articles in planning the burglary; hence, the program might alter his specific objectives.[17] Since tactical displacement could occur regardless of the victimized unit's participation in the program, all residential burglaries from 1970 through 1976 were examined for merchandise taken.

RESULTS

Because of the low number of burglaries committed in the area examined, there is a possibility of distortion by "random" events. Consequently, these findings may be regarded with skepticism by those operating from a purely statistical perspective. From a substantive standpoint, however, knowledge of criminal activity in a community of this type, as well as crime-reporting and recording mechanisms in such an environment, may offset such concerns. Demographically stable residential communities such as this are characterized by stable police patrol operations, as well as low turnover in police departments. Both tend to minimize changes in bookkeeping practices. Demographic stability also minimizes numerous potentially confounding influences on crime rates.

The selection of a police patrol zone for analysis permitted the monitoring of police activity in the area, limiting the confounding influences of variability in crime detection and reporting. Also, it will be recalled, the choice of the particular patrol zone studied was made on the basis of its geographic isolation from adjacent zones, minimizing external influences. Finally, well-integrated communities with good relations between police and the community and low crime rates have considerably higher victim reporting rates than do communities with poor relations between police

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17. Heller et al., *Phase 1—Evaluation of Operation Identification.*

Table 1. Breaking and Entering Offenses for Total Residences and the Participant and Nonparticipant Cohorts (March 1973–August 1976)

Time Period[a]	B&E's[b]	Total Units	Rate[c]	S.A.R.[d]	B&E's	Total Units	Rate	B&E's	Total Units	Rate
		Total Residences			Eventual Participants			Eventual Nonparticipants		
Preprogram										
1	1	889	1.12	1.68	0	131	0	1	758	1.32
2	2	906	2.21	2.31	0	134	0	2	772	2.59
3	3	922	3.25	2.67	0	136	0	3	786	3.82
4	4	961	4.16	3.27	0	142	0	4	819	4.88
5	1	978	1.02	3.97	0	145	0	1	833	1.20
6	7	990	7.07	4.03	1	148	6.76	6	842	7.12
7	4	1000	4.00	3.99	0	150	0	4	850	4.71
8	4	1027	3.89	3.34	1	155	6.45	3	872	3.44
Program Commences										
9	1	1040	.96	3.73	0	151	0	1	882	1.13
10	2	1049	1.91	5.20	0	147	0	2	889	2.25
11	13	1058	12.29	5.76	3	129	23.26	10	896	11.16
12	8	1078	7.42	6.54	1	44	22.73	7	911	7.68
13	2	1087	1.84	6.48	0	21	0	2	917	2.18
14	8	1093	7.32	5.37	0	0	0	8	922	8.68

[a]Three-month intervals starting with March 1973.
[b]Breaking and entering offenses.
[c]Rates per 1,000 occupied units.
[d]Seasonally adjusted rates.

and residents, with large transient populations, and where opinions of police effectiveness are low.[18] Official crime figures in the former type of community more closely depict actual crime rates than do those in the latter type. In Nepean, citizens reported the most trivial offenses imaginable. This lends credence to the comprehensiveness of crime reports in such communities.

Spatial Displacement

In the first part of the study, the burglary rate for all residences was observed. Table 1 indicates that, before the intervention period, the seasonally adjusted rates for total residences fluctuated between 1.68 and 4.03 burglaries per 1,000 occupied dwellings per three-month period studied. Following the onset of the program, the rates ranged from 3.73 to 6.54 per 1,000 dwellings. Although burglary rates continued to climb after O-I's onset, the rate of increase of burglaries declined from an 11.6 percent quarterly growth to 8.8 percent. This did not constitute a statistically significant decrease in the trend as a whole ($t = .2662$; $p > .05$; 10 df). Also, at none of the six observation points in the postprogram period was there a statistically significant departure in the burglary rate from that projected on the basis of the preprogram trend. Therefore, a noticeable, although nonsignificant, decrease in the growth of residential burglary rates accompanied the program.

A comparison of program participants and nonparticipants (the latter group including eventual participants, before participation) reveals distinct differences in the victimization of the two groups (Table 2). Although no participant households were burglarized, during the eighteen-month period after the program began, thirty-four burglaries occurred among nonparticipants, providing a mean of 5.79 incidents for 1,000 nonparticipant dwellings for each three-month period. This constituted a significant difference ($t = 2.82$; $p < .05$; 10 df).

This apparent indication of program effectiveness and displacement can be attributed, at least in part, to initial differences in vulnerability between the participant and nonparticipant cohorts. As Table 1 shows, the participant cohort members had a mean victimization rate of only 1.65 burglaries per 1,000 dwellings before the program, whereas nonparticipant cohort members averaged 3.64 burglaries for the eight points of observation. This difference was found to be significant at the .1 level ($t = 1.52$; 14 df).

The nonequivalence of the two cohorts from the outset made it preferable to gauge program effects through the observation of only one cohort.

18. Leon Radzinowicz and Joan King, *The Growth of Crime: The International Experience* (London, England: Cox and Wyman, 1979), pp. 51-52.

Table 2.   Breaking and Entering Offenses for Program Participants and Nonparticipants

Time Period[a]	Actual Participants			Actual Nonparticipants			
	B&E's[b]	Total Units	Rate[c]	B&E's	Total Units	Rate	S.A.R.[d]
9	0	7	0	1	1033	.97	3.78
10	0	13	0	2	1036	1.93	5.43
11	0	33	0	13	1025	12.68	6.14
12	0	123	0	8	955	8.38	7.12
13	0	149	0	2	938	2.13	7.35
14	0	171	0	8	922	8.68	6.37

[a]Three-month intervals starting with March 1975.
[b]Breaking and entering offenses.
[c]Rates per 1,000 occupied units.
[d]Seasonally adjusted rates.

First, the entire participant cohort was compared before the program's onset with the members of the participant cohort who did not immediately join O-I (see Table 1). Assuming the two groups are fundamentally equivalent, as members of the same cohort, differences could be attributed to the program. The group of dwellings that did not participate immediately would be expected to show a higher burglary rate because they would become more desirable targets as progressively more dwellings entered the program. Indeed, the cohort had a mean burglary rate of 1.65 per 1,000 dwellings before the program; during the program, those cohort members not yet participating had a mean rate of 7.67 incidents. Admittedly, the numbers here are small, precluding inferential analysis and definitive statements on the trend. However, the changes were in the direction predicted.

With respect to this same cohort, program effects were monitored through a comparison of those dwellings yet to join O-I with those already participating. Consistent with the displacement hypothesis, the former group was victimized at a higher rate (7.67 incidents per 1,000 dwellings) than the latter (no victimizations). Again, although the small figures preclude tests of significance, the differences were in the expected direction.

Finally, the effect of the program on the nonparticipant cohort was examined by comparing the victimizations experienced by the group before and following O-I's onset. Before the program, the mean burglary

rate for this group was 3.64 incidents per 1,000 dwellings; after onset, this increased to a rate of 5.51 incidents. Most suggestive of displacement was the fact that the quarterly growth of burglary rates more than doubled after the program was under way. It will be recalled that residences in general and the participant cohort in particular experienced a reduction in this growth rate and an absolute reduction in incidents, respectively.

Target Displacement

In the overall burglary rates for the region (residences and businesses), the figures fluctuated between 4.79 and 7.29 incidents per 1,000 premises before the program (Table 3). After O-I's onset, the rates varied from 7.04 to 9.94 burglaries per 1,000. The average quarterly rate of increase rose to 6.4 percent from 6.1 percent after onset; however, this did not constitute a significant difference (t = .7532; p > .05; 10 df). Indeed, at none of the six points of observation following the program's onset was there a significant departure from the projected rates.

As mentioned above, the quarterly growth rate of burglary among residences in general showed a nonsignificant decline (11.6 percent to 8.8 percent) in conjunction with the program's operation. With respect to businesses (Table 3), the preprogram rates ranged from 10.72 to 14.53 burglaries per 1,000 premises. Once the program was under way, these rates varied between 13.48 and 16.59 incidents. This increase in the quarterly growth of burglary rates from 3.9 percent to 4.2 percent was not significant (t = .9323; p > .05; 10 df). Here again, there was an absence of significant deviations from projected rates at any of the six observation points.

Although the changes in the rates for residences and businesses were not significant, the fact that they occurred in the expected direction is noteworthy. The rate of growth of residential burglaries declined and that of businesses increased. Since the O-I program involved residential participants only (with the exception of one business), these findings can be interpreted in at least three ways.

First, crime may have been displaced from residential to nonresidential premises. An explanation of displacement is insufficient, however, for there were increases in the growth rate of total burglaries (residential plus commercial). A factor in addition to displacement must have produced the rise in the growth rate of commercial burglaries. This may be attributed to a sudden spurt of nonresidential victimizations which coincided with, but was independent of, the introduction of the program. This sudden attractiveness of certain targets to the offender population is referred to as a *multiplier effect.*[19] Such an effect may occur without the impetus of specific prevention programs.

19. Jan M. Chaiken et al., *The Impact of Police Activity on Crime: Robberies on the New York City Subway System* (New York: New York City Rand Institute, 1974).

Table 3. Breaking and Entering Offenses for Total Premises, Residences, and Businesses (March 1973–August 1976)

Time Period[a]	Total Premises				Residences				Businesses			
	B&E's[b]	Total Units	Rate[c]	S.A.R.[d]	B&E's	Total Units	Rate	S.A.R.	B&E's	Units	Rate	S.A.R.
Preprogram												
1	11	1304	8.44	4.79	1	889	1.12	1.68	10	415	24.10	11.48
2	4	1331	3.01	5.80	2	906	2.21	2.31	2	425	4.71	13.26
3	7	1355	5.16	5.73	3	922	3.25	2.67	4	433	9.24	12.21
4	12	1417	8.46	5.67	4	961	4.16	3.27	8	456	17.54	10.72
5	6	1449	4.14	6.42	1	978	1.02	3.97	5	471	10.62	11.53
6	10	1472	6.80	6.85	7	990	7.07	4.03	3	482	6.22	12.62
7	11	1491	7.38	7.29	4	1000	4.00	3.99	7	491	14.26	13.86
8	15	1545	9.72	7.10	4	1027	3.89	3.34	11	518	21.24	14.53
Program Commences												
9	10	1576	6.35	7.04	1	1040	.96	3.73	9	536	16.79	13.55
10	5	1597	3.13	8.09	2	1049	1.91	5.20	3	548	5.47	13.61
11	17	1618	10.51	8.47	13	1058	12.29	5.76	4	560	7.14	13.48
12	25	1668	15.01	9.33	8	1078	7.42	6.54	17	590	28.81	14.32
13	7	1698	4.12	9.94	2	1087	1.84	6.48	5	611	8.18	16.04
14	21	1718	12.22	9.49	8	1093	7.32	5.37	13	625	20.80	16.59

[a]Three-month intervals starting with March 1973.
[b]Breaking and entering offenses.
[c]Rates per 1,000 occupied units.
[d]Seasonally adjusted rates.

Second, a multiplier effect may itself have been responsible for the increased growth rate of burglaries for both businesses and total premises. The decline of the residential burglary growth rate may be attributed to the primary preventive effects of the program (e.g., increased community cohesiveness), to the direct deterrent effects, or to both of these.

Finally, the possibility of improved relations between the police and the community brought about by the program may have increased the reporting rate of residential victims. This may have masked a decline in the growth of actual burglary rates that was greater than was apparent from the official data. In that case, the overall burglary rate may have been stabilized, despite the program, and a genuine displacement effect may have occurred.

Tactical Displacement

The items taken in the residential burglaries were examined for the period from 1970 through 1976. It will be recalled that, in this study, the hypothesized tactical displacement referred to a change in objects taken by burglars from markable (property habitually marked in the program) to nonmarkable items.

In 1970, 37.5 percent of residential burglaries involved the theft of markable merchandise (Table 4). In the following year, markable items

Table 4.   Type of Items Taken during Residential Breaking and Entering Offenses (1970-76)

Year	Total B&E's[a]	Markable Items[b]	% of Total	Non-markable Items[c]	% of Total	No Items Taken/ Items Unknown	% of Total
1970	8	3	37.5	5	62.5	0	0
1971	9	5	55.6	3	33.3	1	11.1
1972	4	0	0	4	100.0	0	0
1973	7	4	57.1	1	14.3	2	28.6
1974	16	9	56.3	4	25.0	3	18.7
1975	21	13	61.9	4	19.0	4	19.0
1976	23	7	30.4	13	56.6	3	13.0

[a]Breaking and entering offenses.
[b]Cases in which markable items were taken.
[c]Cases in which nonmarkable items were taken.
Note: If both markable and nonmarkable items were taken in a burglary the case was registered under the "markable" column, since it was the number of burglaries involving markable items that was of interest.

were taken in 55.6 percent of the cases. In 1972, no markable items were taken; however, only four burglaries were reported in that year. The years 1973 and 1974 were fairly constant, with 57.1 percent and 56.3 percent of burglaries, respectively, involving markable items. In 1975, the year in which the program was initiated, an increase to 61.9 percent occurred, indicating that the program was not demonstrating tactical displacement effects in its initial nine months of operation. However, in 1976, a 50 percent decline occurred in the proportion of cases in which markable items were taken. Conversely, cases before 1975 involving the theft of nonmarkable items fluctuated between 14.3 percent and 100 percent. During 1975, the first year of the program's operation, the theft of non-markable items reached its second lowest point (19 percent). However, as cases involving the theft of markable items declined in the second year of the program (1976), those involving nonmarkable items increased to 56.6 percent. These changes in the patterns of burglary can be attributed to several factors.

Because there was only one measurement for 1976, random effects cannot be dismissed as explanatory factors. Some external event(s) occurring during 1976 may have been responsible for the abrupt change in the nature of items taken.

Regression effects, or the tendency of extreme figures observed at one point to return toward the mean in subsequent observations,[20] may provide an alternative or supplementary explanation. Table 4 shows that the proportion of markable items taken was not at its lowest point in 1976. In 1972, no cases involved such items (although, as mentioned above, the sample was extremely small), and in 1970, only 7.1 percent more cases involved markable items. The percentage of burglaries involving markable items tended to increase through 1975, and the 1976 period could be considered to be a normal fluctuation in the direction of the mean. Similarly, even if one discounts 1972 because of the low number of burglaries in that year, it can be observed that cases in 1976 involving nonmarkable items did not constitute an all-time high; in 1970, 62.5 percent of cases involved such theft. Consequently, the sudden increase in 1976 could be attributed to a movement toward the mean after several low years.

Another possible explanation is that a multiplier effect may have been a factor in burglaries during 1975, the first year of the program. In response to the growing membership of the O-I program, burglars may have specifically sought markable merchandise, knowing that the items would be marked in the near future. At the same time, they may have temporarily avoided nonmarkable items. This could account for an in-

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20. Donald T. Campbell and Julian C. Stanley, *Experimental and Quasi-Experimental Designs for Research* (Chicago: Rand McNally, 1963).

crease in cases involving markable items and the decrease in theft of non-markable items in the initial year of the program. It could also account for the sudden change in the following year as burglars' demand for markable items may have been satiated and their interest in nonmarkable goods renewed. However, the increase in cases involving markable items from 1974 to 1975 was only 5.6 percent, and the decrease in nonmarkable items taken was only 6 percent, so this explanation is not strong.

Finally, a displacement effect can be postulated, since the change from 1975 to 1976 occurred in the expected direction. The failure of the program to demonstrate effects in the first nine months of the program can be attributed to the scarcity of O-I participants for that period.

CONCLUSIONS

Notwithstanding the problems of interpretation discussed above, the findings of all three components of the study indicate that displacement is a plausible reaction to prevention programs and should be a part of evaluative studies. The analysis of displacement promises to illuminate offender behavior patterns and motivation and, consequently, the modes of intervention most appropriate in counteracting antisocial behavior.