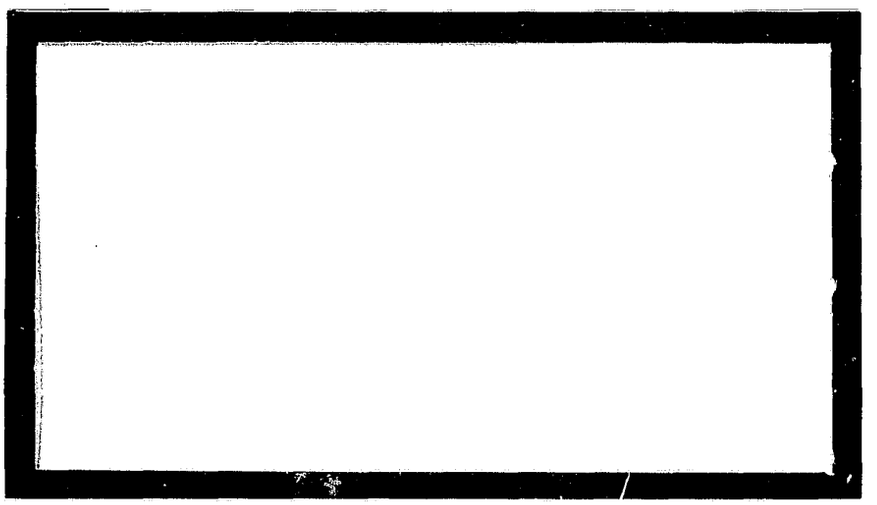

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THE IMPACT OF COMMUNITY CRIME PREVENTION PROGRAMS IN CHICAGO: CAN NEIGHBORHOOD ORGANIZATIONS MAKE A DIFFERENCE?

FINAL REPORT: VOLUME ONE

by

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ABSTRACT

The Ford Foundation funded Northwestern University's Center for Urban Affairs and Policy Research to conduct an evaluation of five neighborhood-based crime prevention programs in Chicago. The central question was whether local community organizations, with outside funding but without substantial assistance from law enforcement, could introduce programs that would have a significant positive impact on local residents' perceptions, attitudes, and behaviors in a manner that is consistent with reducing crime and enhancing the quality of neighborhood life. The intervention was an attempt to organize residents of selected neighborhoods through door-to-door canvassing, block meetings, neighborhood meetings, and related strategies, with emphasis on the block watch model.

A quasi-experimental research design was employed to evaluate these programs, namely, the Untreated Control Group Design with Pretest and Posttest (c.f., Cook & Campbell, 1979). Measurement was taken before and after the implementation of the crime prevention program in the treated neighborhoods, and identical measurement occurred at the same time in untreated comparison neighborhoods, as well as a citywide sample of Chicago residents. A one-year lag was scheduled between the pretest and posttest. Extensive telephone surveys were conducted with residents in two types of samples -- panel samples and independent samples. A total of 3357 interviews were completed at time one and 1652 respondents were reinterviewed one year later to create the panel sample. An additional 1172 posttest-only residents were interviewed at time two to complete the independent samples. Samples were generated through both Random Digit Dialing

(RDD) procedures and the criss-cross directory, with primary reliance on the latter to produce block-level samples.

Analyses were performed to test seven primary hypotheses that embody the program objectives and current theorizing about the impact of community crime prevention programs. Essentially, these interventions were expected to: (a) stimulate residents' awareness of, and participation in crime prevention meetings, (b) enhance feelings of efficacy and personal responsibility for preventive action, (c) produce a number of behavioral changes related to preventing victimization and informally regulating social behavior, (d) enhance social cohesion, (e) reduce crime and various types of disorder, (f) reduce fear of crime and related perceptions of crime, and (g) improve general perceptions of the neighborhood and attachment to the community as a place to live.

The results indicate that, although residents' awareness and participation increased (indicating some success with program implementation), there was a consistent lack of support for the main hypotheses. That is, the large majority of comparisons revealed no significant differential change over time between the treated and untreated areas. Furthermore, the majority of the <u>significant</u> findings ran <u>counter to</u> the main hypotheses. Specifically, the three neighborhoods with the strongest evidence of program implementation showed significant <u>increases</u> in a number of problem areas, such as fear of crime, perceptions of the crime problem, vicarious victimization, and concern about the future of the neighborhood.

Several explanations are discussed for both nonsignificant and significant counter-hypothesis findings. While the interventions may

have been responsible for heightening residents' concerns and fears, other explanations for these unfavorable changes are possible. For example, differential concern about residential transition and differential victimization between treatment and control groups were tested as rival explanations for the observed effects, but controlling for these variables did not eliminate the treatment-effect relationship. Thus, crime prevention meetings may have exacerbated preexisting concerns about neighborhood decline. In any event, the interventions were generally unable to retard these negative processes.

It is worth noting that the one neighborhood which organized numerous block watches showed fewer of these unfavorable results and, in fact, showed some encouraging effects such as (marginally significant) reductions in victimization, increases in surveillance, and increases in home protection behavior. Nevertheless, most of the changes in this neighborhood were either unexpected (e.g. increases in fear of crime) or nonsignificant.

The strongest test of the causal relationship between the block watch intervention and expected outcomes was performed at the block level in the one neighborhood that systematically pursued this crime prevention model. While a check on implementation success revealed large differences in exposure and participation between residents living on treated and untreated blocks within the same neighborhood, analysis of 21 separate outcome scales showed very few differences between the two groups over time. Only one scale registered significant change and only with the panel sample. Specifically, residents on treated blocks were more likely than residents on

untreated blocks to attribute responsibility for crime prevention to citizens instead of police. Three marginally significant findings were produced. In particular, residents of treated (vs untreated) blocks showed: (a) an increase in home protection behaviors, (b) an increase in action taken against neighborhood problems, and (c) a decrease in optimism about change in the neighborhood. In sum, if block watch meetings have any effects, it appears that they stimulate residents to accept more responsibility for crime prevention, secure their homes better, intervene more frequently, and become more concerned about decline in their neighborhood. However, viewing the block-level results as a whole, the general conclusion must be that organizers were quite successful at implementing a program, but that this intervention produced few of the hypothesized effects. Yet given the unfavorable neighborhood-level findings, these nonsignificant block-level results can be viewed as good news for block watch supporters. These data suggest that the observed declines at the neighborhoold level were not the untoward effects of blockwatch, per se, even though they may have been the by-product of other components of the treatment in these neighborhoods, such as neighborhood-wide meetings.

The evaluation results are discussed in terms of several categories of explanations: program failure, measurement failure and theory failure. Evidence of failure in each of these areas is cited, but emphasis is placed on weak implementation and indefensible theory.

Given that our current theorizing about the impact of community crime prevention is largely undeveloped and untested, and given the nonsupportive findings of the present evaluation, there is a pressing

need to rethink our expectations for these popular programs. The causal, intervening, and outcome variables have not been carefully specified to date. Hence, there is considerable room for theory advancement and policy modification in this field.

I. INTRODUCTION

Community crime prevention has experienced tremendous growth in the past decade. Millions of Americans are now participating in both individual and collective actions to protect themselves, their family members, belongings, and neighbors from crime, and to create a sense of safety and social integration in their communities. Given all the excitement about citizen participation in voluntary crime prevention activities, many policy makers, service providers, community leaders, and funding agencies are interested in knowing whether these strategies are effective mechanisms for fighting crime, reducing fear of crime, and building a sense of neighborhood in urban areas. Unfortunately, we know very little about the effectiveness of citizen or police crime prevention programs. (See Rosenbaum, in press).

The Ford Foundation, has demonstrated its interest in this question by funding both community-based programs and evaluations of these programs. One of the major initiatives supported by the Ford Foundation is the Urban Crime Prevention Program directed by the Citizen Information Service in Chicago, Illinois. Ford also funded an evaluation of this program, conducted by the authors at Northwestern University's Center for Urban Affairs and Policy Research. The purpose of this report (Volume One) is to describe and interpret the major <u>impact</u> results of this evaluation. Volume two focuses on the <u>process</u> results, including a detailed description of the community organizations involved and how they operate.

Overview of the Program and the Evaluation

The Citizen Information Service of Chicago (CIS), a branch of the League of Women Voters, directed and monitored the "Organizing Neighborhoods for Crime Prevention" project. This project was a continuation and expansion of programs previously funded by ACTION and the Law Enforcement Assistance Administration as part of the Urban Crime Prevention Program (UCCP). With new funding from the Ford Foundation and assistance from CIS, voluntary citizen organizations in Chicago were able to reach new areas of the city and continue their crime prevention activities.

The new UCCP included funding for nine Chicago community organizations to develop and implement neighborhood crime prevention programs within their own service areas. The "treatment" or intervention was an attempt to organize residents of selected neighborhoods through door-to-door contacts, block meetings, neighborhood meetings, the distribution of educational materials, and related strategies. Unlike the original UCCP, which included a wide variety of crime prevention strategies, (e.g. arson prevention, consumer fraud, dispute settlement), the new project focused on establishing and maintaining block watches and/or youth-focused activities. CIS encouraged participating organizations to adopt the block watch model, but the extent to which this strategy was actually adopted varied considerably across the groups. (For a detailed discussion of the interventions, see Lewis, Grant, & Rosenbaum, 1985).

The Evaluation. The evaluation of these projects was a two-year assessment funded by the Ford Foundation. The central question addressed by the Northwestern evaluation was whether local community

organizations -- with some outside funding but without substantial help from law enforcement -- could introduce programs that would have a significant impact on local residents and the neighborhood as a whole? Could these programs significantly influence residents' perceptions, attitudes, and behaviors in a manner consistent with reducing crime and enhancing the quality of neighborhood life? Much has been written in recent years about the importance of community organizations as vehicles for community crime prevention, but little is known about whether citizen-based programs can make a difference in the community.

To our knowledge, this is the first quasi-experimental evaluation of exclusively citizen-based programs. For the reader's benefit, we should note that this evaluation is dramatically different from the UCCP evaluation conducted by Roehl and Cook (1984) for the National Institute of Justice which included many of the Chicago neighborhoods studied under the present evaluation. Their evaluation was a national assessment of 85 UCCP projects in nine U.S. cities, and by definition, did not collect the type of empirical data at the local level necessary to address impact questions. In contrast, the resources for our evaluation were concentrated in five Chicago neighborhoods to assess changes in residents' behavior and perceptions over a one year period. New target areas were selected to administer the treatment in neighborhoods where some previous organizing had been done.

Before describing the methods and results of the present evaluation, let us briefly review some of the major research and evaluation developments in the field of community crime prevention over the past 15 years to provide the necessary background and context

for our current efforts. Then we will articulate the major hypotheses tested in this evaluation.

B. Early Evaluation Research

In the early to mid-1970s, there were numerous claims of success for community crime prevention programs, especially programs focusing on residential burglary. The early attempts to "evaluate" police-citizen crime prevention programs tended to rely on police statistics to show crime reduction in target areas. The vast majority of programs were not subjected to any rigorous evaluations by professional evaluators. Thus, we are not surprised that the most consistent finding to emerge from the national evaluations of Citizen Crime Reporting Projects (Bickman & Lavrakas, 1976), Operation Identification (Heller et. al, 1975), Security Surveys (I.T.R.E.C., 1977), and Citizen Patrol Projects (Yin, 1977) is that we know very little about the effectiveness of these programs because of the paucity of data.

What little was known about program effectiveness by the mid-1970s came from a few, more rigorous evaluations, such as the assessment of burglary reduction programs in Portland, Oregon (Schneider, 1975), and Seattle, Washington (Cirel et. al, 1977, Matthews, 1976). While these laudable evaluations are often cited as evidence of citizen effectiveness in controlling crime, the difficulties of conducting a good evaluation were never more apparent. For example, we began to see the prohibitive cost of conducting adequate victimization surveys. Enormous sample sizes were required to produce reliable estimates of victimization rates (cf. Skogan, 1978).

These early evaluations were largely designed as impact evaluations, focusing on crime reduction and antecedent changes in a few citizen crime prevention behaviors. Thus, measurement was generally limited to awareness of the program, compliance with crime prevention requests, rates of crime reporting, rates of reported crime, and victimization rates. From these evaluations we gained a clearer understanding of the limitations of official crime statistics for measuring the effectiveness of citizen crime prevention programs (see Schneider, 1975; Skogan, 1978).

The prohibitive cost of measuring crime reduction, along with numerous untold failures to reduce official crime rates, eventually led to a re-examination of both program and evaluation objectives. Many practitioners and evaluators, in concert with the National Institute of Law Enforcement and Criminal Justice (NILECJ), began to question whether crime reduction should be the exclusive goal of citizen-police crime prevention programs. The stated goals for crime prevention programs and evaluations started expanding in the late 1970s to include a reduction in fear of crime and an increase in community cohesiveness.

By focusing on crime reduction, the early evaluations contributed very little to our understanding of program processes and how they affect performance. The complexities of collective reactions to crime had yet to be studied. The "treatment" was assumed to be easily definable and relatively standard across settings, and the outcome was assumed to be crime reduction. More careful scrutiny of the implementation process suggests that the issues are far more complex than first imagined. Hence, we now recognize the importance of defining

the treatment early in the research process (see Yin, 1978), and see the possibilities for program impact in many other areas.

C. Understanding Processes and Outcomes

In the mid-1970s, while the national evaluations funded by NILECJ were telling us how little was known, community crime prevention scholars were also entering a new era of research marked by indepth analyses of individual and collective citizen reactions to crime. As a result of this work, our theoretical understanding of community crime prevention was significantly advanced, along with our ability to measure these processes and their effects. Community crime prevention programs were placed in the larger context of neighborhood activities directed at regulating social behavior and solving neighborhood problems, as well as preventing victimization. Consequently, creating a sense of neighborhood and altering perceptions of the crime problem (both in terms of neighborhood problems and personal fear) became as important as reducing the actual crime rate. The "Reactions to Crime Project" (Skogan et al, 1982), the "Safe and Secure Neighborhoods Project" (Taub, Taylor, & Dunham, 1982), the "Citizen Participation Project" (Lavrakas et. al, 1982), and a handful of other projects in this area (e.g. Greenberg, Rohe, & Williams, 1983; Taylor & Bower, 1980) gave us a much better understanding of the processes involved in citizen responses to crime. This research advanced our understanding of the neighborhood conditions that encourage community crime prevention activities; who participates in various forms of crime prevention, what factors contribute to the maintenance of citizen participation, how participation correlates with fear, perceptions of neighborhood crime and incivility, feelings of efficacy, neighborhood

solidarity and attachment, and other variables. Hence, this research greatly expanded our thinking about possible ways of measuring not only the impact of crime on the community, but also the effects of citizen participation on the individual and the neighborhood. We were able to draw upon this knowledge base to develop hypotheses about program impact conceptualize and operationalize our measurement plans for the present evaluation.

This line of research also reconfirmed the importance of community organizations as vehicles for initiating and maintaining community crime prevention programs. (Lavrakas et. al 1980; Lewis & Salem 1985; Podolefsky & Dubow, 1983). Consistent with this conclusion, national crime prevention policy, as reflected in the Community Anti-Crime Program and the more recent Urban Crime Prevention Program, reached beyond police-sponsored target-hardening programs to recognize the unique role of neighborhood-based initiatives in maintaining various levels of informal social control. Nevertheless, the question remained whether these community-based activities are able to produce changes in the neighborhood.

The "reactions to crime" line of research has not adequately addressed questions related to the effectiveness of individual or collective responses to crime. The major data sets have been cross-sectional (not longitudinal) and collected at the neighborhood level to examine either individual or neighborhood-level effects. The limitations of cross-sectional correlations for understanding program impact are illustrated in a major secondary analysis study by Greenberg et. al.(1983). One of their findings was that neighborhoods with Neighborhood Watch programs show less frequent "neighboring" and

higher rates of personal victimization than neighborhoods without these programs. While such findings are interesting to document the inverse relationship between "formal" and "informal" social controls, (as the authors intended) they highlight the dangers of assessing the effectiveness of Neighborhood Watch and other crime prevention programs by comparing neighborhoods with and without such programs at one point in time. Such comparisons might easily suggest that the program is a total failure, while the findings may simply indicate that neighborhoods with some degree of problems have decided to start a neighborhood watch program to ameliorate these conditions. The real question is whether change or improvement is noted over time in the amount of neighboring, perceptions of crime, etc. within target neighborhoods relative to comparison areas. Evaluations with multiple measurement points can go beyond previous correlational research to test the hypothesis that social control and crime prevention behaviors can be "implanted" in communities where they do not currently exist.

In summary, while the "reactions to crime" research from the mid-1970s to the present has significantly advanced our understanding of the factors which create and maintain collective anti-crime behaviors, it generated little evidence regarding the effectiveness of neighborhood organizations or police in combatting fear of crime, neighborhood deterioration, and related problems. Processes, rather than programs, have been the focal point. However, the growing need for answers to important questions about effectiveness has thrusted us into a period of emphasis on program-focused research and evaluation, with the hope of better understanding and improving the efficacy of police and citizen crime prevention efforts.

D. Recent Developments in Evaluation Research

With the initial Hartford evaluation (Fowler, McCalla, & Mangione, 1979), we see the beginning of a new era in evaluation research, characterized by efforts to look beyond crime rates to measure the broader impact of community crime prevention programs. While fear of crime had been measured in one or two previous evaluations (e.g. Schneider, 1975), the Hartford evaluation expanded the scope of measurement to include perceptions of the neighborhood, social cohesion, and related variables.

Since then, criminal justice scholars have conducted extensive research to conceptually refine and validate measurement in the areas of fear of crime (Rosenbaum & Baumer, 1981), neighborhood attachment and social interaction (Riger & Lavrakas, 1981), protective behaviors (Lavrakas, 1979), participation in organized neighborhood activities (Skogan & Maxfield, 1980), incivility (Lewis & Maxfield, 1980), and other dimensions relevant to program evaluation in this field. However post-Hartford program evaluations utilizing these advancements in theory and measurement were virtually nonexistent in the field of community crime prevention until 1983 when both the Ford Foundation and the National Institute of Justice initiated several major evaluations.[1]

The present evaluation was built on a decade of cummulative experiences described in this brief review. The outcome measures cover many of the major attitudinal, perceptual and behavioral dimensions to emerge from the research on community crime prevention. The quasi-experimental research design (involving pretests, posttests,

and carefully selected control groups) was the best we could utilize in this setting.

E. Statement of Hypotheses

Aside from statements about crime reduction, rarely have the expectations for community crime prevention programs been articulated for the benefit of both theoretical and empirical scrutiny. Dubow and Emmons (1981) have articulated what they call "the community hypothesis" to specify the community benefits that should result from collective crime prevention activities and to clarify the underlying mechanisms. Greenberg et. al (1983) have sought to clarify the role of informal social control processes in explaining community crime prevention outcomes. To help us clarify our thinking about this evaluation, we have combined this type of theorizing with the stated objectives of the UCCP projects to generate some testable hypotheses about the consequences of citizen crime prevention. Although we will not propose any unifying "theory of impact," hopefully the collection of hypotheses stated here will bring us a few steps closer to an integration of previous theoretical statements.

A grand "Community Crime Prevention Hypothesis," which serves as the umbrella for other hypotheses and predictions, can be stated as follows: When citizens voluntarily come together to share and discuss neighborhood problems or issues (including crime), and work collectively toward resolving or preventing these problems, such participatory actions can enhance the psychological and social well-being of the community and eventually reduce both the perceived and actual incidents of such problems. This hypothesis assumes, in the present context, that voluntary community organizations can serve as

effective vehicles for stimulating the citizen participation necessary to effect these changes. While the above hypothesis is clearly a global statement, it provides us with a framework for examining more specific and testable hypotheses. Below we will articulate seven primary hypotheses that were used to conceptually drive this impact evaluation. Frequently, these primary hypotheses are a collection of more specific predictions that will be operationally defined in the Results section of this report.

Hypothesis One. Local community organizations intent on organizing citizens around the issue of crime reduction will, as a first step, be able to improve residents' awareness of local opportunities to participate in crime prevention activities and, secondly, stimulate actual participation in these activities.

Changes in citizen awareness of crime prevention opportunities and behavioral involvement at a minimal level (e.g. attendance at a block watch or neighborhood crime prevention meeting) are considered necessary conditions for producing the effects stated in subsequent hypotheses. In the context of this evaluation, the presence or of these conditions is viewed as direct evidence of treatment implementation.

Hypothesis Two. Exposure to the program -- especially in the way of participating in neighborhood or block meetings -- will enhance feelings of efficacy about individual and collective actions, as well as increase personal responsibility for these actions. Although less likely, informational exposure to crime prevention literature, such as newsletters or flyers, may produce these effects.

By working together to address neighborhood concerns and problems, residents may develop a sense of efficacy and control over their local environment. Historically, "empowerment" is a central objective of community organizers. The mere act of working together with other neighborhood residents or seeing tangible evidence of

success may give participants the feeling that they can change the local crime problem or influence local government to be more responsive to their needs. Meetings to discuss specific crime prevention activities should also strengthen participants' belief in the efficacy of these individual and collective measures. These discussions should also move citizens to attribute more of the responsibility for crime prevention to themselves and less to the police.

Hypothesis Three. At the core of community crime prevention, theorizing is the expectation that organizing efforts will produce behavioral changes among citizens both in terms of attempts to regulate social behavior in the neighborhood and prevent future victimization via protective actions.

The exercise of informal social control by community residents has been posited as a central mechanism in the prevention of crime and disorder (Fowler & Mangione, 1982; Greenberg, et. al., 1982: Jacobs, 1961; Lewis & Salem, 1985; Skogan, 1982; Taub, et. al., 1982; Taylor & Brower, 1980). Considerable attention has also been given to the wide array of protective measures that citizens might employ to prevent victimization (Lavrakas & Lewis, 1980; Baumer & Rosenbaum, 1981). Block Watch is a vehicle for encouraging citizens to engage in behaviors related to informal control or victimization prevention. Hence, in the present case, programs are expected to stimulate more surveillance and "neighboring" (e.g. watching each others homes), increase reporting of victimization experiences, and increase citizen involvement in a variety of personal and household crime prevention behaviors. Assuming that programs encourage the exercise of informal social control and enhance citizens' sense of efficacy, then local

residents should also be more likely to personally intervene and attempt to solve identifiable neighborhood problems.

Hypothesis Four. Collective activities and efforts to prevent crime will enhance social cohesion in the neighborhood.

Several processes may underlie these hypothesized effects. First, organizing residents of a community may contribute to a sense of social cohesion or solidarity by engendering more frequent social interaction. As social comparison theory suggests (Festinger, 1954), social interaction provides a critical opportunity for individuals to share and hence validate their own perceptions and feelings -- a fundamental social need that has been well documented by social psychologists.

Second, the exchange and discussion of information in social settings may generate definitions of neighborhood problems that enhance social cohesion. For example, a block watch group, after much discussion, may arrive at the conclusion that "we have a crime problem in our neighborhood." There is considerable debate about whether crime draws people together and strengthens social bonds (a la Durheim, 1933) or contributes to a reduction of social cohesion (a la Conklin, 1975). We will simply test the hypothesis that interventions can alter residents social interaction patterns.

Hypothesis Five. If, indeed, these programs are able to enhance individual and collective actions that produce less opportunities and more social sanctions for deviant behavior, then we would expect a reduction in crime and other forms of social disorder.

We have changed our thinking a great deal over the past decade regarding possible ways of measuring the benefits of community crime prevention programs and have moved away from emphasizing crime reduction as the primary outcome measure for reasons discussed earlier.

However, we have not reached the point of arguing that crime prevention programs should not be expected to prevent crime and disorder. Even though crime is a complex social problem, we believe that criminal activity can be prevented under a restricted set of test conditions (e.g. small areas, vulnerable target crimes, strong dosage of treatment).

Thus, we will test the crime/disorder hypotheses in the context of the opportunity reduction model, which is the approach that CIS has encouraged the UCCP projects to adopt. In particular, the expected outcomes examined include: a reduction in overall criminal victimization rates, a reduction in what we call "vicarious" victimization (i.e. having personal knowledge of other people in the neighborhood who have been victimized), and reductions in various types of social disorder.

Wilson and Kelling (1982) argue that the police are the central (and best) mechanism for maintaining order. We are exploring the alternative hypothesis that citizen involvement in their community can be a decisive factor in order maintenance through the exercise of informal social control (Greenberg et. al., 1982). Thus, residents in "treated" neighborhoods should report less disorder over time than "untreated" neighborhoods.

Hypothesis Six. Fear of crime and related perceptions of crime should decline as a result of reductions in victimization and disorder or as a direct product of the changes in neighborhood social processes discussed in previous hypotheses.

The correlates of fear of crime have been well documented in previous research (Baumer, 1978; Dubow, MCade & Kaplan, 1979; Lavrakas et. al., 1982; Lewis & Salem, 1985; Skogan & Maxfield, 1981). However, models for predicting changes in fear as a result of

intervention -- especially the impact of community crime prevention -- are virtually nonexistent. Nevertheless, we will venture to posit a few hypotheses.

To the extent that fear of crime is a reflection of the level of crime in the community (and there is some evidence of correlation -- see Skogan, 1977) or a reflection of the level of incivility/disorder (Lewis & Maxfield 1980), then reductions in either of these "causes" of fear should produce a decrease in fear. Clearly, this is simplified analysis of the relationship between fear and crime/disorder, but all things considered, the prediction is not unreasonable. Hence, our theorizing about community crime prevention impact suggests that one of the ultimate fruits of these efforts -- after crime reduction -- is the perception of greater safety and security in the neighborhood.

However, our theorizing also tells us that a reduction in crime or disorder is not a necessary condition for fear reduction. Theoretically, fear can be reduced through the process of involvement in crime prevention or related social processes. Participation in meetings and various crime prevention behaviors, by itself, may enhance feelings of security by allowing participants to feel that "at least I'm doing something to improve the neighborhood and protect myself". Reductions in fear of crime among nonparticipants in the neighborhood is also possible if the benefits of collective action "spill-over" into other types of social interaction in the neighborhood. Awareness that meetings and social gatherings are occurring more often, observing others interact more frequently on the streets, and interacting more oneself are examples of mechanisms by which crime

prevention activities could possibly "spill-over" to reduce fear and distrust in the community at large.

Whether based on real or only perceived changes in the neighborhood, we also expect reductions in the perceived amount of crime in the neighborhood. While fear is a personal response that is more likely to change as a function of the individual's experience, concern about "the crime problem" in the neighborhood may be a better barometer of changes that occur at the neighborhood level, and may be noticeable among both participators and nonparticipators, as well as fearful and nonfearful individuals.

Hypothesis Seven. Residents' general perceptions of the neighborhood's future and attachment to the community as a place to live will be improved by collective action.

Awareness that community crime prevention programs are underway in the neighborhood or experiencing the effects of the program in some way (e.g. greater social interaction or less crime) should produce an optimism about the future of the neighborhood. Residents should begin to feel that things are getting better -- not worse -- in their community, and should be more inclined to stay, rather than move to another neighborhood.

This evaluation was structured as a test of these seven hypotheses to determine if the programs in question had the anticipated effects. In the next section, we describe the methods and procedures used to execute this task.

II. METHODOLOGY

A. Research Design

The basic research design used in this evaluation is what Cook and Campbell (1979) call the "Untreated Control Group Design with Pretest and Posttest." That is, measurement was taken before and after the implementation of the crime prevention program in the "treated" neighborhoods, and identical measurement occurred at the same time in "untreated" (comparison) neighborhoods, as well as a citywide sample of Chicago residents. A one year lag was scheduled between the pretest and the posttest, with most of the quantitative data collected in February and March of 1983 and again in 1984. The field work, however, was continued throughout the implementation period.

Neighborhoods Studied. The selection of treated and untreated neighborhoods was determined by a number of factors, some of which were beyond the control of the evaluators (see the next section for a detailed discussion of site selection procedures). At this point, suffice it to say that five self-selected neighborhoods were used as the areas from which to select treatment sites that had not received any crime prevention programs. Selecting control groups was a much more difficult task. After considering many options, two primary types of comparisons were selected -- a city-wide comparison group and a "roughly equivalent" untreated comparison group.

Untreated areas were selected because of their similarity to the treated neighborhoods on some basic characteristics (described below). Short of conducting a true randomized experiment, there is no easy or

best solution to the problem of how to pick a good control group since any particular selection will be non-equivalent to the treated neighborhood on some dimensions other than the presence or absence of the treatment. To avoid the shortcomings of using a single neighborhood as a control, a decision was made to disperse our resources across three neighborhoods and then pool the results. This "pooled control group" strategy provided more stability and robustness to the data.

Neighborhood level comparisons, however broad, still run the risk of local events distorting the results or non-equivalence on critical variables that could affect the outcome directly or in combination with the treatment. As a partial response to these and other concerns, we utilized a city-wide sample of Chicago residents as our second primary control group. At a minimum, the city-wide sample would provide a stable indicator of city-wide changes that occurred on important outcomes measures.

Types of Samples. Two types of samples were employed in this evaluation: panel samples and independent samples. The primary thrust of the evaluation was to assess the impact of these programs on neighborhood residents over a one year time period. A panel design -- involving repeated measurement on the same respondents over time -- provided a strong test of the individual change hypothesis. Error variance was reduced as each respondent served as his/her own control. Thus, the panel data were given special attention in this evaluation.

However, while a panel design (with proper control groups) is generally stronger that an independent samples design on "internal validity" (i.e. the extent to which it allows a strong inference that

x caused y), it can be weaker on "external validity" (i.e. the extent to which the results can be generalized to other settings, populations, etc.). That is, respondents who remain in the posttest sample one year after the pretest are likely to differ from those who dropped out on variables that may be related to program outcomes. To compensate for this problem and provide an additional test of program impact, posttest data were collected on new independent samples. The entire set of pretest data (including panel respondents) were used as the most appropriate baseline for assessing change in the independent samples design. Thus, the independent pretest-posttest design provided an alternative assessment of the intervention using samples that more closely approximated the target population. The research design, as described above, is illustrated in Figure 1. To see the full picture, imagine this design replicated for each of the programs being evaluated.

B. Selecting Treated and Untreated Neighborhoods

Treated Neighborhoods. Nine well-established volunteer community organizations agreed to work with CIS and seek funding from the Ford Foundation to develop and implement community crime prevention programs. This self-selection and funding process occurred prior to the start-up of the evaluation, and therefore, was not under the control of the evaluators.

However, we selected only five of these nine organizations for inclusion in the evaluation. This reduction was necessary to maintain a high quality evaluation given the resources available. Using program documents, interviews with organization leaders, and neighborhood census data, we selected five communities by applying the

FIGURE 1
Research Design[1]

PANEL SAMPLES

	1983		1984
Treatment Neighborhood	O_1	X	O_2
Citywide Random Sample	O_1		O_2
Comparison Neighborhoods[2]	O_1		O_2

INDEPENDENT SAMPLES

Treatment Neighborhood	O_1	X	O_1
Citywide Random Sample	O_1		O_1
Comparison Neighborhoods[2]	O_1		O_1

[1] This research design was implemented four times to evaluate four separate programs. For a fifth program, data were not collected in the comparison neighborhoods because of implementation failure.

[2] This control group is a composite of three separate neighborhoods with similar characteristics.

O = Observation or measurement subscription indicates first or second measurement for respondents.
X = Treatment implementation.

following criteria: (1) the type of intervention (preference given to those emphasizing block watch or neighborhood watch programs); (2) the probability of impact (preference given to those whose organizations appeared to have adequate planning and resource capabilities to implement the program within a few months), and (3) neighborhood characteristics (some attempt was made to maintain variety between communities along ethnic/racial and socioeconomic dimensions).

Our influence over the selection of treatment neighborhoods within the five chosen community areas was limited to setting two restrictions on the community organizations making these decisions: (1) the programs should be implemented in neighborhoods where their organization has not carried out any community organizing in the past two years, especially crime prevention activities. (Our analysis of pretest differences between treated and untreated areas can be seen as evidence of compliance with this request in several neighborhoods) and (2) the programs should be implemented in contiguous areas whenever possible. This request was respected by 4 of the 5 community organizations.

The geographic outcome of this selection process is shown in Figure 2. The areas selected to serve as treated neighborhoods are shown as shaded patterns within each organization's service area (indicated by dark boundaries). The untreated comparison areas that were selected to serve as control groups are also shown in Figure 2 by a shading pattern that is identical to the treated neighborhood with which it was compared. The selection process for these untreated

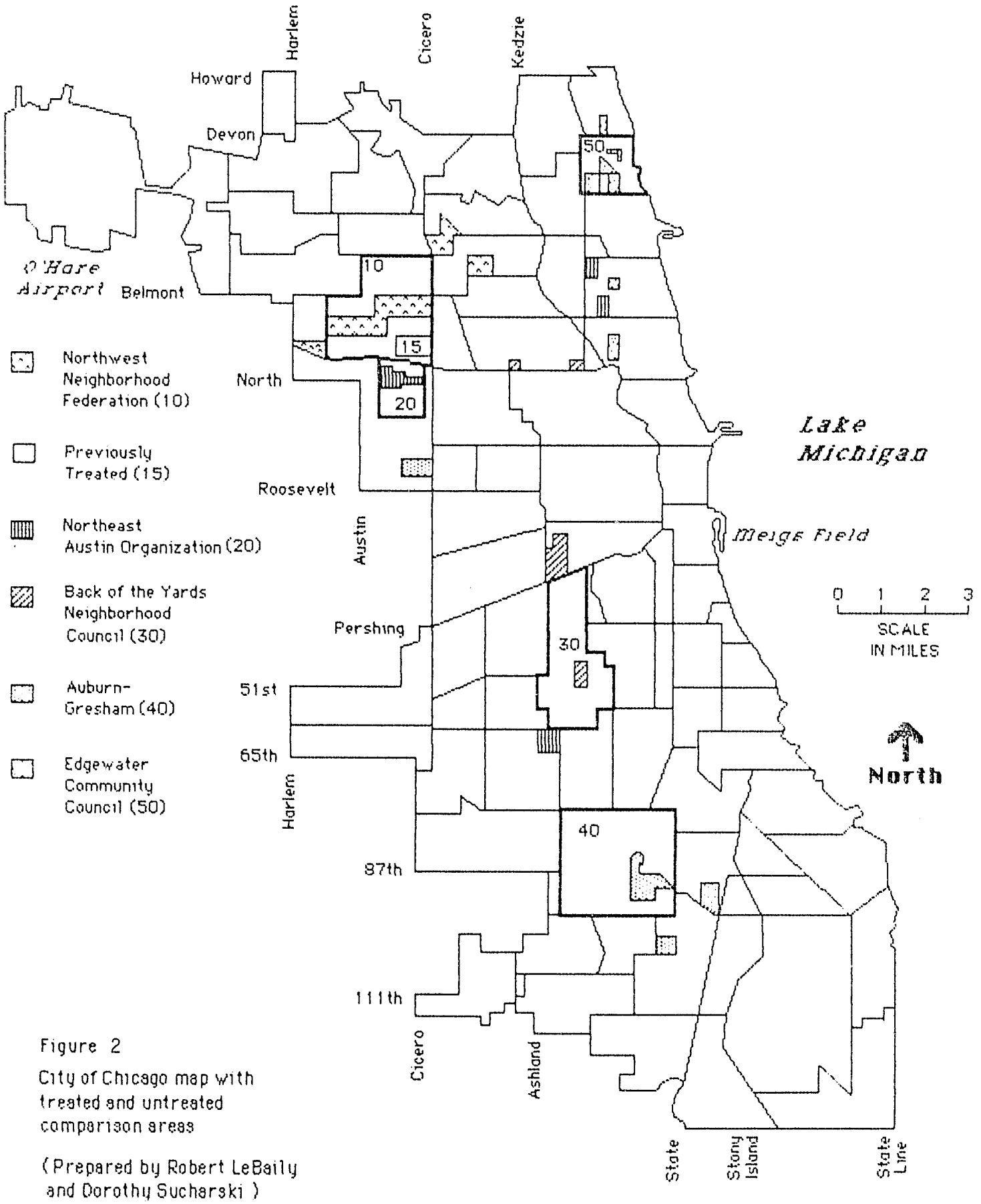


Figure 2

City of Chicago map with treated and untreated comparison areas

(Prepared by Robert LeBailly and Dorothy Sucharski)

neighborhood was more complex and completely controlled by the evaluators, as described in the next section.

A close-up look at the areas selected by the organizations to receive the program is provided in Figures 3 thru 7. As can be seen, some organizations selected large target areas (e.g. NNF), while others selected small areas (e.g. BYNC).

Untreated Comparison Neighborhoods. As noted earlier, our research design called for two primary types of control groups: (1) untreated comparison neighborhoods that are similar to the treated neighborhoods on some basic characteristics, and (2) a city-wide comparison group that is not vulnerable to local history and should detect city-wide changes. The selection of untreated comparison neighborhoods was a process that requires explanation and justification.

A number of strategies were considered for selecting the best possible control groups. Previous evaluations suggest a number of possibilities, including "doughnut" control areas that surround the treated area, adjacent areas, remote areas, and city-wide samples, each of which has some advantages and disadvantages. Furthermore, interest in the equivalence of treated and untreated areas has varied from one evaluation to the next. Because we were interested in neighborhood-level effects (as discussed in the next section), the issue we faced was how to pick similar (yet admittedly non-equivalent) untreated neighborhoods without creating problems such as overmatching or "putting all our eggs in one basket".

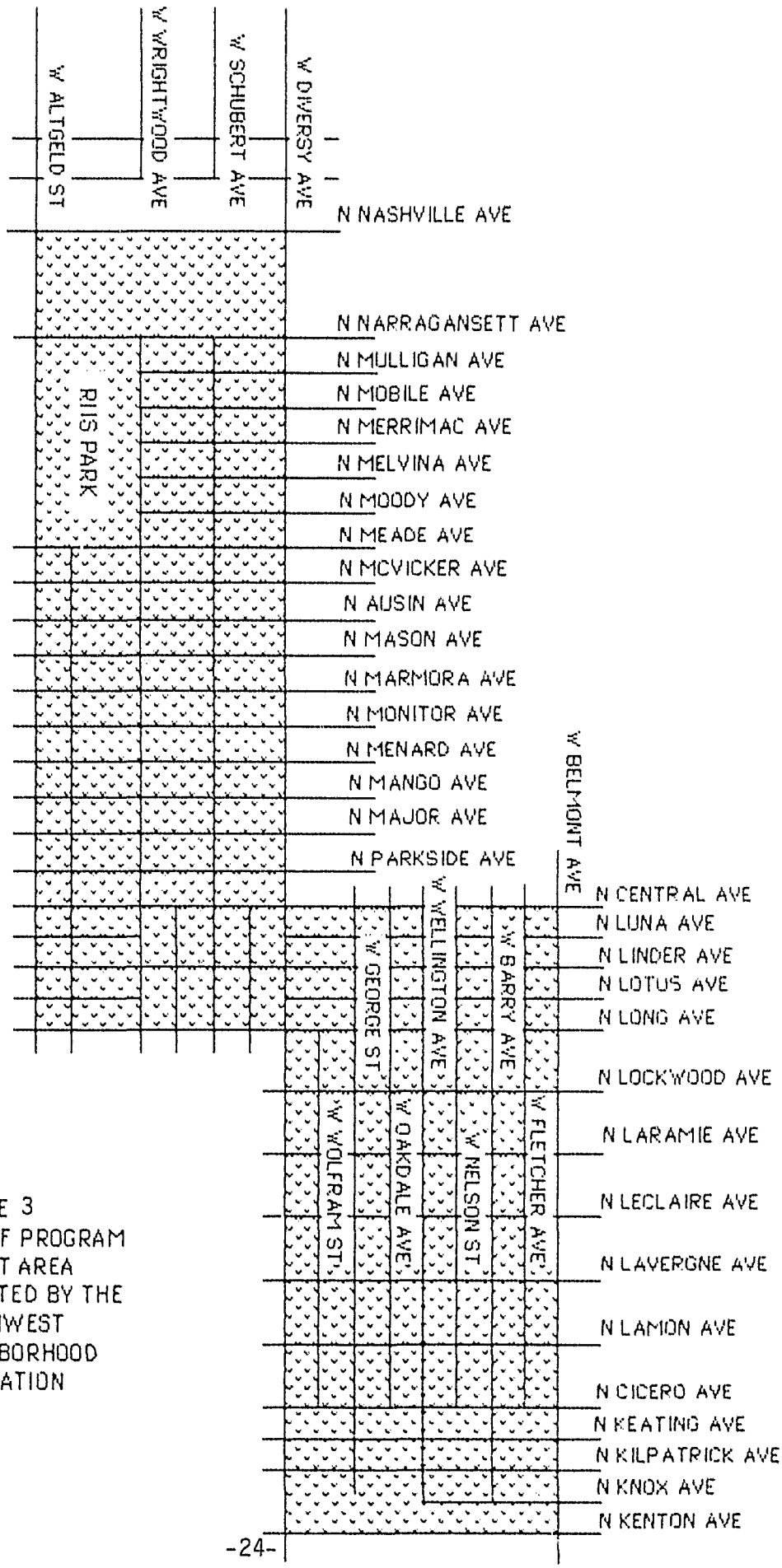


FIGURE 3
MAP OF PROGRAM
TARGET AREA
SELECTED BY THE
NORTHWEST
NEIGHBORHOOD
FEDERATION

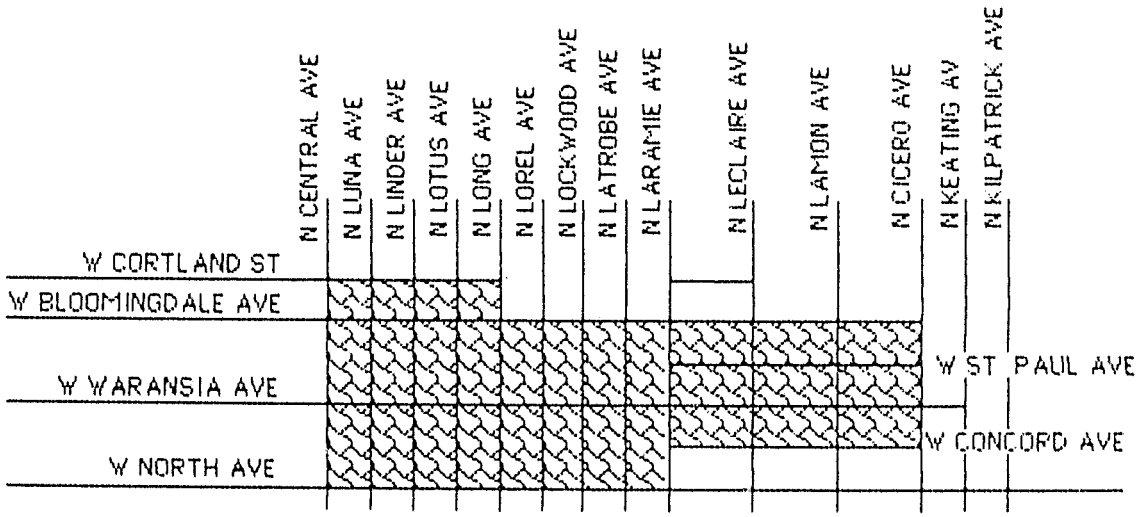


FIGURE 4
MAP OF PRGOGRAM TARGET AREA
SELECTED BY THE
NORTHEAST AUSTIN
ORGANIZATION

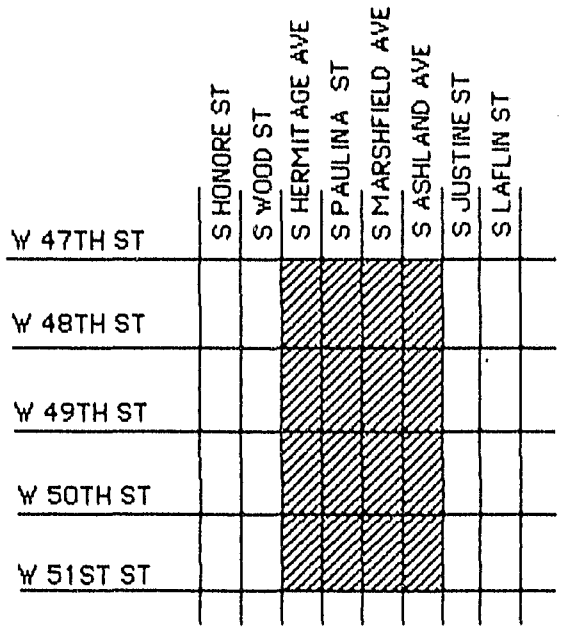


FIGURE 5
MAP OF PROGRAM TARGET AREA
SELECTED BY BACK OF THE YARDS
NEIGHBORHOOD COUNCIL

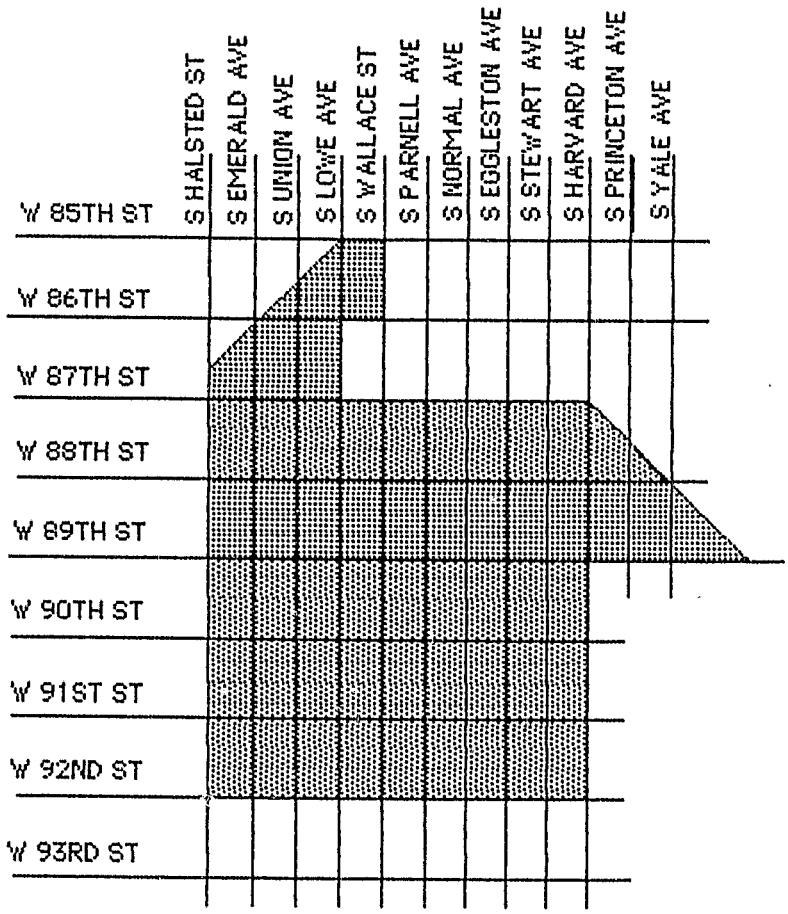


FIGURE 6
MAP OF PROGRAM TARGET AREA
SELECTED BY AUBURN-GRESHAM

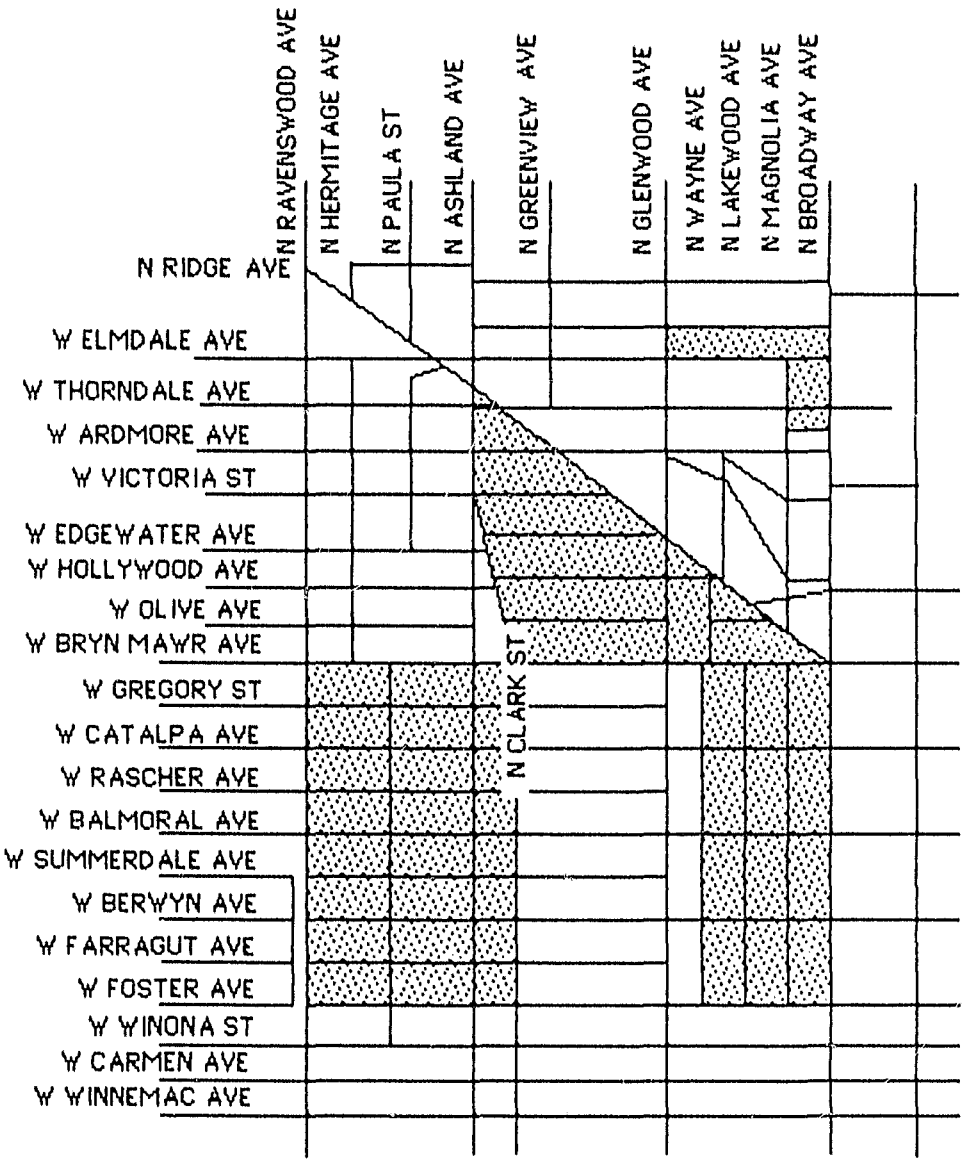


FIGURE 7
MAP OF PROGRAM TARGET
AREA SELECTED BY THE
EDGEWATER COMMUNITY COUNCIL

To avoid the shortcomings of using a single location as a comparison neighborhood, we decided to use a pooled control group that would contain data from three separate locations within the city. We reasoned that this strategy would provide more stability and robustness to the findings. The following procedures were used to identify and select untreated comparison areas:

(1) Each of the treatment sites was profiled using 1980 census data on: median value of owner-occupied units, median contract rent, percentage of housing units that are rentals, racial composition (percent Black, Hispanic, and Caucasian), and age distribution (percentage of youths and older citizens).

(2) For each of these distinct profiles, the pool of more than 800 census tracts in Chicago was searched to identify a group of 10 to 15 sites that could serve as possible comparison areas. (A few sites were eliminated from consideration if a map of the area indicated that these census tracts would be unlikely to represent a "neighborhood" because of physical layout (e.g. railroad tracks that divide an area). A total of 60 different census tracts were identified as possible control sites for the 5 treatment areas.

(3) To confirm the general absence of crime prevention programs, we conducted brief telephone interviews with representatives of local community organizations in more than one-third of the census tracts. None of the interviewers had any knowledge of specific programs in the area of interest. We discontinued these interviews to avoid the possibility of planting ideas about crime prevention, and left this measurement process to the pretest interviewers.

(4) For each treatment site, three census tracts were randomly selected from the pool of 10 to 15 census tracts that were available because of their rough equivalence to the treatment area.

The selection criteria and the outcome of this selection process are shown in Tables 1 thru 5. If a treatment area was comprised of multiple census tracts, the mean and range of scores on the selection criteria were used to establish a range of acceptable values for comparison neighborhoods. The three areas selected as comparisons are

Table 1

Census Criteria Used to Select Comparison Neighborhoods
For Northwest Neighborhood Federation

Selection Criteria

Neighborhood	Primary[1]						Secondary		
	Median[3] Housing Value	Median Rent	Percent[4] Rental	Percent Spanish	Percent White	Percent Black	Percent Pop. Under 5 yrs.	Percent Pop. 5-17 yrs.	Percent Pop. 65 Federation older
Northwest[2] Neighborhood Federation									
Range	51,100-59,800	191-212	36-55	2-14	80- 97	0-0.3	4-6	12-17	13-26
Mean	56,371	204	46	6	91	0	4.9	14.3	19.0
Selection Range[5]	45,000-64,000	185-224	35-59	1-15	80-100	0-4	4-6	12-17	13-26
Comparison Neighborhoods[6]									
1602	57,800	189	48	5	91	0	5	18	15
1609	61,400	207	52	6	91	0	5	19	15
1803	64,500	224	50	4	96	0	4	13	23

[1] "Primary" selection criteria had to be met for inclusion. "Secondary" criteria were examined for extreme values.

[2] Treatment area includes the following 7 census tracts: 1901, 1902, 1903, 1906, 1907, 1908, 1909. Minimum and maximum values are listed for each selection variable.

[3] Median dollar value of owner-occupied units.

[4] Percentage of housing units that are rentals.

[5] Indicates ranges used for selecting comparison census tracts. Ranges are determined by the ranges found in treatment census tracts.

[6] Comparison neighborhoods are identified by census tract.

Table 2

Census Criteria Used to Select Comparison Neighborhoods
For Northeast Austin Organization

Selection Criteria

Neighborhood	Primary[1]						Secondary		
	Median[3] Housing Value	Median Rent	Percent[4] Rental	Percent Spanish	Percent White	Percent Black	Percent Pop. Under 5 yrs.	Percent Pop. 5-17 yrs.	Percent Pop. 65 or older
Northeast Austin[2] Organization									
Range	38,700-40,900	179-188	40-52	29-31	59-62	0-1	7-8	17-20	12-13
Mean	39,800	183	46	30	60	0	7.8	18.5	12.9
Selection Range[5]	32,000-45,000	160-204	35-69	20-40	50-76	0-4	7-8	17-20	12-13
Comparison Neighborhoods[6]									
603	40,000	191	65	23	69	0	7	18	11
627	40,200	161	67	28	67	2	7	17	13
6602	38,400	197	54	26	72	0	8	20	13

[1] "Primary" selection criteria had to be met for inclusion. "Secondary" criteria were examined for extreme values.

[2] Treatment area includes the following 2 census tracts: 2502, 2503. Minimum and maximum values are listed for each variable.

[3] Median dollar value of owner-occupied units.

[4] Percentage of housing units that are rentals.

[5] Indicates ranges used for selecting comparison census tracts. Ranges are determined by the ranges found in treatment census tracts.

[6] Comparison neighborhoods are identified by census tract.

Table 3

Census Criteria Used to Select Comparison Neighborhoods
For Back of the Yards Neighborhood Council

Selection Criteria

Neighborhood	Primary[1]						Secondary		
	Median[3] Housing Value	Median Rent	Percent[4] Rental	Percent Spanish	Percent White	Percent Black	Percent Pop. Under 5 yrs.	Percent Pop. 5-17 yrs.	Percent Pop. 65 or older
Back of Yards[2] Neighborhood Council									
Range[7]	22,900	123	63	40	61	0	8.5	18.6	11.6
Mean	22,900	123	63	40	61	0	8.5	18.6	11.6
Selection Range[5]	18,000-27,000	100-150	50-74	22-50	48-76	0-4	8.5	18.6	11.6
Comparison Neighborhoods[6]									
2220	25,500	138	66	38	61	0	8	20	9
2216	26,100	150	68	45	51	2	9	20	10
3113	23,900	127	69	46	52	0	9	18	13

[1] "Primary" selection criteria had to be met for inclusion. "Secondary" criteria were examined for extreme values.

[2] Treatment area includes the following 1 census tract: 6113.

[3] Median dollar value of owner-occupied units.

[4] Percentage of housing units that are rentals.

[5] Indicates ranges used for selecting comparison census tracts. Ranges are determined by the ranges found in treatment census tracts.

[6] Comparison neighborhoods are identified by census tract.

[7] Treatment area is comprised of only 1 census tract. Hence means are listed rather than ranges.

Table 4

Census Criteria Used to Select Comparison Neighborhoods
For Edgewater Community Council

Selection Criteria

Neighborhood	Primary[1]						Secondary		
	Median[3] Housing Value	Median Rent	Percent[4] Rental	Percent Spanish	Percent White	Percent Black	Percent Pop. Under 5 yrs.	Percent Pop. 5-17 yrs.	Percent Pop. 65 or older
Edgewater Community[2] Council									
Range	53,900-76,000	216-226	69-77	15-27	53-69	0-3	6-7	14-17	11-16
Mean	61,867	221	72	20	62	2	6.4	15.4	14.3
Selection Range[5]	54,000-79,000	190-255	60-80	10-30	50-75	0-6	6-7	14-17	11-16
Comparison Neighborhoods[6]									
109	60,000	247	75	13	65	2	6	18	9
615	55,000	230	77	26	62	1	7	13	13
709	59,400	225	67	19	74	2	6	14	8

[1] "Primary" selection criteria had to be met for inclusion. "Secondary" criteria were examined for extreme values.

[2] Treatment area includes the following 3 census tracts: 305, 308, 309. Minimum and maximum values are listed for each variable.

[3] Median dollar value of owner-occupied units.

[4] Percentage of housing units that are rentals.

[5] Indicates ranges used for selecting comparison census tracts. Ranges are determined by the ranges found in treatment census tracts.

[6] Comparison neighborhoods are identified by census tract.

Table 5

Census Criteria Used to Select Comparison Neighborhoods
For Auburn-Gresham

Selection Criteria

Neighborhood	Primary[1]						Secondary		
	Median[3] Housing Value	Median Rent	Percent[4] Rental	Percent Spanish	Percent White	Percent Black	Percent Pop. Under 5 yrs.	Percent Pop. 5-17 yrs.	Percent Pop. 65 or older
Auburn-Gresham[2]									
Range	36,300-37,200	145-186	15-74	0-0.7	0-1	97- 98	5-6	23-27	5-7
Mean	36,750	165	45	0	0	98	6.1	25.2	6.3
Selection Range[5]	31,000-42,000	135 194	10-74	0-5	0-5	90-100	5-6	23-27	5-7
Comparison Neighborhoods[6]									
2522	36,800	178	69	0	2	97	10	25	4
4907	35,400	187	10	0	1	98	7	27	4
4409	32,900	187	40	1	1	98	7	19	12

[1] "Primary" selection criteria had to be met for inclusion. "Secondary" criteria were examined for extreme values.

[2] Treatment area includes the following 2 census tracts: 4901, 7115. Minimum and maximum values are listed for each variable.

[3] Median dollar value of owner-occupied units.

[4] Percentage of housing units that are rentals.

[5] Indicates ranges used for selecting comparison census tracts. Ranges are determined by the ranges found in treatment census tracts.

[6] Comparison neighborhoods are identified by census tract.

identified in these tables by census tract number and their individual scores on the selection variables are shown.

C. Data Collection Methods and Procedures

Several sources of data and methodologies were employed in this project. The primary methodologies were telephone surveys of neighborhood residents, field work focused on participant observations, interviews with community organization leaders and staff, and reviews of written materials. The telephone surveys generated the primary data for assessing change in residents' perceptions, attitudes, feelings, and behaviors over time, and thus, determining the extent and nature of program impact. In the context of impact assessment, the field work results were used to assist in the planning of survey analyses and the interpretation of survey results.

The same telephone survey was administered under different sampling and design conditions. Specifically, a random digit dial (RDD) telephone survey was conducted, as well as a criss-cross directory survey. Another variant of the telephone survey was whether the residents were interviewed only once (either at time one or time two) or interviewed twice in a panel design. The sampling rationale for these design characteristics are discussed below.

The telephone surveys were completed by trained and supervised interviewers at the Northwestern University Survey Laboratory. The survey, containing more than 200 items on 27 pages, required approximately 30 minutes to complete. The instrument was prepared in both English and Spanish. Hispanic respondents who had difficulty speaking English were interviewed by a Spanish-speaking interviewer.

When the appropriate respondent was selected, the interviewer described the purpose of the study and then initiated the interview. Potential respondents were told that the purpose of the survey was to "assess the quality of life in Chicago neighborhoods."

D. Survey Sampling

Sampling Strategy. Our sampling plan was driven by the central evaluation questions being addressed. The main objective of this research was to assess the impact of specific crime prevention programs on neighborhood residents at the block and neighborhood levels. The impact evaluation was not designed to ascertain the effects of community organizations on local residents, otherwise we would have sampled neighborhoods with and without such organizations. Nor was the research intended to explore how individual participation in crime prevention activities affects individual behavior, otherwise we might have over-sampled the rare network of self-selected individuals that we call "participators." Rather, the central hypothesis under scrutiny suggests that community crime prevention interventions can yield benefits for both participators and nonparticipators who live in the same "treated" areas.

Our prior understanding of the intervention also suggests that there are different versions of the program, depending on the level of implementation. In some areas, the program is implemented at a neighborhood level (e.g. public meetings on neighborhood crime), whereas in other places, it is implemented only at the block level (e.g. block watch meetings). Other organizations pursue a combination of these two strategies. Thus, in the absence of prior knowledge about who would be implementing which strategy, we were forced to

utilize a multi-level sampling scheme. Blockfaces (i.e. pairs of adjacent sides of two city blocks) were randomly selected to represent neighborhood areas and then heads of households were randomly selected to represent each of these selected blockfaces (we will refer to "blockfaces" as "blocks" for ease of discussion).

With emphasis on measuring change in small geographic areas, rather than estimating population parameters, our main sampling frame was the list of published telephone numbers in these target areas. This decision was made after a careful analysis of costs and benefits. The first inclination of any good survey researcher is to use the alternative method of RDD sampling to avoid missing unlisted telephone numbers. While this might be a nice thought, upon closer examination one is forced to conclude that it is extremely difficult and prohibitively costly to execute an RDD survey with screener questions that will allow you to sample respondents in small neighborhoods. It is nearly impossible to screen respondents in (large) prefix areas as to whether or not they live on a particular block. While in-person interviews would have solved the problem of finding unlisted respondents at the block level, the cost of this methodology would have prohibited us from evaluating so many programs in different neighborhoods with adequate control groups.

Leaving aside cost issues, one could argue that the issue of unlisted numbers has been overplayed. First, a reanalysis of RDD data collected by Lavrakas et. al. (1980) in the city of Chicago revealed that the demographic differences between respondents with listed and unlisted numbers was not as great as critics of directory-based samples would have us believe. For example, of the 47 percent of

Chicago residents that were unlisted, only 54 percent were females. More importantly, listing one's home phone number was not strongly related to various crime prevention behaviors or fear of crime. For example, of those who felt "somewhat unsafe" or "very unsafe," 53 percent were unlisted and 47 percent remained in the directory.

Secondly, sample biases do not prevent us from answering the question of whether the intervention produced changes in the perceptions and behaviors of those studied. Especially with panel data, we are able to study changes over time within individual respondents. Again, as evaluators, we can effectively utilize survey methods in a manner that pays less attention to the typical use of survey samples for estimating population responses.

However, we still recognize that directory-based samples may contain biases which limit the generalizability (external validity) of the findings, and external validity is not something we should ignore. In fact, panel data, although still our best test of program effects, can introduce additional biases in our sample. When planning this research we expected that panel attrition would be most likely among renters, younger residents, and those who were generally less attached to the community. Indeed, an analysis of our Chicago data by Lavrakas (1985) confirms this expectation, and documents other differences as well between those who drop out and those who remain in the panel for the posttest. As a response to these expected biases in the panel data, we surveyed a new independent sample of respondents at time two (posttest only). The independent pretest and posttest samples are also directory-based, but at least do not have the attrition problems found in the panel data.

The last defining characteristic of our sampling plan is the method of respondent selection. For all telephone surveys (both RDD and directory-based we used the head-of-household selection technique. The interviewer first determined the number of adults 19 years or older who live at this location and then, following a predetermined selection, asked to speak with the female (or male) head of the household. If there were two (or more) adults of the preselected gender in the household, then the interviewer asked to speak with the one that provided the most financial support to the family. (The Respondent Selection Sheet can be found in Appendix 1).

The respondent selection procedure is an efficient method of producing a head of household sample within each neighborhood. We considered other procedures that would randomly select members of the household, but decided to focus on the neighborhood population for which community crime prevention has the greatest relevance and those who could serve as knowledgeable informants about neighborhood activities. Block watch in particular is a program for adults in the community, especially for those who have property and/or loved ones to protect.

While critics might argue that random selection procedures would produce a vastly different sample, the reality is that the head-of-household technique will capture most of the same respondents. In general, surveys of urban residents tend to find that nearly a third of all respondents live alone, and another 50 percent live in two-person households. If only 15 percent of the respondents live in households of 3 or more persons, then our procedure would only miss roughly 5 percent of the entire population. These are primarily young

adults who are still living at home or older Americans who live with their children. Older persons who live independently would be included in our sample.

Directory Samples. Generating the directory-based samples was a time consuming and complex task. For each of the five treatment neighborhoods, a number of blocks were randomly selected, ranging anywhere from 40 to 100 percent of the total number of blocks in the treatment areas. The number of blocks sampled varied depending on the size of the treatment area and the desired sample size. In four of the treatment neighborhoods, sample sizes of 250 residents were attempted at time one, but because of the small areas targeted in two of these neighborhoods, the average number of completions was 203. In the fifth neighborhood, a larger target area was selected by the community organization, and hence, the time one sample size was set at 400. In fact, 397 interviews were completed. In each of the 15 comparison neighborhoods, the sample size objective was 75 residents at time one or approximately 225 respondents for each pooled control group. A total of 1140 comparison neighborhood surveys were completed at time one, for an average of 228 per pooled control group. The goal of block-level sampling was to complete an average of 4 to 5 interviews per block. Although we ended up with some variation in sample sizes from one block to the next, this average was achieved.

To accomplish this objective, names and phone numbers were randomly selected from each chosen block using the Haines Criss-Cross Directory. Care was taken to exclude commercial blocks or blocks where an adequate sample of residents would not be possible.

RDD Sample. A random sample of all residential telephone numbers in Chicago was generated by matching a "cleaned" set of Chicago residential prefixes with a computer-generated set of 4 random numbers. A total of 812 interviews were completed at time one.

Panel and Independent Samples: The Posttest. The second wave of telephone interviews was instrumental in determining the sample sizes for the panel and independent sample design. The first step was to attempt a second interview with everyone that was interviewed during the pretest. After determining our level of success with this panel sample (i.e. 49.2% of the original sample), the remaining resources were devoted to generating new samples of respondents at time two. For directory-based neighborhood samples, this meant returning to the same blocks used during time one and selecting additional names and phone number of individuals who were not yet interviewed. For the city-wide sample, this meant producing a new RDD sample. The final outcome of this planning is shown in Table 6.

Completion Rates and Sample Sizes. At time one, a total of 3357 interviews were completed. There were 1746 "refusals" of various types, thus yielding a completion rate of 65.8 percent. At time two, we were able to re-interview 1652 respondents from the original sample hence creating our panel sample. In addition, 1172 new "posttest only" residents were interviewed, bringing the total time two sample to 2824. There were 1301 refusals at time two (including over 500 respondents who had moved since time one), thus producing a completion rate of 68.0 percent.

TABLE 6
Sample Sizes

Area	Independent Samples		Panel Samples	
	Pretest[1]	Posttest	Pretest	Posttest
NNF	397	168	226	226
NNF Comparisons	239	85	161	161
NNF Previously Treated	194	92	109	109
NAO	191	103	84	84
NAO Comparisons	221	104	117	117
BYNC	123	56	62	62
BYNC Comparisons	231	111	118	118
ECC	255	112	141	141
ECC Comparisons	216	107	121	121
AG	245	*	149	149
AG Comparisons	233	*	*	*
Citywide	812	234	364	364
TOTAL	3357	1172	1652	1652

* Interviews not conducted because program discontinued.

[1] This is the total pretest data, including panel respondents.

E. Measurement

The survey instrument contained more than 200 items measuring a variety of constructs (The survey can be found in Appendix 2). A careful attempt was made to incorporate "proven" items from previous research on reaction to crime and fear of crime. Instruments from several major studies were reviewed and different measures were compared.

In addition to building upon earlier research we expanded the scope of measurement to include variables in such areas as perceived efficacy and behavioral intervention to solve local problems. Before describing the content of specific measures, the procedures used to develop and test these scales will be summarized.

Scale Development and Testing. Given the number of survey items employed, extensive data reduction was necessary. This effort focused on the development of multi-item scales that could demonstrate the properties of unidimensionality and strong internal consistency. Tests of unidimensionality were performed using Principal Components Factor Analysis in the SPSS program (See Nie et. al., 1975). These analyses were conducted on the entire set of pretest data (N=3357). The results indicate a high degree of success in producing the theoretically expected factor structures at this level. Some of these scaling results were a confirmation of previous research in this field using identical or highly similar items. For example, the identification of two fear of crime dimensions -- one for personal crimes and one for property crimes -- is consistent with the first author's previous research on fear of crime with Terry Baumer (see Baumer & Rosenbaum, 1981; Rosenbaum and Baumer, 1981). Similarly several items

measuring residents' perceptions of neighborhood crime were found to scale nicely, as in our previous work.

Scaling crime prevention behaviors, however, has proven to be a difficult task for researchers, and many alternative conceptualizations have been offered (see Baumer & Rosenbaum, 1981; Lavrakas & Lewis, 1980). Nevertheless, we were able to identify two dimensions of protective behaviors that confirm the few consistent findings of previous research, namely, avoidance and access control behaviors (the latter we call "home protection" behaviors). Additional behaviors were explored, such as home surveillance, because of their relevance to program objectives.

After the initial factor analyses, the scales were tested for internal consistency. A few items that did not contribute significantly to the reliability of the scales as measured by Cronbach's alpha coefficient, and were not conceptually indispensable were deleted. A list of the original scales and their reliability scores are shown in Table 7. The alpha's range from .59 to .91, thus indicating that the items "hang together" to an acceptable degree.

Scale Confirmation. Because most of the analytic comparisons in this evaluation would be performed at the neighborhood level (i.e. comparing one neighborhood against another), we could not assume that the factor structure of these scales would hold up when applied to these smaller subgroups. Hence, we proceeded to replicate the factor analyses for each of the treatment and comparison areas to confirm the unidimensionality of the scales. Table 8 shows the number of factors that emerged in each of the areas tested (The expected/desired outcome was "1" for each test). The results show a high degree of success on

Table 7
List of Original Scales and Reliability Coefficients

Scale Name	No. of Items	Reliability Coefficient[1]
Exposure to Treatment-Related Crime Prevention Activities	4	NA
Participation in Treatment-Related Crime Prevention Activities	2	NA
Efficacy of Block Action	2	.34
Efficacy of Collective Crime Prevention Behaviors	2	.32
Attribution of Responsibility for Crime Prevention	1	NA
Home Protection Behavior	3	.80
Efficacy of Individual Crime Prevention Behaviors	2	.22
Street Avoidance Behavior	2	.49
Percentage of Victimizations Reported to Police	11	NA
Asking Neighbors to Watch Your Home	1	NA
Tendency to Take Action Against Neighborhood Problems	10	NA
Victimization Experience	11	NA
Vicarious Victimization	2	NA
Perceptions of Youth Disorder	4	.78
Youth Rejection of Social Control	4	.91
Neighborhood Deterioration	3	.59
Fear of Personal Crime	2	.63
Fear of Property Crime	2	.36
Perceptions of Neighborhood Crime	4	.78
Optimism About Neighborhood Change	2	.42
Social Cohesion	7	.66

[1]For scales with three or more items, the reliability coefficient is a standardized Cronbach's Alpha coeffcient. For scales with only two items, the coefficient is the zero-order correlation.

NA - additive scale or single-item scale -- reliability not applicable.

Table 8

Confirmation of Scale Unidimensionality
at the Neighborhood Level (Number of Factors)

	Neighborhood Areas									
Scale	NNF	NNF Comparison	NAO	NAO Comparison	BYNC	BYNC Comparison	AG	ECC	ECC Comparison	Chicago Citywide
Neighborhood Crime	1	1	1	1	1	1	1	1	1	1
Group Efficacy	2	1	2	1	1	2	2	1	1	1
Crime Prevention Efficacy	2	2	2	1	1	2	2	2	1	1
Home Protection	1	1	1	1	1	1	1	1	1	1
Youth Disorder	1	1	1	1	1	1	1	1	1	1
Control over Youths	1	1	1	1	1	1	1	1	1	1
Neighborhood Deterioration	1	1	1	1	1	1	1	1	1	1
Social Cohesion	2	1	3	1	3	2	3	2	2	2

most of the scales, but the problem of multi-dimensionality emerged for three of them -- group efficacy, crime prevention efficacy, and social cohesion. After extensive reanalysis, we developed smaller scales or used single items to represent these constructs.

For two-item scales, zero-order correlations were calculated for each of the areas studied. The results in Table 9 show adequate correlates across all scales, with possibly one exception (i.e. efficacy of individual crime prevention). These analyses include some new efficacy scales that were developed in response to the multi-factor problem cited above. Two of the three efficacy scales were used. To measure the third construct -- perceived efficacy of individual crime prevention behaviors -- a single item was employed focusing on target hardening (i.e. perceived helpfulness of "alarm systems, window bars, or special locks" in preventing crime).

Once the scaling analyses were complete, additive composite scales were computed. All variables were assigned equal weight in the equation and, if necessary, the scale was standardized with the assignment of t-score values. The distribution of all variables -- both individual items and composites -- was examined for skewedness, and adjustments were made where appropriate, either by recodes or transformations.

Scale Content. The scales employed covered a broad range of theoretical constructs pertinent to the hypotheses being tested. In total, 23 separate scales were used to assess both the extent of program implementation and the intermediate effects of the intervention on a wide variety of perceptions, emotions, attitudes, and behaviors.

Table 9

ZERO-ORDER CORRELATIONS FOR TWO-ITEM SCALES
AT THE NEIGHBORHOOD LEVEL

Scale	Neighborhood Areas									
	NNF	NNF Comparison	NAO	NAO Comparison	BYNC	BYNC Comparison	AG	ECC	ECC Comparison	Chicago Citywide
Fear of Personal Crime	.61	.65	.50	.54	.53	.74	.52	.57	.64	.63
Fear of Property Crime	.36	.32	.47	.38	.25	.42	.16	.38	.36	.47
Optimism about Neighborhood Change	.38	.30	.47	.54	.33	.35	.33	.54	.56	.38
Street Avoidance Behavior	.42	.56	.50	.54	.27	.66	.49	.49	.37	.51
Efficacy of Block Action	.36	.22	.49	.33	.30	.36	.45	.32	.19	.31
Efficacy of Individual Crime Prevention Behavior	.20	.07	.39	.12	.16	.17	.29	.20	.23	.21
Efficacy of Collective Crime Prevention Behavior	.36	.25	.19	.34	.25	.40	.32	.41	.27	.32

The items contained in each of the scales are shown in Table 10. "Exposure to" and "participation in" treatment-related crime prevention activities were used to measure whether or not a program was actually implemented based on the experiences of local residents. Specifically, their awareness of (4-item scale) and participation in (2-item scale) neighborhood and/or blockwatch meetings were used as the primary indicators of program implementation.

Efficacy was measured in several different ways: (1) the perceived efficacy of individual target hardening behavior in preventing crime, such as locks, bars and alarms; (2) the perceived efficacy of collective crime prevention behaviors, namely blockwatch and citizen patrols, and (3) a more global sense of small group efficacy, i.e. the ability of local residents and block members to effect change in the neighborhood. Along the lines of "empowerment," we have also examined the extent to which citizens alter their perceptions of who is primarily responsible for the prevention of crime--citizens or police.

A variety of self-reported behavioral responses were measured. These include scales focusing on individual protective behaviors (such as avoidance of street crime and home protection measures), as well as items measuring collective responses (such as asking neighborhoods to watch your home while you are away). Other important behavioral measures include the reporting of victimization to the police and the tendency to personally intervene when specific neighborhood problems have been identified.

Two single items were used to measure related aspects of social integration -- the frequency of informal social interaction with

Table 10

ITEMS CONTAINED IN COMPOSITE SCALES

EXPOSURE TO TREATMENT-RELATED CRIME PREVENTION ACTIVITIES

Have you heard or read about any of the following kinds of activities taking place in your neighborhood in the past year or so?

Items: 1. a neighborhood crime prevention meeting? (no, yes)

2. a blockwatch program on your block? (no, yes)

Items: Were you given an opportunity to attend or take part in (3 or 4 below)? Did anyone ask you, or did you see a notice or poster?

3. a neighborhood crime prevention meeting? (no, yes)

4. a blockwatch program on your block? (no, yes)

PARTICIPATION IN TREATMENT-RELATED CRIME PREVENTION ACTIVITIES

Items: Were you able to attend or take part in this...

1. neighborhood crime prevention meeting? (no, yes)

2. blockwatch program on your block? (no, yes)

EFFICACY OF BLOCK ACTION

Please tell me if each statement is mostly true or mostly false, looking at it from your viewpoint.

Items: 1. If we take action, my neighbors and I can make a big difference in in the crime rate around here (mostly true/mostly false)

2. If a few people like me on this block got together, we could get the city to make some improvements in this neighborhood. (mostly true/mostly false)

EFFICACY OF COLLECTIVE CRIME PREVENTION BEHAVIORS

I'd like your opinion on how helpful some things are that people do to prevent crime.

Items: 1. How helpful are block watches where neighbors watch each others homes? (Very, somewhat, not very)

2. What about citizens patrolling their own neighborhoods? (Very, somewhat, not very)

ATTRIBUTION OF RESPONSIBILITY FOR CRIME PREVENTION

Items: 1. When it comes to the prevention of crime in a neighborhood, do you feel that it's more the responsibility of the residents, or more the responsibility of the police? (Residents, police, both, don't know)

HOME PROTECTION BEHAVIOR

Items: 1. Have you installed an alarm system, window bars, or special locks to help prevent break-ins at your home? (no, yes)

2. Have you engraved any of your valuables to help recover them in case they are stolen? (no, yes)

3. Have you had a Home Security Check, where someone made recommendations about new locks and other types of home security? (no, yes)

EFFICACY OF INDIVIDUAL CRIME PREVENTION BEHAVIORS

I'd like your opinion on how helpful some things are that people do to prevent crime.

Items: 1. How helpful are alarm systems, window bars, or special locks in preventing crime? (Very, somewhat, not very)

2. How helpful is marking personal property with an engraving tool? (Very, somewhat, not very)

STREET AVOIDANCE BEHAVIOR

Items: 1. How often do you keep a look out for suspicious people? (Always, most of the time, sometimes, never, don't go out)

2. How often do you avoid walking near certain types of strangers? (Always, most of the time, sometimes, never, don't go out)

PERCENTAGE OF VICTIMIZATIONS REPORTED TO THE POLICE

(For any "yes" responses to the eleven victimization questions listed below under "victimization experience," respondents were asked whether or not the incident was reported to the police. A ratio of victimizations reported to victimizations was calculated).

ASKING NEIGHBORS TO WATCH YOUR HOME

Items: 1. When you are away from home for at least a couple of days, how often do you have a neighbor watch your home? (Always, most of the time, sometimes, never).

TENDENCY TO TAKE ACTION AGAINST NEIGHBORHOOD PROBLEMS

For the big problems in your neighborhood, I'd like to ask if you were able to take any action. First, you said that (1-10 below) was a big problem in your neighborhood. Have you taken any action to try to solve this problem?

Items:
1. Groups of teenagers hanging out on the streets? (yes/no)
2. People selling illegal drugs? (yes/no)
3. Vandalism (like kids breaking windows or writing on walls or things like that)? (yes/no)
4. Noisy neighbors (people playing loud music or having late parties)? (yes/no)
5. Gang activity? (yes/no)
6. Abandoned buildings or vehicles? (yes/no)
7. Garbage or litter on the streets and sidewalks? (yes/no)
8. Certain kinds of people moving into the neighborhoods? (yes/no)
9. Landlords who don't care about what happens to the neighborhood? (yes/no)
10. People who say insulting things or bother people as they walk down the street? (yes/no)

SOCIAL INTEGRATION - SINGLE ITEMS

Items: 1. How many of the people on your block do you know by name -- all of them, most of them, some, hardly any, or none?

2. How often do you chat with your neighbors when you run into them on the street? Do you do this .. (Always, quite often, sometimes, never)

VICTIMIZATION EXPERIENCE

During the past year, in the neighborhood where you live now...

Items:

1. Did anyone enter, or try to enter, your home who didn't belong there, to steal something? (no, yes)

2. Did anyone steal something from inside your home in the past year who had permission to be there, such as a repairman, delivery man, or neighbor? (no, yes)

3. Have you had anything taken that you left outside your home? (Not motor vehicle) (no, yes)

4. Did anyone deliberately damage or deface the building you live in, such as writing on the walls, breaking windows or tearing things up outside? (no, yes)

5. Did anyone steal that (car/truck), or try to steal it? (no, yes)

6. Did anyone deliberately damage that (car/truck) or vandalize it, such as scratching it up, breaking windows, or slashing tires? (no, yes)

7. Did anyone take or try to take something directly from you by using force or threatening you with harm? (no, yes)

8. Has anyone picked your pocket or taken a bag or package directly from you without using force or threatening you? (no, yes)

9. Have you received any threatening or obscene phone calls? (no, yes)

10. Has anyone physically attacked you or has anyone threatened or tried to hurt you even though they did not actually hurt you? (no, yes)

11. (ASKED OF FEMALES ONLY) Has anyone tried to sexually attack you? (no, yes)

VICARIOUS VICTIMIZATION

Items:

1. Do you personally know of anyone, other than yourself, whose property has been stolen, destroyed, or damaged, such as breaking into their home, slashing the tires on their car, or stealing their bicycle? (no, yes)

2. Do you personally know of anyone, other than yourself, who has been robbed or physically attacked, or had someone threaten them or try to harm them during the past year? (no, yes)

PERCEPTIONS OF YOUTH DISORDER

Now I'd like you to tell me whether each of the following is a big problem, some problem or almost no problem in your neighborhood.

Items:
1. Groups of teenagers hanging out on the streets?
2. People selling illegal drugs?
3. Vandalism (like kids breaking windows or writing on walls or things like that)?
4. Gang activity?

YOUTH REJECTION OF SOCIAL CONTROL

Now I'm going to read you a few statements that may be true about children who live in your neighborhood. I'd like you to tell me whether each of these things is a big problem, some problem, or no problem with regard to the children in your neighborhood.

Items:
1. Doing things that might get them in trouble with the police.
2. Not getting enough supervision.
3. Not minding their parents.
4. Not respecting other people and their belongings.

NEIGHBORHOOD DETERIORATION

Now I'd like you to tell me whether each of the following is a big problem, some problem, or almost no problem in your neighborhood.

Items:
1. Abandoned buildings or vehicles?
2. Garbage or litter on the streets and sidewalks?
3. Landlords who don't care about what happens to the neighborhood?

FEAR OF PERSONAL CRIME

Items:
1. How safe do you feel or would you feel being out alone in your neighborhood at night? (Very safe, somewhat safe, somewhat unsafe, or, very unsafe?)
2. When you are walking alone in your neighborhood at night, how concerned are you that someone will harm you or take something from you by force or threat? Are you ... (Not at all concerned, somewhat concerned, quite concerned, or very concerned? Don't go out at night.)

neighbors on the street, and their knowledge of the identity (names) of persons living on their block.

Crime and disorder were measured using five separate scales. An 11-item victimization scale was used that covers the major "Crime Index Offenses" (with the exception of arson), as well as criminal damage to property. Vicarious victimization was measured in terms of the respondent's personal knowledge of other individuals who have been victimized by personal or property crimes. In addition, two disorder scales we utilized, one measuring the extent of youth disorder in the neighborhood (e.g. "hanging out," selling drugs, vandalism, gang activity) and the other serving as an indicator of physical deterioration or disinvestment from the neighborhood (e.g. abandoned buildings or vehicles, garbage or litter, disinterested landlords). Similar social and physical components of disorder have been used in previous research. Finally, we explored another aspect of deviance that has not been carefully studied, but would appear to be central to the concept of informal social control, namely, the extent to which neighborhood youths reject the social control attempts (e.g. not minding their parents, not respecting other people or property, getting in trouble with the police).

Perceptual and emotional responses to crime have been the backbone of most research on reactions to crime over the past few years. As noted earlier, we used two previously validated scales for measuring fear of crime, one focusing on fear of property victimization and the other on fear of personal victimization. Maintaining the well established distinction between perceptions of the general problem and perceptions of personal risk of victimization (cf. Baumer & Rosenbaum,

1981; Lavrakas, Rosenbaum & Kaminski, 1982; Tyler & Cook, 1984), we used another previously validated scale to measure resident's perceptions of the magnitude of the local crime problem (e.g. assessments of general crime rates and frequency-of-occurrence assessments for assaults, robberies, and burglaries).

Finally, we were interested in assessing residents' overall perceptions of neighborhood change and the implications of these perceptions for their own future behavior. Along these lines we used a two-item scale to measure their optimism about neighborhood change -- whether the neighborhood was becoming a better place to live, is getting worse, or is staying about the same (cf. Fowler & Mangione, 1979). To obtain a behavioral measure of confidence in the neighborhood, we asked respondents about the likelihood of their moving in the next two years.

III. RESULTS

A. Analysis Strategy

The overall analysis strategy was to test the seven primary hypotheses articulated earlier using both panel data and independent sample data. Essentially, the analyses performed were testing for the expected differential change over a one year period between the "treated" and "untreated" areas.

More than one analysis strategy was utilized because of differences in the way the "treatment" or program was administered, as well as differences in our sampling procedures. Recall that we expected some programs to be administered at the neighborhood level (e.g. neighborhood-wide meetings) and others to be carried out at the block level (e.g. blockwatch meetings). Indeed, our field work revealed that both types of programs were being implemented, even though only one of the four neighborhoods made a serious investment in the block-level approach. Thus, we performed neighborhood-level analyses on all neighborhoods, using neighborhood as a dummy variable to represent the treatment (0 = untreated neighborhood, 1 = treated neighborhood). Block level analyses (0 = untreated block, 1 = treated block) were conducted on one neighborhood that implemented the program on a block-by-block basis.

Rarely is one justified in analyzing data at more than one level of analysis. However, the circumstances described above were such that potentially different processes were operating at the different levels of treatment implementation. In addition, our sampling of blocks and individuals on blocks paved the way for this analysis

strategy. The bulk of this report is concerned with neighborhood level effects. Near the end of the Results section the block level analyses will be discussed as an alternative test of the main hypotheses.

Type of Analysis. The primary analysis of program impact was conducted within a hierarchical multiple regression framework. This framework was modified, depending on the type of sample being analyzed. The two major types of samples were panel samples and independent random samples.

For panel samples, the posttest scores on the variable of interest were used as the dependent variable and predictor variables were entered into the regression equation in three distinct groupings. First, the pretest scores on the variable of interest were entered to control for pretest differences between the treatment and control groups, as well as control for a large amount of within-person variance. Indeed, our results show repeatedly that the individual's pretest scores often account for more than one-third of all the variance in the posttest. These correlations also provide us with a good indication of the reliability of our measures. Second, we entered a group of important covariates, most of which have been shown to correlate strongly with the outcome measures in prior research. The following covariates were used for virtually all regression analyses: the respondent's sex, age, race, educational level, occupancy status (owner or renter), victimization history, and vicarious victimization history (knowledge of other victims). To illustrate the importance of these variables, previous research has shown that sex and age, for example, are consistently the strongest

predictors of fear of crime. However, as a testimony to the importance of pretest scores, our regression analyses frequently demonstrated that these important covariates no longer made a significant contribution to the prediction equation after controlling for pretest differences. Third, and finally, we entered into the regression equation the dummy variable representing treatment status to determine if the presence or absence of the crime prevention program (as represented by different neighborhoods or blocks) could add significantly to the amount of variance in the posttest already explained by the pretest and other covariates. If so, we would be inclined to say there might be a "treatment effect", i.e., the program made a difference in the outcome variable of interest.

For the independent random samples, a totally different approach was necessary. Rather than settle for a simple independent-groups t-test, we decided to approximate the covariance strategy used with the panel data and to do so, we used a regression framework. Specifically, after merging the two independent samples, we then pooled responses on variables of interest and created a new set of variables representing both groups (time one and time two). Unlike the case of panel data, the variable used to assess treatment effects is no longer simply defined as membership in the treated or untreated neighborhood/block. A treatment effect was defined as an interaction between two variables: treatment status (0 = untreated area; 1 = treated area) and time of measurement (0 = time one pre-program implementation; 1 = time two post-program implementation). A main effect for treatment status using pooled independent samples simply indicates differences between the two groups, but does not address the

question of changes over time. Conversely, the time of measurement variable does not specify which group is changing at what rate. However, a significant interaction between these two variables would indicate differential change over time between the treated and untreated groups, which is what we are interested in when assessing program effects.

Using pooled scores, the same hierarchical multiple regression framework used for analyzing the panel data was applied once again to the independent samples, with the obvious exception that pretest scores were not available as a covariate. First, we entered the covariates used in the panel analyses into the regression equation. Second, we entered treatment status and time of measurement. Finally, we entered the interaction term (treatment status x time of measurement) to test for a treatment effect.

Interpreting Tables and Figures. For all regression analyses, the tables contain information about changes in the proportion of variance accounted for at each step in the hierarchical procedure (See "Cum R^2"), the standardized regression coefficient associated with the treatment variable (See "Beta") and the F value used to test the significance of that beta (See "F Beta"). For the panel analyses, similar information is also provided with regard to the role of the pretest. To avoid the presentation of massive tables, the betas and F values for the seven individual covariates are not shown.

The figures used to illustrate changes over time contain group means that have been adjusted for the covariates listed earlier. Although the regression analyses used to test treatment effects adjusted posttest scores for pretest differences, the pretest

adjustments are not included in the presentation of means so that reasonable comparisons can be made between the pretest (or time one data) and posttest (or time two data). Hence, the adjusted means presented in this report are considered the best approximation of the treatment-related differences that exist between the groups, and a definite improvement over the presentation of unadjusted means.

B. Testing the Main Hypotheses

The results will be presented as they pertain to each of the seven main hypothesis guiding this evaluation. Thus, each hypothesis will be restated below and then examined in light of the available data. The results reported in this section -- which comprise the bulk of the findings -- are based on neighborhood level comparisons of treated and untreated areas. To summarize the possible comparisons, for any given program area and specific outcome measure, there are four possibilities for observing program impact: Each treated area was compared against two control groups -- the "comparison neighborhoods" and the Chicago "citywide sample." These comparisons occur within two samples -- the panel sample and the independent samples.

Testing Hypothesis One: Increased Exposure and Participation

The first hypothesis states that local community organizations, in their efforts to implement the program, should be able to (a) improve residents' awareness of local opportunities to participate in crime prevention activities and (b) stimulate actual participation in these events. These two outcomes are considered direct evidence of treatment implementation.

Exposure/Awareness. The panel results suggest that organizations were quite successful at exposing local residents to the program, i.e., making them aware of opportunities to get involved in crime prevention activities. As Table 11 shows, there were significant "treatment effects" in 7 of the 8 comparisons[3]. That is, after controlling for pretest differences and other covariates, treated neighborhoods showed significant gains relative to untreated neighborhoods over a one year period in terms of having "heard or read about" and having had the "opportunity to attend" a "neighborhood crime prevention meeting" or "blockwatch program on your block".

The adjusted mean changes on the 4-item Exposure scale are shown in Figure 8. All four treated neighborhoods showed increases in residents' exposure to the treatment from 1983 to 1984. However, two of the four -- Northeast Austin Organization (NAO) and Edgewater Community Council (ECC) -- may have capitalized our fairly large (and significant) pretest differences that were present between the treated and untreated areas. In contrast to the panel results, the independent samples showed very little evidence of changes in exposure to crime prevention. As Table 12 reveals, only one treated area -- Northwest Neighborhood Federation (NNF) -- was able to demonstrate a marginally significant increase in exposure, and even here, the difference was evident only when compared to the citywide sample and not NNF's comparison neighborhoods.

We should note at this point that the independent sample results are consistently weaker than the panel results across a wide variety of outcome measures. The possible reasons for this discrepancy will be discussed later. At this point, let us simply say that we have

TABLE 11
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Exposure to the Treatment

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.69	69.69***	.34	52.62***
	Covariates[2]	.23	4.11***	---	---
	Treatment	.24	4.56*	.10	4.56*
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.17	117.97***	.40	108.14***
	Covariates	.21	3.01**	---	---
	Treatment	.22	6.68**	.12	6.68**
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.35	99.20***	.48	52.17***
	Covariates	.39	1.47	---	---
	Treatment	.40	3.23	.12	3.23
Northeast Austin Organization vs. Citywide Sample	Pretest	.23	124.15***	.42	86.55***
	Covariates	.26	2.02*	---	---
	Treatment	.27	8.60**	.14	8.60**
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.23	51.55***	.48	43.66***
	Covariates	.27	.88	---	---
	Treatment	.29	5.34*	.16	5.34*
Back of Yards Neighborhood Council vs. Citywide Sample	Pretest	.17	80.24***	.39	70.24***
	Covariates	.20	1.82*	---	---
	Treatment	.21	4.21*	.10	4.21*
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.14	41.65***	.30	24.83***
	Covariates	.18	1.40***	---	---
	Treatment	.22	12.50***	.21	12.50***
Edgewater Community Council vs. Citywide Sample	Pretest	.18	104.60***	.35	70.60***
	Covariates	.21	2.19***	---	---
	Treatment	.25	23.82***	.22	23.82***
Auburn-Gresham vs. Citywide Sample	Pretest	.17	102.42***	.37	78.25***
	Covariates	.23	3.66*	---	---
	Treatment	.23	4.92*	.12	4.92*

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

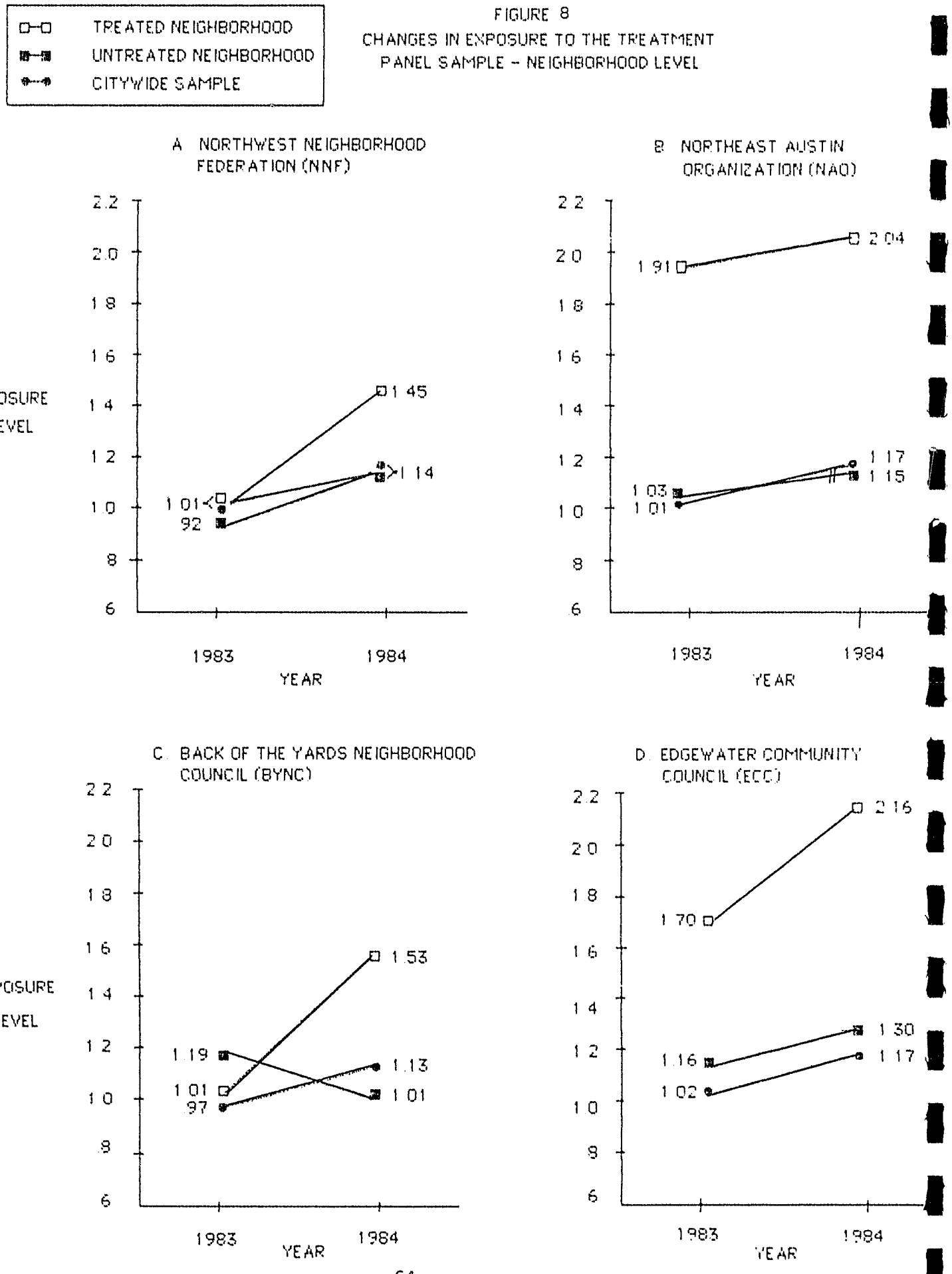


FIGURE 8
CHANGES IN EXPOSURE TO THE TREATMENT
PANEL SAMPLE - NEIGHBORHOOD LEVEL

TABLE 12
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Exposure to the Treatment

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood	Covariates[2]	.05	---	----
Federation vs.	Time & Group Status	.06	---	----
Comparison Neighborhoods	Treatment (TxGS)	.06	-.00	.59
Northwest Neighborhood	Covariates	.04	---	----
Federation vs.	Time & Group Status	.04	---	----
Citywide Sample	Treatment (TxGS)	.04	.07	3.20[1]
Northeast Austin	Covariates	.10	---	----
Organization vs.	Time & Group Status	.20	---	----
Comparison Neighborhoods	Treatment (TxGS)	.20	.08	1.69
Northeast Austin	Covariates	.06	---	----
Organization vs.	Time & Group Status	.10	---	----
Citywide Sample	Treatment (TxGS)	.10	.02	.42
Back of Yards Neighborhood	Covariates	.06	---	----
Council vs.	Time & Group Status	.06	---	----
Comparison Neighborhoods	Treatment (TxGS)	.06	.01	.48
Back of Yards Neighborhood	Covariates	.04	---	----
Council vs.	Time & Group Status	.04	---	----
Citywide Sample	Treatment (TxGS)	.04	.02	.18
Edgewater Community	Covariates	.07	---	----
Council vs.	Time & Group Status	.12	---	----
Comparison Neighborhoods	Treatment (TxGS)	.12	.09	2.55
Edgewater Community	Covariates	.05	---	----
Council vs.	Time & Group Status	.09	---	----
Citywide Sample	Treatment (TxGS)	.09	.06	2.53

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

greater confidence in the panel data for detecting program impact or other changes that may be occurring over time.

A closer look at the items in the Exposure scale revealed that most of the changes were attributable to changes in exposure to block watch meetings rather than neighborhood meetings. The unadjusted panel percentages indicate that the four treated areas showed an overall increase in exposure to block watch by 8 percent (from 21.1% to 29.2% of the residents), while comparison neighborhoods declined by 2.7 percent (from 11.6% to 8.9%) and the citywide remained unchanged, as 16.8 percent of Chicagoans claimed awareness of block watch meetings on their block in both 1983 and 1984. In contrast, a 5.7 percent rise in exposure to neighborhood meetings in treated areas was overshadowed by a 7.8 percent increase in the comparison areas and a 2.8 percent increase citywide.

Participation. Actual levels of participation in relevant crime prevention meetings provide a second and more stringent test of Hypothesis One. It is one thing to make citizens aware of crime prevention meetings, but another to get them to attend and participate. While the effects on participation were not as strong as the effects on exposure (as would be expected), nevertheless, the panel data showed fairly consistent support for the first hypothesis. As shown in Table 13, all four neighborhoods were able to distinguish themselves on participation levels from one of their two control groups, but none was able to distinguish itself from both control groups. Figure 9 shows the direction of these changes over time. Indeed, all four treated neighborhoods demonstrated increases in participation levels among local residents relative to at least one

TABLE 13
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable - Participation in "Treatment" Meetings

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.11	47.90***	.34	41.10***
	Covariates[2]	.15	1.78	---	---
	Treatment	.15	1.90	.07	1.90
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.14	91.12***	.37	87.04***
	Covariates	.16	1.41	---	---
	Treatment	.17	6.97**	.12	6.97**
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.11	22.19***	.28	19.02***
	Covariates	.30	5.57***	---	---
	Treatment	.33	6.45**	.18	6.45**
Northeast Austin Organization vs. Citywide Sample	Pretest	.20	108.41***	.44	101.31***
	Covariates	.23	1.64	---	---
	Treatment	.24	3.22	.09	3.22[1]
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.22	42.27***	.46	43.49***
	Covariates	.25	.73	---	---
	Treatment	.27	3.67	.13	3.67[1]
Back of Yards Neighborhood Council vs. Citywide Sample	Pretest	.17	80.90***	.41	77.98***
	Covariates	.19	1.16	---	---
	Treatment	.19	2.21	.08	2.21
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.11	29.67***	.29	21.88***
	Covariates	.15	1.35	---	---
	Treatment	.16	4.31*	.13	4.31*
Edgewater Community Council vs. Citywide Sample	Pretest	.13	70.86***	.34	59.27***
	Covariates	.16	1.85	---	---
	Treatment	.16	3.21	.08	3.21
Auburn-Gresham vs. Citywide Sample	Pretest	.20	119.94***	.43	105.40***
	Covariates	.21	1.24	---	---
	Treatment	.21	.01	.01	.01

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .01$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

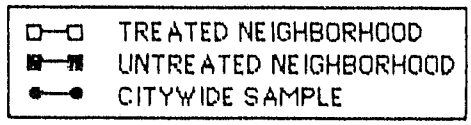
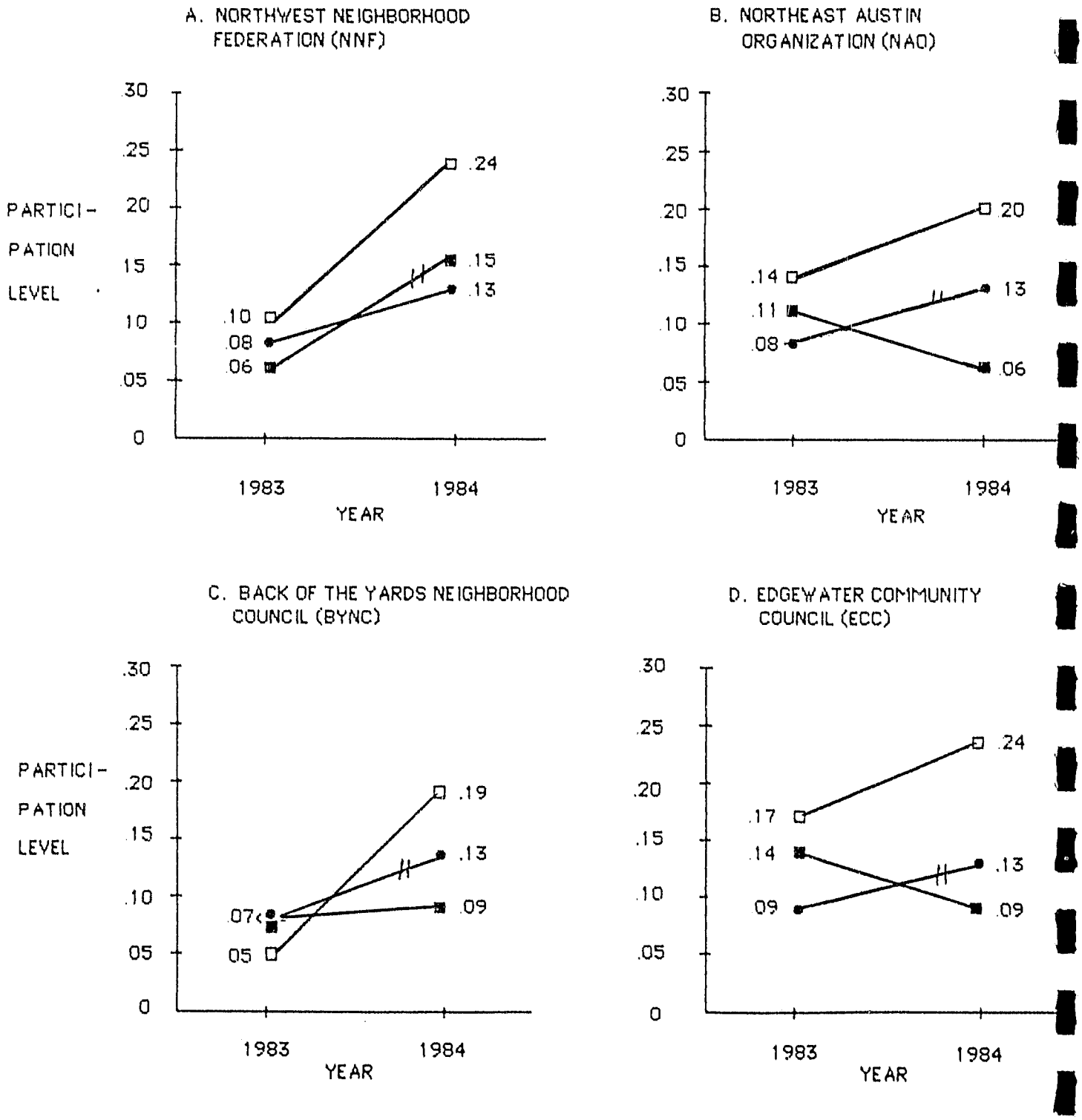


FIGURE 9
CHANGES IN PARTICIPATION
PANEL SAMPLE - NEIGHBORHOOD LEVEL



control group. However, NAO was able to capitalize on declining participation in the untreated comparison neighborhoods. Only NNF was able to demonstrate a significant increase relative to the Chicago citywide sample.

The independent samples revealed changes in participation for two of the four neighborhoods (see Table 14). As Figure 10 shows, these changes were increases in participation in NNF and NAO relative to the controls.

These participation findings should be tempered by our assessment of the amount and prevalence of participation, and by extensive field work on the level of investment by each community organization. First, we should note that, although participation levels have increased significantly relative to certain control groups, both the magnitude of change and the absolute levels of participation remain rather small. For example, the unadjusted panel percentages indicate that treated areas as a whole showed only a 3.9 percent increase in participation, from 12.3 percent of the residents in 1983 to 16.2 percent in 1984. The citywide control group showed an increase of 7.0 percent (from 6.1% to 13.1% of Chicagoans) and the neighborhood control groups showed a 1.4 percent rise in participation (from 8.1% to 9.5%).

Our field work clearly suggests that only one organization -- NNF-- seriously adopted the block watch philosophy and program. Two groups -- NAO and BYNC -- preferred to implement the program via neighborhood meetings geared to the entire community or to specific audiences (e.g., church groups). Thus, holding a few neighborhood meetings over the course of the year might be sufficient to produce a

TABLE 14
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Participation in "Treatment" Meetings

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.02	---	----
	Time & Group Status	.04	---	----
	Treatment (TxGS)	.04	.08	1.48
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.04	---	----
	Time & Group Status	.04	---	----
	Treatment (TxGS)	.04	.11	9.29**
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.04	---	----
	Time & Group Status	.06	---	----
	Treatment (TxGS)	.06	.13	3.85*
Northeast Austin Organization vs. Citywide Sample	Covariates	.04	---	----
	Time & Group Status	.04	---	----
	Treatment (TxGS)	.05	.06	2.20
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.03	---	----
	Time & Group Status	.03	---	----
	Treatment (TxGS)	.03	-.02	.15
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.05	---	----
	Time & Group Status	.05	---	----
	Treatment (TxGS)	.05	.02	.28
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.06	---	----
	Time & Group Status	.07	---	----
	Treatment (TxGS)	.07	.04	.45
Edgewater Community Council vs. Citywide Sample	Covariates	.05	---	----
	Time & Group Status	.06	---	----
	Treatment (TxGS)	.06	.06	2.62

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

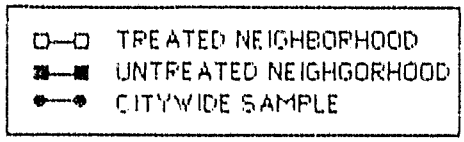
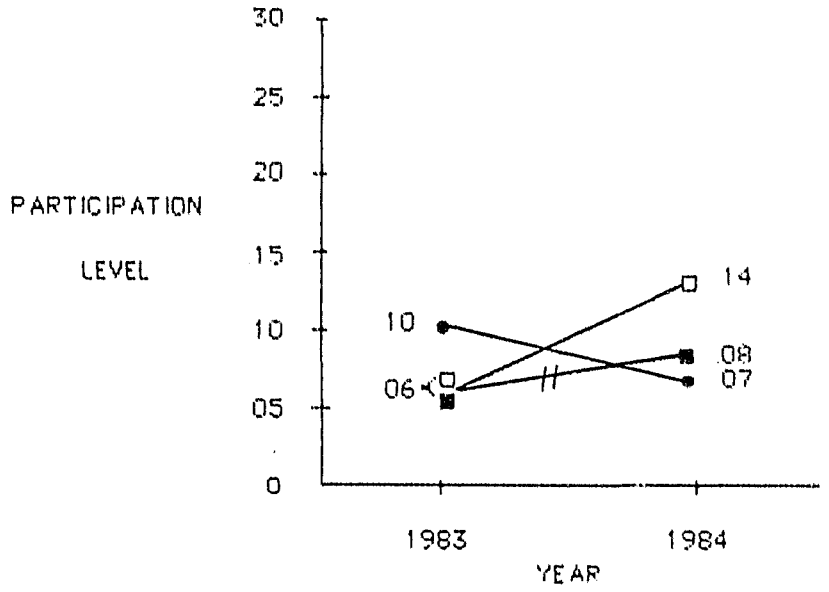


FIGURE 10

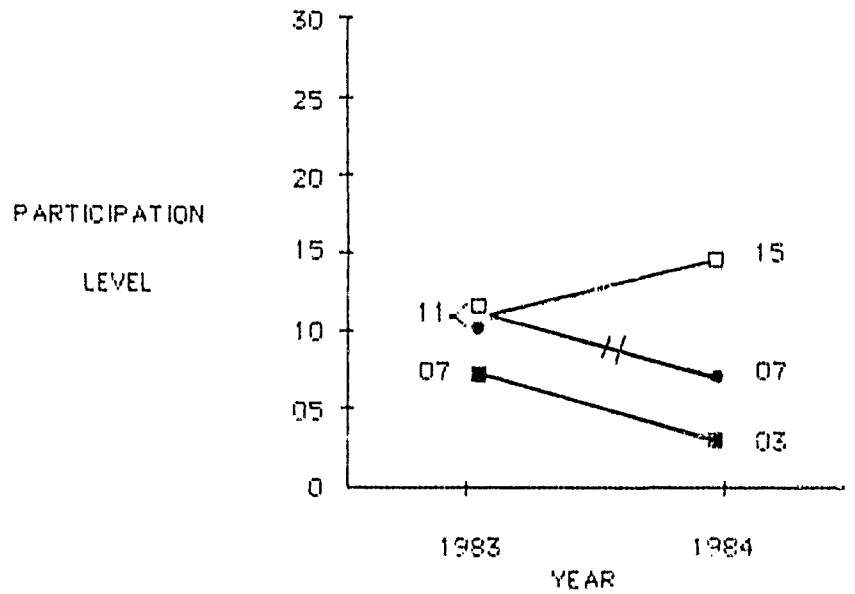
CHANGES IN PARTICIPATION

INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

A NORTHWEST NEIGHBORHOOD FEDERATION (NNF)



B NORTHEAST AUSTIN ORGANIZATION (NAO)



significant increase in neighborhood participation (as measured by our surveys). Our field work indicates that ECC did not start any block watches as part of the program, but the group continued to meet on a regular basis. Thus, attendance at the ECC meetings may have been sufficient to account for the increase in participation, but this is only speculation.

Even the organization that vigorously pursued the block watch model overestimated what it could accomplish in one year. The NNF staff targeted approximately 200 blocks for organizing, of which we sampled approximately 80 blocks for data collection. NNF organized less than half the number of blocks projected between the pretest and posttest and of these, 20 were contained in the sample. Thus, only 1 out of 4 blocks in the NNF "treated" sample were in fact treated. (For a detailed discussion of what actually occurred in the field, see Volume Two of this final report by Lewis, Grant, and Rosenbaum, 1985).

In sum, we found some consistent, albeit, weak support for Hypothesis One in the panel data. Residents in treated neighborhoods showed significant increases in awareness of, and participation in, crime prevention meetings relative to certain untreated areas. While these data stand as encouraging evidence that the organizations did, in fact, implement some type of program, there remain serious doubts about the strength/dosage of the treatment given the limited number of people involved, number of meetings held, and number of blocks organized during the implementation period.

Testing Hypothesis Two: Greater Efficacy and Responsibility

Hypothesis Two states that contact with the program, either in terms of greater awareness or actual participation, should enhance

feelings of efficacy about local collective action and increase the tendency to attribute responsibility to citizens (rather than police) for the prevention of crime. First, we will examine the data on three efficacy scales, and then look at perceived responsibility for crime prevention.

Efficacy of Block Action. Did the intervention help to "empower" the local residents and make them feel that people on their block can make a difference in the neighborhood? For some, the answer is "yes" for others the answer is "no". In fact, the panel data (depicted in Table 15 and Figure 11) indicate that feelings of efficacy about block level action unexpectedly declined in two neighborhoods (NNF and BYNC), while it increased in two others as predicted (NAO and ECC). Although NNF and BYNC showed declines in efficacy relative to comparison neighborhoods, they did not show more rapid declines than the city as a whole. The independent samples were able to replicate the increases in NAO and ECC, but not the declines in NNF and BYNC (see Table 16 and Figure 12).

Efficacy of Collective Crime Prevention Behavior. Did the programs change residents' attitudes about the efficaciousness of collective crime prevention behavior, such as block watches and citizen patrols? With one exception, the results presented in Tables 17 and 18 indicate no effects across all comparisons. (In the independent samples, there was a marginally significant increase in efficacy in NNF relative to Chicago). Thus, overall, residents' beliefs about the helpfulness of collective citizen action in preventing crime were not altered by these programs.

TABLE 15
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Efficacy of Block Action

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood	Pretest	.06	20.79***	.24	20.99***
Federation vs.	Covariates[2]	.07	.85	---	---
Comparison Neighborhoods	Treatment	.10	7.72**	-.15	7.72**
Northwest Neighborhood	Pretest	.12	68.88***	.31	56.75***
Federation vs.	Covariates	.14	1.67[1]	---	---
Citywide Sample	Treatment	.14	.16	-.02	.16
Northeast Austin	Pretest	.17	35.51***	.43	34.55***
Organization vs.	Covariates	.20	.77	---	---
Comparison Neighborhoods	Treatment	.20	.73	.07	.73
Northeast Austin	Pretest	.17	82.54***	.39	69.20***
Organization vs.	Covariates	.19	1.16	---	---
Citywide Sample	Treatment	.20	3.21	.09	3.21[1]
Back of Yards Neighborhood	Pretest	.09	15.75***	.26	10.89***
Council vs.	Covariates	.11	.49	---	---
Comparison Neighborhoods	Treatment	.14	5.07*	-.17	5.07*
Back of Yards Neighborhood	Pretest	.15	68.88***	.35	54.12***
Council vs.	Covariates	.18	1.70[1]	---	---
Citywide Sample	Treatment	.19	.91	-.05	.91
Edgewater Community	Pretest	.13	35.75***	.35	33.40***
Council vs.	Covariates	.16	.89	---	---
Comparison Neighborhoods	Treatment	.17	5.22*	.14	5.22*
Edgewater Community	Pretest	.14	75.68***	.35	61.70***
Council vs.	Covariates	.17	1.52	---	---
Citywide Sample	Treatment	.17	3.78*	.09	3.78*
Auburn-Gresham	Pretest	.13	71.18***	.32	51.70***
vs.	Covariates	.17	2.21	---	---
Citywide Sample	Treatment	.17	.00	.00	.00

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariat = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

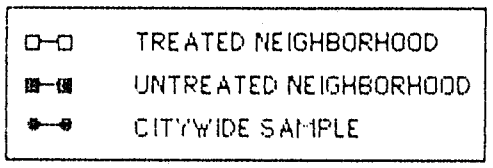


FIGURE 11
CHANGES IN EFFICACY OF BLOCK ACTION
PANEL SAMPLE – NEIGHBORHOOD LEVEL

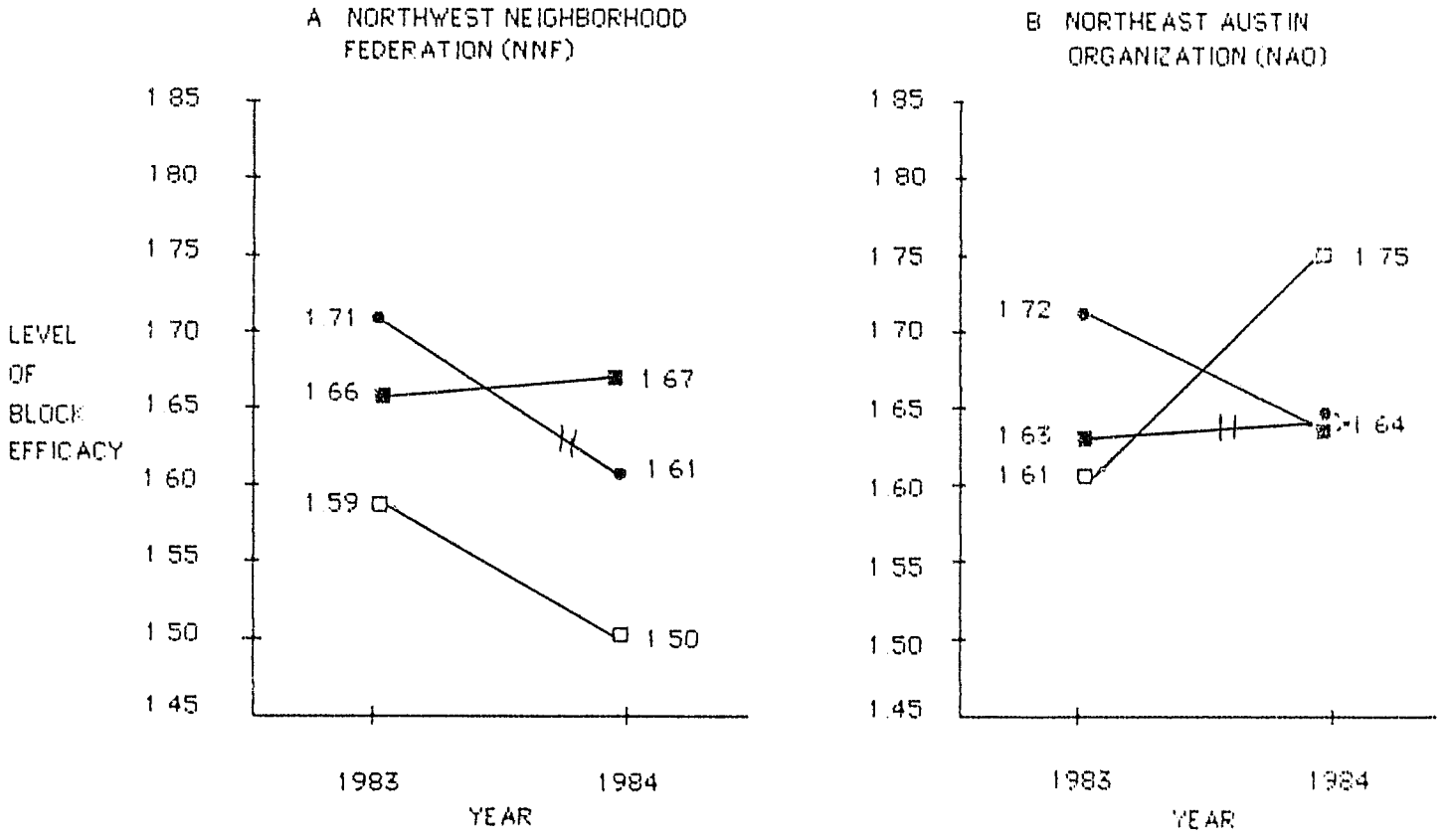


TABLE 16
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Efficacy of Block Action

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.01	---	---
	Time & Group Status	.01	---	---
	Treatment (TxGS)	.01	.06	.90
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.03	---	---
	Time & Group Status	.03	---	---
	Treatment (TxGS)	.04	.03	.84
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.05	---	---
	Time & Group Status	.06	---	---
	Treatment (TxGS)	.06	.04	.35
Northeast Austin Organization vs. Citywide Sample	Covariates	.06	---	---
	Time & Group Status	.06	---	---
	Treatment (TxGS)	.06	.07	3.11[1]
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.04	---	---
	Time & Group Status	.05	---	---
	Treatment (TxGS)	.05	.01	.52
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.05	---	---
	Time & Group Status	.05	---	---
	Treatment (TxGS)	.05	.06	2.15
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.02	---	---
	Time & Group Status	.03	---	---
	Treatment (TxGS)	.03	.10	2.31
Edgewater Community Council vs. Citywide Sample	Covariates	.04	---	---
	Time & Group Status	.04	---	---
	Treatment (TxGS)	.05	.08	3.88*

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

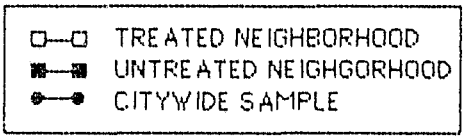
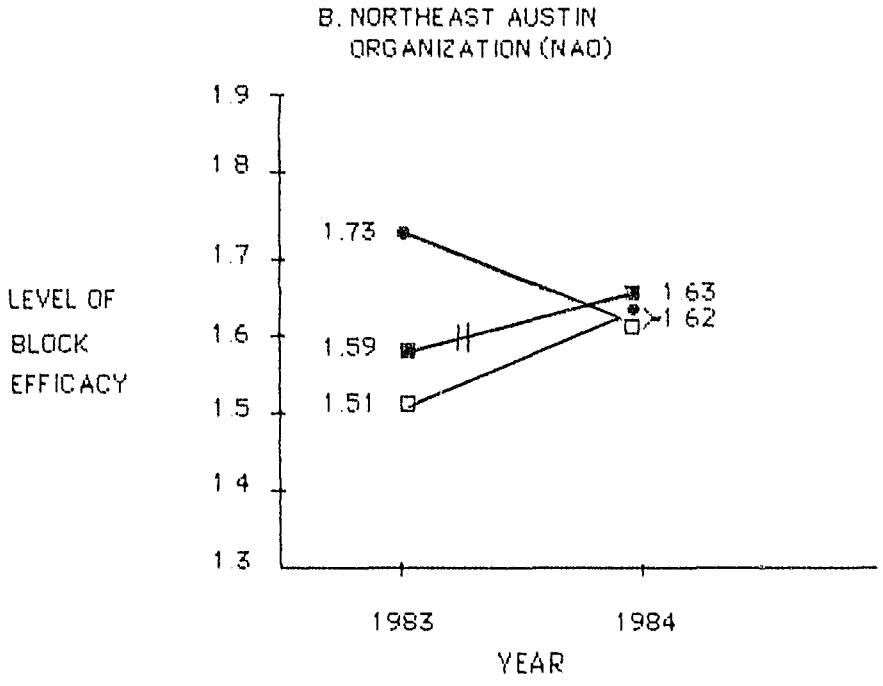


FIGURE 12
CHANGES IN EFFICACY OF BLOCK ACTION
INDEPENDENT SAMPLE - NEIGHBORHOOD LEVEL



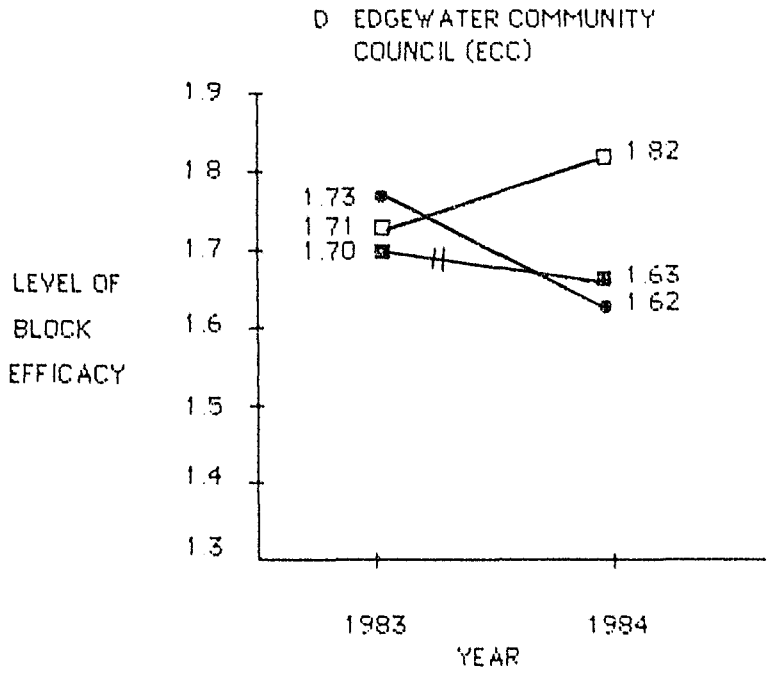


TABLE 17
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Efficacy of Collective Crime Prevention Behavior

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood	Pretest	.10	37.34***	.31	36.79***
Federation vs.	Covariates[2]	.10	.38	---	---
Comparison Neighborhoods	Treatment	.11	.85	.05	.85
Northwest Neighborhood	Pretest	.07	39.51***	.26	35.23***
Federation vs.	Covariates	.09	1.24	---	---
Citywide Sample	Treatment	.09	.55	.04	.55
Northeast Austin	Pretest	.15	32.13***	.37	23.95***
Organization vs.	Covariates	.19	.84	---	---
Comparison Neighborhoods	Treatment	.19	.01	-.01	.01
Northeast Austin	Pretest	.08	34.34***	.26	28.17***
Organization vs.	Covariates	.10	1.12	---	---
Citywide Sample	Treatment	.10	.22	.03	.22
Back of Yards Neighborhood	Pretest	.05	8.37**	.21	7.20**
Council vs.	Covariates	.18	2.62**	---	---
Comparison Neighborhoods	Treatment	.18	.37	.05	.37
Back of Yards Neighborhood	Pretest	.10	40.91***	.29	34.93***
Council vs.	Covariates	.14	1.94	---	---
Citywide Sample	Treatment	.14	.18	.02	.18
Edgewater Community	Pretest	.10	28.03***	.31	25.02***
Council vs.	Covariates	.13	.98	---	---
Comparison Neighborhoods	Treatment	.13	.00	.00	.00
Edgewater Community	Pretest	.07	35.45***	.26	31.22***
Council vs.	Covariates	.09	1.06	---	---
Citywide Sample	Treatment	.09	.29	.03	.29
Auburn-Gresham	Pretest	.11	58.22***	.31	47.10***
vs.	Covariates	.15	2.37**	---	---
Citywide Sample	Treatment	.15	.80	.05	.80

[1] $p \leq .10$; * $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 18
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Efficacy of Collective Crime Prevention Behavior

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.04	---	---
	Time & Group Status	.05	---	---
	Treatment (TxGS)	.05	.09	1.93
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.04	---	---
	Time & Group Status	.05	---	---
	Treatment (TxGS)	.05	.07	3.07[1]
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.06	---	---
	Time & Group Status	.06	---	---
	Treatment (TxGS)	.06	.00	.33
Northeast Austin Organization vs. Citywide Sample	Covariates	.05	---	---
	Time & Group Status	.05	---	---
	Treatment (TxGS)	.05	.02	.18
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.05	---	---
	Time & Group Status	.06	---	---
	Treatment (TxGS)	.06	.00	.22
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.06	---	---
	Time & Group Status	.06	---	---
	Treatment (TxGS)	.06	-.03	.72
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.02	---	---
	Time & Group Status	.03	---	---
	Treatment (TxGS)	.03	.03	.23
Edgewater Community Council vs. Citywide Sample	Covariates	.04	---	---
	Time & Group Status	.04	---	---
	Treatment (TxGS)	.04	.02	.22

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

Efficacy of Individual Target Hardening. Did the interventions persuade residents to believe more strongly in the efficacy of individual home protection measures, such as installing alarm systems or special locks? This was not the case. Tables 19 and 20 reveal a consistent absence of effects on the perceived efficacy of individual target hardening. Only BYNC showed a marginally significant effect, and the direction of change is counter to the hypothesis, i.e., decreased efficacy in comparison to the citywide sample.

Attribution of Responsibility. Did the programs influence residents to think that preventing crime is more the responsibility of citizens than police? Tables 21 and 22 show that the treatments generally had no effect on attributions of responsibility for crime prevention. However, for the one neighborhood where significant effects were observed, the panel results again run counter to the hypothesis. Specifically, NAO residents attributed less responsibility to citizens and more responsibility to the police in comparison to changes in both control groups.

In sum, the support for Hypothesis Two regarding enhancement of efficacy and responsibility is weak, at best. With few exceptions, attributions of responsibility for crime prevention and the perceived efficacy of collective crime prevention were unaffected by the treatment. The third outcome measure -- efficacy of block-level action -- showed contradictory results, as some neighborhoods increased and others decreased. The declines in efficacy occurred in NNF (which used the block watch model) and BYNC (which encouraged residents to rely on BYNC to handle problems) while the increases occurred in NAO (which held neighborhood-wide meetings to respond to pressing issues)

TABLE 19
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Efficacy of Individual Target Hardening

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood	Pretest	.16	62.79***	.39	58.56***
Federation vs.	Covariates[2]	.19	1.36	---	---
Comparison Neighborhoods	Treatment	.19	.05	.01	.05
Northwest Neighborhood	Pretest	.13	75.70***	.36	70.73***
Federation vs.	Covariates	.15	.82	---	---
Citywide Sample	Treatment	.15	.02	-.01	.02
Northeast Austin	Pretest	.06	10.72***	.24	8.67***
Organization vs.	Covariates	.10	.77	---	---
Comparison Neighborhoods	Treatment	.10	.09	.03	.09
Northeast Austin	Pretest	.09	37.83***	.30	36.12***
Organization vs.	Covariates	.11	.73	---	---
Citywide Sample	Treatment	.11	.09	-.02	.10
Back of Yards Neighborhood	Pretest	.17	30.03***	.39	24.29***
Council vs.	Covariates	.20	.61	---	---
Comparison Neighborhoods	Treatment	.20	.82	-.07	.81
Back of Yards Neighborhood	Pretest	.12	47.77***	.33	43.34***
Council vs.	Covariates	.13	.82	---	---
Citywide Sample	Treatment	.14	2.87[1]	-.10	2.87[1]
Edgewater Community	Pretest	.14	35.51***	.37	33.82***
Council vs.	Covariates	.16	.48	---	---
Comparison Neighborhoods	Treatment	.16	1.08	.07	1.08
Edgewater Community	Pretest	.12	58.67***	.35	57.58***
Council vs.	Covariates	.14	.66	---	---
Citywide Sample	Treatment	.14	2.58	.08	2.58
Auburn-Gresham	Pretest	.10	48.86***	.32	45.67***
vs.	Covariates	.12	.85	---	---
Citywide Sample	Treatment	.12	.21	.03	.21

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 20
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Efficacy of Individual Target Hardening

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.01	---	---
	Time & Group Status	.01	---	---
	Treatment (TxGS)	.02	.10	2.45
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.02	---	---
	Time & Group Status	.02	---	---
	Treatment (TxGS)	.02	.00	.16
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.07	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.07	.06	.84
Northeast Austin Organization vs. Citywide Sample	Covariates	.03	---	---
	Time & Group Status	.03	---	---
	Treatment (TxGS)	.03	-.01	.42
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.04	---	---
	Time & Group Status	.04	---	---
	Treatment (TxGS)	.04	.00	.26
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.01	---	---
	Time & Group Status	.02	---	---
	Treatment (TxGS)	.02	-.02	.26
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.02	---	---
	Time & Group Status	.02	---	---
	Treatment (TxGS)	.03	.10	2.59
Edgewater Community Council vs. Citywide Sample	Covariates	.02	---	---
	Time & Group Status	.03	---	---
	Treatment (TxGS)	.03	.03	.54

[1] $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 21
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Attribution of Responsibility for Crime Prevention (Police vs Citizens)

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.12	48.97***	.34	44.12
	Covariates[2]	.15	1.16	---	---
	Treatment	.15	1.01	.05	1.01
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.09	50.93***	.30	50.1***
	Covariates	.11	1.09	---	---
	Treatment	.11	.12	-.02	.12
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.07	12.69***	.27	14.12***
	Covariates	.14	1.54	---	---
	Treatment	.17	6.43*	.20	6.43*
Northeast Austin Organization vs. Citywide Sample	Pretest	.07	27.93***	.25	26.61***
	Covariates	.08	.70	---	---
	Treatment	.09	3.55[1]	.10	3.55[1]
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.01	1.09	.09	1.10
	Covariates	.04	.51	---	---
	Treatment	.04	.50	-.06	.50
Back of Yards Neighborhood Council vs. Citywide Sample	Pretest	.06	22.54***	.25	23.72***
	Covariates	.07	.78	---	---
	Treatment	.08	2.36	.09	2.36
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.08	21.89***	.29	21.97***
	Covariates	.13	1.31	---	---
	Treatment	.13	.01	-.01	.01
Edgewater Community Council vs. Citywide Sample	Pretest	.06	29.46***	.25	29.10***
	Covariates	.08	1.26	---	---
	Treatment	.09	1.13	.05	1.13
Auburn-Gresham vs. Citywide Sample	Pretest	.10	48.60***	.31	47.55***
	Covariates	.12	1.17	---	---
	Treatment	.12	.62	-.05	.62

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 22
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Attribution of Responsibility For Crime Prevention (Police vs. Citizens)

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.04	---	---
	Time & Group Status	.05	---	---
	Treatment (TxGS)	.05	-.04	.32
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.01	---	---
	Time & Group Status	.01	---	---
	Treatment (TxGS)	.01	-.04	1.05
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.03	---	---
	Time & Group Status	.03	---	---
	Treatment (TxGS)	.03	-.07	.96
Northeast Austin Organization vs. Citywide Sample	Covariates	.01	---	---
	Time & Group Status	.01	---	---
	Treatment (TxGS)	.01	-.03	.75
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.02	---	---
	Time & Group Status	.03	---	---
	Treatment (TxGS)	.03	.03	.22
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.01	---	---
	Time & Group Status	.01	---	---
	Treatment (TxGS)	.01	.04	1.05
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.03	---	---
	Time & Group Status	.03	---	---
	Treatment (TxGS)	.03	-.03	.17
Edgewater Community Council vs. Citywide Sample	Covariates	.00	---	---
	Time & Group Status	.00	---	---
	Treatment (TxGS)	.00	-.01	.87

[1] $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

and ECC (whose strategy was never implemented). One might ask -- how could NAO show increases in feelings of efficacy, but decreases in citizens' responsibility for preventing crime? This outcome is possible. One of the repeated messages of community organizers (and NAO in particular) is that citizens can make a difference by organizing themselves and pressuring the police to be accountable and responsive to the needs of their community.

Hypothesis Three: Behavioral Changes

Community crime prevention programs are expected to produce behavioral changes among citizens both in terms of efforts to prevent future victimization and efforts to regulate social behavior. In this section, we will summarize the results from five separate behavioral scales pertinent to this hypothesis, including measures of individual preventive actions, collective preventive actions, and willingness to intervene in neighborhood problems. Overall, the findings are not supportive of Hypothesis Three, and as such, provide little evidence that the interventions were successful at changing residents' behavior over a one year period. The exceptions to this general conclusion are noted.

Home Protection Behavior. We hypothesized that programs would increase individual home protection behaviors, such as installing better locks, engraving valuable property, or having a home security survey. As Table 23 indicates, two of the four neighborhoods (NNF and NAO) showed increases in home protection behavior relative to the citywide sample (NNF was only marginally significant). Figure 13 illustrates these increases and shows why they did not differ from their comparison neighborhoods in the panel sample where increases

TABLE 23
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

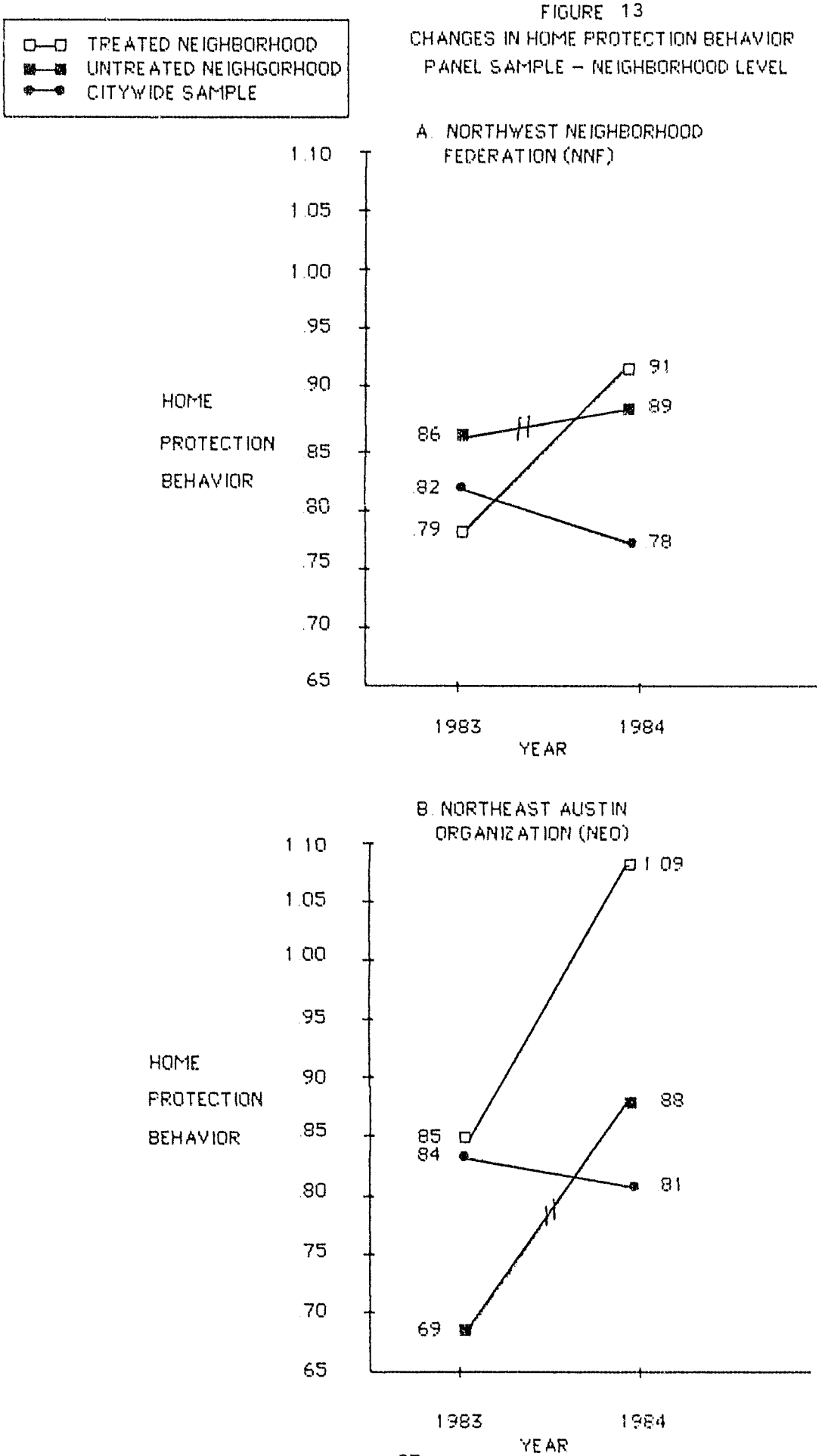
Dependent Variable = Home Protection Behavior

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.21	99.33***	.44	84.63***
	Covariates[2]	.24	1.47	---	---
	Treatment	.24	.10	.01	.10
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.21	150.18***	.44	129.16***
	Covariates	.23	1.69	---	---
	Treatment	.24	3.40	.08	3.40[1]
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.18	41.18***	.33	21.27***
	Covariates	.24	1.64	---	---
	Treatment	.24	.19	.03	.19
Northeast Austin Organization vs. Citywide Sample	Pretest	.21	115.90***	.42	89.89***
	Covariates	.24	1.83**	---	---
	Treatment	.26	6.51	.12	6.51**
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.24	53.86***	.45	42.26***
	Covariates	.32	2.11*	---	---
	Treatment	.32	.03	-.01	.03
Back of Yards Neighborhood Council vs. Citywide Sample	Pretest	.24	128.05***	.46	101.95***
	Covariates	.27	1.80	---	---
	Treatment	.27	.29	.03	.29
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.29	103.51**	.53	94.12***
	Covariates	.35	2.41	---	---
	Treatment	.35	1.03	.06	1.03
Edgewater Community Council vs. Citywide Sample	Pretest	.23	143.56***	.44	120.22***
	Covariates	.27	2.55**	---	---
	Treatment	.27	2.85	.07	2.85
Auburn-Gresham vs. Citywide Sample	Pretest	.26	170.47***	.46	130.44***
	Covariates	.29	2.69**	---	---
	Treatment	.30	4.69*	.11	4.69*

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

FIGURE 13
CHANGES IN HOME PROTECTION BEHAVIOR
PANEL SAMPLE – NEIGHBORHOOD LEVEL



were also occurring. However, in the independent samples, NNF was able to show a marginally significant increase in home protection behaviors relative to its comparison neighborhoods, as shown in Table 24. Nevertheless, all other comparisons were nonsignificant. Overall, only 2 of 8 panel comparisons and 1 of 8 independent sample comparisons revealed significant changes in home protection behavior.

Street Avoidance Behavior. Although not a central element of these programs, we hypothesized that having more information about crime or having crime become a more salient issue might increase personal protective behaviors, such as watching out for suspicious persons or avoiding certain types of strangers. In general, this was not true. As Table 25 reveals, none of the panel sample comparisons showed any program effects, and as Table 26 indicates, only two of the four neighborhoods (BYNC and ECC) revealed any changes in street avoidance behavior in the independent samples. Both of these changes are in the hypothesized direction, showing increases in the treated neighborhoods (See Figure 14). Chicagoans as a whole reported declining levels of avoidance behavior.

Victimization Reporting. Crime prevention programs typically encourage citizens to report crime to the police, especially their own experiences with victimization. We hypothesized that programs would produce an increase in the percentage of victimizations reported to the police. As Tables 27 and 28 reveal, there was little consistent support for this hypothesis. In the panel data, one neighborhood (NAO) showed an increase in reporting relative to Chicago, while another neighborhood (ECC) showed a marginally significant decrease. In the independent samples, one neighborhood (NNF) showed a marginally

TABLE 24
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Home Protection Behavior

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.05	---	----
	Time & Group Status	.05	---	----
	Treatment (TxGS)	.06	.11	3.16[1]
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.06	---	----
	Time & Group Status	.06	---	----
	Treatment (TxGS)	.06	.05	1.65
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.05	---	----
	Time & Group Status	.05	---	----
	Treatment (TxGS)	.05	-.09	1.80
Northeast Austin Organization vs. Citywide Sample	Covariates	.06	---	----
	Time & Group Status	.06	---	----
	Treatment (TxGS)	.06	-.02	.26
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.07	---	----
	Time & Group Status	.07	---	----
	Treatment (TxGS)	.07	.05	.57
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.07	---	----
	Time & Group Status	.07	---	----
	Treatment (TxGS)	.07	.06	2.50
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.06	---	----
	Time & Group Status	.06	---	----
	Treatment (TxGS)	.06	.00	.18
Edgewater Community Council vs. Citywide Sample	Covariates	.06	---	----
	Time & Group Status	.06	---	----
	Treatment (TxGS)	.06	.03	.58

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 25
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Street Avoidance Behavior

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.40	233.45***	.47	105.83***
	Covariates[2]	.48	6.74***	---	---
	Treatment	.48	2.10	.06	2.10
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.33	261.97***	.46	136.08***
	Covariates	.39	6.23***	---	---
	Treatment	.39	.23	-.02	.23
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.28	66.95***	.36	27.76***
	Covariates	.39	3.37***	---	---
	Treatment	.40	1.15	.07	1.15
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.25	134.70***	.41	76.18***
	Covariates	.31	3.71***	---	---
	Treatment	.31	.02	.01	.02
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.26	56.71***	.36	20.44***
	Covariates	.34	1.92*	---	---
	Treatment	.35	2.27	.10	2.27
Back of Yards Neighborhood Council vs. Citywide Sample	Pretest	.27	143.68***	.42	79.01***
	Covariates	.34	4.24***	---	---
	Treatment	.34	.35	.03	.35
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.34	121.32***	.46	55.94***
	Covariates	.39	2.11*	---	---
	Treatment	.39	1.21	.06	1.21
Edgewater Community Council vs. Citywide Sample	Pretest	.29	191.49***	.45	106.99***
	Covariates	.34	3.72***	---	---
	Treatment	.34	.12	-.01	.12
Auburn-Gresham vs. Citywide Sample	Pretest	.27	172.61***	.42	91.54***
	Covariates	.32	3.47***	---	---
	Treatment	.32	.07	-.01	.07

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimizatio (knowledge of victims).

TABLE 26
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Street Avoidance Behavior

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.27	---	---
	Time & Group Status	.27	---	---
	Treatment (TxGS)	.27	.08	1.93
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.27	---	---
	Time & Group Status	.27	---	---
	Treatment (TxGS)	.28	.05	2.34
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.22	---	---
	Time & Group Status	.23	---	---
	Treatment (TxGS)	.23	.00	.21
Northeast Austin Organization vs. Citywide Sample	Covariates	.25	---	---
	Time & Group Status	.25	---	---
	Treatment (TxGS)	.25	.04	1.09
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.27	---	---
	Time & Group Status	.28	---	---
	Treatment (TxGS)	.28	.06	1.41
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.26	---	---
	Time & Group Status	.27	---	---
	Treatment (TxGS)	.27	.08	5.95*
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.29	---	---
	Time & Group Status	.30	---	---
	Treatment (TxGS)	.30	.09	2.93[1]
Edgewater Community Council vs. Citywide Sample	Covariates	.28	---	---
	Time & Group Status	.29	---	---
	Treatment (TxGS)	.29	.07	5.28*

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

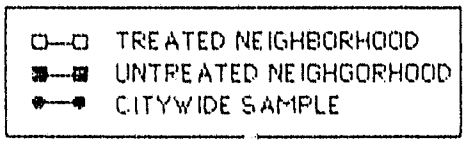
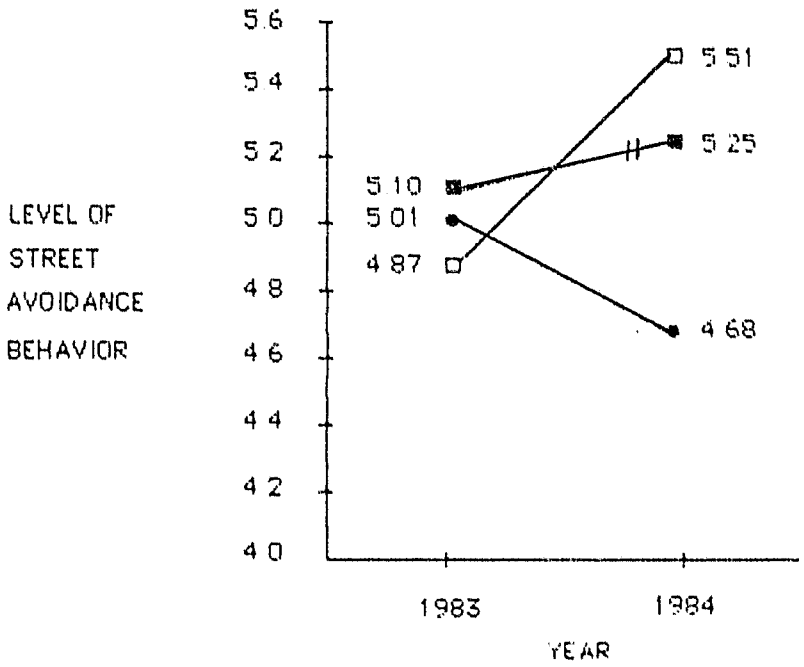


FIGURE 14
CHANGES IN STREET AVOIDANCE BEHAVIOR
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

C. BACK OF THE YARDS NEIGHBORHOOD COUNCIL (BYNC)



D. EDGEWATER COMMUNITY COUNCIL (ECC)

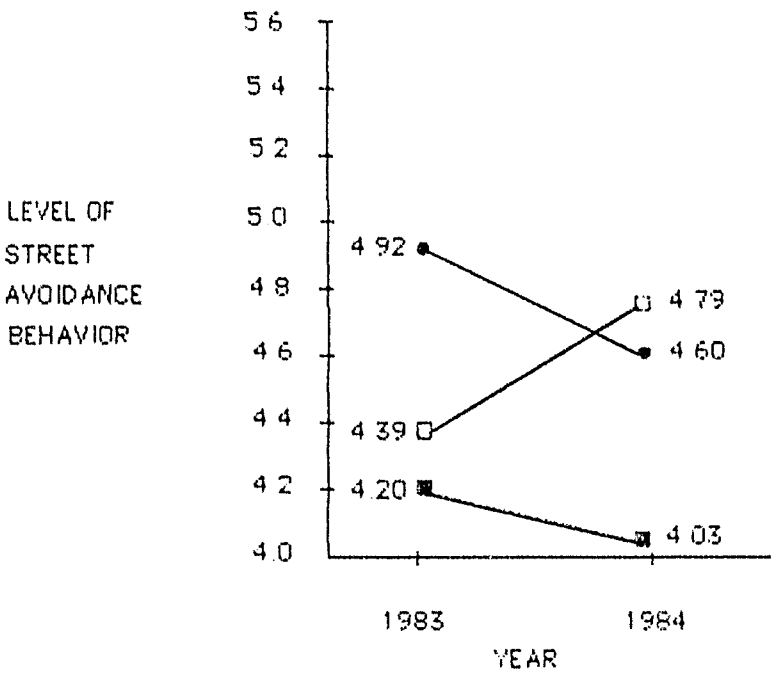


TABLE 27
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Percentage of Victimizations Reported to Police

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood	Pretest	.19	28.17***	.45	26.19***
Federation vs.	Covariates[2]	.22	.59	---	---
Comparison Neighborhoods	Treatment	.22	.33	-.05	.33
Northwest Neighborhood	Pretest	.11	24.92***	.30	18.47***
Federation vs.	Covariates	.16	1.09	---	---
Citywide Sample	Treatment	.17	2.20	-.12	2.20
Northeast Austin	Pretest	.10	9.62**	.23	4.97
Organization vs.	Covariates	.20	1.33	---	---
Comparison Neighborhoods	Treatment	.24	4.28*	.24	4.28*
Northeast Austin	Pretest	.09	16.51***	.25	9.87**
Organization vs.	Covariates	.13	.73	---	---
Citywide Sample	Treatment	.14	1.99	.12	1.99
Back of Yards Neighborhood	Pretest	.12	10.17**	.32	8.65**
Council vs.	Covariates	.27	1.69	---	---
Comparison Neighborhoods	Treatment	.28	.91	-.10	.91
Back of Yards Neighborhood	Pretest	.13	24.60***	.36	20.39***
Council vs.	Covariates	.17	.78	---	---
Citywide Sample	Treatment	.17	.17	-.04	.17
Edgewater Community	Pretest	.11	13.47**	.30	10.24**
Council vs.	Covariates	.16	.72	---	---
Comparison Neighborhoods	Treatment	.17	1.63	-.12	1.63
Edgewater Community	Pretest	.13	28.13***	.33	21.91***
Council vs.	Covariates	.16	.82	---	---
Citywide Sample	Treatment	.18	3.51[1]	-.14	3.51[1]
Auburn-Gresham	Pretest	.09	16.79***	.28	12.42***
vs.	Covariates	.15	1.31	---	---
Citywide Sample	Treatment	.15	.01	.01	.01

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 28
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Percentage of Victimizations Reported to Police

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.07	---	---
	Time & Group Status	.10	---	---
	Treatment (TxGS)	.12	-.32	2.81[1]
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.07	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.08	-.16	3.59[1]
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.14	---	---
	Time & Group Status	.14	---	---
	Treatment (TxGS)	.15	-.10	.50
Northeast Austin Organization vs. Citywide Sample	Covariates	.06	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.07	-.05	.26
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.06	---	---
	Time & Group Status	.06	---	---
	Treatment (TxGS)	.06	.03	.59
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.06	---	---
	Time & Group Status	.08	---	---
	Treatment (TxGS)	.08	-.01	.42
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.07	---	---
	Time & Group Status	.09	---	---
	Treatment (TxGS)	.09	.06	.16
Edgewater Community Council vs. Citywide Sample	Covariates	.06	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.07	-.06	.55

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

significant decrease relative to both control groups. Thus, only 4 of 16 comparisons were at all significant, and 3 of these ran counter to the hypothesis, showing decreases in the percentage of victimizations reported to the police.

Collective Surveillance. Central to the block watch concept is the notion of "neighboring", whereby block residents take on a territorial interest in their immediate environment and collectively protect each other and their property from criminal intruders. In this context, we hypothesized that residents in the treated areas would show increases in the tendency to ask neighbors to watch their home while they were away. The results in Tables 29 and 30 indicate almost no support for this hypothesis. Only NNF -- the neighborhood which fully adopted the block watch model -- was able to show a marginally significant increase in requests for neighbors to watch their homes. This difference is displayed in Figure 15. In sum, 15 of 16 comparisons showed no changes in this type of "neighboring" behavior.

Taking Action Against Neighborhood Problems. Assuming that these programs encourage the exercise of informal social control and enhance citizen efficacy, we hypothesized that local residents, when faced with identifiable neighborhood problems, would be more inclined to intervene and take some form of action to help solve these problems. Looking at the percentage of "big problems" in the neighborhood for which residents took some action (10-item scale), we found little support for this hypothesis in the panel data (See Table 31). In fact, the one significant finding is in the opposite direction, showing a decrease in the tendency to take action among NAO residents

TABLE 29
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Asking Neighbors To Watch Your Home

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood	Pretest	.21	88.97***	.45	77.31***
Federation vs.	Covariates[2]	.22	.37	---	---
Comparison Neighborhoods	Treatment	.22	.72	.04	.72
Northwest Neighborhood	Pretest	.19	115.31***	.42	104.13***
Federation vs.	Covariates	.20	1.09	---	---
Citywide Sample	Treatment	.21	2.84[1]	.08	2.84[1]
Northeast Austin	Pretest	.12	21.49***	.27	14.25***
Organization vs.	Covariates	.30	4.48***	---	---
Comparison Neighborhoods	Treatment	.30	.22	-.04	.22
Northeast Austin	Pretest	.16	69.22***	.38	60.91***
Organization vs.	Covariates	.17	.93	---	---
Citywide Sample	Treatment	.17	.00	.00	.00
Back of Yards Neighborhood	Pretest	.05	7.31**	.18	5.08*
Council vs.	Covariates	.18	2.45*	---	---
Comparison Neighborhoods	Treatment	.18	.03	-.01	.03
Back of Yards Neighborhood	Pretest	.15	61.85***	.36	52.49***
Council vs.	Covariates	.17	.88	---	---
Citywide Sample	Treatment	.17	.01	.01	.01
Edgewater Community	Pretest	.19	51.82***	.42	46.66***
Council vs.	Covariates	.24	1.60*	---	---
Comparison Neighborhoods	Treatment	.24	.28	-.03	.28
Edgewater Community	Pretest	.18	93.74***	.42	86.93***
Council vs.	Covariates	.19	.56	---	---
Citywide Sample	Treatment	.19	.04	-.01	.04
Auburn-Gresham	Pretest	.14	71.09***	.34	54.03***
vs.	Covariates	.15	.91	---	---
Citywide Sample	Treatment	.17	6.30**	.14	6.30**

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

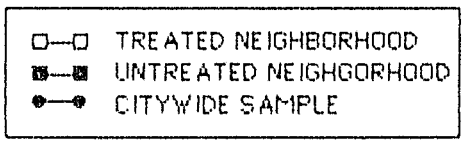


FIGURE 15

CHANGES IN ASKING NEIGHBORS TO WATCH HOME
PANEL SAMPLE-NEIGHBORHOOD LEVEL

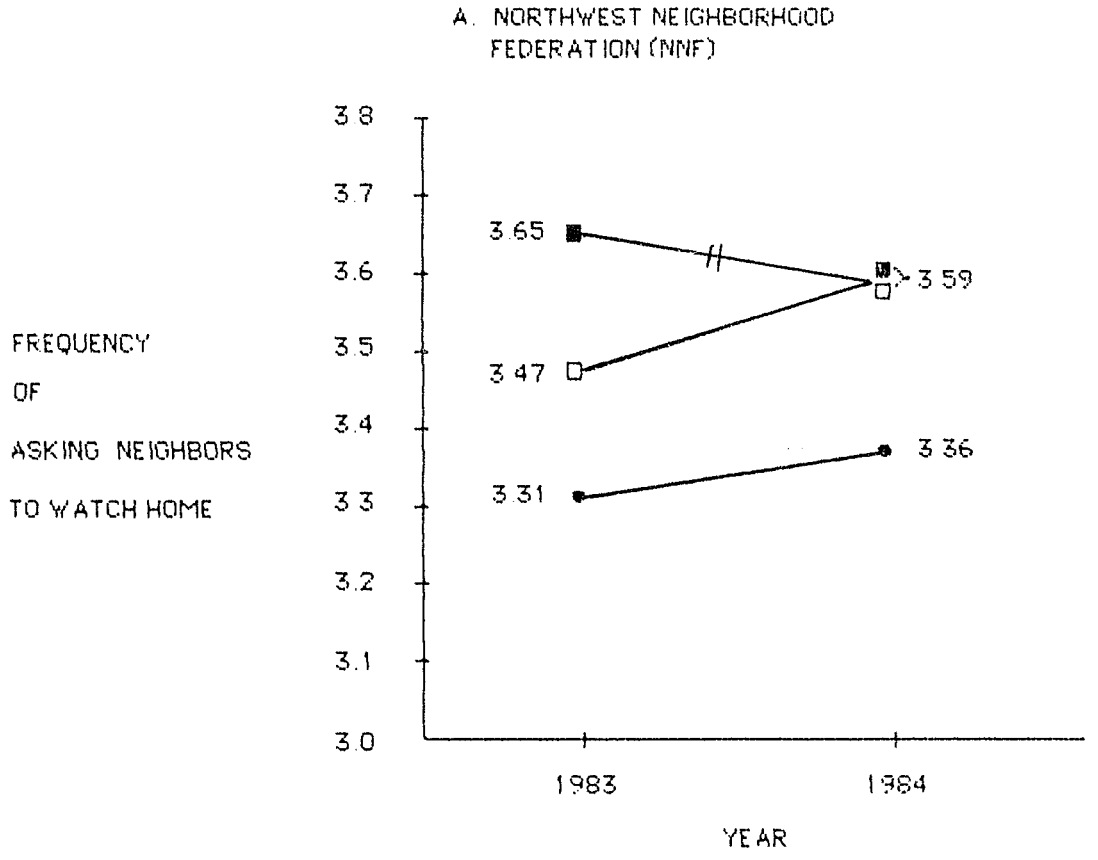


TABLE 30
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Asking Neighbors to Watch Your Home

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.06	---	---
	Time & Group Status	.06	---	---
	Treatment (TxGS)	.06	.09	2.18
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.04	---	---
	Time & Group Status	.04	---	---
	Treatment (TxGS)	.04	-.01	.97
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.07	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.07	-.05	.44
Northeast Austin Organization vs. Citywide Sample	Covariates	.04	---	---
	Time & Group Status	.04	---	---
	Treatment (TxGS)	.04	-.04	.88
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.08	---	---
	Time & Group Status	.08	---	---
	Treatment (TxGS)	.08	.01	.17
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.04	---	---
	Time & Group Status	.05	---	---
	Treatment (TxGS)	.05	.01	.10
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.07	---	---
	Time & Group Status	.08	---	---
	Treatment (TxGS)	.08	.09	2.20
Edgewater Community Council vs. Citywide Sample	Covariates	.05	---	---
	Time & Group Status	.05	---	---
	Treatment (TxGS)	.05	.04	1.30

[1] $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 31
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Tendency to Take Action Against Neighborhood Problems

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood	Pretest	.29	16.68***	.54	14.71***
Federation vs.	Covariates[2]	.42	1.11	---	---
Comparison Neighborhoods	Treatment	.43	.26	-.07	.27
Northwest Neighborhood	Pretest	.22	27.48***	.46	20.07***
Federation vs.	Covariates	.28	.77	---	---
Citywide Sample	Treatment	.28	.34	.07	.34
Northeast Austin	Pretest	.07	2.18	.09	.20
Organization vs.	Covariates	.48	1.99	---	---
Comparison Neighborhoods	Treatment	.59	4.79*	-.55	4.80*
Northeast Austin	Pretest	.15	13.16***	.43	13.19***
Organization vs.	Covariates	.20	.53	---	---
Citywide Sample	Treatment	.20	.01	-.01	.01
Back of Yards Neighborhood	Pretest	.09	4.34*	.32	4.45*
Council vs.	Covariates	.37	2.11[1]	---	---
Comparison Neighborhoods	Treatment	.38	.63	.11	.63
Back of Yards Neighborhood	Pretest	.14	13.14***	.40	12.55***
Council vs.	Covariates	.17	.34	---	---
Citywide Sample	Treatment	.18	.43	.09	.43
Edgewater Community	Pretest	.07	2.43	.27	1.14
Council vs.	Covariates	.30	.77	---	---
Comparison Neighborhoods	Treatment	.31	.08	-.07	.08
Edgewater Community	Pretest	.20	20.79***	.46	19.04***
Council vs.	Covariates	.27	.81	---	---
Citywide Sample	Treatment	.27	.37	.07	.37
Auburn-Gresham	Pretest	.19	17.85***	.45	15.33***
vs.	Covariates	.26	.71	---	---
Citywide Sample	Treatment	.26	.19	.06	.19

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

relative to the comparison neighborhoods. In contrast, the independent samples showed some support for this prediction. (See Table 32). As Figure 16 shows, NAO, BYNC, and ECC all demonstrated increases in citizens' tendency to get involved, relative to the citywide sample, but did not differ from their comparison neighborhood.

In sum, the total picture with regard to behavioral changes is not supportive of Hypothesis Three. The vast majority of comparisons were nonsignificant. Furthermore, some of the significant findings showed no consistent pattern across neighborhoods. For example, the tendency to report victimizations and to intervene in neighborhood problems each showed significant increases and decreases, depending on the neighborhood of interest.

Hypothesis Four: Social Integration

A central tenant of community crime prevention theorists is that collective activity has the capacity to enhance social integration among community residents, thus making the neighborhood a better social environment in which to live. Two measures of social integration were used to test hypothesis four -- the self-reported frequency of spontaneous verbal interaction with neighbors on the street, and the proportion of block residents that they know by name.

Chatting with Neighbors. As shown in Tables 33 and 34, there was no support for the hypothesis that these programs would increase the frequency of informal "chatting" on the street among neighbors.

Residents Known by Name. According to the panel data, residents in three out of four neighborhoods showed no evidence of an increase in the proportion of block residents they know by name (See Table 35).

TABLE 32
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Tendency to Take Action Against Neighborhood Problems

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.04	---	---
	Time & Group Status	.06	---	---
	Treatment (TxGS)	.07	-.21	1.93
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.10	---	---
	Time & Group Status	.10	---	---
	Treatment (TxGS)	.11	-.08	1.84
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.07	---	---
	Time & Group Status	.12	---	---
	Treatment (TxGS)	.12	.09	.52
Northeast Austin Organization vs. Citywide Sample	Covariates	.09	---	---
	Time & Group Status	.10	---	---
	Treatment (TxGS)	.12	.15	5.05*
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.07	---	---
	Time & Group Status	.08	---	---
	Treatment (TxGS)	.09	.11	1.29
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.09	---	---
	Time & Group Status	.09	---	---
	Treatment (TxGS)	.10	.15	5.32*
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.13	---	---
	Time & Group Status	.13	---	---
	Treatment (TxGS)	.16	.26	4.38*
Edgewater Community Council vs. Citywide Sample	Covariates	.12	---	---
	Time & Group Status	.12	---	---
	Treatment (TxGS)	.12	.12	3.62[1]

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

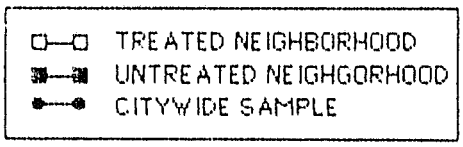
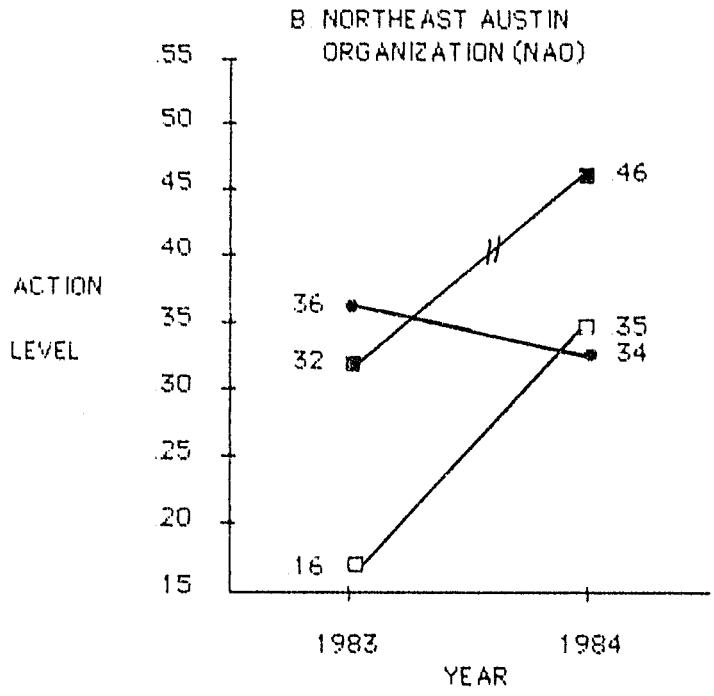
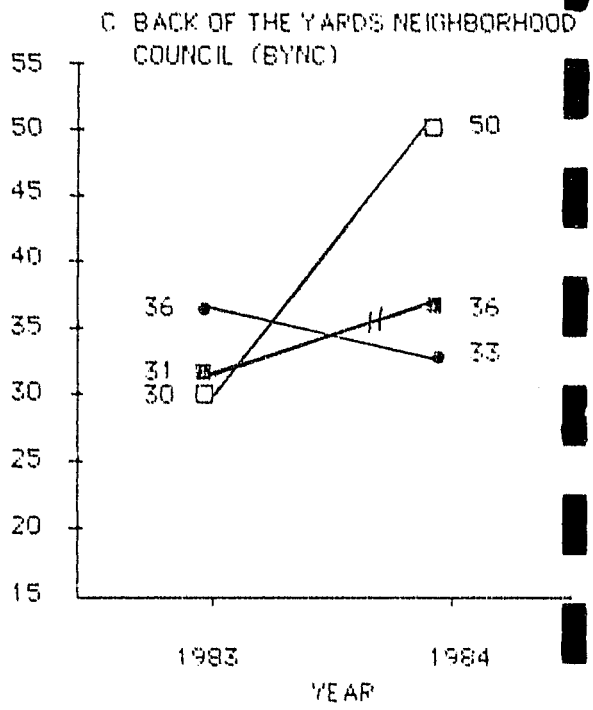


FIGURE 16
CHANGES IN TENDENCY TO TAKE ACTION
AGAINST NEIGHBORHOOD PROBLEMS
INDEPENDENT SAMPLE - NEIGHBORHOOD LEVEL





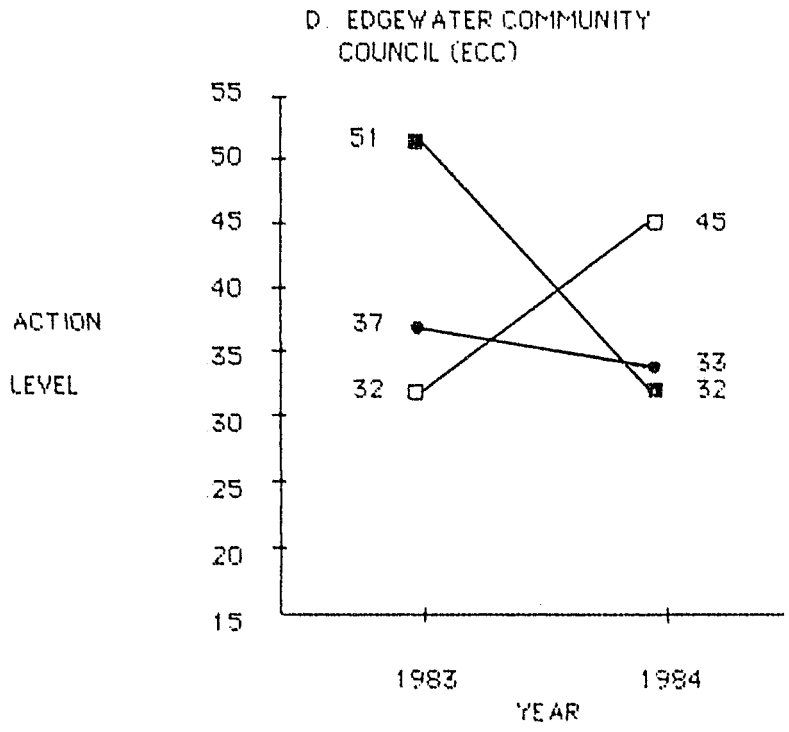


TABLE 33
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Frequency of "Chatting" with Neighbors

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.23	108.35***	.44	90.06***
	Covariates[2]	.27	2.69**	---	---
	Treatment	.27	.17	.02	.17
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.21	147.96***	.41	110.09***
	Covariates	.24	2.72**	---	---
	Treatment	.24	.79	.04	.79
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.22	53.58***	.40	36.52***
	Covariates	.28	1.65	---	---
	Treatment	.29	1.74	-.10	1.74
Northeast Austin Organization vs. Citywide Sample	Pretest	.20	104.75***	.41	80.74***
	Covariates	.22	1.48	---	---
	Treatment	.23	2.14	-.07	2.14
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.23	50.22***	.39	30.33***
	Covariates	.31	2.10*	---	---
	Treatment	.31	.48	.05	.48
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.24	126.00***	.44	91.16***
	Covariates	.26	1.50	---	---
	Treatment	.26	.41	.03	.41
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.23	74.87***	.45	56.69***
	Covariates	.26	.97	---	---
	Treatment	.26	.69	.05	.69
Edgewater Community Council vs. Citywide Sample	Pretest	.21	130.48***	.42	---
	Covariates	.24	2.10*	---	---
	Treatment	.24	.47	.03	.47
Auburn-Gresham vs. Citywide Sample	Pretest	.21	132.06***	.43	106.72***
	Covariates	.23	1.36	---	---
	Treatment	.24	1.54	.06	1.54

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 34
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Frequency of "Chatting" With Neighbors

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.08	---	---
	Time & Group Status	.08	---	---
	Treatment (TxGS)	.09	.06	.94
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.08	---	---
	Time & Group Status	.09	---	---
	Treatment (TxGS)	.09	.03	.59
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.09	---	---
	Time & Group Status	.10	---	---
	Treatment (TxGS)	.10	.00	.48
Northeast Austin Organization vs. Citywide Sample	Covariates	.08	---	---
	Time & Group Status	.08	---	---
	Treatment (TxGS)	.08	.01	.54
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.09	---	---
	Time & Group Status	.09	---	---
	Treatment (TxGS)	.09	-.02	.10
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.09	---	---
	Time & Group Status	.09	---	---
	Treatment (TxGS)	.09	-.01	.46
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.10	---	---
	Time & Group Status	.11	---	---
	Treatment (TxGS)	.11	-.03	.19
Edgewater Community Council vs. Citywide Sample	Covariates	.09	---	---
	Time & Group Status	.10	---	---
	Treatment (TxGS)	.10	-.02	.30

[1] $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 35
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Number of Block Residents You Know by Name

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.37	214.63***	.55	153.90***
	Covariates[2]	.40	2.78**	---	---
	Treatment	.40	.00	.00	.00
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.37	325.62***	.59	288.22***
	Covariates	.38	1.85[1]	---	---
	Treatment	.38	.14	.01	.14
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.34	95.44***	.56	88.84***
	Covariates	.37	1.06	---	---
	Treatment	.40	10.06**	-.21	10.06**
Northeast Austin Organization vs. Citywide Sample	Pretest	.40	281.38***	.61	253.06***
	Covariates	.42	1.63	---	---
	Treatment	.44	14.37***	-.16	14.37***
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.35	89.63***	.63	86.99***
	Covariates	.39	1.33	---	---
	Treatment	.40	.72	-.05	.72
Back of Yards Neighborhood Council vs. Citywide Sample	Pretest	.41	279.79***	.63	250.94***
	Covariates	.43	1.62	---	---
	Treatment	.43	1.22	-.05	1.22
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.43	187.20***	.61	143.27***
	Covariates	.47	2.36**	---	---
	Treatment	.47	.09	-.02	.09
Edgewater Community Council vs. Citywide Sample	Pretest	.42	353.59***	.63	303.28***
	Covariates	.44	2.02*	---	---
	Treatment	.44	.00	.00	.00
Auburn-Gresham vs. Citywide Sample	Pretest	.47	438.51***	.65	330.59***
	Covariates	.50	2.46**	---	---
	Treatment	.50	2.46	.07	2.46

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

The one neighborhood that did change provides evidence against the hypothesis. Specifically, NAO showed a significant reduction in the proportion of residents known by name relative to both control groups, as displayed in Figure 17. The independent samples revealed no changes in residents' familiarity with people on their block (See Table 36).

Hypothesis Five: Reduced Crime and Incivility

Assuming that all the mechanisms and processes posited in earlier hypotheses are in place, two major outcomes that should be expected are (a) a reduction in criminal victimization and (b) a reduction in various types of incivility or disorder. To test this hypothesis, we examined the impact of the programs on five different scales -- two measuring victimization and three measuring different forms of disorder.

Victimization Experience. The panel results revealed an interesting mixture of changes in victimization experience. Three of the four neighborhoods showed significant changes in the average number of victimizations per respondent, but two of these three run counter to the hypothesis. As suggested by the betas in Table 37 and confirmed in Figure 18, NAO and BYNC showed significant increases in the number of victimizations per respondent between 1983 and 1984. In contrast, NNF showed a marginally significant decrease in victimization experiences. As Figure 18 shows, victimization levels in Chicago remained very stable between 1983 and 1984. The independent sample results show no changes in victimization across all comparisons (See Table 38).

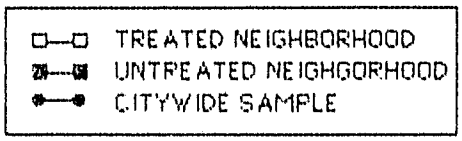


FIGURE 17

CHANGES IN NUMBER OF BLOCK RESIDENTS
THAT RESPONDENT KNOWS BY NAME
PANEL SAMPLE - NEIGHBORHOOD LEVEL

B. NORTHEAST AUSTIN
ORGANIZATION (NAO)

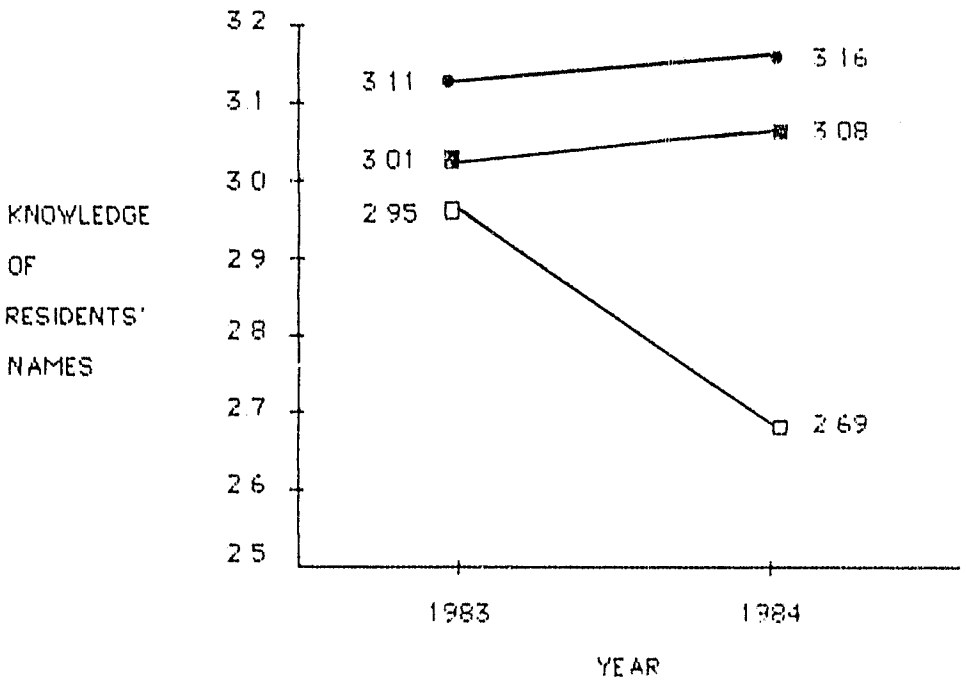


TABLE 36
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Number of Block Residents You Know by Name

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.15	---	---
	Time & Group Status	.17	---	---
	Treatment (TxGS)	.17	.01	.43
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.10	---	---
	Time & Group Status	.11	---	---
	Treatment (TxGS)	.11	.02	.22
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.06	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.07	.02	.10
Northeast Austin Organization vs. Citywide Sample	Covariates	.08	---	---
	Time & Group Status	.09	---	---
	Treatment (TxGS)	.09	.00	.10
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.06	---	---
	Time & Group Status	.06	---	---
	Treatment (TxGS)	.06	.01	.57
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.10	---	---
	Time & Group Status	.11	---	---
	Treatment (TxGS)	.11	.03	.52
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.16	---	---
	Time & Group Status	.16	---	---
	Treatment (TxGS)	.16	.03	.22
Edgewater Community Council vs. Citywide Sample	Covariates	.12	---	---
	Time & Group Status	.13	---	---
	Treatment (TxGS)	.13	.03	.77

[1] $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 37
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Victimization Experience

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood	Pretest	.17	78.15***	.43	79.20***
Federation vs.	Covariates[2]	.20	1.49	---	---
Comparison Neighborhoods	Treatment	.20	.30	-.03	.30
Northwest Neighborhood	Pretest	.26	192.72***	.50	181.62***
Federation vs.	Covariates	.27	.90	---	---
Citywide Sample	Treatment	.27	3.17	-.08	3.17[1]
Northeast Austin	Pretest	.22	56.66***	.47	50.25***
Organization vs.	Covariates	.25	.94	---	---
Comparison Neighborhoods	Treatment	.28	8.83**	.21	8.83**
Northeast Austin	Pretest	.30	180.27***	.53	167.78***
Organization vs.	Covariates	.31	.61	---	---
Citywide Sample	Treatment	.31	4.97*	.10	4.97*
Back of Yards Neighborhood	Pretest	.20	42.09***	.42	31.72***
Council vs.	Covariates	.23	.85	---	---
Comparison Neighborhoods	Treatment	.24	2.45	.11	2.45
Back of Yards Neighborhood	Pretest	.31	179.39***	.53	156.39***
Council vs.	Covariates	.31	.39	---	---
Citywide Sample	Treatment	.32	4.28*	.10	4.28*
Edgewater Community	Pretest	.19	59.08***	.43	57.84***
Council vs.	Covariates	.24	2.18	---	---
Comparison Neighborhoods	Treatment	.25	.74	-.05	.74
Edgewater Community	Pretest	.29	195.56***	.53	182.20***
Council vs.	Covariates	.30	1.13	---	---
Citywide Sample	Treatment	.30	.20	-.02	.20
Auburn-Gresham	Pretest	.30	209.97***	.52	184.88***
vs.	Covariates	.32	1.57	---	---
Citywide Sample	Treatment	.32	.75	-.04	.75

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

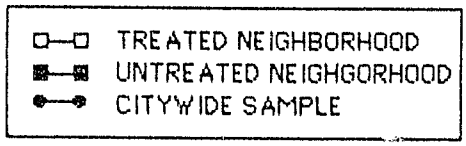


FIGURE 18
CHANGES IN VICTIMIZATION EXPERIENCE
PANEL SAMPLE - NEIGHBORHOOD LEVEL

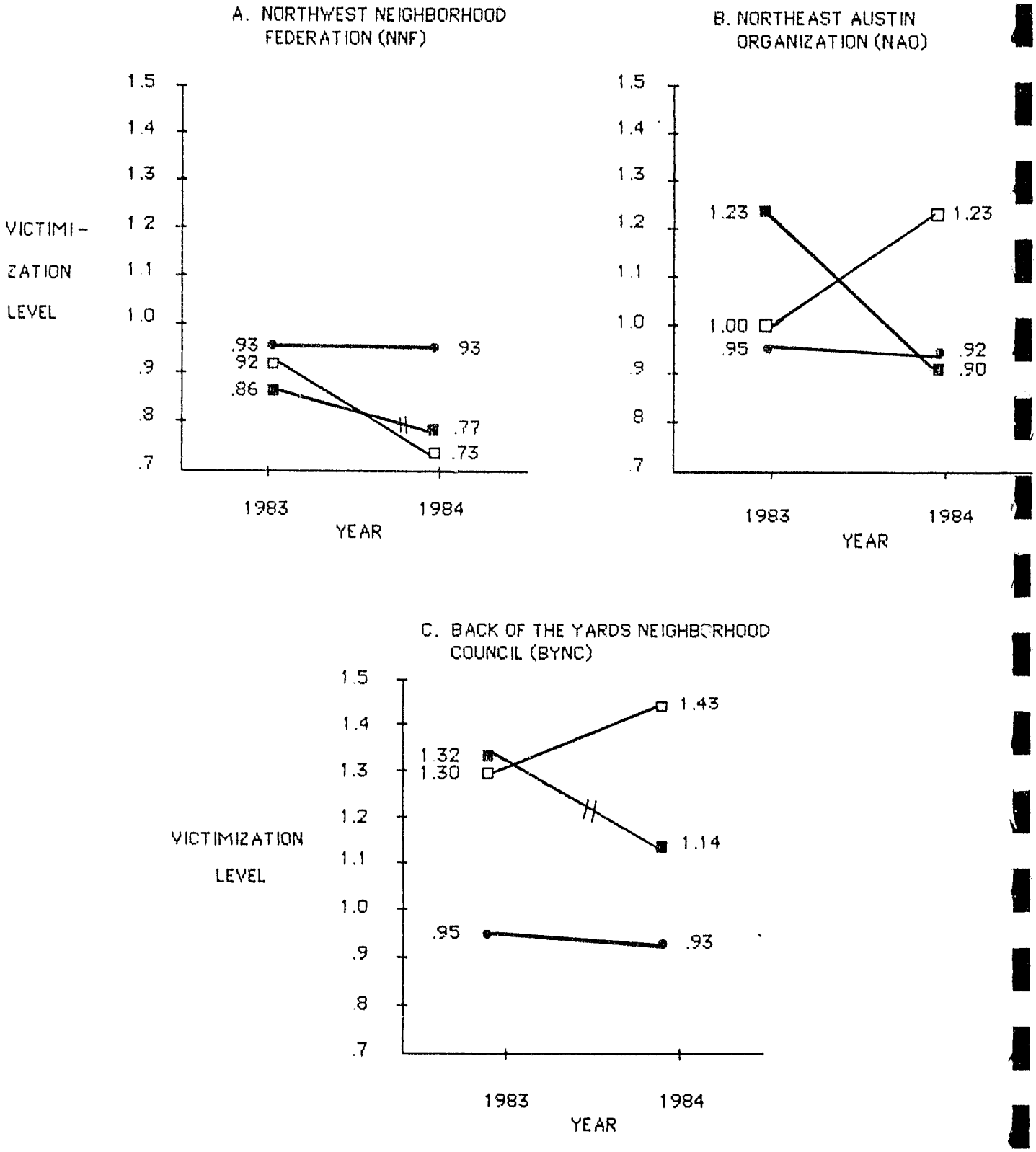


Table 38
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Victimization Experience

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.06	---	---
	Time & Group Status	.06	---	---
	Treatment (TxGS)	.06	-.07	1.31
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.06	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.07	-.05	1.51
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.09	---	---
	Time & Group Status	.09	---	---
	Treatment (TxGS)	.09	-.03	.19
Northeast Austin Organization vs. Citywide Sample	Covariates	.07	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.07	-.05	1.80
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.18	---	---
	Time & Group Status	.18	---	---
	Treatment (TxGS)	.18	-.04	.48
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.07	---	---
	Time & Group Status	.08	---	---
	Treatment (TxGS)	.08	-.05	1.72
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.08	---	---
	Time & Group Status	.08	---	---
	Treatment (TxGS)	.08	.02	.12
Edgewater Community Council vs. Citywide Sample	Covariates	.07	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.07	-.03	.59

[1] $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

Vicarious Victimization. Indirect or vicarious victimization was measured by asking respondents if they "personally know anyone" (other than themselves) who has been a victim of serious crime in the past year (respondents were asked about a cluster of personal and property crimes in two items). Paralleling the victimization results, the panel data revealed increases in the number of vicarious victimizations in NAO. (See Table 39 and Figure 19). While the betas in Table 39 suggest unfavorable differential change in NNF and BYNC as well, an inspection of the means in Figure 18 revealed that vicarious victimization was actually decreasing in these areas, but just not as rapidly as the comparison neighborhoods. The independent samples produced a similar pattern for NNF, but other comparisons were nonsignificant (See Table 40). In sum, there is no support for the hypothesis that programs will yield reductions in vicarious victimization, and some evidence to the contrary.

Youth Disorder. The amount of youth disorder in these neighborhoods (e.g."hanging out," grafitti, drugs, gangs) was expected to decline as another indicator of program success. There were only four significant changes across both samples, and three of these disconfirm the hypothesis (See Tables 41 and 42). In the panel sample, youth disorder increased in NAO relative to comparison neighborhoods. In the independent samples, BYNC showed significant increases in youth disorder relative to both control groups. Only ECC showed a marginally significant decline in youth disorder as predicted.

Youth Rejection of Social Control. Another measure of disorder which more directly examines the success of social control efforts is the extent to which neighborhood youths are viewed as respectful of

TABLE 39
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Vicarious Victimization

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood	Pretest	.16	68.55***	.27	28.11***
Federation vs.	Covariates[2]	.24	6.03***	---	---
Comparison Neighborhoods	Treatment	.25	2.87[1]	.08	2.87[1]
Northwest Neighborhood	Pretest	.14	89.51***	.27	44.48***
Federation vs.	Covariates	.20	5.63***	---	---
Citywide Sample	Treatment	.20	.30	-.02	.30
Northeast Austin	Pretest	.18	41.97***	.35	23.81***
Organization vs.	Covariates	.25	2.08*	---	---
Comparison Neighborhoods	Treatment	.30	12.51***	.25	12.51***
Northeast Austin	Pretest	.16	81.01***	.31	40.88***
Organization vs.	Covariates	.21	3.14***	---	---
Citywide Sample	Treatment	.22	6.47**	.12	6.47**
Back of Yards Neighborhood	Pretest	.23	51.22***	.39	24.28***
Council vs.	Covariates	.30	2.00*	---	---
Comparison Neighborhoods	Treatment	.31	2.78[1]	.11	2.78[1]
Back of Yards Neighborhood	Pretest	.17	82.83***	.31	41.66***
Council vs.	Covariates	.23	3.87	---	---
Citywide Sample	Treatment	.23	.05	-.01	.05
Edgewater Community	Pretest	.11	32.73***	.28	19.45***
Council vs.	Covariates	.15	1.23	---	---
Comparison Neighborhoods	Treatment	.15	1.38	.07	1.38
Edgewater Community	Pretest	.15	87.84***	.29	40.13***
Council vs.	Covariates	.21	4.17***	---	---
Citywide Sample	Treatment	.21	.47	.03	.47
Auburn-Gresham	Pretest	.12	66.70***	.26	32.38***
vs.	Covariates	.18	4.42***	---	---
Citywide Sample	Treatment	.18	.66	.04	.66

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience.

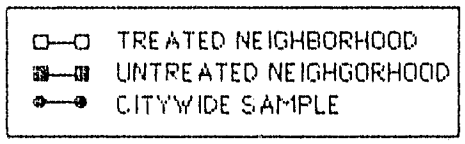
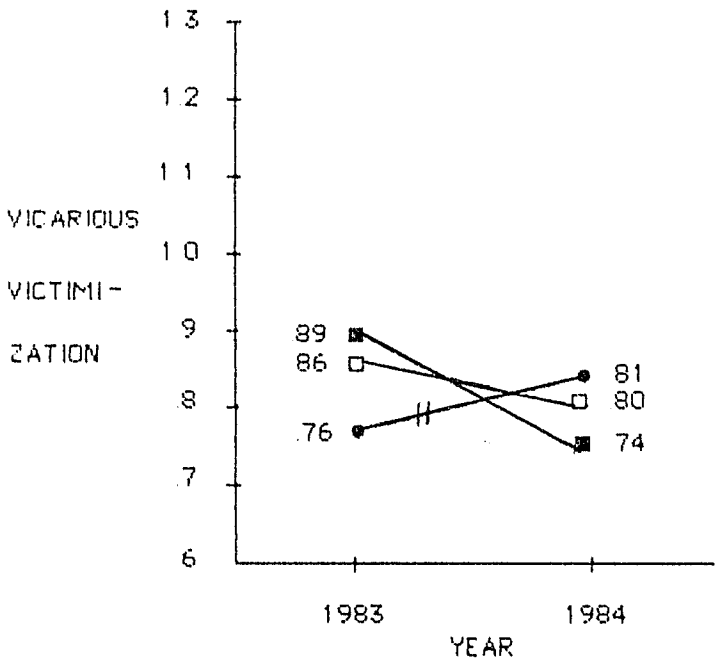
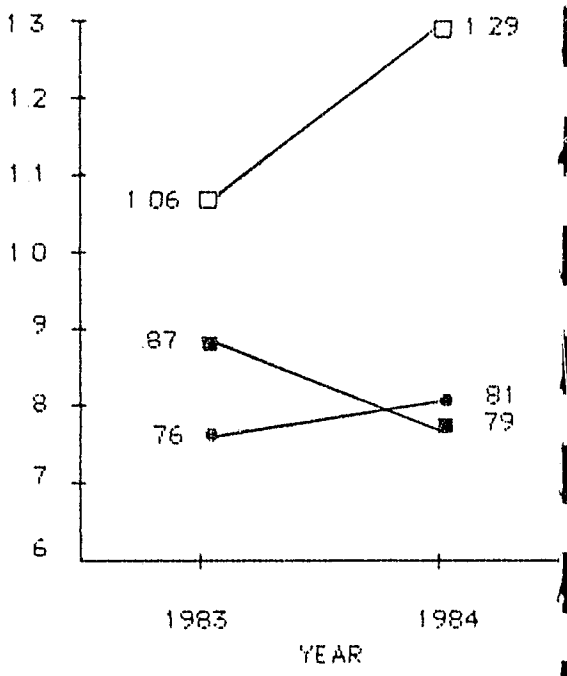


FIGURE 19
CHANGES IN VICARIOUS VICTIMIZATION
PANEL SAMPLE - NEIGHBORHOOD LEVEL

A NORTHWEST NEIGHBORHOOD FEDERATION (NNF)



B NORTHEAST AUSTIN ORGANIZATION (NAO)



C BACK OF THE YARDS NEIGHBORHOOD COUNCIL (BYNC)

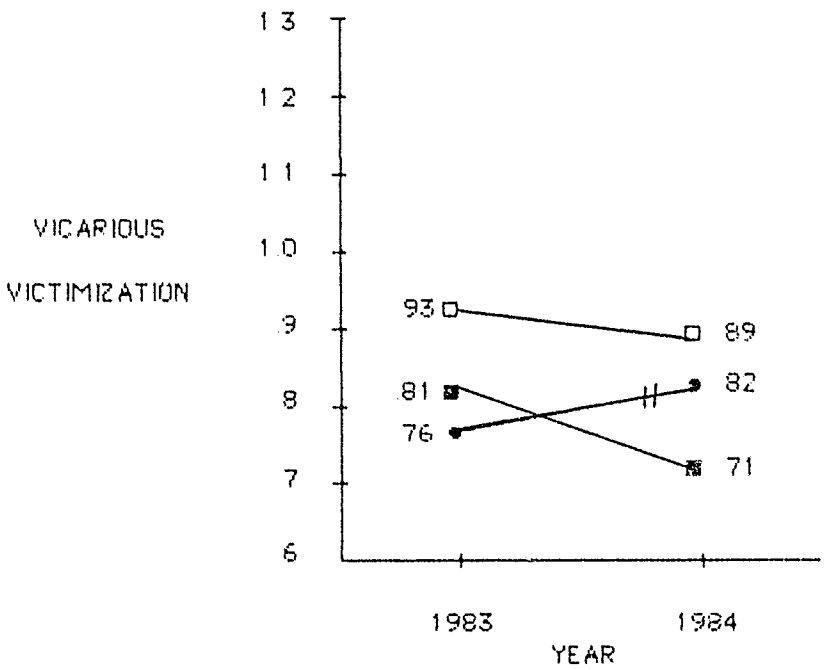


Table 40
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Vicarious Victimization

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.11	---	---
	Time & Group Status	.11	---	---
	Treatment (TxGS)	.11	.03	.32
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.09	---	---
	Time & Group Status	.09	---	---
	Treatment (TxGS)	.10	.07	3.72[1]
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.12	---	---
	Time & Group Status	.13	---	---
	Treatment (TxGS)	.13	.03	.23
Northeast Austin Organization vs. Citywide Sample	Covariates	.10	---	---
	Time & Group Status	.11	---	---
	Treatment (TxGS)	.11	.02	.22
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.16	---	---
	Time & Group Status	.16	---	---
	Treatment (TxGS)	.16	.05	.67
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.09	---	---
	Time & Group Status	.10	---	---
	Treatment (TxGS)	.10	.05	1.64
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.08	---	---
	Time & Group Status	.11	---	---
	Treatment (TxGS)	.11	.04	.58
Edgewater Community Council vs. Citywide Sample	Covariates	.10	---	---
	Time & Group Status	.11	---	---
	Treatment (TxGS)	.11	-.05	1.90

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience.

TABLE 41
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Youth Disorder

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.27	136.36***	.46	85.48***
	Covariates[2]	.30	2.21*	---	---
	Treatment	.30	.04	-.01	.04
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.39	361.54***	.56	226.12***
	Covariates	.42	3.23***	---	---
	Treatment	.42	.05	-.01	.05
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.35	101.55***	.50	62.72***
	Covariates	.44	3.06**	---	---
	Treatment	.47	12.15***	.22	12.15***
Northeast Austin Organizaion vs. Citywide Sample	Pretest	.41	300.67***	.57	185.18***
	Covariates	.44	1.84[1]	---	---
	Treatment	.44	1.24***	.05	1.24
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.40	112.59***	.55	78.74***
	Covariates	.53	4.97***	---	---
	Treatment	.53	1.31	.06	1.31
Back of Yards Neighborhood Council vs. Citywide Sample	Pretest	.40	265.23***	.55	162.25***
	Covariates	.42	2.10*	---	---
	Treatment	.42	.14	.02	.14
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.37	149.08***	.58	110.08***
	Covariates	.41	1.56	---	---
	Treatment	.41	.08	-.01	.08
Edgewater Community Council vs. Citywide Sample	Pretest	.40	325.87***	.57	215.61***
	Covariates	.44	3.07***	---	---
	Treatment	.44	3.28[1]	-.07	3.28[1]
Auburn-Gresham vs. Citywide Sample	Pretest	.41	341.42***	.57	219.23***
	Covariates	.44	2.52**	---	---
	Treatment	.44	1.99	-.06	1.99

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

Table 42
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Youth Disorder

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.18	---	---
	Time & Group Status	.18	---	---
	Treatment (TxGS)	.18	.02	.99
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.22	---	---
	Time & Group Status	.22	---	---
	Treatment (TxGS)	.22	.01	.34
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.21	---	---
	Time & Group Status	.21	---	---
	Treatment (TxGS)	.21	.05	.80
Northeast Austin Organization vs. Citywide Sample	Covariates	.20	---	---
	Time & Group Status	.20	---	---
	Treatment (TxGS)	.20	.01	.40
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.22	---	---
	Time & Group Status	.23	---	---
	Treatment (TxGS)	.24	.16	8.45**
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.19	---	---
	Time & Group Status	.21	---	---
	Treatment (TxGS)	.21	.09	6.85**
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.12	---	---
	Time & Group Status	.13	---	---
	Treatment (TxGS)	.13	-.06	1.17
Edgewater Community Council vs. Citywide Sample	Covariates	.18	---	---
	Time & Group Status	.18	---	---
	Treatment (TxGS)	.18	-.01	.14

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

property and people, law abiding, and responsive to parental requests. Consistent with the hypothesis, two neighborhoods (NNF and BYNC) showed significant reductions in the perceived amount of youth rejection of social control or youth deviance. (See Table 43). However, the independent samples produced no significant findings (See Table 44).

Neighborhood Deterioration. In addition to addressing social disorder, community crime prevention programs oftentimes seek to improve the physical environment because of the close connection between neighborhood deterioration and crime. Using a 3-item neighborhood deterioration scale (covering abandoned buildings or vehicles, garbage or litter, and disinterested landlords), we sought to measure residents' perceptions of the physical environment in a way that might detect disinvestment in the community. The panel results (Table 45) indicate that only one neighborhood -- NNF -- was able to show any perceived improvement in the physical environment and this change was in relationship to the citywide sample. The independent samples yielded no significant findings (See Table 46).

In sum, we found no consistent support for Hypothesis Five regarding reductions in crime, social disorder, and "physical" disorder. In fact, the significant changes were generally in the direction of increases rather than decreases in these problems areas. Specifically, there were more increases than decreases in both direct and vicarious victimization levels, as well as youth disorder. Scales measuring youth rejection of social control and neighborhood deterioration were generally unchanged, but NNF was able to show some consistent support for the hypothesis across each of these scales.

TABLE 43
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Youth Rejection of Social Control

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.26	121.10***	.43	77.72***
	Covariates[2]	.31	3.12**	---	---
	Treatment	.31	3.72*	.09	3.72*
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.29	216.25***	.46	135.52***
	Covariates	.33	3.27***	---	---
	Treatment	.33	.48	-.03	.48
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.32	80.96***	.46	42.40***
	Covariates	.40	2.44**	---	---
	Treatment	.40	.00	.00	.00
Northeast Austin Organization vs. Citywide Sample	Pretest	.30	168.97***	.46	101.74***
	Covariates	.35	3.69***	---	---
	Treatment	.35	.01	.00	.01
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.36	90.04***	.44	43.67***
	Covariates	.51	4.82***	---	---
	Treatment	.52	4.07*	.12	4.07*
Back of Yards Neighborhood Council vs. Citywide Sample	Pretest	.29	156.81***	.46	95.33***
	Covariates	.33	2.36**	---	---
	Treatment	.33	1.24	.05	1.24
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.28	92.48***	.43	49.93***
	Covariates	.33	1.82	---	---
	Treatment	.33	.28	.03	.28
Edgewater Community Council vs. Citywide Sample	Pretest	.27	167.58***	.44	99.45***
	Covariates	.32	3.16	---	---
	Treatment	.32	.43	-.03	.43
Auburn-Gresham vs. Citywide Sample	Pretest	.26	159.65***	.43	95.61***
	Covariates	.31	3.61***	---	---
	Treatment	.31	.99	-.05	.99

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

Table 44
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Youth Rejection of Social Control

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.15	---	---
	Time & Group Status	.15	---	---
	Treatment (TxGS)	.15	.03	.25
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.18	---	---
	Time & Group Status	.19	---	---
	Treatment (TxGS)	.19	.01	.21
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.21	---	---
	Time & Group Status	.22	---	---
	Treatment (TxGS)	.22	.01	.46
Northeast Austin Organization vs. Citywide Sample	Covariates	.19	---	---
	Time & Group Status	.19	---	---
	Treatment (TxGS)	.19	.04	1.03
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.26	---	---
	Time & Group Status	.26	---	---
	Treatment (TxGS)	.26	.05	.99
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.19	---	---
	Time & Group Status	.20	---	---
	Treatment (TxGS)	.20	.03	.71
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.12	---	---
	Time & Group Status	.12	---	---
	Treatment (TxGS)	.12	.01	.15
Edgewater Community Council vs. Citywide Sample	Covariates	.16	---	---
	Time & Group Status	.16	---	---
	Treatment (TxGS)	.16	.01	.48

[1] $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 45
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Neighborhood Deterioration

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.23	110.59***	.43	82.33***
	Covariates[2]	.26	2.05*	---	---
	Treatment	.27	.85	-.04	.85
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.28	214.37***	.46	146.63***
	Covariates	.31	2.75**	---	---
	Treatment	.32	7.01**	-.11	7.01**
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.16	35.74***	.39	31.18***
	Covariates	.21	1.16	---	---
	Treatment	.21	1.36	.09	1.36
Northeast Austin Organization vs. Citywide Sample	Pretest	.24	136.00***	.43	91.61***
	Covariates	.28	2.47**	---	---
	Treatment	.28	.09	.01	.09
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.23	50.60***	.40	32.19***
	Covariates	.33	2.62**	---	---
	Treatment	.33	.01	.01	.01
Back of Yards Neighborhood Council vs. Citywide Sample	Pretest	.27	146.90***	.48	107.18***
	Covariates	.29	1.29	---	---
	Treatment	.29	.40	-.03	.40
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.30	105.82***	.50	79.69***
	Covariates	.33	1.38	---	---
	Treatment	.33	.00	.00	.00
Edgewater Community Council vs. Citywide Sample	Pretest	.30	203.29***	.50	154.47***
	Covariates	.32	1.62	---	---
	Treatment	.32	.53	-.03	.53
Auburn-Gresham vs. Citywide Sample	Pretest	.25	162.62***	.45	122.31***
	Covariates	.29	2.94**	---	---
	Treatment	.29	3.08[1]	-.09	3.08[1]

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

Table 46
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Neighborhood Deterioration

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.06	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.07	-.00	.43
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.14	---	---
	Time & Group Status	.15	---	---
	Treatment (TxGS)	.15	-.00	.54
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.12	---	---
	Time & Group Status	.15	---	---
	Treatment (TxGS)	.15	.01	.58
Northeast Austin Organization vs. Citywide Sample	Covariates	.13	---	---
	Time & Group Status	.13	---	---
	Treatment (TxGS)	.14	.04	1.18
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.19	---	---
	Time & Group Status	.19	---	---
	Treatment (TxGS)	.20	.08	1.78
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.14	---	---
	Time & Group Status	.16	---	---
	Treatment (TxGS)	.16	.04	1.25
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.11	---	---
	Time & Group Status	.12	---	---
	Treatment (TxGS)	.12	-.06	.99
Edgewater Community Council vs. Citywide Sample	Covariates	.12	---	---
	Time & Group Status	.12	---	---
	Treatment (TxGS)	.12	.01	.68

[1] $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

Hypothesis Six: Reduced Fear and Perceived Crime Rates

According to Hypothesis Six, one of the major outcomes of community crime prevention programs should be a reduction in residents' fear of crime and a drop in the amount of crime they perceive in their neighborhood. Although fear of crime and perceptions of the crime problem are conceptually and empirically distinct constructs (See Baumer & Rosenbaum, 1981), for simplicity of presentation, we will discuss them together under a general hypothesis about psychological responses. Three scales were employed: fear of personal crime, fear of property crime, and perceptions of neighborhood crime.

Fear of Personal Crime. This fear scale measures individual concern about being harmed or threatened while walking alone in ones own neighborhood. Contrary to the hypothesis, the panel results indicated significant increases in fear of personal crime in three of the four neighborhoods. Table 47 shows that for NNF, these changes occurred in relationship to comparison neighborhoods but not in relationship to the city as a whole. For BYNC and NAC, however, the increase in fear of personal crime was observed relative to both control groups. The independent samples revealed no changes on this scale. (See Table 48). The significant changes in fear of personal crime among panel respondents are displayed in Figure 20.

Fear of Property Crime. This scale measures residents' fear of being victimized by property crime, primarily their concern about residential burglary. The results do not support the hypothesis. As shown in Tables 49 and 50, there are many nonsignificant findings, and the significant results are generally in the opposite direction,

TABLE 47
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Fear of Personal Crime

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood	Pretest	.48	338.90***	.54	160.79***
Federation vs.	Covariates[2]	.54	5.81***	---	---
Comparison Neighborhoods	Treatment	.55	6.48**	.09	6.48**
Northwest Neighborhood	Pretest	.45	459.01***	.59	279.32***
Federation vs.	Covariates	.48	3.82***	---	---
Citywide Sample	Treatment	.48	.08	.01	.08
Northeast Austin	Pretest	.44	147.07***	.45	55.42***
Organization vs.	Covariates	.53	4.01***	---	---
Comparison Neighborhoods	Treatment	.55	6.84**	.15	6.84**
Northeast Austin	Pretest	.41	296.92***	.56	181.24***
Organization vs.	Covariates	.45	2.83**	---	---
Citywide Sample	Treatment	.45	3.59[1]	.08	3.59
Back of Yards Neighborhood	Pretest	.38	101.25***	.52	63.86***
Council vs.	Covariates	.44	1.98*	---	---
Comparison Neighborhoods	Treatment	.49	15.92***	.24	15.92***
Back of Yards Neighborhood	Pretest	.41	277.61***	.58	187.45***
Council vs.	Covariates	.44	2.43**	---	---
Citywide Sample	Treatment	.46	12.79***	.15	12.79**
Edgewater Community	Pretest	.47	226.80***	.53	97.15***
Council vs.	Covariates	.54	4.09***	---	---
Comparison Neighborhoods	Treatment	.54	.42	-.03	.42
Edgewater Community	Pretest	.45	393.77***	.59	221.71***
Council vs.	Covariates	.48	2.59**	---	---
Citywide Sample	Treatment	.48	.08	-.01	.08
Auburn-Gresham	Pretest	.45	398.61***	.60	244.93***
vs.	Covariates	.48	2.63**	---	---
Citywide Sample	Treatment	.48	.17	.02	.17

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

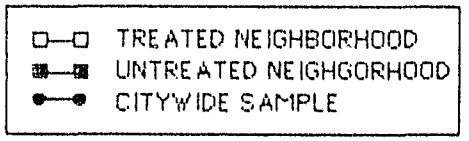
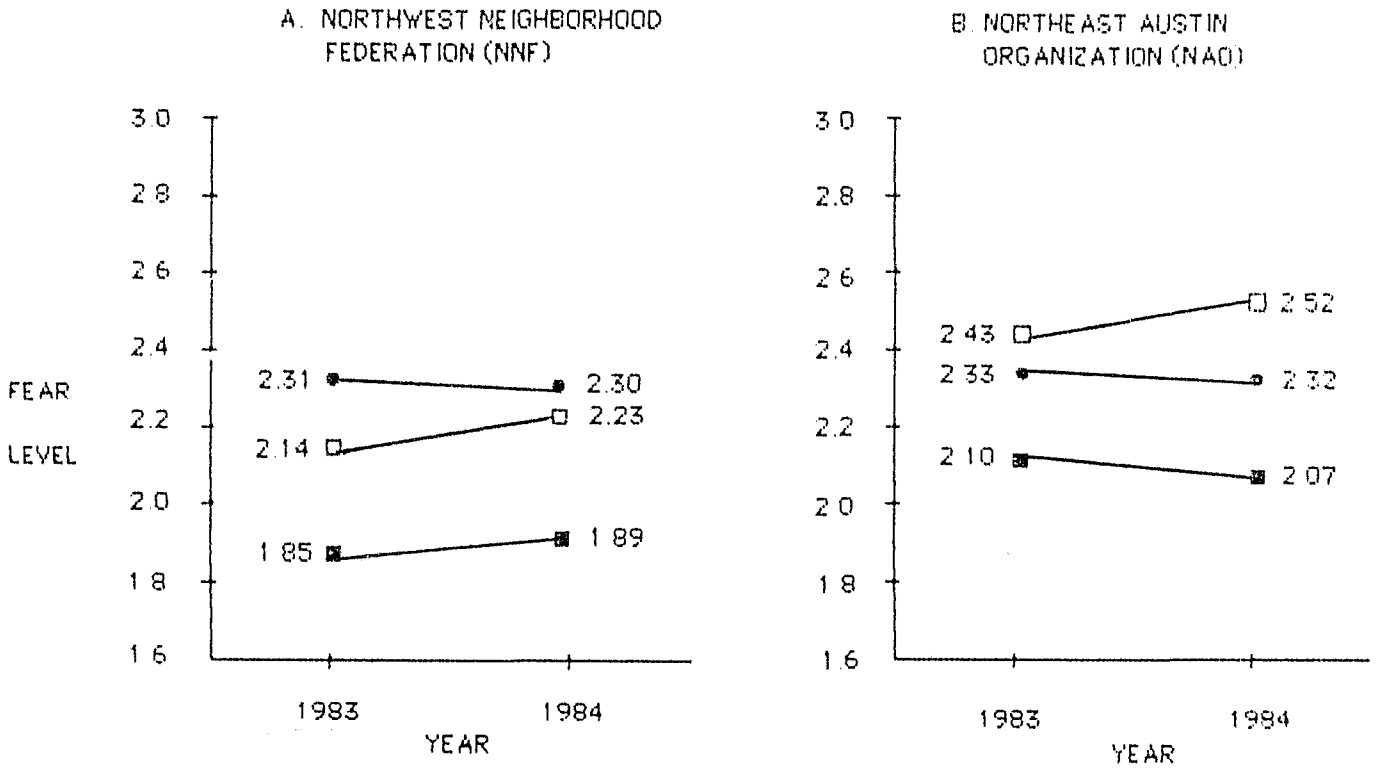


FIGURE 20
CHANGES IN FEAR OF PERSONAL CRIME
PANEL SAMPLE - NEIGHBORHOOD LEVEL



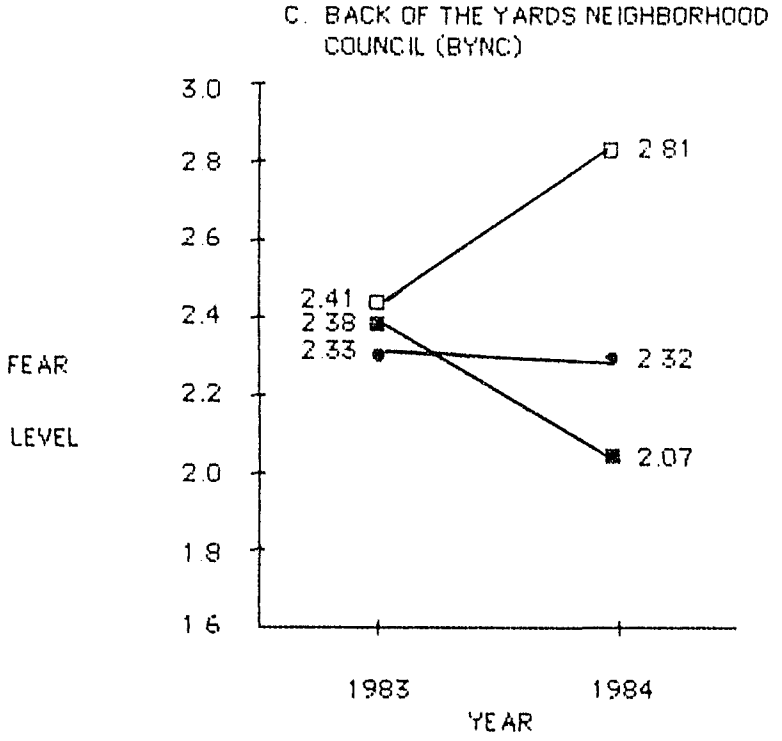


Table 48
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Fear of Personal Crime

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.26	---	----
	Time & Group Status	.28	---	----
	Treatment (TxGS)	.28	-.01	.53
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.23	---	----
	Time & Group Status	.23	---	----
	Treatment (TxGS)	.23	.00	.89
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.26	---	----
	Time & Group Status	.30	---	----
	Treatment (TxGS)	.30	.03	.22
Northeast Austin Organization vs. Citywide Sample	Covariates	.22	---	----
	Time & Group Status	.23	---	----
	Treatment (TxGS)	.23	.03	.88
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.25	---	----
	Time & Group Status	.27	---	----
	Treatment (TxGS)	.27	.00	.23
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.21	---	----
	Time & Group Status	.22	---	----
	Treatment (TxGS)	.22	.05	2.17
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.32	---	----
	Time & Group Status	.32	---	----
	Treatment (TxGS)	.33	.07	1.63
Edgewater Community Council vs. Citywide Sample	Covariates	.24	---	----
	Time & Group Status	.25	---	----
	Treatment (TxGS)	.25	.03	.73

[1] $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 49
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Fear of Property Crime

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood	Pretest	.29	149.49***	.51	126.78***
Federation vs.	Covariates[2]	.31	1.68	---	---
Comparison Neighborhoods	Treatment	.31	.23	.02	.23
Northwest Neighborhood	Pretest	.26	196.80***	.47	159.14***
Federation vs.	Covariates	.29	2.48**	---	---
Citywide Sample	Treatment	.29	.49	.03	.49
Northeast Austin	Pretest	.21	49.67***	.43	40.45***
Organization vs.	Covariates	.30	2.39**	---	---
Comparison Neighborhoods	Treatment	.30	.40	.05	.40
Northeast Austin	Pretest	.26	148.81***	.45	107.87***
Organization vs.	Covariates	.31	3.24***	---	---
Citywide Sample	Treatment	.31	.07	.01	.07
Back of Yards Neighborhood	Pretest	.14	27.69***	.33	18.27***
Council vs.	Covariates	.20	1.39	---	---
Comparison Neighborhoods	Treatment	.22	2.98	.13	2.98
Back of Yards Neighborhood	Pretest	.24	127.55***	.43	94.24***
Council vs.	Covariates	.29	3.05***	---	---
Comparison Neighborhoods	Treatment	.30	4.75*	.10	4.75*
Edgewater Community	Pretest	.24	149.92***	.45	116.58***
Council vs.	Covariates	.26	1.58	---	---
Comparison Neighborhoods	Treatment	.26	.05	.01	.05
Edgewater Community	Pretest	.25	84.35***	.44	57.34***
Council vs.	Covariates	.29	1.64	---	---
Citywide Sample	Treatment	.29	.07	.01	.07
Auburn-Gresham	Pretest	.24	149.92***	.45	116.58***
vs.	Covariates	.26	1.58	---	---
Citywide Sample	Treatment	.26	.05	.01	.05

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

Table 50
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Fear of Property Crime

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.08	---	----
	Time & Group Status	.08	---	----
	Treatment (TxGS)	.08	.04	.51
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.10	---	----
	Time & Group Status	.10	---	----
	Treatment (TxGS)	.10	.09	5.92*
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.12	---	----
	Time & Group Status	.13	---	----
	Treatment (TxGS)	.14	-.11	2.92[1]
Northeast Austin Organization vs. Citywide Sample	Covariates	.12	---	----
	Time & Group Status	.13	---	----
	Treatment (TxGS)	.13	.01	.63
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.12	---	----
	Time & Group Status	.13	---	----
	Treatment (TxGS)	.13	.11	3.41[1]
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.12	---	----
	Time & Group Status	.12	---	----
	Treatment (TxGS)	.13	.14	14.98***
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.12	---	----
	Time & Group Status	.13	---	----
	Treatment (TxGS)	.13	-.10	2.68
Edgewater Community Council vs. Citywide Sample	Covariates	.11	---	----
	Time & Group Status	.12	---	----
	Treatment (TxGS)	.12	.04	1.03

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

showing increases in fear of property crime. The panel data produced only one significant effect, and this was an increase of fear in BYNC relative to its comparison neighborhoods. The independent samples data showed a significant decline in fear of property crime in NAO (in support of the hypothesis), but significant increases were observed in NNF and BYNC. These changes are illustrated in Figure 21.

Perceptions of Neighborhood Crime. A four-item scale measured residents' perceptions of the amount of crime in their neighborhood. Contrary to the hypothesis, the panel results in Table 51 indicate that residents in two of the four treated neighborhoods experienced increases in the amount of crime in their immediate environments. These changes are shown in Figure 22. NNF residents perceived more neighborhood crime relative to residents in comparison areas, but not relative to the citywide sample. NAO residents felt a considerable rise in local crime rates relative to both control groups. The independent samples showed no significant changes in perceptions of neighborhood crime. (See Table 52).

In sum, the available evidence from three outcome measures not only failed to support Hypothesis Six, but showed unexpected changes in the opposite direction. Specifically, there were some consistent findings in the panel samples which showed increases in fear of personal crime and increases in perceptions of local crime rates. Also, the independent samples show increases in fear of property crime in two neighborhoods.

Hypothesis Seven: Increased Optimism and Attachment to Neighborhood

Hypothesis Seven addresses the final outcome of these interventions. If all goes well, the programs should improve

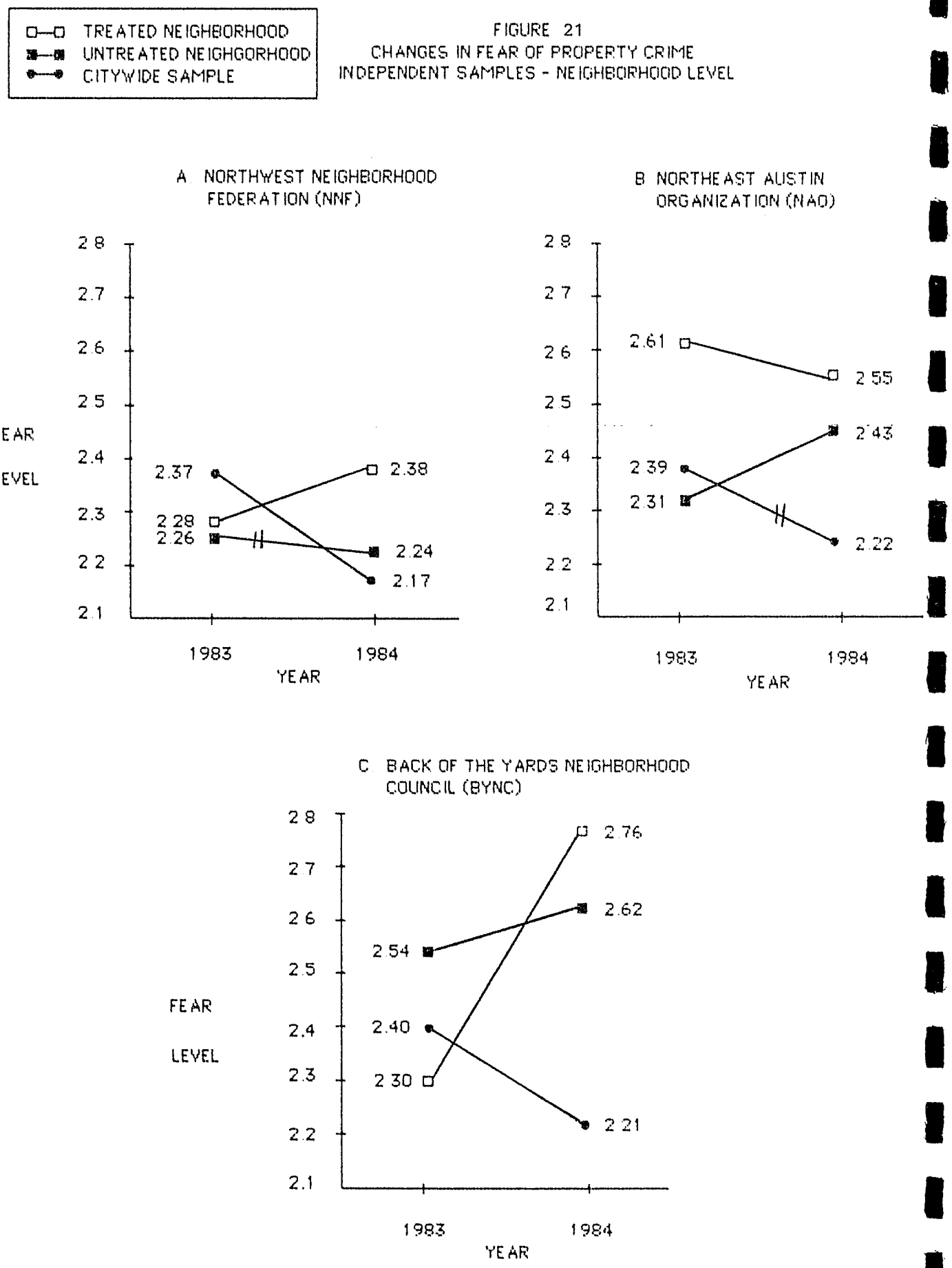


FIGURE 21
CHANGES IN FEAR OF PROPERTY CRIME
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

TABLE 51
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Perceptions of Neighborhood Crime

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood	Pretest	.30	157.52***	.51	120.58***
Federation vs.	Covariates[2]	.32	1.06	---	---
Comparison Neighborhoods	Treatment	.33	4.49*	.10	4.49*
Northwest Neighborhood	Pretest	.37	331.44***	.55	239.78***
Federation vs.	Covariates	.40	2.57	---	---
Citywide Sample	Treatment	.40	.00	.00	.00
Northeast Austin	Pretest	.23	55.38***	.39	33.61***
Organization vs.	Covariates	.29	1.67	---	---
Comparison Neighborhoods	Treatment	.38	25.53***	.34	25.53***
Northeast Austin	Pretest	.37	243.82***	.50	150.21***
Organizaion vs.	Covariates	.41	3.34***	---	---
Citywide Sample	Treatment	.44	18.25***	.18	18.25***
Back of Yards Neighborhood	Pretest	.31	73.04***	.41	32.77***
Council vs.	Covariates	.40	2.76**	---	---
Comparison Neighborhoods	Treatment	.41	1.54	.08	1.54
Back of Yards Neighborhood	Pretest	.37	236.70***	.51	143.85***
Council vs.	Covariates	.41	3.11***	---	---
Citywide Sample	Treatment	.42	2.86	.07	2.86
Edgewater Community	Pretest	.37	143.01***	.53	90.14***
Council vs.	Covariates	.40	1.63	---	---
Comparison Neighborhoods	Treatment	.41	1.56	.07	1.56
Edgewater Community	Pretest	.40	310.64***	.54	195.66***
Council vs.	Covariates	.43	3.56***	---	---
Citywide Sample	Treatment	.43	.00	.00	.00
Auburn-Gresham	Pretest	.40	320.29***	.56	217.96***
vs.	Covariates	.43	2.88**	---	---
Citywide Sample	Treatment	.43	.61	.03	.61

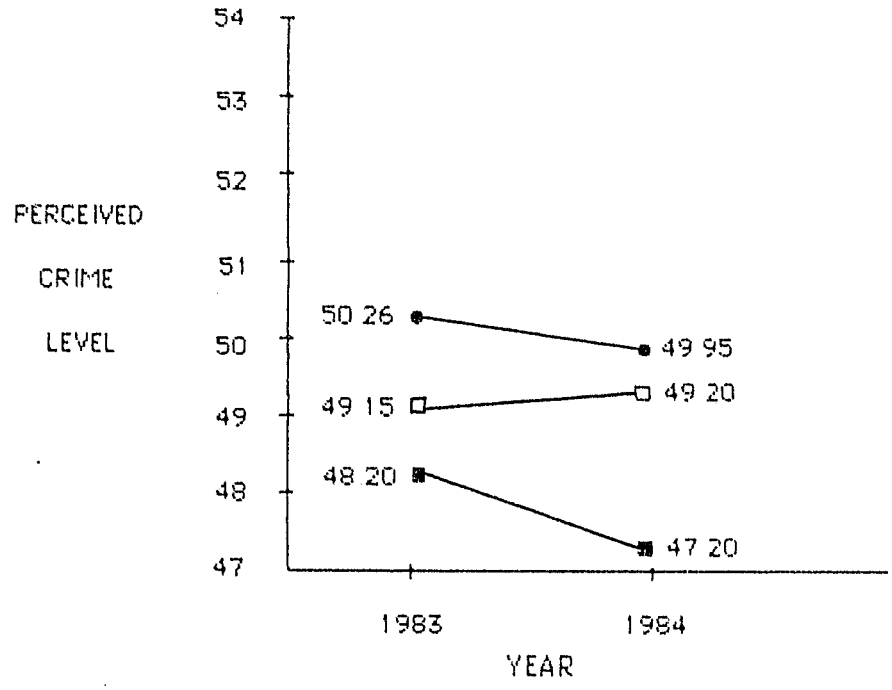
[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TREATED NEIGHBORHOOD
UNTREATED NEIGHGORHOOD
CITYWIDE SAMPLE

FIGURE 22
CHANGES IN PERCEPTIONS OF NEIGHBORHOOD CRIME
PANEL SAMPLE - NEIGHBORHOOD LEVEL

A NORTHWEST NEIGHBORHOOD FEDERATION (NNF)



B NORTHEAST AUSTIN ORGANIZATION (NAO)

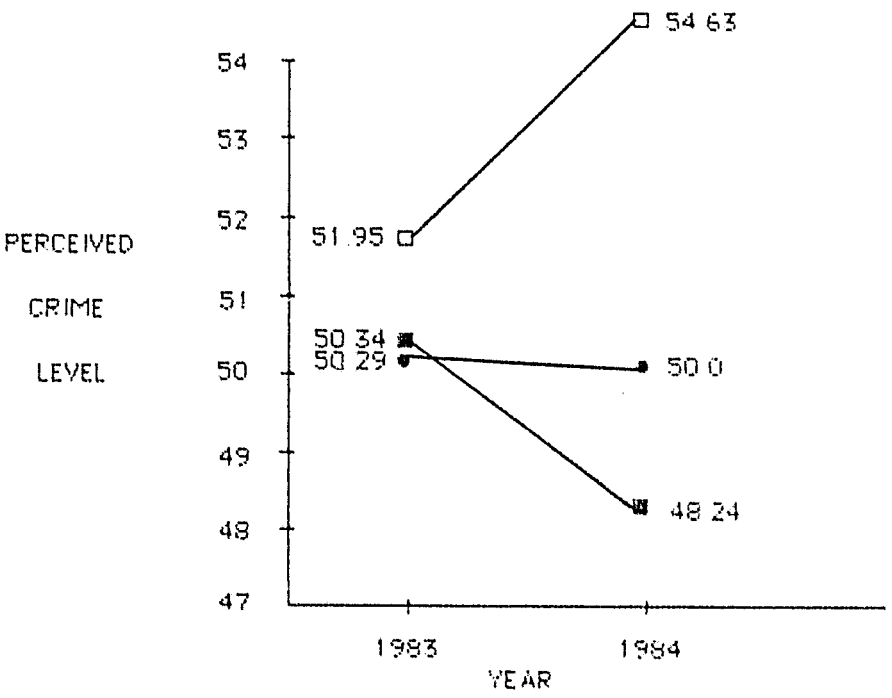


Table 52
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Perceptions of Neighborhood Crime

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.14	---	---
	Time & Group Status	.15	---	---
	Treatment (TxGS)	.16	.09	2.12
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.17	---	---
	Time & Group Status	.18	---	---
	Treatment (TxGS)	.18	-.02	.28
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.27	---	---
	Time & Group Status	.29	---	---
	Treatment (TxGS)	.29	.04	.42
Northeast Austin Organization vs. Citywide Sample	Covariates	.20	---	---
	Time & Group Status	.20	---	---
	Treatment (TxGS)	.20	.00	.37
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.24	---	---
	Time & Group Status	.25	---	---
	Treatment (TxGS)	.25	.01	.28
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.21	---	---
	Time & Group Status	.22	---	---
	Treatment (TxGS)	.22	.01	.15
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.16	---	---
	Time & Group Status	.16	---	---
	Treatment (TxGS)	.16	-.03	.29
Edgewater Community Council vs. Citywide Sample	Covariates	.19	---	---
	Time & Group Status	.19	---	---
	Treatment (TxGS)	.19	-.01	.12

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

residents' optimism about the future of their neighborhood and increase their attachment to the area as a place of live. Two scales were used to test this hypothesis -- a two-item scale measuring residents' optimism about neighborhood change in the past year and in the two years ahead (ie. whether the neighborhood is getting "better" "worse" or "staying about the same"), and a single-item scale asking residents about their likelihood of moving in the next two years.

Optimism About Neighborhood Change. Contrary to the hypothesis, the panel results showed significant declines in residents' optimism about neighborhood change in three of the four neighborhoods (See Table 53). In other words, residents were more inclined after one year to report that their neighborhood is getting "worse" rather than "better". Although the control groups also showed declines in optimism, the treatment areas were declining at a faster rate. The fourth neighborhood -- ECC -- showed a significant increase in optimism about the neighborhood. These The independent samples showed no effects on optimism (See Table 54).

As Figure 23 shows, the changes in optimism within the panel sample are complicated by pretest differences in two of the neighborhoods. That is, NAO and BYNC residents were significantly less optimistic about the future of their neighborhoods than residents in the respective control groups. Hence, there is a greater possibility of selection differences interacting with other factors (including the treatment) to produce these effects.

Likelihood of Moving. Changes in the likelihood of moving out of the neighborhood also ran counter to the hypothesis. In the panel sample, residents from two of the four neighborhoods (NAO and BYNC)

TABLE 53
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Optimism About Neighborhood Change

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.16	68.74***	.36	54.69***
	Covariates[2]	.20	2.51**	---	---
	Treatment	.21	5.10*	-.11	5.10*
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.16	104.37***	.36	79.21***
	Covariates	.19	2.18*	---	---
	Treatment	.19	.40	-.03	.40
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.44	146.47***	.58	81.51***
	Covariates	.45	.33	---	---
	Treatment	.48	11.41***	-.23	11.41***
Northeast Austin Organization vs. Citywide Sample	Pretest	.25	141.25***	.39	76.73***
	Covariates	.28	2.05*	---	---
	Treatment	.32	22.38***	-.22	22.38***
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.33	82.73***	.46	40.33***
	Covariates	.37	1.08	---	---
	Treatment	.43	16.35***	-.29	16.35***
Back of Yards Neighborhood Council vs. Citywide Sample	Pretest	.20	100.96***	.37	60.31***
	Covariates	.24	2.41**	---	---
	Treatment	.26	7.60**	-.14	7.60**
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.41	171.71***	.62	142.98***
	Covariates	.43	1.03	---	---
	Treatment	.43	1.60	.06	1.60
Edgewater Community Council vs. Citywide Sample	Pretest	.20	123.11***	.41	96.19***
	Covariates	.24	2.59**	---	---
	Treatment	.25	4.02*	.09	4.02*
Auburn-Gresham vs. Citywide Sample	Pretest	.13	73.76***	.34	63.62***
	Covariates	.16	2.14*	---	---
	Treatment	.17	.80	.05	.80

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

FIGURE 23
CHANGES IN OPTIMISM ABOUT FUTURE
PANEL SAMPLE - NEIGHBORHOOD SAMPLE

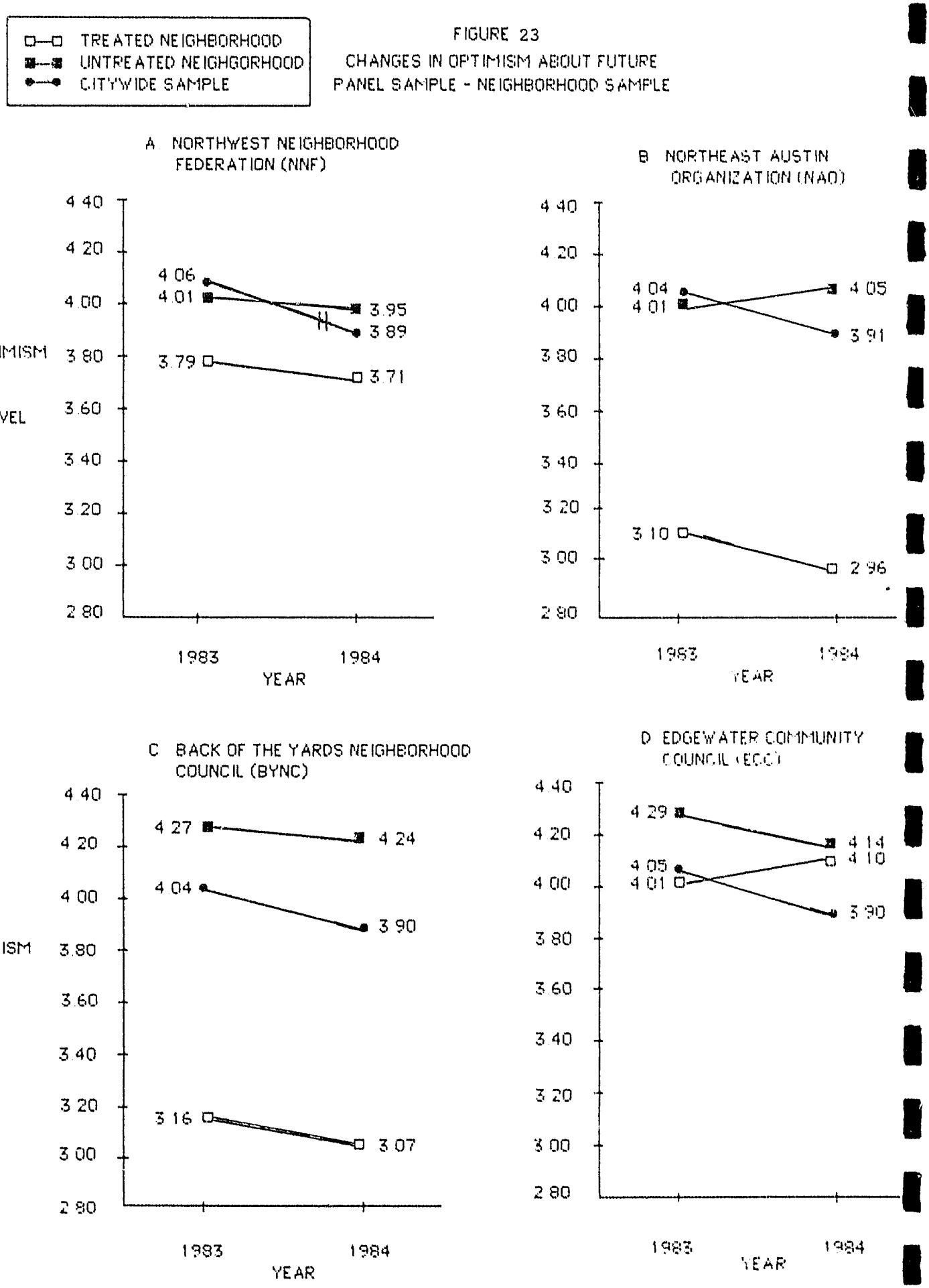


Table 54
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Optimism About Neighborhood Change

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.05	---	----
	Time & Group Status	.07	---	----
	Treatment (TxGS)	.07	-.07	1.36
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.08	---	----
	Time & Group Status	.09	---	----
	Treatment (TxGS)	.09	.03	.50
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.14	---	----
	Time & Group Status	.25	---	----
	Treatment (TxGS)	.25	.01	.82
Northeast Austin Organization vs. Citywide Sample	Covariates	.12	---	----
	Time & Group Status	.19	---	----
	Treatment (TxGS)	.19	.03	.78
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.11	---	----
	Time & Group Status	.24	---	----
	Treatment (TxGS)	.24	.01	.55
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.12	---	----
	Time & Group Status	.16	---	----
	Treatment (TxGS)	.16	-.01	.21
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.12	---	----
	Time & Group Status	.16	---	----
	Treatment (TxGS)	.16	-.09	2.55
Edgewater Community Council vs. Citywide Sample	Covariates	.08	---	----
	Time & Group Status	.08	---	----
	Treatment (TxGS)	.08	.01	.11

[1] $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

revealed significant increases in their likelihood of moving relative to both control groups (See Table 55). The independent samples showed no changes in the likelihood of moving (See Table 56).

In sum, the available evidence, goes against Hypothesis Seven. In the panel samples, three of the four neighborhoods showed decreases in optimism about changes in the neighborhood, and two of the four neighborhoods showed a greater likelihood of moving among residents.

C. Summary of Neighborhood Hypothesis Testing

Tables 57 and 58 are designed to summarize the neighborhood-level results presented up to this point. Each table is a matrix of the major outcome variables crossed with neighborhood comparisons. Significant findings and the direction of change in relationship to the hypothesis are shown.

These summary tables are useful for highlighting several facts. First, the large majority of comparisons revealed no differential change between the treated and untreated areas, thus failing to support most of the main hypotheses. Second, patterns of significant changes are apparent by neighborhood, with some neighborhoods showing very little change and others showing change on a number of outcome variables. Third, the majority of significant findings run counter to the main hypotheses, as indicated by the asterisks in Tables 57 and 58.

The differences in outcomes between the target neighborhoods are noteworthy (Outcomes numbered 13 to 23 in Tables 57 best illustrate these differences). ECC and AG -- areas where "programs", per se, were not implemented to our knowledge -- showed very few changes relative to control groups. In contrast, NNF, NAO, and BYNC showed a

TABLE 55
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
PANEL SAMPLE - NEIGHBORHOOD LEVEL

Dependent Variable = Likelihood of Moving[3]

Comparison	Predictors	Cum R^2	F Change	BETA	F BETA
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Pretest	.28	139.84***	.48	109.68***
	Covariates[2]	.35	4.55***	---	---
	Treatment	.35	.13	-.02	.13
Northwest Neighborhood Federation vs. Citywide Sample	Pretest	.36	293.37***	.50	180.69***
	Covariates	.41	4.79***	---	---
	Treatment	.41	.61	.03	.61
Northeast Austin Organization vs. Comparison Neighborhoods	Pretest	.34	91.74***	.54	68.90***
	Covariates	.39	1.48	---	---
	Treatment	.46	20.48***	-.31	20.48***
Northeast Austin Organization vs. Citywide Sample	Pretest	.39	258.72***	.54	155.22***
	Covariates	.42	1.83[1]	---	---
	Treatment	.44	15.76***	-.17	15.76***
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Pretest	.20	37.38***	.37	23.84***
	Covariates	.25	1.13	---	---
	Treatment	.28	6.94**	-.19	6.94**
Back of Yards Neighborhood Council vs. Citywide Sample	Pretest	.37	216.59***	.52	133.25***
	Covariates	.40	2.28*	---	---
	Treatment	.41	5.69*	-.11	5.69*
Edgewater Community Council vs. Comparison Neighborhoods	Pretest	.34	125.70***	.56	93.78***
	Covariates	.36	.87	---	---
	Treatment	.36	.50	.04	.50
Edgewater Community Council vs. Citywide Sample	Pretest	.39	293.28***	.55	185.27
	Covariates	.43	2.88**	---	---
	Treatment	.43	.10	.01	.10
Auburn-Gresham vs. Citywide Sample	Pretest	.37	265.35***	.49	137.88***
	Covariates	.42	4.06***	---	---
	Treatment	.42	2.16	.07	2.16

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariate = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

[3] Coded such that higher scores indicate less likelihood of moving

Table 56
HIERARCHICAL MULTIPLE REGRESSION ANALYSIS
INDEPENDENT SAMPLES - NEIGHBORHOOD LEVEL

Dependent Variable = Likelihood of Moving[3]

Comparison	Predictors	Cum R^2	Beta	F of Beta
Northwest Neighborhood Federation vs. Comparison Neighborhoods	Covariates[2]	.13	---	---
	Time & Group Status	.15	---	---
	Treatment (TxGS)	.15	-.01	.14
Northwest Neighborhood Federation vs. Citywide Sample	Covariates	.17	---	---
	Time & Group Status	.19	---	---
	Treatment (TxGS)	.19	-.02	.45
Northeast Austin Organization vs. Comparison Neighborhoods	Covariates	.10	---	---
	Time & Group Status	.20	---	---
	Treatment (TxGS)	.21	.02	.75
Northeast Austin Organization vs. Citywide Sample	Covariates	.13	---	---
	Time & Group Status	.20	---	---
	Treatment (TxGS)	.20	.04	1.23
Back of Yards Neighborhood Council vs. Comparison Neighborhoods	Covariates	.19	---	---
	Time & Group Status	.22	---	---
	Treatment (TxGS)	.22	-.03	.34
Back of Yards Neighborhood Council vs. Citywide Sample	Covariates	.18	---	---
	Time & Group Status	.19	---	---
	Treatment (TxGS)	.19	-.02	.28
Edgewater Community Council vs. Comparison Neighborhoods	Covariates	.16	---	---
	Time & Group Status	.18	---	---
	Treatment (TxGS)	.18	-.01	.81
Edgewater Community Council vs. Citywide Sample	Covariates	.18	---	---
	Time & Group Status	.20	---	---
	Treatment (TxGS)	.20	-.02	.47

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

[3] Coded such that higher scores indicate less likelihood of moving.

TABLE 57

MATRIX OF SIGNIFICANT CHANGES[1]
AREAS BY OUTCOME MEASURES

PANEL SAMPLE - NEIGHBORHOOD LEVEL

	Area Comparisons								
	NNF vs Comparison	NNF vs Chicago	NAO vs Comparison	NAO vs Chicago	BYNC vs Comparison	BYNC vs Chicago	ECC vs Comparison	ECC vs Chicago	AG vs Chicago
1. Exposure to the Treatment	Increase	Increase		Increase	Increase	Increase	Increase	Increase	Increase
2. Participation in the Treatment		Increase	Increase	Increase	Increase		Increase		
3. Efficacy of Block Action	Decrease*			Increase	Decrease*		Increase	Increase	
4. Efficacy of Collective Crime Prevention Behavior									
5. Efficacy of Individual Target Hardening						Decrease*			
6. Attribution of Responsibility for Crime Protection			Decrease*	Decrease*					
7. Home Protection Behavior		Increase		Increase					Increase
8. Street Avoidance Behavior									
9. Percentage of Victimizations Reported to Police			Increase					Decrease*	
10. Asking Neighbors to Watch Your Home		Increase							Increase
11. Willingness to Take Action Against Neighborhood Problems			Decrease*						
12. Frequency of Chatting With Neighbors									

[1] "Increase" and "decrease" should not be interpreted literally. They indicate significant (or marginally significant) differential change over time between the treatment and control areas as expressed by positive or negative betas. The actual direction of change requires inspection of the means.

* Opposite direction to that hypothesized.

TABLE 57 (continued)
MATRIX OF SIGNIFICANT CHANGES[1]
AREAS BY OUTCOME MEASURES

PANEL SAMPLE - NEIGHBORHOOD LEVEL

	Area Comparisons								
	NNF vs Comparison	NNF vs Chicago	NAO vs Comparison	NAO vs Chicago	BYNC vs Comparison	BYNC vs Chicago	ECC vs Comparison	ECC vs Chicago	AG vs Chicago
13. Number of Block Residents you Know by Name			Decrease*	Decrease*					
14. Victimization Experience		Decrease	Increase*	Increase*		Increase*			
15. Vicarious Victimization	Increase*		Increase*	Increase*	Increase*				
16. Youth Disorder			Increase*					Decrease	
17. Youth Rejection of Social Control	Increase*				Increase*				
18. Neighborhood Deterioration		Decrease							Decrease
19. Fear of Personal Crime	Increase*		Increase*		Increase*	Increase*			
20. Fear of Property Crime					Increase*				
21. Perceptions of Neighborhood Crime	Increase*		Increase*	Increase*					
22. Optimism About Neighborhood Change	Decrease*		Decrease*	Decrease*	Decrease*	Decrease*		Increase	
23. Likelihood of Moving			Increase*	Increase*	Increase*	Increase*			

[1] "Increase" and "decrease" should not be interpreted literally. They indicate significant differential change over time between the treatment and control areas as expressed by positive or negative betas. The actual direction of change requires inspection of the means.

* Opposite direction to that hypothesized.

TABLE 58
MATRIX OF SIGNIFICANT CHANGES[1]
AREAS BY OUTCOME MEASURES

INDEPENDENT SAMPLE - NEIGHBORHOOD LEVEL

	Area Comparisons							
	NNF vs Comparison	NNF vs Chicago	NAO vs Comparison	NAO vs Chicago	BYNC vs Comparison	BYNC vs Chicago	ECC vs Comparison	ECC vs Chicago
1. Exposure to the Treatment		Increase						
2. Participation in the Treatment		Increase	Increase					
3. Efficacy of Block Action				Increase				Increase
4. Efficacy of Collective Crime Prevention Behavior		Increase						
5. Efficacy of Individual Target Hardening								
6. Attribution of Responsibility for Crime Protection								
7. Home Protection Behavior								
8. Street Avoidance Behavior						Increase	Increase	Increase
9. Percentage of Victimizations Reported to Police	Decrease*	Decrease*						
10. Asking Neighbors to Watch Your Home								
11. Tendency to Take Action Against Neighborhood Problems							Increase	Increase
12. Frequency of Chatting With Neighbors								

[1] "Increase and "decrease" should not be interpreted literally. They indicate significant (or marginally significant) differential change over time between the treatment and control areas as expressed by positive or negative betas. The actual direction of change requires inspection of the means.

* Opposite direction to that hypothesized.

TABLE 58 (continued)
MATRIX OF SIGNIFICANT CHANGES[1]
AREAS BY OUTCOME MEASURES

INDEPENDENT SAMPLE - NEIGHBORHOOD LEVEL

	Area Comparisons							
	NNF vs Comparison	NNF vs Chicago	NAO vs Comparison	NAO vs Chicago	BYNC vs Comparison	BYNC vs Chicago	ECC vs Comparison	ECC vs Chicago
13. Number of Block Residents You Know by Name								
14. Victimization Experience								
15. Vicarious Victimization		Increase						
16. Youth Disorder					Increase	Increase		
17. Youth Rejection of Social Control								
18. Neighborhood Deterioration								
19. Fear of Personal Crime								
20. Fear of Property Crime		Increase*	Decrease		Increase*	Increase*		
21. Perceptions of Neighborhood Crime								
22. Optimism About Neighborhood Change								
23. Likelihood of Moving								

[1] "Increase and "decrease" should not be interpreted literally. They indicate significant (or marginally significant) differential change over time between the treatment and control areas as expressed by positive or negative betas. The actual direction of change requires inspection of the means.

* Opposite direction to that hypothesized.

number of significant unexpected changes. NAO and BYNC in particular revealed the strongest pattern of counter-hypothesis findings, ranging from increases in victimization and fear of crime to declines in optimism about the future of neighborhood and increases in the likelihood of moving out.

As one might expect, these "treated" neighborhoods differed in ways other than the extent to which they received a strong intervention. Our field work and pretest data suggest that the two areas which showed few changes are relatively stable middle class neighborhoods, while the other three areas are experiencing more "residential transition." Thus, the apparent negative effects of the intervention in these neighborhoods may be attributable to neighborhood decline rather than the intervention itself. With two control groups, however, this type of alternative explanation is not as compelling as it might be otherwise. We will explore this issue further in the next section.

The NNF pattern is also noteworthy. While the comparisons between NNF and its neighborhood control group revealed a pattern of negative findings similar to NAO or BYNC, the comparisons with the citywide control group paint a more positive picture that is consistent with the hypotheses. At best, we can conclude that the significant results are mixed, and we must keep in mind that the bulk of the findings indicate no change.

Finally, there was a striking difference in the number of significant findings yielded by the panel sample and the independent samples. Few of the independent sample analyses showed significant differential change between the treatment and control groups. There

are several possible reasons for this discrepancy. For example, community crime prevention tends to be geared to, or more appealing to, the stable, "rooted" members of the community, who are over-represented in panel samples. They are over-represented because the less rooted portion of the sample (e.g. renters) is more likely to "drop out" between the pretest and the posttest. If any of the observed changes can be attributed to the interventions, then the panel sample is most likely to manifest these changes because these individuals are more likely to be exposed to (and listen to) the message of community organizers who are interested in neighborhood improvement.

Another possible reason for the differences in results between the panel and independent samples is a statistical one. Panel designs are simply more powerful for detecting changes in individual responses. The error term is reduced as we control for pretest differences between individuals. In any event, these two explanations also imply that the treatment effects are limited to certain populations and/or are too weak to be detected in independent samples.

D. Testing Alternative Explanations

Should we attribute the unexpected neighborhood changes to the treatment or can we propose some plausible alternative explanations for these largely unfavorable outcomes? Specifically, why did three of the four primary treatment areas (NNF, NAO, and BYNC) show significant increases in various problems, such as fear of crime, perceptions of the crime problem, vicarious victimization, and concern about future changes in the neighborhood? While it is possible that the intervention heightened these concerns and fears among residents, another possibility is that the neighborhoods selected for these programs were already on the decline at the time of program implementation, and this trend simply continued after the programs were initiated. Our field work offers some support for this interpretation, as organizers sometimes spoke of working in "front line" neighborhoods and fighting the battle against "residential transition". In addition, our survey data suggest that these neighborhoods were worse off than the control groups on some of these critical outcome measures at the time of the pretest (although most of the pretest comparisons between treatment and control groups were nonsignificant). For example, in comparison to their control groups, NAO and BYNC residents were significantly lower in optimism about the future of their neighborhood at the time of the pretest.

If these neighborhoods were, in fact, experiencing a process of decline, such changes may have resulted in a greater volume of crime and more victimizations -- outcomes than are not easily explained by the treatment (i.e. one would be hard pressed to specify how programs work to produce increases in victimization). There is a real

possibility that significant increases in victimization experience between the pretest and posttest -- both direct and indirect victimization -- can account for the increases in fear of crime and other perceptual changes. Hence, we tested the hypothesis that victimization experiences can explain differences in fear of crime to the point that apparent "treatment" effects will no longer be significant. That is, we tested the hypothesis that the treatment-fear relationship was spurious.

Using panel data, the predictors in our regression analysis included pretest scores, the covariates used in other regression equations, victimization experience at both the pretest and the posttest, and the treatment. The results indicate that for NAO and BYNC comparisons (where both fear and victimization showed increased in earlier analyses), victimization experience was unrelated to fear of crime. Thus, residents who were victimized between the pretest and posttest did not show significant increases in fear of crime. Furthermore, controlling for victimization between the pretest and posttest did not eliminate or fundamentally alter the significant "treatment" effect on fear. Thus, differential victimization is not a plausible rival explanation for increases in fear of crime.

A second alternative hypothesis is that increases in vicarious (indirect) victimization can account for increases in fear, as well as other psychological responses, such as perceptions of crime and declining optimism about the future of the neighborhood. The same regression procedures were applied using vicarious victimization at the posttest as the additional covariate. Similar to the direct victimization results, the findings indicate that vicarious

victimization between the pretest and posttest was unrelated to fear of crime and did not provide any evidence that the treatment-fear relationship was spurious.

We did find, however, that vicarious victimization was a strong and consistent predictor of decreased optimism and perceptions of increased neighborhood crime in NNF, NAO, and BYNC. Nonetheless, controlling for vicarious victimization did not reduce the significance of the relationship between the treatment and these outcome variables. Thus, vicarious victimization was also unable to stand as a plausible alternative explanation for the apparent treatment effects.

Returning to the question of "residential transition", another rival hypothesis is that increases in fear of crime are directly due to resident's growing concern about transition. Fear of crime is sometimes conceptualized as fear of strangers or fear of minority members. First, to assess the transition problem, residents were asked about whether "certain types of people moving into the neighborhood" is a "big problem", "some problem", or "almost no problem". Although NNF showed no changes, the NAO and BYNC neighborhoods showed significant differential increases in the size of the residential transition problem relative to control groups. Having documented these processes, the question becomes -- does this growing concern about transition explain the growing fear of crime originally attributed to the treatment? For both NAO and BYNC, changing concern about transition was unrelated to changing fear of crime, and the treatment-fear relationship remained significant after controlling for perceptions of residential transition. Although concern about

transition was related to fear in NNF, the treatment effect was still significant.

In sum, we have posited several alternative hypotheses to account for the unexpected outcomes observed at the neighborhood level. Although certain variables were able to explain a significant amount variance in the outcome measures of interest, nevertheless, they did not serve as plausible rivals to the hypothesis that the treatment was responsible for these effects. Of course, concluding that we can find no reasonable alternative explanations is not the same as concluding that we have a great deal of confidence in the original hypothesis that the treatment is causing the observed effects. Therefore, we have gone one step farther in search of more compelling evidence that community efforts do make a difference, for better or worse. To intensify our pre-planned search, we lowered our "research microscope" from the neighborhood level to the block level where a different treatment was administered in one particular neighborhood.

E. Block-Level Analyses: The NNF Test Case

One might be tempted to conclude that community crime prevention programs not only fail to meet our expectations, but even make matters worse. However, such a conclusion would be premature. Given a quasi-experimental research design and great concern about the strength of treatment implementation, our confidence in the inference that the program produced these effects is limited. However, there is another test of our hypotheses available to us and we have pursued this alternative; namely, a block-level analysis within the NNF neighborhood.

Evaluations can only test a theory of impact when there is minimal doubt about (a) the nature of the treatment and (b) the "dosage" of the treatment. For one particular neighborhood (NNF), we could at least identify and define the treatment, and we were fairly confident that the treatment dosage was higher than what we had observed in other neighborhoods.

Specifically, as noted earlier, NNF pursued the block watch approach to community crime prevention. A minimum of two meetings was necessary before NNF organizers would consider a block "organized". As described in our field notes:

> The organizer responsible for an area would canvass a block, talk to residents, and find someone who was willing to host the first meeting in their home. Then flyers would be distributed door-to-door telling other block residents about the meeting and where and when it would take place. At the first meeting, people were given a chance to meet each other, express what they saw as the block problems, and then find out about the program. A second meeting would be held to solidify the watch and let new participants attend. A block watch map would be established with the names, addresses, and phone numbers of all participants. A block rep would also be chosen to attend the local Anti-Crime Committee meetings. Block watch participants received monitoring sheets and current crime statistics on a regular basis from the Federation.

As evaluators, the question for us was whether the "theory" of community crime prevention (as delineated earlier in seven main hypotheses) would hold up under empirical scrutiny when the program was implemented more or less "according to the book"? Although NNF successfully organized many blocks in the manner described above, using experienced organizers, the number of blocks organized was short of their objective. Because community organizers were able to organize only about one-half of the blocks in the treatment area,

this created a unique opportunity for us to compare treated and untreated blocks within the same neighborhood, and apply the Pretest-Posttest Control Group Design used earlier.

This NNF test case did not suffer from the same types of (unmeasured) nonequivalence between treated and untreated areas that may have occurred with neighborhood comparisons, since both groups of blocks are in the same geographic area. Also, there was little evidence that organizers systematically selected certain blocks and not others. Residents who lived on treated and untreated blocks did not differ on a number of demographic characteristics, such as sex, age, and race. There were marginally more home owners living on treated blocks. However, our analysis controlled for occupancy status by using it as a covariate.

Implementation Results. The first analytic task was to check the success of implementation. That is, did residents on treated blocks report more exposure to (awareness of) the program and were they more likely to have participated in crime prevention meetings than residents on untreated blocks?

The results indicate that NNF organizers were highly successful at stimulating citizen awareness of and participation in local meetings. As illustrated in Figure 24, the magnitude of change over a one year period suggests that residents on the treated blocks received a higher dosage of the treatment than we observed at the neighborhood-level, or at least a higher percentage of the residents were involved with the program. There were no significant pretest differences in exposure or participation between residents on treated and untreated blocks.

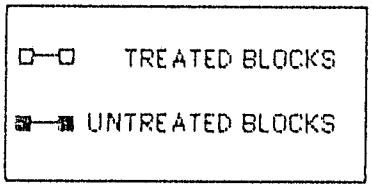
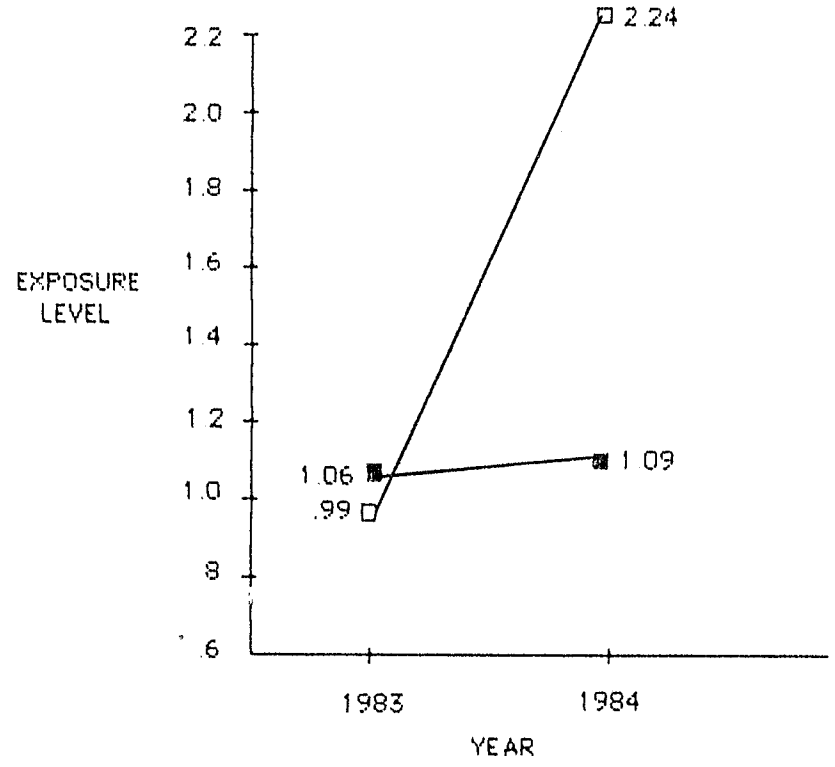
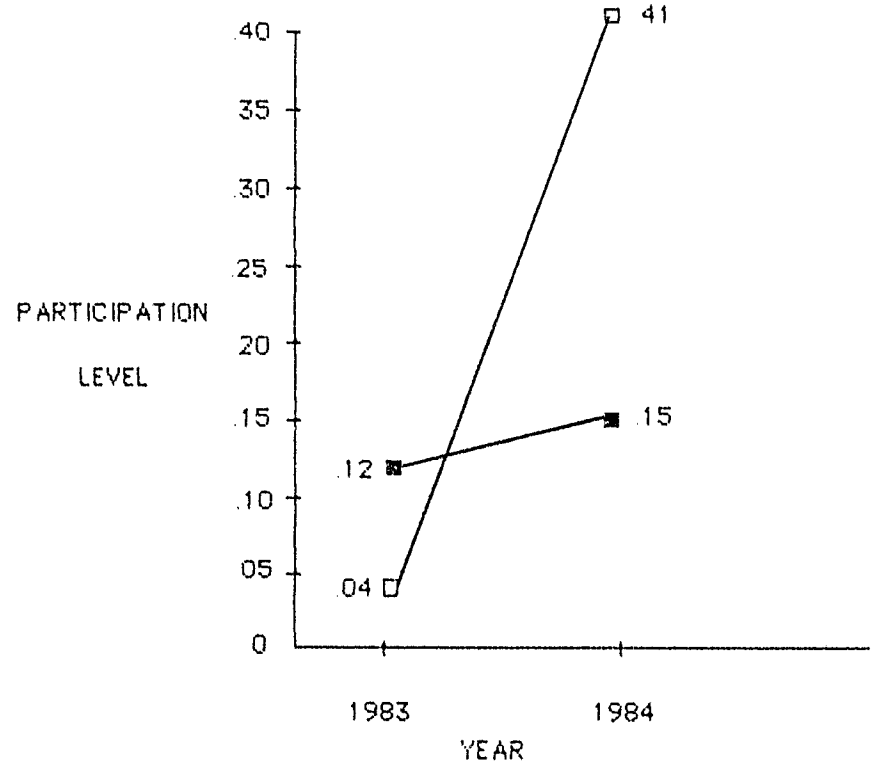


FIGURE 24
CHANGES IN EXPOSURE AND PARTICIPATION
PANEL SAMPLE - BLOCK LEVEL
NORTHWEST NEIGHBORHOOD FEDERATION





Outcome Results. The important question is -- did these organizing efforts make a difference? Did residents of treated and untreated blocks display differential changes in attitudes, perceptions, and/or behavior as a result of differential participation in the block watch program? Tables 59 and 60 tell the story. Across 21 separate outcome measures (i.e. dependent variables #3 thru #23) and across two different samples, the results indicate that neighborhood organizing had very few effects on local residents. Only one scale registered significant change and only with the panel sample. Specifically, residents on treated blocks were more likely than residents on untreated blocks to attribute responsibility for crime prevention to citizens instead of police. Three marginally significant findings were produced. In particular, residents of treated (vs untreated) blocks showed: (a) increases in home protection behaviors, (b) increases in action taken against neighborhood problems, (c) decreases in optimism about change in the neighborhood. Given that "action taken" may be confounded with participation in the treatment (i.e. treated block residents may have defined attendance at a block watch meeting as "action taken") only two of the three changes may be indicative of program impact. In sum, these data suggest that block watch meetings, if they have any effects, stimulate residents to accept more responsibility for crime prevention, secure their homes better, intervene more frequently when problems arise, and become more concerned about neighborhood decline.

However, when viewing the block-level results as a whole, the general conclusion must be that organizers were quite successful at

TABLE 59
HIERARCHICAL MULTIPLE REGRESSION ANALYSES
PANEL SAMPLE BLOCK LEVEL

Northwest Neighborhood Federation

Treated vs Untreated Blocks

	Dependent Variable	Predictors	Cum R^2	F Change	Beta	F Beta
1.	Exposure to the Treatment	Pretest	.18	46.48***	.39	50.64***
		Covariates[2]	.29	4.64***	---	---
		Treatment	.39	33.26***	.33	33.26***
2.	Participation in "Treatment" Meetings	Pretest	.10	23.95***	.33	26.93***
		Covariates	.14	1.41***	---	---
		Treatment	.20	15.52***	.26	15.52***
3.	Efficacy of Block Action	Pretest	.07	14.97***	.26	13.24***
		Covariates	.08	.42	---	---
		Treatment	.09	.29	.04	.29
4.	Efficacy of Collective Crime Prevention Behavior	Pretest	.04	9.26**	.21	8.63**
		Covariates	.06	.54	---	---
		Treatment	.07	.65	.06	.65
5.	Attribution of Responsibility for Crime Prevention	Pretest	.11	24.56***	.31	21.13***
		Covariates	.13	.78*	---	---
		Treatment	.15	4.39*	-.15	4.39*
6.	Home Protection Behavior	Pretest	.19	49.25***	.44	47.43***
		Covariates	.24	1.73	---	---
		Treatment	.25	2.96	.11	3.00[1]
7.	Efficacy of Individual Target Hardening	Pretest	.17	38.72***	.41	36.91***
		Covariates	.21	1.16	---	---
		Treatment	.21	.50	-.05	.50
8.	Street Avoidance	Pretest	.40	133.83***	.45	58.93***
		Covariates	.51	6.35	---	---
		Treatment	.52	1.92	.07	1.93
9.	Percentage of Victimization Reported to Police	Pretest	.10	7.12**	.30	5.45*
		Covariates	.15	.51	---	---
		Treatment	.15	.02	-.02	.02

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$.

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 59 (continued)
HIERARCHICAL MULTIPLE REGRESSION ANALYSES
PANEL SAMPLE BLOCK LEVEL

Northwest Neighborhood Federation

Treated vs Untreated Blocks

Dependent Variable	Predictors	Cum R^2	F Change	Beta	F Beta
10. Asking Neighbors to Watch Your Home	Pretest	.26	65.99***	.47	55.82***
	Covariates	.32	1.98*	---	---
	Treatment	.32	.01	-.01	.01
11. Tendency to Take Action Against Neighborhood Problems	Pretest	.21	7.67**	.50	8.44**
	Covariates	.41	1.25	---	---
	Treatment	.49	2.89[1]	.31	2.89[1]
12. Frequency of Chatting with Neighbors	Pretest	.17	44.65***	.35	32.24**
	Covariates	.25	2.68**	---	---
	Treatment	.26	1.73	-.08	1.73
13. Number of Block Residents You Know By Name	Pretest	.28	83.11***	.50	66.50**
	Covariates	.30	.99	---	---
	Treatment	.30	.02	-.01	.02
14. Victimization Experience	Pretest	.19	50.82***	.44	47.42**
	Covariates	.20	.52	---	---
	Treatment	.21	1.82	-.09	1.83
15. Vicarious Victimization	Pretest	.03	7.79**	-.17	6.65**
	Covariates	.07	1.25	---	---
	Treatment	.07	0.41	-.04	0.41
16. Youth Disorder	Pretest	.31	97.09***	.49	55.87***
	Covariates	.35	1.65	---	---
	Treatment	.35	.12	-.02	.73
17. Social Control Over Neighborhood Youth	Pretest	.29	83.80***	.46	52.89**
	Covariates	.37	3.27**	---	---
	Treatment	.38	.75	-.05	.75
18. Neighborhood Deterioration	Pretest	.18	46.78***	.37	33.92**
	Covariates	.27	3.16**	---	---
	Treatment	.27	.64	-.05	.64
19. Fear of Personal Crime	Pretest	.47	190.69***	.53	93.71**
	Covariates	.55	5.26	---	---
	Treatment	.55	.53	.04	.53

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$.

TABLE 59 (continued)
HIERARCHICAL MULTIPLE REGRESSION ANALYSES
PANEL SAMPLE BLOCK LEVEL

Northwest Neighborhood Federation

Treated vs Untreated Blocks

Dependent Variable	Predictors	Cum R^2	F Change	Beta	F Beta
20. Fear of Property Crime	Pretest	.27	78.72***	.47	67.83***
	Covariates	.29	.83	---	---
	Treatment	.29	.02	.00	.00
21. Perceptions of Neighborhood Crime	Pretest	.34	110.47***	.57	91.98***
	Covariates	.36	.56	---	---
	Treatment	.36	1.56	.07	1.57
22. Optimism About Neighborhood Change	Pretest	.12	29.73***	.30	20.14***
	Covariates	.17	1.53	---	---
	Treatment	.18	3.41	-.12	3.41[1]
23. Likelihood of Moving	Pretest	.27	77.72***	.46	52.18***
	Covariates	.33	2.24*	---	---
	Treatment	.34	.12	-.02	.12

[1] $p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$

TABLE 60
HIERARCHICAL MULTIPLE REGRESSION ANALYSES
INDEPENDENT SAMPLE BLOCK LEVEL

Northwest Neighborhood Federation

Treated vs Untreated Blocks

	Dependent Variable	Predictors	Cum R^2	Beta	F Beta
1.	Exposure to the Treatment	Covariates	.05	---	---
		Time & Group Status	.06	---	---
		Treatment (TxGS)	.07	.12	5.11*
2.	Participation in "Treatment" Meetings	Covariates	.03	---	---
		Time & Group Status	.04	---	---
		Treatment (TxGS)	.07	.24	20.08***
3.	Efficacy of Block Action	Covariates	.01	---	---
		Time & Group Status	.01	---	---
		Treatment (TxGS)	.01	.06	1.06
4.	Efficacy of Collective Crime Prevention Behavior	Covariates	.04	---	---
		Time & Group Status	.06	---	---
		Treatment (TxGS)	.06	-.03	.37
5.	Attribution of Responsibility for Crime Prevention	Covariates	.04	---	---
		Time & Group Status	.05	---	---
		Treatment (TxGS)	.05	-.09	2.78[1]
6.	Home Protection Behavior	Covariates	.05	---	---
		Time & Group Status	.05	---	---
		Treatment (TxGS)	.05	.07	1.72
7.	Efficacy of Individual Target Hardening	Covariates	.02	---	---
		Time & Group Status	.02	---	---
		Treatment (TxGS)	.02	.06	1.02
8.	Street Avoidance Behavior	Covariates	.25	---	---
		Time & Group Status	.25	---	---
		Treatment (TxGS)	.25	-.04	.50
9.	Percentage of Victimization Reported to Police	Covariates	.04	---	---
		Time & Group Status	.05	---	---
		Treatment (TxGS)	.05	-.03	.54

[1] $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$.

[2] Covariates = sex, age, race, education, home ownership, victimization experience, vicarious victimization (knowledge of victims).

TABLE 60 (continued)
HIERARCHICAL MULTIPLE REGRESSION ANALYSES
INDEPENDENT SAMPLE BLOCK LEVEL

Northwest Neighborhood Federation

Treated vs Untreated Blocks

Dependent Variable	Predictors	Cum R^2	Beta	F Beta
10. Asking Neighbors to Watch Your Home	Covariates	.05	---	---
	Time & Group Status	.05	---	---
	Treatment (TxGS)	.05	.00	.16
11. Willingness to Take Action Against Neighborhood Problems	Covariates	.06	---	---
	Time & Group Status	.12	---	---
	Treatment (TxGS)	.12	.06	.19
12. Frequency of Chatting with Neighbors	Covariates	.09	---	---
	Time & Group Status	.10	---	---
	Treatment (TxGS)	.10	.02	.97
13. Number of Block Residents You Know by Name	Covariates	.10	---	---
	Time & Group Status	.11	---	---
	Treatment (TxGS)	.11	-.02	.12
14. Victimization Experience	Covariates	.07	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.08	-.04	.43
15. Vicarious Victimization	Covariates	.09	---	---
	TxG Status	.10	---	---
	Treatment (TxGS)	.10	-.06	1.11
16. Youth Disorder	Covariates	.20	---	---
	Time & Group Status	.21	---	---
	Treatment (TxGS)	.21	-.01	.26
17. Social Control Over Neighborhood Youth	Covariates	.16	---	---
	Time & Group Status	.16	---	---
	Treatment (TxGS)	.16	-.01	.19
18. Neighborhood Deterioration	Covariates	.06	---	---
	Time & Group Status	.06	---	---
	Treatment (TxGS)	.06	.06	1.01
19. Fear of Personal Crime	Covariates	.26	---	---
	Time & Group Status	.26	---	---
	Treatment (TxGS)	.26	-.04	.52

[1] $p<.10$; $^{*}p<.05$; $^{**}p<.01$; $^{***}p<.001$.

TABLE 60 (continued)
HIERARCHICAL MULTIPLE REGRESSION ANALYSES
INDEPENDENT SAMPLE BLOCK LEVEL

Northwest Neighborhood Federation

Treated vs Untreated Blocks

Dependent Variable	Predictors	Cum R^2	Beta	F Beta
20. Fear of Property Crime	Covariates	.07	---	---
	Time & Group Status	.08	---	---
	Treatment (TxGS)	.08	-.01	.16
21. Perceptions of Neighborhood Crime	Covariates	.12	---	---
	Time & Group Status	.12	---	---
	Treatment (TxGS)	.12	.00	.89
22. Optimism About Neighborhood Change	Covariates	.07	---	---
	Time & Group Status	.07	---	---
	Treatment (TxGS)	.07	-.04	.44
23. Likelihood of Moving	Covariates	.16	---	---
	Time & Group Status	.18	---	---
	Treatment (TxGS)	.18	.03	.42

$^{1}p<.10$; $^{*}p<.05$; $^{**}p<.01$; $^{***}p<.001$

implementing a program, but that this intervention produced few of the hypothesized effects.

IV. DISCUSSION

This evaluation generated very little empirical support for the seven main hypotheses that we feel embody the primary objectives of community crime prevention programs in general and these Chicago efforts in particular. Most of the findings were nonsignificant and another group of findings were significant in the direction opposite to the hypotheses. The basic question we are left with is -- why did this happen? How should these results be interpreted? Did these programs fail or is there a better way to interpret the findings?

When evaluators do not observe program effects that are expected, there are many possible reasons for this failure. As Suchman (1969) and Weiss (1972) have noted, there are two general categories of reasons: either (a) the program did not set in motion the "causal process" that would produce the desired goals (referred to as "program failure") or (b) the program activated the supposed "causal process" but this process did not produce the desired effects (referred to as "theory failure"). In the context of interpreting evaluation results, there is a third general category of reasons for observed failure, namely measurement or research problems that cause the evaluators to overlook significant program effects. In an attempt to shed some light on the present evaluation results, we will discuss these three categories of reasons as they apply to the circumstances in Chicago.

Program Failure. One explanation for nonsignificant findings is "program failure" or sometimes referred to as "implementation failure." In the field of evaluation research, we continually experience this problem. Often, evaluators are unable to test the underlying theory of impact because the program was not implemented as

planned or never implemented at all. The theory specifies that certain causal processes must be activated before the hypothesized effects will be observed (e.g., attendance at block watch meetings, discusssions of local crime issues, and planning crime prevention activities are necessary to increase preventive behaviors, reduce fear of crime, etc.).

Although we have reported some evidence of successful program implementation in the current evaluation, nevertheless, we must ask ourselves whether these efforts were adequate to test the "theory" of community crime prevention impact? Looking at all five neighborhoods, we would have to conclude that the level of implementation success was marginal, at best. Our field work indicates that only one of the five organizations used the outside funding primarily for organizing block watches. (In defense of the other organizations, the reader should know that these groups were strongly encouraged, but not required, to follow the block watch model). The question of <u>why</u> community organizations in this case (and in other cases across the country) have failed to seriously adopt this model is an important policy question, and one that is addressed in Volume Two of this final report.

Aside from the intent of community organizations, one of the evaluation issues is how much "treatment" is needed to show an impact? What is the minimum "dosage" of the treatment that is necessary? Increasing community participation in crime prevention meetings from 12 to 16 percent (as was observed in the treated neighborhoods) hardly seems sufficient to produce community-wide effects. Holding several meetings over a 12-month period, with few additional organized social

activities, would appear to be a very weak treatment. Why should we expect such limited activities to produce long-term changes in residents' perceptions and behavior? To the extent that the theory sets the parameters for the treatment, this problem is indicative of theory failure rather than program failure, as we discuss below.

Although the argument can be made that the treatment dosage was quite weak for most comparisons, the NNF case study (with treated and untreated blocks) contained a much stronger implementation, and therefore, offered a stronger test of the hypotheses. Even though the NNF organizers followed the suggested models, very few of the expected outcomes occurred. Hence, these particular data encourage us to conclude that the problem lies not with the program but rather the theory itself. Also, the failure to replicate the unfavorable neighborhood changes in the NNF case study suggests that these counter-hypothesis findings are not untoward effects of block watch, per se, even though they may be a byproduct of other components of the treatment in these neighborhoods (e.g., neighborhood-wide meetings).

Measurement Failure. Another possible explanation for the observed findings is that the evaluation research simply failed to detect the real changes that occurred or falsely detected "changes" that run counter to the hypotheses. However, for a number of reasons, we do not feel that this a plausible or defensible explanation for the results. It would be inappropriate here for us to assess all aspects of the research design and measurement in terms of validity issues, but several comments about possible "measurement failure" are relevant to this discussion.

Using statistical significance criteria (i.e. probability less than .05) helped us immensely in guarding against the "Type I" error of falsely detecting changes. In the present evaluation a large number of comparisons were performed, and hence, we increase our chances of making Type I errors. However, the frequency and pattern of significant "unexpected" findings simply cannot be explained as a statistical artifact when our chances of making such errors remain much smaller than the number of observed differences.

There are several other categories of possible research problems that might be proposed as explanations for the results. These include weaknesses in the evaluation design and measurement problems. In terms of research design, quasi-experimental designs never satisfactorily answer the question of causality because of the possible nonequivalence of the treatment and control groups. At this point, suffice it to say that the Pretest-Posttest Control Group Design used in this evaluation controls for many of the known threats to internal validity (see Campbell & Stanley, 1963; Cook & Campbell, 1979). Furthermore, through statistical controls, we have tested and dismissed some basic rival hypotheses.

In terms of possible measurement problems, there are several points we wish to emphasize. Evaluation measurement can be either weak or inappropriate. First, we have a fair degree of confidence in the reliability and construct validity of the measures employed, and therefore, do not view measurement weakness as a plausible rival hypothesis. Many of the scales and items used have been tested and validated as part of this project and through previous research. Second, we feel the measures used, to the extent that they correspond

to the theoretical constructs of interest, are very appropriate for testing the "community crime prevention hypothesis." However, we are not suggesting that the stated hypotheses or theoretical underpinnings are the most appropriate for understanding the phenomena of interest (as discussed below under "theory failure") nor are we suggesting that these measures are the most appropriate for assessing the primary objectives of these community organizations (as opposed to their crime prevention/block watch objectives). For example, the organizing activities of these community groups may be highly effective at increasing group membership, changing public attitudes about the organization, improving cohesion among group members, influencing residents' voting behavior, affecting city policies, or changing a host of other factors. However, these outcomes do not reflect the theories and stated organizational objectives with regard to community crime prevention impact.

Theory Failure. The third possible explanation for the nonsignificant and unexpectedly significant findings is theory failure. If the programs were properly implemented (i.e., the hypothesized causal process was set in motion), and the expected effects did not occur, then we would need to rethink the theoretical basis for our expectations. The available data, although not conclusive, have caused us to re-examine the theoretical underpinning of our current thinking about community crime prevention programs. Specifically, the counter-hypothesis findings at the neighborhood level and the nonsignificant findings at the block level raise questions about whether the theory of impact is defective in specifying one or more of the following: (a) the amount or dosage

treatment needed to produce the desired effects; (b) the content of the treatment needed to produce these effects or (c) the content of the effects themselves (i.e., the appropriate outcome measures).

First, there is the question of dosage. We must ask ourselves, in all seriousness, whether a one hour meeting that occurs only once a month or every few months, and is attended by only a few local residents, should be expected to change the quality of life in the neighborhood. We have tried to spell out some of the mechanisms by which this might occur (in our hypotheses), but the real question is whether the treatment -- even if appropriate -- is strong enough to activate the causal process delineated. We do not believe it is.

Given a weak treatment, it is easy to ask "what if" questions. What if we were able to increase the dosage of the treatment -- would it make a difference? It might, but for policy reasons, we must keep in mind what is practical and realistic in most neighborhood settings where voluntary organizations have limited resources and multiple objectives that reach far beyond crime prevention. We have witnessed a recent surge of expert technical assistance in other community crime prevention projects to insure a strong implementation, but if successful, the external validity (replicability) of these findings in more natural settings is questionable. In any event, the point we wish to make is not about program failure, but rather about theory failure. We are suggesting that current theorizing about community crime prevention may be flawed for presuming that such a small dosage of the treatment (if properly implemented) is adequate to produce the desired effects.

Issues about the quantity of the treatment concern us less than those pertaining to the nature of the treatment. At the heart of any solid theoretical statement is the specification of causal, intervening, and outcome variables, as well as the relationships among them. Although a complete impact theory for community crime prevention has yet to be developed, one can question whether the current thinking (as represented by our hypotheses) is defensible. For example, what reasons do we have for thinking that blockwatch activities will reduce fear of crime or improve perceptions of the neighborhood? One could easily predict just the opposite outcome given that the intervention involves citizens coming together to discuss the crime problem in their neighborhood. Oftentimes such discussions take the form of exchanging victimization stories or validating each others assessment of the severity of the local crime problem. Thus, one could easily imagine how these discussions could influence not only fear of crime, but also perceptions of the crime problem and the neighborhood as a whole.

As another example, what reasons do we have for thinking that blockwatch activities will help to empower citizens by enhancing their feelings of individual or collective efficacy? What about the possibility that small group discussions serve as a "consciousness raising" process whereby citizens end up feeling more (rather than less) helpless in the face of uncontrollable political and social realities? For example, citizens might come to recognize that residential transition is beyond their control; or that the police department has been successful at ignoring their pressure; or that

criminal victimization is sometimes a random event that can strike anyone regardless of his/her precautionary measures.

Finally, what good reasons do we have for thinking that crime prevention meetings will cause residents to arrive at new conclusions and change attitudes about crime prevention behavior? By what process or influence strategy will citizens come to accept greater responsibility for their neighborhood or believe that crime prevention really works? In the absence of a persuasive change agent, is it not just as reasonable to think that small group processes will reinforce existing stereotypes about police and citizen roles and strengthen existing beliefs about the effectiveness of various crime reduction strategies?

The above examples are provided simply to illustrate that the current state of theorizing in community crime prevention is still rather primitive. Greater specificity in prediction could be obtained through continued observations of the actual social processes involved and through greater utilization of existing research and theory in relevant disciplines. Over the past few decades, for example, there has been extensive social psychological research on small group processes (e.g., conformity, group conflict, leadership) and individual-social processes (e.g., social comparison, social influence, social judgement, coping with stress) that could be applied to the topic of community crime prevention, and may help to clarify some of the underlying mechanisms that are operating to produce the observed effects.

One aspect of the theoretical problem facing community crime prevention scholars is the need for a clearer specification of who

will be affected by these interventions and under what conditions? How widespread is the impact of these programs? Is it reasonable to expect block watch programs to have "collective benefits" for neighborhood residents who have never heard of the concept and never become involved? Should we expect neighborhood-wide effects, block effects or only individual participator effects? In reality, the effects of these programs may not even extend to all participators, but to only certain types of participators (e.g., "joiners," civic-minded citizens, victims, property owners, fearful residents). If the model suggests that the impact eventually spreads to the entire community, then how long do we have to wait before we can expect these effects? Is it reasonable to think that the desired outcomes could be achieved within one year? Given the possibility of activating different components of the causal process at different points in time (e.g. immediate, intermediate, and long-range goals), what are these time parameters?

In sum, there is a clear need for more research in this field to clarify the processes and impact of community crime prevention programs. The results force us to seriously address the possibility of both theory failure and program failure in this field. We have suggested how each could be deficient. Before we implement or evaluate more community crime prevention programs, we should rethink the principles and expectations that guide our actions. The problem may lie more in our way of thinking about community crime prevention than in the actions of local community organizations.

FOOTNOTES

1. The Ford Foundation and the National Institute of Justice, U.S. Department of Justice, have funded important programs and evaluations in the past two years. In addition to the current Chicago project, Ford has supported the Eisenhower Neighborhood Anti-Crime Self-Help Program in the ten U.S. cities and Northwestern's evaluation of this program via the Eisenhower Foundation. NIJ has funded the Police Foundation's evaluation of the Fear Reduction Program in Houston and Newark. Both of these projects are still ongoing.

2. Our original design called for a "spatial displacement control group" that would be used to test for displacement of crime from the treated area into an untreated bordering area. However, the Chicago police data were shown to be so inaccurate in a media investigative report and subsequent audits that we decided to drop this idea.

3. Nine comparisons are shown in all panel tables because we have included results from Auburn-Gresham, a community group whose program and funding were discontinued in the middle of the evaluation. We collected panel data only at time two and only in the "treated" neighborhoods. The results are not discussed in relationship to other programs, but rather are presented to give the reader some idea of neighborhood change in the absence of a complete program. Auburn-Gresham was the only predominantly black neighborhood in our sample.

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APPENDIX 1

Respondent Selection Sheets

Chicago Crime Prevention
Evaluation Survey

Respondent Selection

Hello, is this _____? My name is _____.
I'm calling from Northwestern University. We are conducting a survey for the Center for Urban Affairs to assess the quality of life in Chicago neighborhoods.

In order to randomly select which adult I can speak to in your household, would you please tell me how many adults live here?

_____ *# of adults 19 yrs. or older* 24-25

(IF "one", ask for that adult and start interview, repeating intro if necessary)

I don't need any names but would you also tell me their relationship to each other?

Enumerate adult members of household (e.g., "husband", "wife", "husband's father", etc.):

(1) _____
(2) _____
(3) _____
(4) _____
(5) _____
(6) _____

*** Interview HUSBAND in primary couple ***

(CHECK RESPONDENT)

If primary couple not clear, pick economic dominant by asking:

"Who provides the major share of financial support for the family?"

(Ask for that adult and start interview, repeating intro if necessary)

(If not home at this time, arrange time for call-back: _____*)*

1984 Chicago Panel Selection Sheet - Wave II

Hello, is this _____? My name is _____, and I'm calling from Northwestern University. About a year ago (March, 1982) we conducted an interview for the Center for Urban Affairs

with a _____ at this number.

May I please speak with (her) (him)?

If already speaking with identified respondent proceed:

The information you gave us last year was a big help to the Center for Urban Affairs in understanding the concerns of Chicago residents like yourself. We are calling you back now to help the Center find out some things about the quality of life in Chicago neighborhoods during the past year.

Proceed to Q1

If not already speaking with identified respondent repeat introduction when (she) (he) comes to the telephone:

Hello, my name is _____, and I'm calling from Northwestern University. About a year ago we conducted an interview with you for the Center for Urban Affairs. The information you gave us last year was a big help to the Center in understanding the concerns of residents like yourself. We are calling you back now to help the Center find out some things about the quality of life in Chicago neighborhoods during the past year.

Proceed to Q1

If identified respondent is unavailable at this time, determine when you can call back to reach (her) (him):

_____

_____

_____

APPENDIX 2

Telephone Survey

Chicago Crime Prevention Evaluation - Wave II, 1984

CARD 01

Time Interview Began _____ AM / PM

** First of all, I have a few questions about the neighborhood where you live.

1. How many years have you lived in your present neighborhood? 26-27

_____ years

Don't know. 98

2. In the past year or so, has your neighborhood become a better place to live, has it gotten worse, or has it stayed about the same? 28

Better 3

Worse 1

About the Same 2

Don't Know 8

3. Do you really feel a part of your neighborhood or do you think of it more as just a place to live? 29

Feel part of your neighborhood 2

Just a place to live 1

Used to feel part of neighborhood. . . .7

Don't know 8

4. What kind of neighborhood would you say you live in -- is it mostly one where people help each other or one where people go their own ways? 30

People help each other 1

People go their own ways 2

Somewhere in between 7

Don't know 8

5. How hard is it to tell a stranger in your neighborhood from somebody who lives there? Is it pretty hard or pretty easy most of the time? 31

Pretty hard 1

Pretty easy. 2

Don't know 8

Now it's pretty hard . . .7

CARD 01

6. In general, how similar would you say most adults on your block are to you? Would you say they are... 32

Very similar to you,. 4
Pretty similar, ... 3
Not very similar, or. 2
Not at all similar to you?. . . . 1
Don't know 8

7. How many of the people on your block do you know by name -- all of them, most of them, some, hardly any, or none? 33

All of them 5
Most of them 4
Some 3
Hardly any 2
None 1
Don't know 8

8. On the whole, how do you feel about living in your neighborhood? Would you say that you... 34

Like living there, or 3
Dislike living there?. 1
Don't have feelings one way or the other . . . 2
Don't know 8

9. In general, would you describe the crime rate in your neighborhood as... 35

Very high,. 5
Higher than average,. 4
About average,. 3
Lower than average, or 2
Very low?. 1
Don't know 8

CARD 01

10. How often are people robbed of their money, beaten up, or assaulted on the streets in your neighborhood. Does this happen ... 36

Very often,. 4
Quite often, ...3
Not too often, or 2
Almost never?. 1
Don't know 8

11. How about people breaking in or sneaking into homes to steal things. Does this happen... 37

Very often,. 4
Quite often, ...3
Not too often, or. 2
Almost never?. 1
Don't know 8

12. In the past year or so, has the amount of crime in your neighborhood increased, decreased, or stayed about the same? 38

Increased 3
Decreased 1
Stayed about the same . . .2
Don't Know 8

13. Considering crime in your neighborhood as a whole, how much of it would you say is committed by youths who live in the neighborhood? Would you say... 39

Almost all of it, ...5
More than half,. 4
About half,. 3
Less than half, or 2
Almost none of it?. 1
Don't know 8

CARD 0

14. Now, I'd like your opinion on how helpful some things are that people do to prevent crime.

	Very Helpful,	Somewhat Helpful, or	Not very Helpful?	*Don't Know*	
a. How helpful are alarm systems, window bars, or special locks in preventing crime? Are they...	3	2	1	*8*	40
b. How helpful are youth programs to help kids stay out of trouble?	3	2	1	*8*	41
c. How helpful is marking personal property with an engraving tool? Is this...	3	2	1	*8*	42
d. How helpful are block watches where neighbors watch each others homes?	3	2	1	*8*	43
e. What about police patrolling the neighborhoods? Is this...	3	2	1	*8*	44
f. What about citizens patrolling their own neighborhds? Is this...	3	2	1	*8*	45

15. Do you think the city should provide funds to community groups that sponsor neighborhood crime prevention activities?

Yes 1 4

No. 2

Don't know. *8*

** The next few questions are about your neighborhood and friends. CARD 01

16. How likely is it that you will move out of your neighborhood in the next two years? Would you say you will...

Definitely move,. 5 47
Probably move,. 4
Probably not move, or 2
Definitely not move?. 1
Fifty-fifty chance 3
Would like to move, but can't. . . .6
Don't know 8

17a. All things considered, what do you think your neighborhood will be like two years from now? Will it be a <u>better</u> place to live, will it have <u>gotten worse</u>, or will it be <u>about the same</u> as it is now?

Better 3 48
Worse 1
About the same . . . 2 *(Skip to Q. 18)*
Don't know . . . . . 8 *(Skip to Q. 18)*

b. Why do you think it might get (better) (worse)?

_____

_____

_____

18. About how often do you spend a social evening with one of your neighbors? Do you do this...

Once a week or more,. 4 49
A few times a month, ... 3
Less than once a month, or 2
Never?. 1
Don't know 8

19. How often do you chat with your neighbors when you run into them on the street? Do you do this... 50

Always,. 4
Quite often,. 3
Sometimes, or 2
Never?. 1
Don't know *8*

** Now I'd like to ask you some questions about getting involved in voluntary groups.

20. Would you generally describe yourself as a "joiner," that is, someone who likes to join together with groups of people for some specific purposes? 51

Yes 1
No 2
Don't know *8*

21a. Are you currently a member of...

	Yes	No	*Don't Know*	
(1) A group at your church or synagogue <u>other than</u> a prayer or study group?	1	2	*8*	52
(2) A PTA or local school council?	1	2	*8*	53
(3) A block group or neighborhood-based community group?	1	2	*8*	54
(4) Any other kind of group?	1	2	*8*	55

(IF ALL NOs, SKIP TO Q. 22)

b. Approximately how many hours <u>per month</u> do you spend in activities related to this/these groups(s)? Would you say... 56

None 0
1-4 hours 1
5-10 hours 2
More than 10 hours . . .3
Don't know *8*

22. *(Ask a 1-5 before asking b)*

	a. Have you heard or read about any of the following kinds of activities taking place in your neighborhood in the past year or so?			*(If yes to a)* b. Were you given an opportunity to attend or take part in _____? Did anyone ask you, or did you see a notice or poster?			*(If yes to b, ask all relevant c questions before turning page)* c. Were you able to attend or take part in this?		
	No	Yes		No	Yes		Yes	No	
(1) a neighborhood crime prevention meeting?	1	2	57	1	2	→ 62	(1) 1 *(GO TO PINK)*	2	63
(2) a blockwatch program on your block? *(NOT THE SAME AS (1) ABOVE)*	1	2	58	1	2	→ 64	(2) 1 *(GO TO BLUE)*	2	65
(3) a Beat Representative program?	1	2	59	1	2	→ 66	(3) 1	2	67
4) a WhistleSTOP program	1	2	60	1	2	→ 68	(4) 1	2	69
(5) any other crime prevention program or activity in your neighborhood? *(SPECIFY: _____)*	1	2	61	1	2	→ 70	(5) 1	2	71

(NOW FOLLOW UP ALL "YES'S")

(IF BOTH (1) AND (2) ARE "NO," SKIP TO WHITE)

CARD 01

** Now, I'd like to ask you some questions about the meeting you attended.

23a. How many meetings have you attended? _____ Meetings 72-73

Don't Know 98

b. On the average, how many other persons participated (each time)?

_____Persons 74-76

Don't know 998

c. Who sponsored or organized (this)(these) meetings? *(IF NEIGHBORHOOD ORGANIZATION MENTIONED, ASK FOR NAME)*_____

d. What did you do as part of (this)(these) meetings?

_____

_____

e. Since participating in this, is there anything different you now do or have done for crime prevention reasons?

Yes 1 77

No . . . . . .0 *(SKIP TO Q. 23g)*

Don't know . . . . . .7 (SKIP TO Q. 23g)

f. What have you done?_____

_____

g. Are you still attending neighborhood crime prevention meetings?

Yes 1 78

No 2

h. Approximately how many hours do (did) you spend per month attending these meetings? _____ hours 79-80

Don't Know. . . 88

(IF DID NOT ATTEND BLOCKWATCH MEETING, IN ADDITION TO NEIGHBORHOOD CP MEETING, SKIP TO WHITE)

BEGIN CARD 02

** Now, I'd like to ask you some questions about the block watch program.

24a. How many times have you done this?

_____Times 6-7

Don't Know 98

b. On the average, how many other persons participated (each time)?

_____Persons 8-10

Don't know 998

c. Who sponsored or organized this activity or program? (*IF NEIGHBORHOOD ORGANIZATION MENTIONED, ASK FOR NAME*_____

d. What did you do as part of this activity or program?

_____

_____

e. Since participating in this, is there anything different you now do or have done for crime prevention reasons?

Yes 1 11

No . . . . . .0 *(SKIP TO Q. 24g)*

Don't know . . . . . .7 (SKIP TO Q. 24g)

f. What have you done?_____

_____

g. Are you still actively involved in this activity or program?

Yes 1 12

No 2

h. Approximately how many hours do (did) you spend per month on this (activity) (program)?

None 0 13

1-4 hours 1

5-10 hours 2

More than 10 hours 3

CARD 02

i. How many of the people on your block are (were) actively involved in the block watch program? 14

Almost all of them, ... 5
More than half, ... 4
About half, ... 3
Less than half, or. 2
Almost none of them?. 1
Don't know. 8

j. Based on what you know right now, how likely is it that your block group will stay together and have regular meetings one year from now? Is it very likely the group will be strong one year from now, somewhat likely, somewhat unlikely, or very unlikely? 15

Very likely 4
Somewhat likely 3
Somewhat unlikely 2
Very unlikely 1
Don't know 8
Already Inactive 7

CARD 02

25. Now, I'd like to ask you about some things you may do to protect yourself. In your neighborhood...

	Always,	Most of the time,	Some times, or	Never?	*Don't Know*	*Refused*	*Don't Go Out*	
a. How often do you keep a look out for suspicious people? Do you do this...	4	3	2	1	*7*	*8*	*9*	16
b. How often do you avoid being outside alone at night because of crime.	4	3	2	1	*7*	*8*	*9*	17
c. How often do you avoid walking near certain types of strangers?	4	3	2	1	*7*	*8*	*9*	18
d. How often do you take something with you at night that could be used for protection from crime-- like a dog, whistle, or a weapon?	4	3	2	1	*7*	*8*	*9*	19
When you are away from home for at least a couple of days...								
e. How often do you have a neighbor watch your home? Do you do this...	4	3	2	1	*7*	*8*	*9*	20

26. Now I'd like you to tell me whether each of the following is a big problem, some problem, or almost no problem in your neighborhood.

	Big Problem,	Some Problem, or	Almost No Problem?	*Don't Know*	
a. Groups of teenagers hanging out on the streets? Is this a ...	3	2	1	8	21
b. People selling illegal drugs? Is this a ...	3	2	1	8	22
c. Vandalism (like kids breaking windows or writing on walls or things like that)? Is this a ...	3	2	1	8	23
d. Noisy neighbors (people playing loud music or having late parties)?	3	2	1	8	24
e. Gang activity? Is this ...	3	2	1	8	25
f. Abandoned buildings or vehicles? Is this a ...	3	2	1	8	26
g. Garbage or litter on the streets and sidewalks?	3	2	1	8	27
h. Certain kinds of people moving into the neighborhood? Is this a...	3	2	1	8	28
i. Landlords who don't care about what happens to the neighborhood?	3	2	1	8	29
j. People who say insulting things or bother people as they walk down the street?	3	2	1	8	30

(ASK Q. 27 FOR EACH "BIG" PROBLEM
IF NO "BIG" PROBLEMS, SKIP TO Q. 23)

27. For the big problems in your neighborhood, I'd like to ask if you were able to take any action. First, you said that _____ was a big problem in your neighborhood.

	Have you taken any action to try to solve this problem?			If Yes	Have you dealt directly with the person or persons responsible?				Have you gotten together with the neighbors to try to solve this problem?				Have you talked to police/public officials about this problem?			
	DK	No	Yes		Yes	No	*DK*		Yes	No	*DK*		Yes	No	*DK*	
a.	8	2	1	31 →	1	2	8	32	1	2	8	33	1	2	8	34
b.	8	2	1	35 →	1	2	8	36	1	2	8	37	1	2	8	38
c.	8	2	1	39 →	1	2	8	40	1	2	8	41	1	2	8	42
d.	8	2	1	43 →	1	2	8	44	1	2	8	45	1	2	8	46
e.	8	2	1	47 →	1	2	8	48	1	2	8	49	1	2	8	50
f.	8	2	1	51 →	1	2	8	52	1	2	8	53	1	2	8	54
g.	8	2	1	55 →	1	2	8	56	1	2	8	57	1	2	8	58
h.	8	2	1	59 →	1	2	8	60	1	2	8	61	1	2	8	62
i.	8	2	1	63 →	1	2	8	64	1	2	8	65	1	2	8	66
j.	8	2	1	67 →	1	2	8	68	1	2	8	69	1	2	8	70

2 80

BEGIN CARD 03

** Now, I have a few questions about walking in your neighborhood.

28a. In the summer months, how often do you walk around in your neighborhood just for exercise or pleasure? Do you walk....

Every day,.	5		6
Several times a week,.	4	*(SKIP to Q.29)*	
Once a week,.	3		
Less than once a week, or . . .	2		
Never?.	1		
Don't know	*8*	*(SKIP to Q. 29)*	

b. Why is that?_____

29. How safe do you feel or would you feel being out alone in your neighborhood at night?

Very safe, ...	4	7
Somewhat safe,.	3	
Somewhat unsafe, or	2	
Very unsafe?.	1	
Don't know	*8*	

30. When you are walking alone in your neighborhood at night, how concerned are you that someone will harm you or take something from you by force or threat? Are you...

Not at all concerned,.	1	8
Somewhat concerned,.	2	
Quite concerned, or	3	
Very concerned?.	4	
Don't go out at night	*7*	
Don't know	*8*	

CARD 03

31. How concerned are you that someone will break into your home to steal something when no one is home? Are you ...

Not at all concerned,. 1 9

Somewhat concerned,. 2

Quite concerned, or 3

Very concerned?. 4

Don't know *8*

32. How likely do you think it is that someone will break into your home in the next couple of years? Would you say it is...

Very likely,. 4 10

Somewhat likely,. 3

Somewhat unlikely, or 2

Very unlikely?. 1

Don't know. *8*

	Yes	No	*Don't Know*	*Refused*	
33a. Have you installed an alarm system, window bars, or special locks to help prevent break-ins at your home?	1	2	*7*	*8*	11
b. Have you engraved any of your valuables to help recover them in case they are stolen?	1	2	*7*	*8*	12
c. Have you had a Home Security Check, where someone made recommendations about new locks and other types of home security?	1	2	*7*	*8*	13

34. If you found out that some kids in the neighborhood were stealing things, would you first tend to call their parents or call the police or just ignore it?

Call their parents 3	14
Call the police 2	
Ignore it 1	
Call both parents and police. . 6	
Other 7	
Don't know 8	

** Now I have a few questions about your experiences with television:

35. On the average weekday, how much time do you usually spend watching television from the time you get up until you go to sleep?

_____ Hours _____ Minutes 15-18

Don't know 8888
None . . . . . 0000 (SKIP TO Q 38)

36. How often do you watch police, crime, or detective programs on television? Do you watch them very often, pretty often, not too often, or almost never?

Very often. 4	19
Quite often 3	
Not too often 2	
Almost never 1	
Don't know 8	

37. When you come across news stories about crime on television, do you usually pay close attention to them, some attention, or not much attention at all?

Close attention 3	20
Some attention. 2	
Not much attention at all . . . 1	
Don't know. 8	

CARD 03

** Now I have a few questions about the police.

38. About how often do you see Chicago police officers in your neighborhood patrolling the streets in a car or on a motorcycle?

Several times a day 6	21
Almost every day 5	
Several times a week 4	
Once a week 3	
A few times a month 2	
Almost never 1	
Don't know *8*	

39. About how often do you see Chicago police officers on foot in your neighborhood?

Several times a day 6	22
Almost every day 5	
Several times a week 4	
Once a week 3	
A few times a month 2	
Almost never 1	
Don't know *8*	

40. If you called the Chicago police for help, how long do you think it would take for them to come?

_____ Hours _____ Minutes	23-26
Don't know *8888*	
Wouldn't come *9999*	

41. Overall, how satisfied are you with the police service in your neighborhood? Are you...

Very satisfied,. 4	27
Somewhat satisfied,. 3	
Somewhat dissatisfied, or. . . . 2	
Very dissatisfied? 1	
Don't know *8*	

42a. As far as you know, have there been any changes in the police service to your neighborhood in the last year or so? (e.g. new programs, or less police officers).

Yes 2 28
No . . . . . .1 *(SKIP to Q. 43a)*
Don't know . . . . . .8 (SKIP to Q. 43a)

b. Please tell me about these changes.

_____

_____

_____

43a. Have you had the occasion to call the Chicago Police Department in the last year or so?

Yes 2 29
No . . . . . .1 *(SKIP TO Q. 46)*
Don't know . . . . . .8 (SKIP TO Q. 46)

b. How many times did you call the police during that time period?

_____ times 30-31
Don't know 88

44. In the past year, did you call the Chicago Police to report a crime that happened, or was about to happen, to one of your neighbors?

Yes 2 32
No. 1
Don't know. 8

45. In the past year, did you call the police to report any strangers in your neighborhood who were hanging around, or acting suspicious?

Yes 2 33
No. 1
Don't know. 8

CARD 03

46. How much crime would you say there is on the few blocks right around your home? Would you say there is ...

Quite a lot, ...4	34
Some, ... 3	
Only a little, or. 2	
None? 1	
Don't know *8*	

47. When it comes to the prevention of crime in a neighborhood, do you feel that it's more the responsibility of the residents, or more the responsibility of the police?

Residents 1	35
Police. 3	
Both. 2	
Don't know. 8	

** Now I have a few questions about children.

48. How many children (of your own) do you have, under 19, living at home with you? This could include adopted children, foster children, and children from a previous marriage.

_____Children *(If None, SKIP to GREEN)*	36-37
Refused . . . . . *77 (SKIP to GREEN)*	

49. (Is this child)(Are any of these children) 5 years of age or older:

Yes 1	38
No . . . . . 2 *(SKIP to GREEN)*	
Refused . . . . . .*7 (SKIP to GREEN)*	

50. The problems and challenges of raising children these days are things that concern many parents. I'd like you to tell me whether each of the following things is a big problem, some problem, or no problem with regard to your (child) (oldest child at home).

	Big Problem,	Some Problem,	or No Problem?	*Don't Know*	
a. watching too much TV. Is this a...	3	2	1	*8*	39
b. spending time with kids you don't know. Is this a...	3	2	1	*8*	40
c. finishing homework...	3	2	1	*8*	41
d. hanging around with the wrong kids.	3	2	1	*8*	42
e. doing things that might get them in trouble with with the police.	3	2	1	*8*	43
f. not minding you.	3	2	1	*8*	44
g. getting passing grades in school.	3	2	1	*8*	45

(IF ALL "1s" or "8s", SKIP TO Q. 52)

51. Would you say these are problems you are trying to change or problems you've decided to live with because they're too hard to change? 46

Trying to change 1
Decided to live with 2
Don't know *8*

52. On the average weekday, how much time does your (child) (oldest child at home) usually spend watching television from morning to night? 47-49

_____ Hours _____ Minutes
Don't Know *888*

CARD 03

53. Now I'm going to read you a few statements that may be true about children who live in your neighborhood. I'd like you to tell me whether each of these things is a big problem, some problem, or no problem with regard to the children in your neighborhood.

What about...	Big Problem	Some Problem, or	No Problem?	*Don't Know*	
a. Doing things that might get them in trouble with the police. Is this a...	3	2	1	*8*	50
b. Not getting enough supervision. Is this a...	3	2	1	*8*	51
c. Not minding their parents.	3	2	1	*8*	52
d. Not respecting other people and their belongings.	3	2	1	*8*	53

54. Now I'm going to read a few statements that may apply to you and your neighbors. Please tell me if each statement is mostly true or mostly false, looking at it from your viewpoint.

	Mostly True, or	Mostly False?	*Don't Know*	
a. The people on my block work together to solve problems. Is this ...	1	2	*8*	54
b. There is very little my neighbors and I can do to change things around here. Is this...	1	2	*8*	55
c. People like me don't have any say about what the city government does...	1	2	*8*	56
d. If we take action, my neighbors and I can make a big difference in the crime rate around here...	1	2	*8*	57
e. In many cases, calling the police to report something you saw happen in the neighborhood is not worth the hassle of getting yourself involved...	1	2	*8*	58
f. If a few people like me on this block got together, we could get the city to make some improvements in this neighborhood...	1	2	*8*	59

** Now, I'd like to ask you about some things that may have happened to you in your neighborhood during the past year or so.

55. During the past year, in the neighborhood where you live now,...	*IF YES, ASK Q. 56* No	Yes		56. Did this come to the attention of the police? *(IF MORE THAN ONE INCIDENT, USE MOST RECENT INCIDENT)* No	Yes	*Don't Know*	
a. Did anyone enter, or try to enter, your home who didn't belong there, to steal something?	1	2	→ 60	1	2	8	61
b. Did anyone steal something from inside your home in the past year who had permission to be there, such as a repairman, delivery man, or neighbor?	1	2	→ 62	1	2	8	63
c. Have you had anything taken that you left outside your home? *(Not motor vehicle)*	1	2	→ 64	1	2	8	65
d. Did anyone deliberately damage or deface the building you live in, such as writing on the walls, breaking windows or tearing things up outside?	1	2	→ 66	1	2	8	67
e. Have you or anyone in your household owned a car or truck in the past year?	1	2	68				

(SKIP TO Q 55i)

CARD 03

During the past year, in the neighborhood where you live now...

	No	Yes		Did this come to the attention of the police? (IF MORE THAN ONE, USE MOST RECENT INCIDENT) No	Yes	*Don't Know*	
f. Did any one steal that (car/truck), or try to steal it?	1	2	→ 69	1	2	*8*	70
g. Did anyone take anything from that (car/truck), or try to steal any parts from it?	1	2	→ 71	1	2	*8*	72
h. Did anyone deliberately damage that (car/truck) or vandalize it, such as scratching it up, breaking windows, or slashing tires?	1	2	→ 73	1	2	*8*	74
i. Did anyone take or try to take something directly from you by using force or threatening you with harm?	1	2	→ 75	1	2	*8*	76
j. Has anyone picked your pocket or taken a bag or package directly from you without using force or threatening you?	1	2	→ 77	1	2	*8*	78

3 8

BEGIN CARD 04

During the past year, in the neighborhood where you live now...	No	Yes		Did this come to the attention of the police? (IF MORE THAN ONE, USE MOST RECENT INCIDENT) No	Yes	*Don't Know*	
k. Have you received any threatening or obscene phone calls?	1	2	6 →	1	2	*8*	7
l. Has anyone physically attacked you or has anyone threatened or tried to hurt you even though they did not actually hurt you?	1	2	8 →	1	2	*8*	9
(ASK FEMALES ONLY)							
m. Has anyone tried to sexually attack you ?	1	2	10 →	1	2	8	11

57a. Do you personally know of anyone, other than yourself, whose property has been stolen, destroyed, or damaged, such as breaking into their home, slashing the tires on their car, or stealing their bicycle? 12

Yes 1
No . . . . . 2 *(SKIP to Q. 58a)*
Don't know . . . . . *8* *(SKIP to Q. 58a)*

b. Did this happen in your present neighborhood, elsewhere in Chicago, or outside of Chicago?

	1st	2nd	
Present neighborhood.	1	1	13
Elsewhere in Chicago.	2	2	14
Outside of Chicago.	3	3	
Don't know	*8*	*8*	

CARD 04

58a. Do you personally know of anyone, other than yourself, who has been robbed or physically attacked, or had someone threaten them or try to harm them during the past year? 15

Yes	1	
No	2	*(SKIP to Q. 59)*
Don't know	*8*	*(SKIP to Q. 59)*

b. Did this happen in your present neighborhood, elsewhere in Chicago, or outside of Chicago?

	1st	2nd	
Present neighborhood	1	1	16
Elsewhere in Chicago	2	2	17
Outside of Chicago	3	3	
Don't know	*8*	*8*	

** Now, I'd like to finish by asking you some background questions that we need to analyze our survey results.

59. Do you own your home or do you rent it? 18

Own	1
Rent	2
Don't Know	*8*
Refused	*7*

60. Is your residence a single family house, a duplex, a rowhouse, or a bigger building? 19

Single family	1
Duplex/two flat	2
Rowhouse/townhouse	3
Bigger building	4
Other (specify)_____	5
Refused	*7*

CARD 04

61. In what year were you born?

_____year 20-22

Don't know *998*

Refused. *997*

62. What was the highest grade or year of school you completed?

None 00 23-24

Elementary. . .01 02 03 04 05 06 07 08

High School 09 10 11 12

Some College 13

College graduate (Bachelors) 14

Some graduate school 15

Masters degree 16

Doctoral degree. 17

Don't know *98*

Refused. *97*

63. Are you presently working full or part-time, keeping house, retired, unemployed, or something else?

Working full-time 1 25

Working part-time 2

Keeping house 3

In School 4

Retired 5

Unemployed 6

Disabled 7

Other (Specify):_____). 8

Refused *9*

CARD 0

64a. Are you currently...

Married,. . . . . .1 — 26
Living with someone as married, .2
Widowed, ... 3
Divorced,. 4
Separated, or 5
Never been married?. 6
Don't know 8
Refused. 7

SKIP to Q.65a

b. Is your (husband) (wife) (person you live with) presently working full or part-time, keeping house, retired, unemployed or something else?

Working full-time . . . . . .1 — 27
Working part-time 2
Keeping house 3
In School 4
Retired 5
Unemployed 6
Disabled 7
Other (Specify _____) . 8
Refused 9

65a. For 1983, was your total household income from all sources, before taxes,...

(Repeat until "no")

More than $ 6,000? No . . .0 — 28
More than $10,000? No . . .1
More than $15,000? No . . .2
More than $20,000? No . . .3
More than $30,000? No . . .4
More than $50,000? No . . .5
Yes. . . 6

(SKIP To Q. 66)

Don't know 8
Refused 7

b. Could you just tell me if it was above or below $15,000?

Above. 1 29
Below 0
Don't know *8*
Refused *7*

66. What is your racial-ethnic background? Are you...

Asian, ...1 30
Black, ...2
Hispanic, ... 3
White, ...4
American Indian, or 5
Something else? (Specify_____). 6
Don't know *8*
Refused *7*

67a. Now I don't want to know your exact address, but could you please tell me what street you live on?

_____ *(record exact spelling)*

Don't Know *998*
Refused *997*

b. What street crosses it at the corner nearest your home?

_____ *(record exact spelling)*

Don't Know *998*
Refused. *997*

68a. Altogether, how many different telephone numbers are there in your household?

Total # _____ 31-32
Don't know *98*
Refused *97*

CARD 04

b. (Are any of these) (Is this number) listed in the current directory? 33

Yes 1
No 2
Don't know 8
Refused 7

** Thank you very much for your cooperation.

Time ended _____ *A.M.* / *P.M.*

69. *Sex of respondent:* 34
Male 0
Female 1

70. *Was respondent's English...* 35
Good 3
Fair, or 2
Poor? 1

71. *Was respondent...* 36
Very cooperative. 3
Fairly cooperative. 2
Not very cooperative . . 1

72. *Did respondent seem...* 37
Very interested in interview 3
Somewhat interested, or 2
Not interested; hard to hold attention. . . 1

73a. *Do you think the information given by respondent was...* 38
Accurate, or 1
Inaccurate? 0

b. *(If inaccurate)*
Why? _____

_____

_____

4 80