CITIZEN CONTACT PATROL:

THE HOUSTON FIELD TEST

TECHNICAL REPORT

by

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PREFACE

This technical report describes the Houston Police Department's Citizen Contact Patrol Program and the evaluation of it conducted by the Police Foundation. As the report describes, the program was developed by a team of Houston police officers. They worked out of the Department's Research and Planning Division, under the direction of the Division Head and the Chief of Police. Without their creativity and cooperation there would have been no program to evaluate. The following members of the Houston Police Department were actively involved in the planning and execution of the program:

Lee Brown, Chief of Police
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Frank Yorek, Deputy Chief
L. B. Alsup, Captain
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Staff members of the Police Foundation and research consultants were involved in the design and execution of the program evaluation, or gave advice to those who were. They included:

Sampson Annan, Survey Director
Gretchen Eckman, Houston Site Observer
Antony Pate, Newark Project Director
Mary Ann Wycoff, Houston Project Director

Research Consultants

David Bayley	Richard McCleary
Richard Berk	Albert J. Reiss, Jr.
George Kelling	Peter Rossi
Paul J. Lavrakas	Jerome Skolnick

Bonnie Fisher worked at Northwestern University preparing and analyzing the data. Virginia Burke performed the arduous task of producing the final report.

The project was supported by the National Institute of Justice. The staff of the Institute provided continuous encouragement and advice. Those actively involved in this project included James K. Stewart, Director, William Saulsbury, the original project monitor, and Larry Bennett and Gil Kerlikowske, who shared the monitor role as it neared completion.

The entire project, including the evaluation, was conducted under the direction of Lawrence Sherman, then the Vice President for Research of the Police Foundation. Patrick V. Murphy, then the President of the Police Foundation, was active in establishing the project and representing it to the policing community.

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INTRODUCTION

The strategy of Citizen Contact Patrol evaluated in this report is a variation of motorized patrol which was implemented by the Houston Police Department in 1983 and 1984 so that its effectiveness as a fear reduction technique could be tested. This strategy was one of several designed by the Houston and Newark Police Departments as part of the Fear Reduction Project which was funded by the National Institute of Justice and evaluated by the Police Foundation. That project, the various strategies and the methods of strategy design and implementation in both cities are described in Appendix A of this report.

The strategies were designed with the particular characteristics of the two cities in mind. Houston police must cover an enormous geographic area (565 square miles), a fact which leaves them few alternatives to motorized patrol if they are to respond to calls for service. Citizen Contact Patrol was designed as a means of increasing police-citizen contact while still leaving officers readily mobile for most of their tour.

This report documents the way in which the strategy was implemented and the impact it appears to have had on levels of fear and satisfaction among Houston residents in one neighborhood.

THE CITIZEN CONTACT STRATEGY

PROBLEM AND PLAN

The Houston Police Department's Fear Reduction Task Force was concerned that one source of fear in that sprawling urban area might be the widespread sense of a physical, social and psychological distance between ordinary citizens and the police. In early 1983, Houston was a city of 1.8 million residents and 3357 police officers. This ratio of 1.92 officers for every 1000 citizens is spread across an area of 565 square miles. Almost all patrolling is done in cars which citizens may seldom see, and residents are unlikely to have reason to talk with officers unless they call with a complaint. In police systems based almost entirely on motorized patrol, there is seldom much opportunity for police interaction with residents and business persons, outside of giving tickets, responding to calls for service, and dealing with criminal incidents. Indeed, the police officer working a patrol beat may have little understanding of the priorities and concerns of people living or working there. Thus, the officer's free patrol time is likely to be directed by his or her own sense of task priorities rather than by those of the neighborhood. This lack of information about neighborhoods can cause officers to be unresponsive to important neighborhood problems, and the result may be a decline in public satisfaction with police service and an increase in fear of crime.

The Citizen Contact program was intended to give citizens an increased sense of police presence and, at the same time, to produce better quality

police service. The officers assigned to the project area were to initiate face-to-face contacts with residents and business people in which they would ask citizens what problems they were having in their neighborhood. The officer would then tell the citizen what had been, could be, or would be done to deal with the problem, and the officer would either take responsibility for handling the problem or would give the person information about another city agency to contact for assistance. The officers planned to leave their personal business cards with the people they contacted with the invitation for citizens to call them directly with any additional questions or information.

It was believed that these contacts had the potential to:

1. Reduce residents' fear of personal victimization and related worries about crime and disorder in the area;
2. Increase their level of satisfaction with their neighborhood as a place to live; and
3. Increase residents' satisfaction with the quality of police service they received.

The contacts and any subsequent service would be reinforced by a police-produced newsletter which would be sent once a month to each person who had been contacted. The newsletter would contain general departmental news of interest to the community, safety and crime tips, and "feature stories" which would describe citizens and/or police working to prevent crimes or apprehend criminals. One section of the four page paper would focus on news directly relevant to the neighborhood, including items about the contact program. (See Appendix L for a copy of one newsletter and an analysis of newsletter content.)

In addition to the direct effects of the contacts and the newsletters, there were likely to be unintended, but potentially beneficial, consequences of the structuring of the contact program. The officers believed that in order to have sufficient time to make the contacts and learn about the neighborhood, they would need to maintain beat integrity, meaning they would spend their shift within the program area and respond to calls outside Golfcrest only in an emergency. Maintenance of beat integrity should make it possible for officers to be more familiar with an area than they could be when performing patrol duties across a much larger area. And because it would increase patrol time in the program area, beat integrity could be expected to make people living and working in Golfcrest aware of a substantial increase in police presence. This increased police presence and the greater police familiarity with the area were predicted to reinforce the positive outcomes predicted for the contacts themselves.

Ever since the idea of police patrol was first articulated in Nineteenth Century England, the question of <u>how</u> to patrol most effectively has remained unresolved. The debate over method has often focused on the means of transportation police should use. Horse, bicycle, motorcycle, and foot patrol have all been used and advocated, even since the advent of the radio dispatched patrol car. But the means of transportation may not be nearly as important as what police <u>do</u> while they are on patrol.

The debate over transportation arises from the critique of automobile patrols as having isolated police from the community, cutting off the opportunity for informal contacts between pedestrian citizens and officers.

The 1968 Report of the National Advisory Commission on the Causes and Prevention of Civil Disorder identified such "stranger policing" as a cause of urban riots.

The Kansas City Preventive Patrol Experiment (Kelling, Pate, et al., 1974) focused the growing concern over patrol method. By finding that variations in the numbers of patrol cars in residential neighborhoods made little difference in the crime rate, that experiment suggested to some people that we could safely reduce the size of police departments. Other people drew what may be a more useful conclusion: that police should be doing something else on patrol besides merely driving around while waiting to be dispatched to a call for service.

The 1970s produced many attempts to patrol neighborhoods more effectively. Wilson (1983) divides these attempts into "community service" and "crime attack" strategies. The community service approach encourages officers to become more familiar with their neighborhoods, developing contacts with citizens that can lead to better intelligence about crime and higher arrest rates. The crime attack approach bypasses neighborhood residents in a direct attempt to catch criminals (through decoys or stakeouts) or deter potential criminals (through aggressive field interrogations).

The problem with the community service innovations of the 1970s was a general failure of implementation. "Team policing," the most common name for such efforts, usually attempted radical change in police activity, relations among police and supervisors, and systems of dispatching officers.

Few departments could actually produce team policing's key elements of increased personal contact with the community, meetings and supervisory coordination among all police working a patrol beat, and a ban on calls outside of that beat (Sherman, et al, 1973).

The Houston Citizen Contact Patrol test, in contrast, succeeded in implementing two of these elements: personal contacts and beat integrity. Unlike the team policing efforts, it did not try to create an area police "team," or try to restructure the role of the supervisor, or otherwise threaten the professional autonomy of the officers - - as the earlier team policing experiments had done.

The contribution of this field test to the patrol method problem is that, unlike earlier "community service" efforts, it emphasizes patrol method rather than patrol organization or patrol officer numbers. It provides a fairly clear test of the different effects of doing patrol with and without some primarily police-initiated, friendly personal contacts.

PLANNING CONSTRAINTS

Design of all the Fear Reduction strategies was constrained by several requirements, among them that: the strategy could be evaluated in a sound way; the strategy could be implemented and evaluated within a year; it could be implemented using existing department resources; and the strategy could be easily transferred to other police agencies. The Citizen Contact program met all of these conditions.

The evaluation condition. The evaluation of the strategy would use a quasi-experimental design in which fear, other attitudes and reported

behaviors would be measured with surveys conducted in program and comparison areas prior to implementation of the strategy and then again one year after the initial survey. Changes in attitudes in this neighborhood would be compared with those in the neighborhood in which no new programs would be undertaken during the year.

Implementation and evaluation within a year. Of the several Fear Reduction strategies which were designed and tested, Citizen Contact required the least complicated preparations. Once the card which would be used for recording contacts had been designed and the substation personnel briefed, the officers assigned to the test area were ready to initiate the program. The one year deadline was not a threat to the successful test of this particular strategy.

Existing resources. Implementation of Citizen Contact required some reallocation of personnel within the patrol area involved in order to maintain the beat integrity which was considered necessary, but no additional patrol officers, equipment or funds were required by this strategy.

Easy transferability. The straight-forward nature of the program would make it simple to describe and present to other agencies.

THE PROGRAM AREA

Figure 1 presents the neighborhood, Golfcrest, selected for the contact strategy. It is approximately one square mile in size and constitutes about one-third of the patrol beat of which it is a part. The 1980 Census reported a population of 3106 persons in 1209 occupied housing units and a racial and ethnic mix that was 47 percent white, 37 percent hispanic, 15 percent black and 2 percent Asian (Table 3). The pre-test survey conducted by the Police Foundation in the summer of 1983 found that almost 20 percent of the houses and apartments which were sampled were vacant, and it found 9 percent more black residents than the 1980 census had documented (Table 4).

It appears that the Golfcrest population was in a state of flux between 1980 and 1984, when the total population apparently declined, people moved in and out, and the ethnic mix changed. Although the size of the black population in the area was increasing, the racial housing pattern within the area was not mixed; blacks and hispanics tended to live in apartment buildings and in one housing project in the area, while the single family buildings were occupied primarily by whites.

All of the commercial and other non-residential establishments in Golfcrest are on perimeter streets. In the summer of 1983 there were approximately 155 businesses and other establishments on the sides of the perimeter streets immediately adjacent to the residential area. These establishments were a mix of retail and wholesale businesses and service

HOUSTON
GOLFCREST

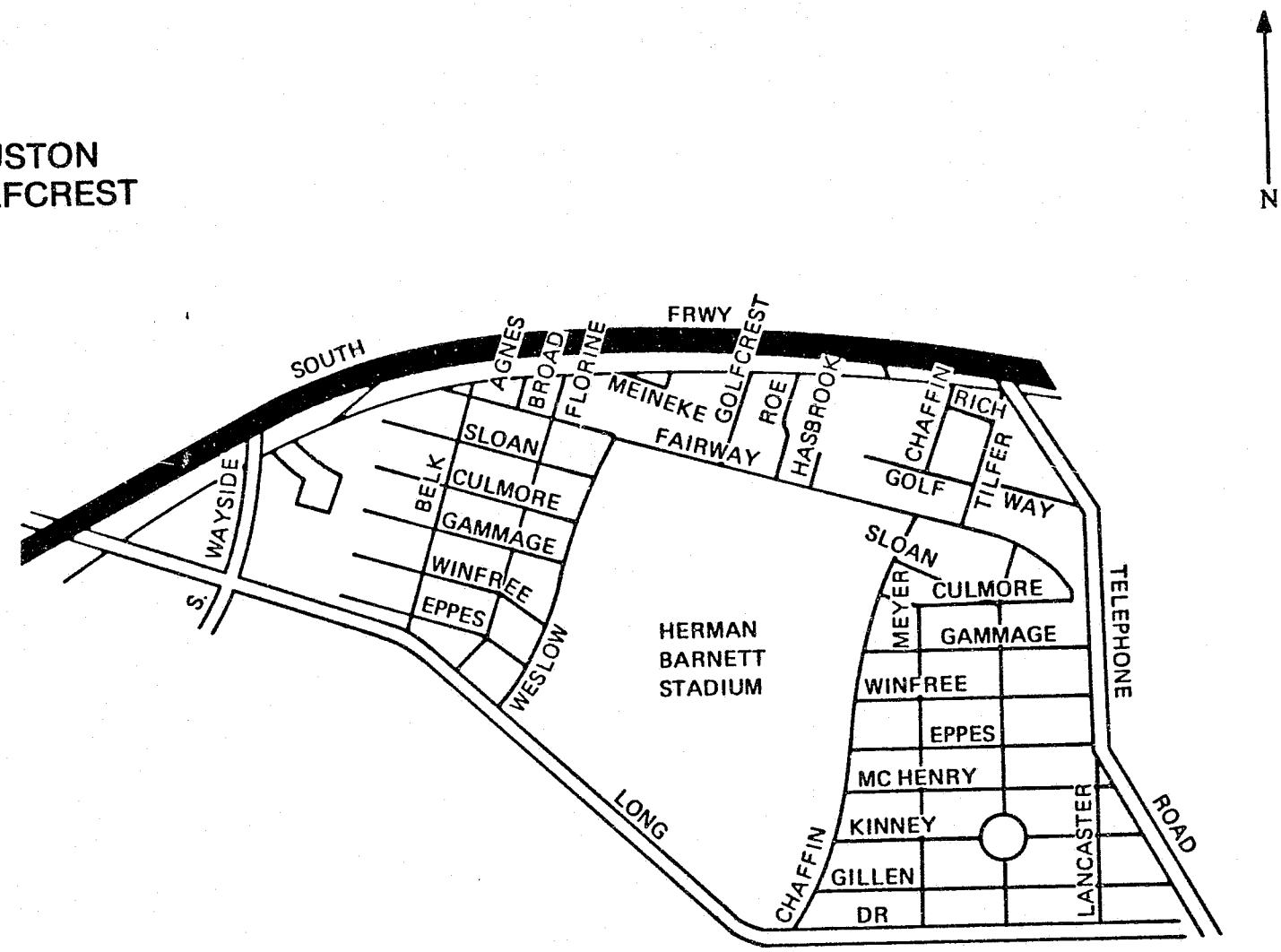


FIGURE 1

organizations. Approximately 10 percent of the businesses were manufacturing or construction firms. (See Appendix N.)

In the six months prior to the survey, 12 percent of these non-residental establishments had been the site of actual or attempted robberies and 40 percent had been burglarized or been the scenes of attempted burglaries. Vandalism had been committed at 29 percent of these places.

Twenty-six percent of the 1983 residential respondents had been the victims of actual or attempted robberies, pursesnatchings or pocketpickings during the previous six months. Forty-one percent lived in households which had experienced some type of property crime during the same period; of these, 15 percent lived in households which had been burglarized. All residential respondents were asked to rate a number of problems on a three point scale in which 1 = not a problem, 2 = somewhat a problem, and 3 = a big problem. In Golfcrest, burglary was assigned a 2 (somewhat of a problem), auto vandalism and auto theft were rated at 1.7; public drinking, the use and sale of drugs and robbery and pursesnatching were all scored at 1.6; stranger assault was assigned a 1.5 while breaking windows, graffiti, gangs and rape were scored at 1.4. There were no problems which Golfcrest residents, on the average, rated as more than "somewhat of a problem."

In 1983 the Golfcrest area was not characterized by high levels of citizen fear. Respondents were asked whether they were not at all worried, somewhat worried or very worried about several crimes. They indicated they were "somewhat" worried about robbery, slightly more than somewhat worried about burglary, and slightly less than somewhat worried about home invasion,

and assaults. With regard to these indicators of fear, Golfcrest was similar to the four other Houston neighborhoods surveyed for the Fear Reduction studies.

In 1983, 34 percent of the Golfcrest respondents reported having seen an officer in the area within the previous 24 hours but another 32 percent could not recall having seen an officer within the previous week.*

Residents reported a moderately positive attitude toward the police who served their area. They rated their performance of a number of police services as between "fair" and "good" and, as in other Houston areas which were surveyed, Golfcrest residents thought their police were not quite strict enough when it came to traffic enforcement.

PROGRAM ORGANIZATION

During the Fear Reduction Project's planning phase, preparation of plans for the Citizen Contact Program was headed by Officer Phil Brooks, a member of the Department's Fear Reduction Planning Task Force. He studied reports of a directed contact strategy which had been used in Grand Rapids, Michigan, and met with three lieutentants from the Oakland, California department who were conducting a similar program there. He and Officer Charles Epperson, who would lead the strategy team during the implementation phase, designed a citizen contact card (see Figure 2) and the filing system which would be used to collect and store the information obtained during the program.

*This measure of the recency of seeing an officer is not treated in this study as a measure of police visibility in the area. It is a measure subject--like all attitude measures--to personal differences among respondents and is used here as a measure of respondents' sense of police presence in their neighborhood.

FIGURE 2

THE CITIZEN CONTACT CARD

FRONT

HOUSTON POLICE DEPARTMENT	CITIZEN CONTACT CARD		NO.	
	CONTACT	RES. BUS. PED. VEH.	OFFICER	
		PRO-ACTIVE RE-ACTIVE	UNIT NO.	DATE / TIME
NAME			RACE / SEX / AGE	
ADDRESS			HOME PHONE	BUS. PHONE
PROBLEM DESCRIPTION: CRIME RELATED NON-CRIME RELATED				

FORM NO. PD-0016

REVERSE

DISPOSITION: ACTION TAKEN OR RE-CONTACTS

Officers Brooks and Epperson explained the strategy to each of the officers who would be assigned to the treatment area. One officer on each shift would be assigned exclusively to the area and would be responsible for the contacts during that shift. Relief officers who would replace the regular officers on sick days and days off were also instructed in the use of the contact cards, and the strategy was explained to the appropriate lieutenants and sergeants. Captain Alsup, commander of the substation from which the strategy would be conducted, and Officers Brooks and Epperson agreed that beat integrity was essential to the success of the project; if the officers were to reach their goal of contacting most of the households and other establishments within the year, they would need to be free from the expectation of taking calls outside the strategy area. This provision was a substantial change from normal operating procedures which typically left patrol units free to be dispatched over a large geographical area.

The major organizational problem for this strategy was one which plagues any effort to coordinate a program operating across shifts. It was difficult for Officer Epperson to meet regularly with the officers who were making the contacts during other shifts, and he had no formal supervisory authority over them. He provided the initial information about how and when to make contacts and in what areas, but he was in no position to monitor anything other than the location of the contacts and he could not request that supervisors for the other shifts monitor the program. He could pass

information through routine channels to officers on other shifts, but there was no respect in which the various officers responsible for implementing the strategy were part of a coherent team.

A lesser, but still important, problem involved the issue of workspace for Officer Epperson who needed a desk, telephone, and small set of files. The only space at the substation which could be freed was a small corner of the sergents' office; the necessity of locating the desk here probably did not increase the supervisors' appreciation for the program.

PROGRAM DOCUMENTATION

The Foundation intended to document the way in which the program was carried out so that (1) it would be possible to determine and describe the extent to which the program had been implemented as designed, and (2) so that the actual operation of the program could be described in detail to any other agency which might wish to adopt the strategy. When an evaluation of a program fails to demonstrate any program impact, it is frequently impossible to know whether the lack of impact was due to the inappropriateness of the program concept, or whether it was due to failure of the implementing agency to put a potentially good idea into the planned action. Documenting and evaluating a program allows for the distinction between failure of an idea and failure of implementation of the idea.

The Police Foundation's full-time site observer for Houston, Gretchen Eckman, rode randomly selected tours with the contact officers and systematically recorded her impressions of the contacts made during that tour. A copy of the data collection instrument is available in Appendix B. Approximately 40 contacts were observed and data were recorded formally for 20 of these.

A combination of survey data, administrative data gleaned from the citizen contact cards, and the field observations leads us to conclude that the program was implemented and the nature (if not the extent) of it was essentially that which was planned and is described in this report.

PROJECT IN ACTION

The approach to contacts was not highly aggressive. During September, 1983, the most active month of the program, 92 contacts were made. This was approximately 3 per day or somewhat less than one per shift per day. However, the contacts were not equally distributed across shifts; of the approximately 500 contacts made during the course of the program, roughly 50 percent were made during the shift which worked from 2 p.m. to 10 p.m. This was true in part because the officer who worked that shift was especially aggressive, and also because these hours are good ones in which to find residents at home. The officer, who worked the 6 a.m. to 2 p.m. shift found it necessary to spend considerable time handling the record keeping which the project and its evaluation required, and officers who worked the shift from 10 p.m. until 2 a.m. found it difficult to

identify non-threatening situations in which they could speak to residents. The observer found that one officer working the late shift would try to intercept residents in apartment parking lots or find legitimate reason to make auto stops which would then be used for conducting the interview. Whether this approach would be fear-reducing is not known, but the persons who were stopped probably did experience a sense of relief at discovering they were being detained for information and friendly conversation rather than for a presumed violation.

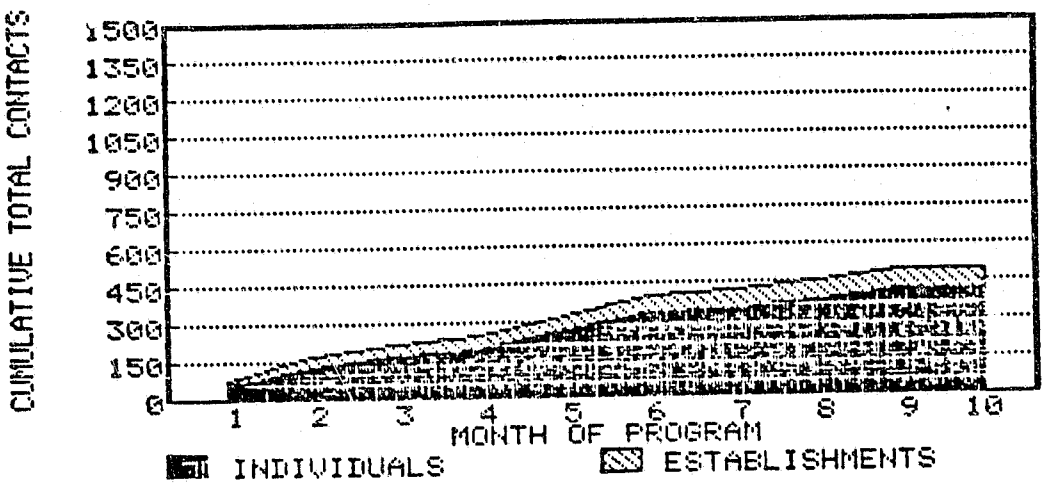
Nature of the Contacts

The contact cards completed by the officers indicate that 427 contacts were made among residents. Based on the 1983 survey estimate of 1146 occupied housing units (the relevant unit for the bulk of the contacts), contacts were made at 37 percent of the units (and with approximately 14 percent of the people living in the neighborhood). The 1984 evaluation survey found 12 percent of the respondents able to recall that an officer had "come to the door" to ask about problems in the neighborhood.

Most of these contacts were proactive in nature; that is, police officers initiated the contact with citizens. Other interviews were conducted during reactive encounters with citizens, usually because the citizen had summoned the police for some reason. Overall, 68 percent of the non-residental contacts and 73 percent of residential and street contacts were proactive rather than reactive. Among the latter, 79 percent were

FIGURE 3

NUMBER OF CONTACTS BY MONTH



Note: It was estimated that 1300 would be the maximum number of contacts made during the program period. This figure is a combination of the 1146 occupied housing units and the 155 non-residential establishments documented by the Foundation survey team in the summer of 1983.

conducted at homes while 9 percent were with citizens who were walking in the area and 9 percent were with citizens who were driving cars or trucks in the area. (The bulk of these vehicle-related contacts occurred in or near parking lots of apartment complexes.)

One of the officers made 47 percent of all contacts.* Four other officers each made 10-15 percent of the contacts and five relief officers each made approximately 2 percent of the contacts.

Those contacts which were observed tended to range from 3 to 6 minutes in length; rarely did they consume 10 minutes. Typically, the officer introduced him or herself, explained that she/he was the officer who worked the area and that they were trying to become familiar with the people who lived there and with their problems. They asked for a few minutes of the person's time and then proceeded to ask whether there were any problems in the neighborhood they wanted the police to know about. The officer listed any problems identified on the contact card (Figure 2), gave advice as to what could or could not be done about the problem and then usually left a business card so that the person could contact the officer at the district station should she/he ever feel need to do so.

Among the 40 contacts which the observer witnessed, there were only two which she personally considered to be "poor," and these were conducted by rookie officers who were still learning the program and were nervous about the contact and (or) about being observed. Ten percent of the contacts were

*This officer worked the shift (2 p.m. to 10 p.m.) in which it was convenient to make contacts. In addition, this officer was reputedly a very productive worker even prior to the contact program.

rated as "adequate" and 89 percent were considered to be "good." The observer never witnessed a negative response on the part of the citizen. In ten percent of the contacts citizen response was rated as neutral, and in 90 percent it was scored as positive. Citizens were very friendly in 80 percent of the contacts, very relaxed in 65 percent of the contacts and very cooperative in 100 percent of the contacts. The contacting officers were rated as very friendly in 90 percent of the contacts and very relaxed in 65 percent of them. "Level of relaxation" is the quality with the most variance across contacts. It is not possible to sort out causes, but it was clearly the case that officers were more relaxed the more often they previously had been observed, and they reported their level of relaxation during a given encounter as being related to their sense of how well they were accepted in the prior contact.

Although most of the contacts which were observed were judged to be friendly and relaxed on the part of both the citizen and the officer, the style and content of the conversation varied by officer and at times depended on the mood of the officer. Some officers were more likely to leave a business card; some were more likely to give the citizen information about possible solutions to the problems identified, and some were more adept at explaining the purpose of the contact. The officer who made the most contacts was likely to leave a card, provide information, and give a good explanation. However, his style was not always consistent. The observer judged him most successful when he began the conversation by explaining why the contact was being made. Residents appeared to be less

comfortable when officers asked questions first and only later said they were trying to contact everyone in the neighborhood in order to become more familiar with the area and its people.

The Fear Reduction Task Force considered the nature of the planned contacts to be so similar to the normal interviewing done by patrol officers as to make unnecessary special training for the contact officers. However, training would perhaps have made officers more sensitive to the order of their presentation and to the need to reassure the citizen that she/he was not being singled out for special attention, and might have resulted in a more consistent treatment of persons contacted. Training should be an integral part of future implementation of this strategy.

The Persons Contacted

Table 1 reports the distribution by sex, age and race of the persons contacted and compares these figures to those for the entire area. As they were conducting the interviews over the course of the project, the team monitored these distributions and tried deliberately to reach all major segments of the population. The characteristics of the persons contacted match those of the Golfcrest population as a whole fairly well, with some oversampling of young people, and white males.

Problems Identified

The officers recorded on the contact cards the problems or conditions which were of concern to the respondents. These cards were then used by the officers to monitor the program and to analyze patterns of problems in the

TABLE 1

DEMOGRAPHIC CHARACTERISTICS OF CONTACTED INDIVIDUALS AND TOTAL PROGRAM AREA POPULATION

Percentage of Each Group Which Were:	Contacted Individuals*	Total Population (1983 Survey)
Male	51	46
Female	49	54
15-24 years old	26	19
25-49 years old	47	55
50-98 years old	26	26
Black	25	24
White	51	41
Hispanic	22	33
Other	1	2

*Including residents and representatives of non-residential establishments.

area. For this report, the problems which were named were first grouped into 54 detailed descriptive categories. Those are described in Appendix C. Then, thirteen summary categories were developed to facilitate presentation and analysis of the problems.

The summary categories and examples of their content include:

- domestic violence (disturbances, disputes, assaults, child abuse)
- suspicion (persons, events, circumstances)
- vehicle-related problems (parking, speeding, drinking)
- juvenile problems (gangs, truancy, kids causing problems)
- disputes (neighbors, threats, fights, trespassing, personal confrontations)
- disorders (prostitution, panhandlers, drinking, noisy parties, disturbed persons)
- environmental decay (noise, dogs, trash, abandonment)
- vehicle crimes (theft from, of, vandalism to vehicles)
- burglary
- personal crimes (robbery, rape, general assault)
- vandalism
- general theft
- other (including drugs, fears, police problems)

Table 2 indicates the frequency of problems or conditions, as reported by residents and representatives of non-residential establishments in the area. For both groups, the most frequent response was "no problem." Forty percent of residential contacts and 28 percent of those in various establishments indicated this was the case. Among problems mentioned, vehicle crimes and burglary predominated. Respondents from non-residential establishments expressed concern about personal crimes in the area (16 percent) and theft (12 percent) as well. Nine percent of both groups expressed concern about various "disorders."

A detailed analysis of the distribution of problems expressed by different types of residents also can be found in Appendix C. In summary it indicates that:

TABLE 2

PROBLEMS NAMED DURING CONTACTS

Problem Category	Contacts With Residents	Establishments
Domestic violence	6	1
Suspicion	5	3
Vehicle problems	6	4
Juvenile problems	3	4
Disputes	4	-
Disorders	9	9
Environment	4	-
Vehicle crime	14	15
Burglary	12	15
Personal crime	3	16
Vandalism	2	7
General theft	3	12
Other	3	7
No problems mentioned	41	28
Total	115%	121%
(N)	(427)	(73)

SOURCE: gathered by police officers during citizen contacts in Houston 1983-84. Percentages are based upon total problems mentioned divided by the number of individuals contacted, so they may sum to more than 100%.

- blacks were more likely to express concern about domestic violence and, with hispanics, shared concern about vehicle crime. Whites more often than others were concerned about burglary. Hispanics more frequently described disorder problems in the community.
- Women were more concerned than men about domestic violence and suspicious circumstances, while males more frequently mentioned vehicle problems and vehicle crime.
- Younger people were more concerned about domestic violence, while older people were more likely to describe area problems with burglary, vehicles, and disorderly persons and conditions. Vehicle crimes concerned those in the age category, 26-49, but not others.
- Not surprisingly, those who had summoned the police usually had a complaint about the area (only 9 percent did not), and were more likely to be concerned about domestic violence, vehicle crime, suspicious persons and circumstances, and burglary.
- Those who were interviewed in their homes registered more complaints than those contacted on the street, and they were less likely to be concerned about disorderly conditions and more likely to mention burglary. Pedestrians were concerned about vehicle problems.

Police Response to Problems

Among the observed contacts, the most common response was for the officer to tell the citizen that the police were now spending more time patrolling in the area and then to give the citizen a business card with

the instruction to call the officer at the station if there were any further problems.* Although officers intended to record their responses to problems, the record of problems is much more complete than the record of responses. This is due, in part, to the fact that the response or solution (other than a promise to "look into it") did not always occur at the time the contact was recorded. For example, the officer might tell the citizen which other city agency to call in order to get some desired service (e.g., an overgrown lot mowed or an abandoned car towed). This might be the only response on the part of the officer. But there were several instances recalled by the officers in which they later received a telephone call from the citizen who reported that they had not been able to get a satisfactory response from another agency; at that point, the officer might have called the agency and got the desired response. Days after the initial contact, officers in these situations were not inclined to take the time to seek the contact card in the file and update the information about the response.

Similarly, for example, after hearing related complaints over several days from residents of a particular apartment complex, the officer might later have a talk with the apartment manager, suggesting ways in which the manager might help solve the problem. In one case, an officer asked an apartment manager to talk with parents residing in the complex whose children were reportedly breaking windows in the neighborhood. The officer believed the conversation took place and the problem ended.

After several residents in a retirement complex complained that cars were being stolen through a back entrance of their parking lot, the contact

*If the officer was out of the station when the call came in, a message was taken and the officer who initially made the contact returned the call. There is no record of the number or nature of these calls.

officer suggested to the building manager that the lot's rear entrance be permanently blocked or at least closed at night. The manager took no immediate action but the officers began to watch the lot more closely.

Arrests were another type of response which usually were not recorded on the contact card since they typically occurred some time after the contact or contacts which produced the information which led to the arrest. There is no way to know how many arrests were directly attributable to the contacts, but when the most active officer was asked what he thought he got out of working the strategy, he replied "eighty-three good informants."* This was an officer who, prior to his participation in the program, was known as one who made a high number of arrests, so his interest in the crime-related, informational aspects of the contacts was not surprising. He believed that several arrests resulted either directly or indirectly from his conversations with residents and, at the conclusion of the formal test of the strategy, he was continuing to stay in touch with some of the people he had met through the contact program. Because of his own orientation to policing, this officer never considered the contact program to be anything other than "real" police work. In this respect he differed from some other officers who perhaps thought of police work as being only that which was assigned by the dispatcher or that which was done in reaction to observed offenses. The officer who used the program to develop informants was working as a field-training officer to teach the rookies assigned to him to make proactive contacts.

*This, like the information about program responses, is based on officer impressions rather than any count of actual events.

While it is not possible to document which or how much of the information provided by contacted citizens resulted in specific responses, participating officers all agreed that the contacts led to an increase in useful information. They may have received it at the time of the contact or it may have come to them days or weeks later through a telephone call from the citizen. Although the calls were not systematically recorded, the officers reported having received several. Sometimes a citizen wanted to add information to a case report which the officer had made in the course of a reactive contact and, at other times, simply wanted to let the officer know about something happening in the neighborhood. The contact officers believed they received information which they would not have been given if the citizen had had to try to reach them through the police dispatcher.

Whatever the information, it was used primarily by the individual officer who received it. Problem response in Golfcrest never took the form of officers who worked the area meeting together to discuss a solution for a given situation. There was no supervisor in charge of the program who might have reviewed the cards to determine whether an integrated response was appropriate and who might have assumed the responsibility for coordinating the effort across shifts. There was not even a means of letting the officer on each shift know which addresses had not yet been contacted. Had there been more officers active in the area and had the program been conducted over a longer period of time, there probably would have been a problem of duplicate contacts. This potential for analyzing the individual incidents or problems in order to identify of general problems that might call for responses beyond the capacity of the individual officer should be considered

by organizations that might wish to adopt this strategy. This is one aspect of the larger management issue for this type of project. Had this strategy been implemented over a large area and involved several officers, it would have required the full attention of a good supervisor.

The Newsletters

After a household or business was contacted, the address was added to those which were mailed a monthly police-produced newsletter. Depending on the month in which the contact was made, those contacted could have received up to five newsletters prior to the 1984 post-intervention survey. (See Appendix L for a copy of one newsletter and an analysis of newsletter content.)

SUMMARY

The Houston Police Department's Fear Reduction Task Force was concerned that one source of fear might be a sense of physical, social and psychological distance betwen ordinary citizens and the police. They decided to test a Citizen Contact strategy in which officers in one neighborhood would contact people living and working in their area at their homes, in businesses and on the streets to introduce themselves and discuss citizens' perceptions of neighborhood problems. Leaving business cards behind, officers made it possible for people in this one community to contact them directly by telephone. By maintaining beat integrity through the course of the project, officers increased their visibility in the neighborhood.

A contact card was completed after each meeting, and from these it was determined that the most frequently mentioned concerns of both residents and

non-residential contacts were vehicle crimes and burglaries. Non-residential contacts also cited personal crimes frequently as well as theft, generally. In forty-one percent of the contacts, citizens said they had no problems.

Any police actions in response to the problems were taken by the officers who made the initial contact. Responses ranged from giving citizens information they could use to solve the problem (e.g, the name and number of the city office to call to get trash picked up) to working to apprehend offenders. The contact officers felt they received information from the contact which was useful in efforts to prevent crime and to accomplish apprehensions.

In the eight month period of the project, officers made contacts at approximately 37 percent of the housing units and 45 percent of the non-residential establishments. The major demographic characteristics of the contacted residents closely matched those of the neighborhood population as a whole.

A Police Foundation observer who accompanied the officers to 40 of the contacts judged all but two to be good interactions in which both the officer and citizen were comfortable, the officer was informative, and the citizen was cooperative.

Golfcrest, the program neighborhood, was approximately one square mile in size with a mixed-race population, in 1980, of 3106 residents. There were 155 non-residential establishments in the area.

EVALUATION DESIGN AND METHODOLOGY*

THE DESIGN

As mentioned in the introduction in this report, the evaluation of the Citizen Contact program was based on a quasi-experimental design in which citizen attitudes, reported experiences, and behaviors were measured using face-to-face interviews in the Houston neighborhoods in the summer of 1983 (pre-intervention) and again in the summer of 1984 (post-intervention). The contact strategy was begun in Golfcrest (the program area) approximately two weeks after the completion of the Wave 1 (pre-intervention) survey and had been in operation eight months when the Wave 2 (post-intervention) survey was begun. Shady Acres, the comparison area, was located approximately 20 miles from Golfcrest and was designated as the survey area in which no new police programs were to be implemented between the Wave 1 and Wave 2 surveys.

The following sections describe the groups which were surveyed, the levels of analysis and tests of program effects, the program and comparison areas, the survey procedures, and the variables used to determine program effect.

THE SURVEYED GROUPS

Two different groups in the Golfcrest area were considered targets of the contact strategy. Residents or household members constituted the largest group, and the contact officers hoped to reach half of the

*The design and methodology are discussed in the methodology report of the Fear Reduction Project. See Annan, et al., 1985.

approximately 1146 households which were estimated by the 1983 pre-intervention survey to be occupied. It was the attitudes of the residents which were considered to be critical to the future stability of the neighborhood, and it was among residents that the officers would make their greatest efforts.

However, businesses and other non-residential establishments are also important to the viability of a community and it is the abandonment of commercial property which is often the first sign that a neighborhood is declining. These non-residential establishments were to be surveyed in an effort to determine whether the business community and other local organizations were responding to the program. The contact officers did expect to visit most of these locations during the project period.

PROGRAM AND COMPARISON AREAS

The Houston Police Department and the Police Foundation together identified five areas of the city, closely matched in terms of their size, demographic characteristics, land use, level of disorder and other characteristics to participate in the Houston Fear Reduction Program. To accomplish this, the Department began by obtaining from the City Planning Department a list of 51 areas of the city which previously had been identified as neighborhoods and for which demographic data had been compiled. Foundation and Department personnel agreed that the areas should

be racially mixed, and of similar racial patterns, so that programs would not be tested among only one racial group--a condition which would be unrepresentative of the city's population. Using this criterion, Foundation staff analyzed the neighborhood data and narrowed the list to approximately 20 neighborhoods which met the racial mix criterion and were similar in terms of other major demographic features. Department personnel then provided crime data for these areas.

Foundation staff visited each of the substations in Houston to ask the station captains and the crime analysts to describe the neighborhoods on the list which were in their district and also to identify any other areas which might be suitable for the study. They were asked to think of areas which were experiencing social disorder problems which might be reduced if addressed for a year with a special program. Officers from the districts took Foundation staff for tours of the neighborhoods and provided extensive information from their own patrol experience in the areas. Through this process, some neighborhoods were eliminated from the original list* and others were added. Demographic and crime data were collected for the latter, and all of the areas were again studied for comparability.

*In two cases because officers believed the racial mix had changed substantially since the 1980 Census and in another because a freeway which divided an area prevented it from being a "neighborhood."

A final conference of district captains, district crime analysts, Police Department Research and Planning staff, and Police Foundation staff produced a list of nine areas which were considered sufficiently similar in terms of problems and demographic charcteristics to serve as "matched" areas for the program. The selection of five areas in four districts was based on considerations of distances among the areas and other programs being conducted within some of the districts.

From among the five areas, Golfcrest was selected to be the area exposed to the Citizen Contact program. Shady Acres was designated the comparison area in which no new police programs would be introduced. Any changes in this area, then, could be taken to be generally representative of prevailing trends in the city during the time of the study. 1980 Census data for these two areas are presented in Table 3. As the table indicates, the two areas were quite similar in most respects. The program area, Golfcrest, had a population, in 1980, of 3,106 pesons and 1,309 housing units. Of that population, 46 percent were white, 37 percent were hispanic, 15 percent were black, and 2 percent were Asian and other. Twenty-six percent of the population were under 18 years of age and eleven percent were aged 65 or older. Fifty-nine percent of the housing units were for single families and of these, 87 percent were occupied. Of the 1,209 number of units which were occupied in Golfcrest, 40 percent were occupied by their owners. An average of 2.6 persons lived in each unit.

Table 3

Demographic Data for Citizen Contact Patrol Program and Comparison Areas

Area	Population							Housing Units			Occupied Units		
		Ethnicity				Age							
	Total	% Black	% API	% W	% SO	% Below 18	% 65 and above	Total	% Single Family	% Occupied	Persons Per Unit	Total	% Owner Occupied
Program Area (Golfcrest)	3106	15	2	46	37	26	11	1390	59	87	2.6	1209	40
Comparison Area (Shady Acres)	3690	22	-	52	26	26	15	1626	62	90	2.7	1468	39

Source: 1980 Census

The comparison neighborhood, Shady Acres, had a 1980 population of 3,690 persons and 1,626 housing units. Fifty-two percent of the population were white, 26 percent were hispanic, 22 percent were black and there were no Asians counted. Twenty-six percent of the population were less than 18 years old and 15 percent were 65 years old or older. Sixty-two percent of the housing units were for single families and 90 percent of these were occupied. Of the 1,468 units in the area which were occupied, 39 percent were occupied by their owners.

SURVEY PROCEDURES

Area Listing and Household Selection. Once the program and comparison areas were selected, Police Foundation staff used updated 1980 Census block maps to compile sample frames for both the residential and non-residential samples. Area survey supervisors conducted an area listing, walking the streets and recording on Listing Sheets all addresses within the defined boundaries. After being put on computer-readable tape, these listings were divided into two sub-lists, one for residences and one for non-residential establishments such as businesses, churches, offices and other such places. Each address on both lists was assigned an identification number. Selection of sample addresses was accomplished by dividing the universe (the number of addresses listed) by the desired sample size to arrive at a sampling

interval. Starting with a random number and selecting every Nth case (where N was equal to the sampling interval), this procedure was used to produce a random sample of addresses in the program and comparison areas.

There were many advantages to this procedure, among them that sample households were separated physically by the number of addresses in the sampling interval, a condition which should help in reducing diffusion effects attributable to household visits.

Respondent Selection Within The Household. Once the sample of addresses was selected, the next step was the selection of a respondent within the household. This selection was accomplished by listing all household members who were 19 years old or older and assigning them numbers, starting with the oldest male and listing through the youngest female. The interviewer then used a random selection table assigned to that household to determine who should be the respondent. No substitution was permitted for the selected respondent. (This is a standard "Kish-table" selection procedure.)

The plan for Wave 2 was to contact all sample addresses (including those at which no interview was conducted at Wave 1), and interview the respondents from Wave 1 when possible, thus creating a panel sample. A replacement respondent was selected at sample addresses where the Wave 1 respondent was no longer a resident of the household. These respondents, however, were excluded from the panel analysis, but were included in the pooled cross-sectional analysis. For an address at which no interview was

completed during Wave 1, a respondent was selected on the initial contact, using the same selection table that was assigned to that address for Wave 1. Thus, for this evaluation, the completed panel sample is a subset of the Wave 1 and Wave 2 area samples, and is included with them when area-level analyses are reported.

Respondent Selection Within an Establishment. In each non-residential establishment, the goal was to inteview the owner or the manager of the establishment. In 10 percent of the cases, because the owner or manager was unavailable, the most knowledgeable staff member was selected as the actual respondent.

Supervisor/Interviewer Training. The interview operations for Wave 1 began with the recruitment of supervisors, who were given a two-day training session, followed by the recruitment and hiring process for interviewers. After general advertising for interviewers, several orientation sessions were held for screening and selection purposes. The selected interviewers were then invited to a three day training session, after passing a police record check to which they had agreed as part of the hiring process. The final hiring decisions were made after the training session by the Police Foundation's Survey Director and the Foundation's Houston field supervisor.

The interviewers' training was conducted by the Survey Director with the assistance of the Project Director, a trainer and the site supervisor. Prior to attending the training sessions, an Interviewer Training Manual was sent to each interviewer. This manual was designed as a programmed learning text with questions which interviewers were to answer as they reviewed each section. The training agenda consisted of general introductory remarks (including background on the study and the Foundation role), general and

specific instructions on procedures for respondent selection, a complete review of the questionnaire with special attention to the victimization series, a practice review session, and role-playing sessions.

Contacting Sampled Households and Non-Residential Establishments. About one week before interviewing began, an advance letter from the Mayor of Houston was mailed to the selected addresses. The letter, addressed to "resident" or "owner," informed the recipient of the main objectives of the research in an effort to give credibility to the study and encourage cooperation with it.

Wave 1 interviewing began on May 29, 1983 and was completed for all project areas on September 8, 1984, after which the police department started the implementation of the programs. The post implementation survey (Wave 2) began on May 18, 1984 and continued in various project areas until July 20, 1984.

All interviewing was conducted in person. Following the initial face-to-face contact, telephone contacts were used occasionally to schedule an in-person interview with the selected respondent.

Call Back Procedures. Interviewers made a minimum of five attempts to complete an in-person interview. Each attempt was recorded on a Call Record Sheet. The attempts were made at different times of the day and different days of the week to maximize the chances of finding the respondent at home. About 40 percent of the interviews were completed on the first and second visits.

A Non-Interview Report (NIR) was completed for each selected address at which an interview could not be completed. The supervisor reviewed each NIR to decide whether or not the case should be reassigned to another interviewer. Most refusal cases were reassigned and interviewers were successful in converting nearly 40 percent of the initial refusals to completed interviews.

In-Field Editing. Completed questionnaires were returned to the supervisor on a daily basis. The supervisor and her clerical staff were then responsible for the field editing of all completed questionnaires. This process enabled the supervisor to provide the interviewers with feed back concerning their performance and insure that they did not repeat the errors they previously had committed. It also permitted the identification of missing information which could be completed, before interview schedules were sent to the home office.

Validation. About thirty percent of the respondents were recontacted to verify that the interview was indeed completed with the selected respondent. The validation process also helped to provide feedback about the interviewers. Thirty percent of each interviewer's questionnaires were randomly chosen for validation. Validations were completed either by telephone or in-person.

If one of an interviewer's completed questionnaires could not be validated, the supervisor conducted a 100 percent validation of that interviewer's work. Cases that failed validation were either reassigned or dropped from the data base.

Towards the end of the field work period for Wave 1, when the interviewers' mode of payment was changed from an hourly basis to "per completed" basis, a 100 percent validation was conducted on all completed interviews. The validations were carried out from the home office by telephone. Cases in which the telephone number was no longer working and cases without telephone numbers were sent back to the field for in-person validation. The "per completed" mode of payment for interviewers was continued for the Wave 2 survey; after the supervisor had successfully validated the initial five completed interviews for each interviewer, he or she continued to check 33 percent of the interviewer's work.

Response Rates. The final survey results are presented in Tables 4 and 5. As indicated, Wave 1 residential response rates of 77.9 percent and 74.7 percent were achieved in the program and comparison areas. Response rates of 82.7 percent and 78.1 percent, were achieved during Wave 2. Such high response rates indicate that the samples can be taken as generally representative of the populations living in the two areas.

As Table 5 indicates, in the panel survey, 58.0 percent of the Wave 1 residential respondents were reinterviewed in the program area, and 47.0 percent were reinterviewed in the comparison area.* The panel response rate in the program area was 70.6 percent; it was 53 percent in the comparison area.

Table 6 indicates response rates of approximately 96 percent in the program area and 81 pecent in the comparison area for the Wave 1 non-residential surveys. During Wave 2, these response rates were 94 and 88 percent, respectively.

*The high vacancy rates which contributed to the low panel response rates are discussed in the methodology report of the Fear Reduction Project. See Annan, et al., 1985.

TABLE 4

WAVE 1 RESIDENT SURVEY RESULTS
(Numbers in Parentheses are Percentages of Sample Size)

Area	Total Units	Sample Size [1]	Completed	Refusals	Vacant	Bad Address	Maximum Calls	Ineligible, Duplicates	Other [2]	Area Response Rate [3]
Program Area (Golfcrest)	1427	875	543 (62.1%)	63 (7.2%)	173 (19.7%)	3 (0.2%)	54 (6.2%)	2 (0.2%)	37 (4.2%)	77.9%
Comparison Area (Shady Acres)	1486	613	389 (63.4%)	64 (10.4%)	58 (9.5%)	0 (0.0%)	46 (7.5%)	34 (5.5%)	22 (3.6%)	74.7%

WAVE 2 RESIDENT SURVEY RESULTS
(Numbers in Parentheses are Percentages of Sample Size)

Area	Total Units	Sample Size [1]	Completed	Refusals	Vacant	Bad Address	Maximum Calls	Ineligible, Duplicates	Other [2]	Area Response Rate [3]
Program Area (Golfcrest)	1427	875	560 (64.0%)	26 (2.9%)	191 (21.8%)	3 (0.2%)	26 (3.0%)	4 (0.5%)	65 (7.4%)	82.7%
Comparison Area (Shady Acres)	1486	613	403 (65.7%)	30 (4.9%)	79 (12.9%)	4 (0.7%)	42 (6.9%)	14 (2.9%)	41 (6.7%)	78.1%

1. The sample size was based on the assumption that the survey operations would produce completion rates of 75 percent for the area sample and 66 percent for the panel (re-interview) sample.

2. "Other" includes the number of respondents who were in hospital, ill, on vacation, or had a language problem, plus completed interviews which were invalidated during quality control checks.

3. "Area Response Rate" equals Number Completed ÷ (Sample Size - (Number Vacant + Number with Bad Address + Number Ineligible))

TABLE 5

PANEL RESIDENT SURVEY RESULTS
(Numbers in Parentheses are Percentages of Sample Size)

Area	Sample[1] Size	Completed Same Address Same Respondent	Completed Same Address Different[2] Respondent	Refusals	Vacant	Bad Address	Maximum Calls	Ineligible, Duplicates	Other[3]	Panel Response Rate[4]
Program Area (Golfcrest)	543	315 (58.0%)	75 (13.8%)	9 (1.7%)	96 (17.7%)	0 (0.0%)	16 (2.9%)	1 (0.2%)	31 (5.7%)	70.6%
Comparison Area (Shady Acres)	389	183 (47.0%)	102 (26.2%)	21 (5.4%)	39 (10.0%)	2 (0.5%)	18 (4.6%)	3 (0.8%)	21 (5.4%)	53.0%

1. The panel sample consists only of those households in which an interview was completed at Wave 1.

2. Interviews that were completed with a different respondent in the panel households were excluded from the panel analysis but were included in the analysis of the pooled cross-sectional data.

3. "Other" includes the number of respondents who were in hospital, ill, on vacation, or had a language problem, plus completed interviews which were invalidated during quality control checks.

4. "Panel Response Rate" equals Number Completed at Same Address with same Respondent ÷ (Sample Size - (Number Vacant + Number Bad Address + Number Ineligible).

TABLE 6

WAVE 1 NON-RESIDENTIAL SURVEY RESULTS
(Numbers in Parentheses are Percentages of Sample Size)

Area	Total Establishments	Sample Size	Completed	Refusals	Vacant	Maximum Calls	Ineligible, Duplicates	Other [1]	Area Response Rate [2]
Program Area (Golfcrest)	155	77	68 (88.3%)	2 (2.6%)	6 (7.8%)	0 (0.0%)	1 (1.3%)	0 (0.0%)	95.8%
Comparison Area (Shady Acres)	127	63	39 (61.9%)	4 (6.3%)	12 (19.0%)	4 (6.3%)	3 (4.8%)	1 (1.6%)	81.2%

WAVE 2 NON-RESIDENTIAL SURVEY RESULTS
(Numbers in Parentheses are Percentages of Sample Size)

Area	Total Establishments	Sample Size	Completed	Refusals	Vacant	Maximum Calls	Ineligible, Duplicates	Other [1]	Area Response Rate [2]
Program Area (Golfcrest)	155	76	67 (88.2%)	1 (1.3%)	5 (6.6%)	2 (2.6%)	0 (0.0%)	1 (1.3%)	94.4%
Comparison Area (Shady Acres)	127	60	44 (73.3%)	3 (5.0%)	10 (16.7%)	3 (5.0%)	0 (0.0%)	0 (0.0%)	88.0%

1. "Other" includes language problem and establishment temporarily closed.

2. "Area Response Rate" equals number completed ÷ (Sample Size - (Number Vacant + Number Bad Address + Number Ineligible))

MEASUREMENT

Survey questionnaires were designed to collect information about exposure to the program as well as to measure the effects on each of the dimensions on which the program was hypothesized to have some impact. One version was created for residents; another shorter version was created for use with owners and managers of non-residential establishments. Copies of both instruments are included in a separate methodology report. Appendix D describes in detail the measures used in the residential survey and how they were created. Appendix E presents the same information about the measures used in the non-residential survey. A brief summary of the measures used is presented below.

o Recalled Program Exposure. Both before and after the program, respondents in both areas were asked whether they recalled an officer coming to their door to discuss neighborhood problems with them, when they had last seen an officer in the area, and whether they knew an officer who worked in the neighborhood.

o Perceived Area Social Disorder Problems. To measure perceived social disorder problems, residential respondents were asked a series of questions about how much of a problem each of the following activities were:

- Groups hanging around on corners,
- People saying insulting things,
- Public drinking,
- People breaking windows,
- Writing or painting on walls,
- Gangs, and
- Sale or use of drugs in public.

The responses to each of these questions were combined to form one composite scale. A similar set of items was used among non-residential respondents.

o Perceived Area Physical Deterioration Problems. Perceived physical deterioration was measured among residential respondents by combining the responses to questions about how much of a problem each of the following were in the area:

- Dirty streets and sidewalks,
- Abandoned houses and buildings, and
- Vacant lots filled with trash and junk.

A similar set of items was utilized among non-residential respondents.

o Fear of Personal Victimization in Area. A composite scale was created combining the responses of residential respondents to four questions which asked about:

- Perceived safety while in area alone,
- Whether there was a place in the area where the respondent was afraid to go,
- Worry about being robbed in the area,
- Worry about being assaulted in the area.

Similar items were combined among non-residential respondents.

o Perceived Concern About Crime Among Employees and Patrons. Responses to two questions were combined to form a measure of the concern expressed by the employees and patrons of the establishment:

- Frequency of hearing employees express concern about their personal security in the area, and
- Frequency of hearing patrons express concern about their personal safety in the area.

o Worry About Property Crime Victimization in Area. A scale combined responses of residential respondents to two items asking about the extent of worry about:

- Burglary, and
- Auto theft.

Among non-residential respondents the responses to items concerning worry about burglary and vandalism were combined.

o Perceived Area Personal Crime Problems. This scale combined responses to three questions which asked about the extent to which each of the following were perceived as problems in the area:

- People being attacked or beaten up by strangers in the area,
- People being robbed or having their money, purses or wallets taken, and
- Rape or other sexual attacks.

o Perceived Area Property Crime Problems. This scale combined responses to three questions which asked about the extent to which each of the following were perceived in the area:

- Burglary,
- Auto vandalism, and
- Auto theft.

o Victimization. Residents were asked whether they had been victims of various types of attempted and successful crimes during the six-month period prior to being interviewed. Because many individual types of victimization were relatively infrequent, respondents have been categorized for this analysis as to whether they were victims of:

--personal crimes, including actual and attempted robbery, pursesnatching and pocketpicking, actual and attempted or threatened assault, threats, and sexual assault;

--property crimes, including actual and attempted burglary, theft, mailbox and bicycle theft, as well as motor vehicle theft, vandalism of home and automobile.

Representatives of non-residential establishments were asked whether their establishment had been victimized by each of the following crimes during the six months prior to being interviewed:

- Robbery or attempted robbery,
- Burglary or attempted burglary, and
- Vandalism.

o Evaluations of Police Service and Aggressiveness. Two scales were created to measure respondents' evaluations of the police. The first scale, designed to indicate general attitudes toward police service, was composed of the responses to the following individual items:

- How good a job do the police in the area do at preventing crime,
- How good a job do the police in the area do in helping victims,
- How good a job do the police in the area do in keeping order on the street,
- How polite are police in the area in dealing with people,
- How helpful are police in the area in dealing with people, and
- How fair are police in the area in dealing with people.

The second measure, to serve as an indicator of perceived police aggressiveness, was created by combining the responses to questions concerning the extent to which each of the following were thought to be problems in the area.

- Police stopping too many people on the streets without good reason, and
- Police being too tough on people they stop.

o Defensive Behaviors to Avoid Personal Crime. To measure the extent to which respondents take restrictive, defensive precautions to protect themselves against crime, the answers to the following questions were combined:

- Whether the respondent goes out with someone else after dark in order to avoid crime,
- Whether the respondent avoids certain areas,
- Whether the respondent avoids certain types of people, and
- Whether the respondent avoids going out after dark.

These are used in this evaluation as behavioral measures of fear of crime.

o Household Crime Prevention Efforts. To measure the extent to which respondents had made efforts to prevent household crime, the responses to the following questions concerning whether the following household crime prevention efforts had been made:

- Install special locks,
- Install outdoor lights,
- Install timers,
- Install special windows or bars, and
- Is a neighbor asked to watch home when respondent is away for a day or two.

These are used in this evaluation as indicators of positive effects upon purposive crime prevention.

o Change in Business Environment. To measure the extent to which business conditions had changed in the recent past, the responses of non-residential representatives to the following two questions were combined:

- Change in the number of people who came in the establishment during the past year, and
- Change in the amount of business at the establishment during the past year.

o Satisfaction with Area. To ascertain the extent to which residential respondents were satisfied with the area, responses were combined for two items which explored:

- Their perception of the extent to which the area had become a better or worse place in the past year, and
- The extent to which they were satisfied with the area as a place to live.

The answers were combined for two questions asked of non-residential respondents:

- The extent to which the respondent was satisfied with the area as a place for the establishment, and
- The extent to which the area had become better or worse in the past year.

Recorded Crime Data Collection

In additon to the survey measures of attitudes and behaviors, data were collected by the Houston Police Department for the Golfcrest and Shady Acres areas for the periods January through June, 1983 and January through June, 1984.

SUMMARY

The basic evaluation design compared measures of attitudes and reported behaviors collected before and ten months after the introduction of the program. These measures were obtained by conducting inteviews with

random samples of residents and representatives of non-residential establishments in both a program area and in a comparison area, similar to the program area in size and demographic characteristics, in which no new fear reduction activities were undertaken.

The surveys produced area response rates ranging from 75 to 83 percent, easily high enough to allow the results to be taken as representative of the persons living in these neighborhoods. Attempts to conduct interviews with a set of respondents both before and after the program began produced panel response rates of approximately 70 and 53 percent, in the program and comparison areas respectively. Interviews were also conducted with owners, managers or employees of non-residential establishments. The response rates were were consistently higher than 81 percent.

Survey questionnaires were designed to collect information about each of the following:

- Recalled Program Exposure
- Perceived Area Social Disorder Problems
- Perceived Area Physical Deterioration Problems
- Fear of Personal Victimization in Area
- Worry About Property Crime Victimization in Area
- Perceived Area Personal Crime Problems
- Perceived Area Property Crime Problems
- Victimization
- Evaluations of Police Service and Aggressiveness
- Defensive Behaviors to Avoid Personal Crime
- Household Crime Prevention Efforts
- Satisfaction with Area.

Recorded crime data for Part I crimes were also collected, by month, for both areas for the periods January through June, 1983 and January through June, 1984.

ANALYSES AND RESULTS FOR RESIDENTIAL RESPONDENTS

THE RESIDENTIAL DATA

To determine program consequences for residents, the Wave 1 and Wave 2 survey data have been analyzed in two different ways. The first is a pooled cross-sectional analysis which utilizes all respondents in the pre- and post-intervention surveys. Because the respondents involved in the cross-sectional analysis were selected at both Wave 1 and Wave 2 by a statistically randomizing process, these data can be analyzed to provide our best estimate of the effects of the program on the neighborhood as a whole. In Golfcrest, the program area, the Wave 1 survey sample contained 543 respondents; the Wave 2 sample included 560 people. In Shady Acres, the comparison area, the Wave 1 sample was 389; the Wave 2 sample was 403.

The second analysis is of a panel subset which includes all of the respondents in the Wave 1 survey who could be located and reinterviewed at Wave 2. Respondent attrition between the Wave 1 and Wave 2 surveys (see Table 5) would have diminished the likelihood that the panel respondents would be representative of area residents as a whole. Representativeness is more nearly achieved in the cross-sectional analysis. Analysis of the panel data, however, provides our best estimate of the effects of the program on individuals.* In the program area, there were 315 panel respondents; in the comparison area there were 183.

* It should be noted that while the panel data are analyzed completely independently of the cross-sectional data, the panel constitutes 52 percent of the cross-sectional data set.

For the cross-sectional and the panel data sets, three types of analyses have been conducted:

1. Comparisons of means with t-tests to measure the size and significance of Wave 1-Wave 2 differences in levels of program awareness within the program and comparison areas,

2. calculations, for descriptive purposes, of Wave 1-Wave 2 mean scores on outcome measures in the program and comparison areas, and

3. tests of program effects based on regression models. For both the cross-sectional and panel data sets, the data from both survey waves and both areas have been merged and analyzed as one set.

For the panel data only, two additional types of analysis have been conducted:

1. Regression analysis to explore the possible impact of the program on people in the program area who report being aware of the program, and

2. regression analysis to explore possible program impact for demographic subgroups in the program and comparison areas.

The regression models used for the pooled cross-sectional analysis and for the various panel analyses will be discussed in subsequent sections of this chapter.

CROSS SECTIONAL ANALYSIS

Cross-Sectional Respondents: Characteristics

Table 7 provides information about the characteristics of the area level sample in the program and comparison areas for both pre- and post-intervention surveys. In the comparison area, there was a significant ($p \leq .01$)* decrease in the percentage of white respondents.

*In this report, we use a one-tailed test of statistical significance of $p \leq .01$ for simple t-tests. For the regression analysis, where it is possible to control for covariates, the significance level employed is $\leq .05$ in both pooled and panel analyses.

Table 7

CROSS-SECTIONAL SAMPLE DESCRIPTION

WAVE ONE - WAVE TWO SURVEYS

	Golfcrest Program Area		Shady Acres Comparison Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Sex				
Males	46	49	52	50
Females	54	51	48	50
	(543)	(560)	(389)	(402)
	p <.50		p <.70	
Race				
Black	24	25	20	20
White	41	41	55	48
Hispanic	33	32	24	27
Other	2	2	1	6
	(539)	(558)	(388)	(403)
	p <.98		p <.01	
Housing				
Own	41	41	40	35
Rent	59	59	60	65
	(537)	(559)	(388)	(399)
	p <.98		p <.20	
Education				
Not High School	39	35	46	50
High School Graduate	61	65	54	50
	(542)	(559)	(385)	(395)
	p <.30		p <.30	
Income				
Under $15,000	53	46	46	54
Over $15,000	47	54	54	46
	(516)	(523)	(355)	(360)
	p <.05		p <.10	
Age Category				
15-24	19	15	16	17
25-49	55	57	50	48
50-98	26	28	34	34
	(540)	(559)	(385)	(400)
	p <.20		p <.90	

Table 7 continued

CROSS-SECTIONAL SAMPLE DESCRIPTION

WAVE ONE - WAVE TWO SURVEYS

	Golfcrest Program Area		Shady Acres Comparison Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Children at Home				
None	45	53	58	55
One	19	17	18	22
Two+	36	30	24	23
	(542)	(558)	(389)	(399)
	p <.05		p <.50	
Number of adults in household				
One	30	37	31	28
Two	57	53	49	50
Three+	13	10	20	21
	(543)	(559)	(389)	(402)
	p <.02		p <.70	
Marital Status				
Single	42	46	47	46
Married*	58	54	53	54
	(542)	(558)	(386)	(402)
	p <.30		p <.95	
Employment				
Work full-part	62	65	66	67
Other	38	35	34	33
	(542)	(558)	(387)	(402)
	p <.50		p <.80	
Length of Residence				
0--2 years	47	42	47	47
3-5 years	20	21	16	13
6-9 years	9	11	7	8
10 years +	24	25	30	31
	(543)	(558)	(389)	(401)
	p <.50		p <.50	
Know Area Victim				
No	68	77	74	73
Yes	32	23	26	27
	(543)	(560)	(389)	(403)
	p <.01		p <.70	

* Includes "living with someone as partner."

Cross-Sectional Respondents: Program Awareness

As reported earlier, approximately 37 percent of the occupied housing units (and about 14 percent of the individual residents) in Golfcrest were contacted directly by the officers working in the area. Table 8 reports the percentage of area residents in the program and comparison areas who, at the time of the Wave 1 and Wave 2 surveys, reported having received a contact and also the percentage who recalled the recent sighting of a police officer in the neighborhood. Figures are presented from the Wave 2 survey for the percentage of people in each area who said they knew a police officer who worked in the area. In Golfcrest, the program area, there was a statistically significant ($p \leq .01$) nine percentage point, positive Wave 1 - Wave 2 difference in the number of respondents who recalled having a police officer come to their door. There was no difference over time in Shady Acres. In both areas there was a statistically significant, positive difference in the percentage of people reporting they had seen a police officer in their area within the past 24 hours.

Tables 9 through 11 report levels of program awareness for demographic subgroups within the program area. Whites, home owners, persons over 50 years of age, and respondents who have lived in the area more than five years are all more likely than other respondents to say they recall that a police officer came to their door.

There are no statistically significant differences among subgroups in terms of reports of having seen an officer in the area in the previous 24 hours.

Whites, persons earning more than $15,000 a year, home owners, and persons who have lived in the area more than 5 years are more likely than other respondents to say they know an officer who works in the neighborhood.

TABLE 8

PROGRAM AWARENESS
PERCENTAGE OF RESIDENTIAL RESPONDENTS RECALLING
ASPECTS OF THE PROGRAM, PROGRAM AND COMPARISON AREAS

(Cross-Sectional Sample)

		Program Area (Golfcrest)				Comparison Area (Shady Acres)			
		Wave 1	Wave 2	Diff.	Sigf.	Wave 1	Wave 2	Diff.	Sigf.
Type of Exposure									
Recalled police came to door to ask about problems or provide information		3	12	+ 9	.001*	3	3	0	.95
	[N]	[535]	[560]			[386]	[400]		
Had seen police officer									
more than 1 week ago		32	22	- 10		32	25	- 7	
within past week		35	35	0	.001*	38	34	- 4	.01*
within past 24 hours		34	43	+ 9		30	41	+ 11	
	[N]	[543]	[560]			[389]	[403]		
Knew a police officer who worked in the area		---	18			---	8		
	[N]		[557]				[401]		

*Statistically significant at $p \leq .01$.
Chi-square tests of significance.

TABLE 9

PROGRAM AWARENESS

PERCENTAGE OF RESIDENTIAL RESPONDENT BY DEMOGRAPHIC SUBGROUPS WHO RECALLED THAT POLICE CAME TO THE DOOR

(Cross-Sectional Sample, Wave 2, Program Area Only)

Group	Percentage Reporting Recall	(N)	Group	Percentage Reporting Recall	(N)
Sex			Housing		
Male	12	(272)	Own	18	(230)
Female	13	(288)	Rent	8	(329)
	$p < .79$			$p < .002$*	
Race			Age		
Black	11	(138)	15-24	2	(83)
White	18	(227)	25-49	12	(317)
Hispanic	6	(180)	50+	19	(159)
	$p < .002$*			$p < .001$*	
Income			Adults in Household		
Under $15,000	13	(243)	1	10	(208)
Over $15,000	12	(280)	2	14	(295)
	$p < .92$		3+	12	(56)
				$p < .44$	
Education			Length of Residence		
Not H.S.	10	(195)	0-2 years	8	(237)
High School	14	(364)	3-5 years	6	(119)
	$p < .22$		6-9 years	23	(61)
			10+ years	20	(141)
				$p < .001$*	

*Statistically significant at $p \leq .01$.

TABLE 10

PROGRAM AWARENESS

PERCENTAGE OF RESIDENTIAL RESPONDENT BY DEMOGRAPHIC SUBGROUPS WHO RECALLED SEEING OFFICER IN PREVIOUS 24 HOURS

(Cross-Sectional Sample, Wave 2, Program Area Only)

Group	Percentage Reporting Recall	(N)	Group	Percentage Reporting Recall	(N)
Sex			Housing		
Male	45	(272)	Own	40	(230)
Female	41	(288)	Rent	45	(329)
	$p < .31$			$p < .23$	
Race			Age		
Black	49	(138)	15-24	43	(83)
White	40	(227)	25-49	46	(317)
Hispanic	42	(180)	50+	36	(159)
	$p < .23$			$p < .09$	
Income			Adults in Household		
Under $15,000	44	(243)	1	39	(208)
Over $15,000	44	(280)	2	43	(295)
	$p < .92$		3+	52	(56)
				$p < .24$	
Education			Length of Residence		
Not H.S.	40	(195)	0-2 years	45	(237)
High School	44	(364)	3-5 years	49	(119)
	$p < .24$		6-9 years	44	(61)
			10+ years	33	(141)
				$p < .06$	

TABLE 11

PROGRAM AWARENESS

PERCENTAGE OF RESIDENTIAL RESPONDENT BY DEMOGRAPHIC SUBGROUPS WHO RECALLED KNOWING OFFICER IN THE AREA

(Cross-Sectional Sample, Wave 2, Program Area Only)

Group	Percentage Reporting Recall	(N)	Group	Percentage Reporting Recall	(N)
Sex			Housing		
Male	20	(269)	Own	24	(229)
Female	17	(288)	Rent	14	(327)
	$p < .41$			$p < .001$*	
Race			Age		
Black	12	(136)	15-24	13	(83)
White	26	(226)	25-49	18	(314)
Hispanic	13	(180)	50+	20	(159)
	$p < .001$*			$p < .41$	
Income			Adults in Household		
Under $15,000	11	(241)	1	17	(206)
Over $15,000	24	(279)	2	19	(294)
	$p < .001$*		3+	19	(56)
				$p < .85$	
Education			Length of Residence		
Not H.S.	16	(195)	0-2 years	12	(234)
High School	19	(361)	3-5 years	15	(119)
	$p < .50$		6-9 years	31	(61)
			10+ years	26	(141)
				$p < .001$*	

*Statistically significant at $p \leq .01$.

Cross-Sectional Respondents: Wave 1 and Wave 2 Mean Outcome Scores

Table 12 reports Wave 1 and Wave 2 mean scores for measures of fear of victimization, perceptions of area crime and disorder problems, citizen satisfaction with the area in which they live, and attitudes toward the police, reported use of defensive behaviors to avoid personal victimization, and reported victimization. The size and statistical significance of differences in Wave 1 and Wave 2 scale scores are reported for respondents in both the program area, Golfcrest, and the comparison area, Shady Acres. The scores are based on data for all residential respondents in both survey waves. Wave 1 and Wave 2 values for individual items within the scales are presented in Appendix D.

Although levels of significance are reported for these data, they do not represent tests of program effect. These data merely give us a picture of what was happening over time within the areas. They also provide a basis for speculating about alternative explanations of findings of program effects to be presented in a later section.

Table 13 reports data for another outcome measure--Prevalence of Victimization. These figures represent the percentage of persons who recalled being victimized,* in their area, by:

--personal crimes, including: actual and attempted robbery, pursesnatching and pocketpicking, actual and attempted or threatened assault, threats, and sexual assault,

*This measure is different from the "crime rate" or even the "victimization rate." It does not take into account the extent to which persons were multiply victimized during these six-month periods. The survey questionnaire did ask victims "how many times" they were victimized by each type of incident, but those data are prone to recall error. The measures of victimization employed in Table 13 are necessarily insensitive to whether or not fewer people were victimized, but victimized more frequently. However, during a six-month recall period relatively few persons are multiply victimized by the same type of incident, so there will be few differences between the dichotomous measures employed in Table 13 and victimization rate accounts for individuals.

TABLE 12

DIFFERENCES IN WAVE 1-WAVE 2 OUTCOME SCORES FOR CROSS-SECTIONAL SAMPLE RESPONDENTS PROGRAM AND COMPARISON AREAS

		Program Area (Golfcrest)				Comparison Area (Shady Acres)			
		Wave 1	Wave 2	Diff.	Sigf.	Wave 1	Wave 2	Diff.	Sigf.
Outcome Scale									
Fear of Personal Victimization in Area	(sd)	1.80	1.63	- .17	.001*	1.69	1.65	- .04	.25
	(sd)	(.57)	(.58)			(.56)	(.61)		
	[N]	[543]	[559]			[389]	[403]		
Perceived Area Personal Crime Problems		1.54	1.32	- .22	.001*	1.44	1.38	- .06	.10
	(sd)	(.64)	(.51)			(.57)	(.55)		
	[N]	[529]	[554]			[372]	[394]		
Worry About Property Crime Victimization in Area		2.16	1.98	-.18	.001*	1.93	1.85	-.08	.10
	(sd)	(.65)	(.68)			(.67)	(.72)		
	[N]	[542]	[557]			[387]	[401]		
Perceived Area Property Crime Problems		1.84	1.58	-.26	.001*	1.60	1.55	-.05	.25
	(sd)	(.65)	(.65)			(.60)	(.59)		
	[N]	[535]	[555]			[380]	[397]		

*Statistically significant at $p < .01$.
One-tailed significance t-tests.

TABLE 12
(continued)

DIFFERENCES IN WAVE 1-WAVE 2 OUTCOME SCORES FOR CROSS-SECTIONAL RESPONDENTS, PROGRAM AND COMPARISON AREAS

		Program Area (Golfcrest)				Comparison Area (Shady Acres)			
		Wave 1	Wave 2	Diff.	Sigf.	Wave 1	Wave 2	Diff.	Sigf.
Outcome Scale									
Perceived Area Social Disorder Problems	(sd)	1.49	1.30	- .19	.001*	1.40	1.39	- .01	.40
	(sd)	(.46)	(.39)			(.46)	(.47)		
	[N]	[543]	[560]			[387]	[402]		
Satisfaction with Area		2.42	2.64	+ .22	.001*	2.51	2.60	+ .09	.025
	(sd)	(.62)	(.59)			(.61)	(.60)		
	[N]	[543]	[558]			[389]	[403]		
Evaluation of Police Service		3.24	3.49	+ .25	.001*	3.23	3.37	+ .14	.005*
	(sd)	(.71)	(.64)			(.63)	(.71)		
	[N]	[535]	[552]			[372]	[388]		
Perceived Police Aggressiveness		1.22	1.13	- .09	.001*	1.15	1.11	- .04	.10
	(sd)	(.48)	(.35)			(.40)	(.32)		
	[N]	[509]	[552]			[363]	[375]		
Defensive Behaviors to Avoid Personal Victimization		.49	.47	- .02	.25	.44	.47	+ .03	.25
	(sd)	(.34)	(.36)			(.34)	(.35)		
	[N]	[542]	[560]			[387]	[403]		

*Statistically significant at $p \leq .01$.
One-tailed significance t-tests.

--property crimes, including: actual and attempted burglary, thefts from, in, and around the home, mailbox and bicycle theft, home and auto vandalism and motor vehicle theft.

Table 13 reports the frequency of victimization by these broad categories of crimes and also by selected types of incidents, including burglary, motor vehicle crime, and other types of thefts. Also reported is a test of the statistical significance of differences in victimization between the first and second waves of the surveys in each area. These data indicate a significant reduction over time in all types of property crimes in the program area.

We see across all the outcome measures many more statistically significant Wave 1-Wave 2 differences in the program than in the comparison area. The only significant difference in the comparison area was the increase on Evaluation of Police Services. Because this difference occurred in both areas (and in all the Houston test areas), it is likely that there was something happening all over Houston which contributed to this more positive attitude toward the police in all areas. During the project test period, the Houston Police Department appeared to be receiving more positive coverage from the local press than it had in previous years. Some of the stories were related to the Fear Reduction program itself and news of the program also focused national press attention on the Houston Police Department. But Houston's new police chief, Lee Brown, was seen frequently on television during this period discussing various operational aspects of his community-oriented policing philosophy, and there were a number of programs or organizational changes implemented or tested during the program period, both the fact of which and the publicity of which may have contributed to an increasingly positive public image of the Houston police. Again, while interesting in their own right, these data do not provide good evidence of program-based causality. This type of analysis does not control

TABLE 13

PERCENTAGE OF RESPONDENTS REPORTING THEMSELVES TO HAVE BEEN VICTIMS
BY TYPE OF CRIME, WAVE 1 - WAVE 2, PROGRAM AND COMPARISON AREAS

Cross-Sectional Sample

	Program Area (Golfcrest)				Comparison Area (Shady Acres)			
	Wave 1	Wave 2	Diff.	Sigf.	Wave 1	Wave 2	Diff.	Sigf.
Type of Crime								
Personal Crimes	26	21	-5	.05	17	18	+1	.95
Property Crimes	41	24	-17	.001*	31	29	-6	.80
Burglary	15	8	-7	.001*	16	10	-6	.20
Motor Vehicle Crime	18	11	-7	.001*	10	13	+3	.10
Other Theft	18	10	-8	.001*	12	13	+1	.80

*Statistically significant at $p \leq .01$.
One-tailed significance t-tests.

for many possible population differences between the two areas (and over time within each area), and does not tell us whether the changes in the program area are statistically significantly greater than those in the comparison area.

Cross-Sectional Respondents: Program Effects

The much stronger test of area or neighborhood-level effects is provided by a regression analysis in which potentially important outcome covariates can be controlled. Such an analysis was done on a data set which pooled the Wave 1 and Wave 2 data for both the program and comparison areas. The regression model which provides controls for survey wave, area of residence, and covariates is as follows:

$$Y = a + b*COVARIATES + b*WAVE + b*TREAT + b*INTER$$

Where:

Y = an outcome measure;
a = intercept
WAVE = pretest (coded 0) or posttest (coded 1) wave
TREAT = residence in comparison (coded 0) or program (coded 1) area;
INTER = interaction term coded 1 if respondent lives in the program area and it is a posttest interview, and a 0 otherwise;
COVARIATES = indicators modeling differences between residents of the program and comparison areas which potentially are related to the outcome measures (see below.).

The covariates are critical. One of the major design flaws of an area-level quasi-experiment is that residents are not randomly assigned to treatment or comparison status, but rather opt (or are forced, in one fashion or another) into one of the areas. The factors which lie behind their selection of, or assignment to, the program or comparison areas potentially are confounded with the treatment. Program and comparison areas can never be perfectly matched. The goal of the analysis, therefore, is to model the selection process in order to statistically "control" the factors which led them to one neighborhood or the other and which are related to the outcome measures.

The covariates used in this analysis include many of the known correlates of most of the outcome measures for the evaluation. They reflect the respondent's crime experiences and physical vulnerability, the anonymity of their immediate environment, cultural and ethnic differences in experiences with the police, and social supports. Many factors which affect fear and assessments of the police also are linked to residential choice, including income, education, race, household organization, and employment status. Most of the covariates listed here are "demographic" because it is important that they be conceptually and temporally antecedent to the program, and not be affected by it. This is especially critical in the pooled cross-sectional analysis, for half of the respondents were interviewed after the program took place. If factors were included among the covariates which could have been affected by the program (like recent experiences with the police or victimization), controlling for them would "take out" variance also associated with the treatment, and could lead to an underestimate of program effect. Note, however, that their exclusion contributes to the specification bias in the structural models of fear and assessments of the police which guided the selection of the covariates, for the examples given above are important determinants of both outcomes. This problem is rectified in the analysis of panel data (reported in a later section of this chapter), where measures of victimization and assessments of the police taken before the onset of the program can be used as covariates.

Covariates Used in Pooled Cross-Sectional Analyses

Race-black	Origin-hispanic	High school graduate
Age in years	Elderly-over 60	Income (dichotomy)
Gender-female	Married	Length of residence
Own home	Single family home	Work full-part time
Live alone	Household size	Single family head
Poor English	Apartment complex	Number of children

There were scattered missing data for most of the covariates. These were coded at median values or mid-ranges where appropriate. There were more missing data for income (8.5 percent), and those cases were coded midway between the low and high categories. Appendix M reports two analyses which compare results based on "complete cases" data sets and on those excluding missing-data cases. These analyses suggest there is no systematic bias introduced by this procedure.

In addition to identifying the structural model of the selection process, it is important to understand how its components were measured. Unlike the outcome measures, which have known estimated reliabilities, are single factored, and are well distributed, the covariates analyzed here were all measured using single indicators. However, because the interviews were conducted in-person, some covariates (such as sex, observed building type) probably are usually accurate. Others, like race, are conceptually thorny, but are at least respondent-identified categories, and most of the remainder ("working," "married") should be fairly reliably measured by the questionnaire. Income level doubtless is the worst-measured of the covariates, but there are no reliability estimates for any of them.

Because they are intended to model the selection process and adjust for unmatched differences between the treatment and control areas, in this analysis the covariates were forced in before an assessment was made of the significance of other components of the model.

The WAVE measure controls for the main effects of wave of interview. It identifies interviews conducted before and after the onset of the program, and its inclusion should take out the simple, linear effects of history, maturation, and other general over-time changes in both program and comparison areas. It will not account for <u>differences</u> in the magnitude of general temporal shifts between the two areas, however.

The TREATment measure controls for the main effects of area of residence. This is an interesting factor in the model. If the covariates (which were entered first) adequately accounted for selection differences between the two areas which are related to the outcome measures, the regression coefficient for TREAT should approximate zero ("significance" is not the best criterion in this case); there should be no independent effect of area of residence. If the selection model were less adequate, the inclusion of TREAT will serve to take out further unmodeled (or ill-measured) differences between respondents from the two areas. However, as we shall see shortly, the problem of multicollinearity makes this a less desirable solution to the problem than is modeling differential area selection.

Treatment effect is estimated in this analysis by the size and significance of the unstandardized regression coefficient associated with the INTERaction indicator. INTER identifies interviews with (a) residents of the program area conducted (b) after the onset of the program.

One problem with this analysis model is that there inevitably will be a substantial amount of multicollinearity between the WAVE, TREAT, and INTER indicators. This makes it less likely that any significant program effects will be identified. However, because they perform important analytic functions, it clearly would be incorrect to leave out either of the main effect indicators--unless the coefficient associated with area of residence (TREAT) approximates zero because of adequate modeling of the selection process. Unfortunately, while the coefficients for area of residence frequently were insignificant in the multivariate analyses, they sometimes were significant and rarely were zero; thus, they were included in each analysis.

The before-and-after surveys are designed to draw representative sketches of area residents at two points in time. They may better reflect the community-wide effects of a program. However, the absence of a pretest forces us to rely upon covariates which were measured in the surveys to factor out non-program differences between treatment and control individuals, and important differences between residents of the program and comparison areas may not have been included or may have been badly measured.

Note that, after all of this, INTER will continue to be a biased estimator of program affect due to unaccounted-for treatment-by-history and tratment-by-maturation threats to validity, if present.

The results of the pooled analysis are presented in Table 14. The first column reports the sign and size of the regression coefficient associated with living in the program area and being interviewed after program implementation. This is the measure of program effect after the other variables in the model have been taken into account. The second column reports the level of statistical significance of the coefficient.

At the area-level, the citizen contact program appears to be negatively and significantly ($p \leq .05$) associated with indicators of:

Fear of Personal Victimization in the Area,
Perceived Area Personal Crime Problems,
Perceived Area Property Crime Problems,
Perceived Area Social Disorder Problems,
Police Aggressiveness, and
Property Crime Victimization.

Further, the program is positively and significantly associated with the scale, "Satisfaction With The Area."

The contact program appears to have had statistically significant, predicted effects on six of the eight attitude measures of program impact. For the other two attitudes, the effects were in the predicted direction but were not significant.

TABLE 14

PROGRAM EFFECTS FOR CROSS-SECTIONAL SAMPLE RESPONDENTS: REGRESSON COEFFICIENTS AND LEVELS OF SIGNIFICANCE

Outcome Scale	Regression Coefficient (b)	Level of Significance
Fear of Personal Victimization in Area	-.12	.02*
Perceived Area Personal Crime Problems	-.14	.01*
Worry About Area Property Crime Problems	-.10	.10
Perceived Area Property Crime Problems	-.21	.01*
Perceived Area Social Disorder Problems	-.15	.01*
Satisfaction with Area	+.13	.02*
Evaluations of Police Service	+.09	.13
Perceived Police Aggressiveness	-.04	.04*
Defensive Behaviors to Avoid Victimization	-.03	.32
Property Crime Victimization	-.15	.01*
Personal Crime Victimization	-.06	.08
(N)	(1893)	

*Statistically significant at $p \leq .05$.

The program appears to have had no impact, at the area-level, on the two behavioral measures--"Defensive Behaviors" and "Household Crime Prevention."

Somewhat surprisingly, since this effect was not predicted for the program, it is negatively and significantly associated with the measure of property crime victimization. We will consider below possible alternative explanations for this and other findings in the cross-sectional analysis. At this point, however, it might be useful to discuss why we think this program could have had this impact.

The Potential Impact of the Contact Program on Crime. It is possible that the increased presence of officers in the neighborhood would cause potential criminals to avoid the area or to commit criminal acts there less frequently. Not only were these officers in the area more frequently but they were seen in places where people were not accustomed to seeing them (e.g., at doorsteps and in parking losts), engaged in activities which may have appeared unusual (i.e. interviewing a lot of ordinary citizens), and were active at unusual, late hours. (One officer made late night traffic stops in order to chat with people and stopped others as they walked through parking lots to their apartments.) This obvious and extraordinary behavior may have frightened potential offenders away, or the presence and apparent interest of the police may have given local parents of troublesome children more "moral authority" in dealing with disciplinary problems. Perhaps, the information gathered through the contacts might lead officers to make arrests which could in turn deter criminal activity in the area.

Alternative Explanations of Program Effects Detected in Regression Analysis

The two most significant threats to the reliability of these findings (and of those to be presented below for the panel subset) are posed by the possibility of a statistical artifact and by the possibility of differential history in the two areas. The statistical artifact which could be operating in these data is regression toward the mean--a phenomenon that occurs when pre-intervention outcome scores are abnormally high (or low) in the program area and return, over the course of the program period, to their "normal" state (the mean score) for reasons entirely unrelated to the implementation of the program.

There is some support to be found for this alternative explanation in the Wave 1 outcome scores reported in Table 12. In almost every case, the Wave 1 mean outcome score is higher in the program area than in the comparison area. Furthermore, the Wave 2 scores do not differ dramatically between the two areas. And, except for burglary, the same can be found in respondent reports of victimization summarized in Table 13. It is possible that the apparent program impacts on attitudes and reported victimization were the function of a statistical anomaly.

This is not something for which we can test in these data; it is a possible problem to which we can only point with some consternation, noting that this is a condition not unlikely to plague tests in which there is only one program and one comparison area, and only two data points. We might note, however, that regression toward the mean is most likely to occur in those cases in which the program area has been selected precisely because it is perceived as a problem area and one in which the planned program might be

expected or hoped to have an impact. This was not the reason Golfcrest was selected as the site for the Citizen Contact strategy. So far as the Houston officers or researchers knew, there was no reason to expect marked differences in Wave 1 outcome scores among any of the areas considered as test sites. Rather than being chosen for the contact program because it was perceived as an area in need of that particular program, Golfcrest was selected from among the final five matched areas because it was in the patrol district of the officer who had conceived the program and was interested in seeing it implemented. While this was not strictly a random assignment of treatment to area, it was not based on presumptions about area conditions. Table 15 compares Wave 1 outcome scores in the four areas which were used as test sites and the comparison area for the three area-level programs implemented in Houston. On only three of the ten outcome measures in Table 15 was Golfcrest on the highest (or lowest) end of the area scores. This is not an argument that regression toward the mean might not still have occurred in Golfcrest, only an argument that the possibility was not made more probable by the nature of the study design.

Regression toward the mean could have affected reports of victimization in the same way it might have affected other outcome measures. If there was an abnormally high rate of crime in Golfcrest just prior to program implementation, crime might have "slid" back toward the mean, regardless of program efforts. Judging from the data in Table 13, this appears to be a possible explanation; there were more persons in Golfcrest who reported themselves to have been victims at Wave 1 than was the case in Shady Acres. However, when we look at the police department's reports of crime for that same period, (Table 16) we find the incidence of reported burglaries and thefts to have been <u>lower</u> in Golfcrest at Wave 1 than in Shady Acres

TABLE 15

WAVE 1 OUTCOME SCORES FOR FOUR HOUSTON NEIGHBORHOODS

	Areas			
	Golfcrest	Northline	Langwood	Shady Acres
Outcome Scale				
Fear of Personal Victimization in Area	1.80	1.77	1.63	1.69
Perceived Area Personal Crime Problems	1.54	1.61	1.35	1.44
Worry About Area Property Crime Victimization	2.16	2.20	2.00	1.93
Perceived Area Property Crime Problems	1.84	1.98	1.57	1.60
Perceived Area Social Disorder Problems	1.49	1.56	1.41	1.40
Satisfaction with Area	2.42	2.39	2.43	2.51
Evaluations of Police Service	3.24	3.22	3.33	3.23
Perceived Police Aggressiveness	1.22	1.17	1.14	1.15
Property Crime Victimization	26	27	24	17
Personal Crime Victimization	41	36	28	31

TABLE 16

ROBBERIES, BURGLARIES AND THEFTS REPORTED TO POLICE
JANUARY - JUNE, 1983 AND 1984
PROGRAM AND COMPARISON AREAS

	Program Area (Golfcrest)			Comparison Area (Shady Acres)		
	1983	1984	Diff.	1983	1984	Diff.
Type of Crime						
Robberies	19	12	-7	9	10	+1
Burglaries and Thefts (excluding auto)	108	83	-25	137	138	+1

(despite the fact that the percentage of respondents reporting themselves to have been victims of burglaries at Wave 1 was similar in both areas.) Robberies may have been unusually high in Golfcrest at Wave 1, but this does not appear to have been the case for property crime.

There is another alternative explanation for the finding of reduced victimization in Golfcrest; this is the possibility that persons who had been victimized by the time of the Wave 1 survey were more likely to leave the area before the Wave 2 survey. If, for some reason, this movement was more likely to have occurred in Golfcrest than in Shady Acres, reports of prior victimization might be artificially low in Golfcrest at Wave 2. Table 17 explores this possibility. When the survey attrition rate is compared for persons who were or were not victims at Wave 1, we find that Wave 1 victims in Golfcrest were as likely to be reinterviewed at Wave 2 as were respondents who were not victims at Wave 1. In Shady Acres, respondents who had been Wave 1 victims were slightly less likely to be reinterviewed than residents who had been victims. This difference should have produced a bias against finding effects of reduced victimization in the program area.

All of the findings are subject to the possible effects of differential history in the program and comparison areas. It is possible, for example, that something other than the program occurred in Golfcrest--and not in the comparison area--which had the effect of reducing fear and the prevalence of victimization in one area but not the other. This alternative explanation is one which the evaluation plan anticipated. An evaluation observer made regular contact with police personnel in both the program and comparison areas to make certain there were no new police operations being introduced into either area during the period of the test. In addition, she monitored

TABLE 17

REINTERVIEW RATES OF PERSONS WHO WERE VICTIMS* OR NON-VICTIMS AT WAVE 1
PROGRAM AND COMPARISON AREAS

Respondents Who Were:	Program Area (Golfcrest)		Comparison Area (Shady Acres)	
	Percentage	Number	Percentage	Number
Not Victims* at Wave 1				
Reinterviewed at Wave 2	81	[151]	65	[115]
Not found at Wave 2	19	[35]	35	[62]
	100%	[186]	100%	[177]
Victims at Wave 1				
Reinterviewed at Wave 2	81	[164]	60	[66]
Not found at Wave 2	19	[39]	40	[44]
	100%	[203]	100%	[110]

* Includes all forms of victimization.

the media for stories about the area. She learned that a Neighborhood Watch program had been started in Golfcrest almost mid-way between the Wave 1 and Wave 2 surveys. She interviewed the leader of the group and found that in the initial organizing period, the group had been in contact with only about 20 Golfcrest residents. By the end of the test period, the group had not yet undertaken any projects which would have made it highly visible to the neighborhood, and it is believed by both the evaluation observer and the officers working the area that the Neighborhood Watch program had not yet developed to the point that it could have had any measurable effects on levels of fear and worry in the program area.

There also occurred during the test period the brutal murder of a young woman whose body was found on the edge of the program area. If this incident had any effect, it would have been to raise the levels of fear in the Golfcrest neighborhood.

There remains, however, an alternative explanation for the effects reported in Table 14 which we cannot test and about which we can only speculate. It is possible that the reported improvements in attitudes were brought about by the reported reductions in victimization. There is no proof that the reductions in victimization resulted in some way from the contacts made in Golfcrest. It is possible that reduced victimizations were the consequence of something unrelated to the contacts. It is conceivable that one or more individuals had been responsible for a substantial number of property crimes in Golfcrest and that during the program period these offenders left the area, either of their own will or through the actions of officers other than those working in Golfcrest. If the thieves were arrested by officers outside the Golfcrest area, we would not have known about the arrests and even though the persons arrested might have been responsible for crimes in Golfcrest, it is possible that the Golfcrest

officers would not have known of the arrests. It remains a possibility that something unrelated to the contact program caused the decrease in reported property victimization and that this decrease, rather than any aspect of the contact program itself, was the actual cause of the improved attitudes in Golfcrest.

Finally, alternative explanations may lurk in uncontrolled differences between the program and comparison areas and between the people who live in them. Those are confounded with potential program effects because there was no random allocation of persons into treatment or control status to equate them on other factors. That is, we cannot be sure that outcome differences between people in the program and control areas, or even changes in the outcomes for two areas over the course of a year, were due to the program, or to those other factors. Regression-based, quasi-experimental analyses attempt to compensate for this by "controlling" statistically for those other differences between people. This is typically done using multiple regression, entering a measure of program exposure along with other control variables to predict outcome scores. The more credible the claim that (a) all relevent differences between people in the two areas other than program exposure have been identified, that (b) those differences have been perfectly measured, and (c) that linear regression (or any other statistical model) perfectly captures their relationship to the outcome measure, the more credible the quasi-experiment.

We make no such claims here. In the absence of firm data on a-c above, the best substitute is a pre-test outcome score. A pretest score for an outcome variable should capture most of the measurable sources of variation in the post-test outcome variable which are not attributable to the program. To make use of these pretest scores, we must now turn to the analysis of the data from the panel samples.

PANEL ANALYSIS

Panel Respondents: Characteristics

In the program area there were 315 respondents in the panel sample; there were 181 in the comparison area.

The second and fourth columns of Table 18 provide descriptive data about the characteristics of the panel respondents in both the program and comparison areas. The first and third columns provide the same information for the first wave of the cross-sectional respondents. As tends to be the case in panel studies, the persons who were relocated for Wave 2 were more likely to be home owners, to have lived in the area a longer time, and to be older than the larger sample interviewed at Wave 1.

Panel Respondents: Program Awareness

Table 19 reports the extent to which panel respondents recalled elements of the contact program. In Golfcrest, the program area, there was a statistically significant ($p \leq .01$) eleven percentage point, positive Wave 1-Wave 2 difference in the number of respondents who recalled having a police officer come to their door. There was no difference over time in Shady Acres. In both areas there was a statistically significant, positive difference in the percentage of people reporting they had seen a police officer in their area within the past 24 hours. The findings are very similar to those for the cross-sectional sample.

Panel Respondents: Wave 1 and Wave 2 Mean Outcome Scores

Table 20 presents for the panel respondents in each area the mean outcome scores for both waves of the survey. Within the program area there were significant differences on 7 out of 9 outcome measures over time; there were no significant Wave 1-Wave 2 differences in the comparison area. As with the cross-sectional sample, these data are presented for their descriptive utility and are not to be taken as tests of program effect.

TABLE 18

COMPARISON OF CROSS-SECTIONAL SAMPLE AND PANEL SAMPLE CHARACTERISTICS, PROGRAM AND COMPARISON AREAS, WAVE 1

Sample Characteristics	Program Area (Golfcrest) Cross-Sectional	Program Area (Golfcrest) Panel	Comparison Area (Shady Acres) Cross-Sectional	Comparison Area (Shady Acres) Panel
Sex				
Males	46	41	52	47
Females	54	59	41	53
	(543)	(315)	(389)	(181)
	$p < .10$		$p < .30$	
Race				
Black	24	21	20	24
White	41	45	55	55
Hispanic	33	32	24	20
Other	2	2	1	1
	(539)	(314)	(388)	(181)
	$p < .80$		$p < .70$	
Housing				
Own	42	56	40	54
Rent	58	44	60	46
	(537)	(315)	(388)	(181)
	$p < .001$*		$p < .01$*	
Education				
Not high school	39	37	46	55
High school graduate	61	63	54	45
	(542)	(314)	(385)	(179)
	$p < .70$		$p < .05$	
Income				
Under $15,000	53	46	46	47
Over $15,000	47	54	54	52
	(516)	(299)	(355)	(163)
	$p < .05$		$p < .90$	
Age				
15-24	19	14	16	8
25-49	55	51	50	45
50-98	26	35	34	47
	(542)	(313)	(385)	(180)
	$p < .02$		$p < .01$	

continued

*Statistically significant at $p \leq .01$.
Note: Both columns for each area drawn from Wave 1 data.

TABLE 18
(continued)

COMPARISON OF CROSS-SECTIONAL SAMPLE AND PANEL SAMPLE CHARACTERISTICS, PROGRAM AND COMPARISON AREAS, WAVE 1

Sample Characteristics	Program Area (Golfcrest)		Comparison Area (Shady Acres)	
	Cross-Sectional	Panel	Cross-Sectional	Panel
Children at Home				
None	46	49	58	60
One +	54	51	42	40
	(542)	(315)	(389)	(181)
	$p < .50$		$p < .70$	
Number of adults in Household				
One	30	33	31	28
Two	57	57	49	52
Three +	13	10	19	20
	(542)	(315)	(389)	(181)
	$p < .30$		$p < .80$	
Marital Status				
Single	48	48	52	46
Married	52	52	48	54
	(542)	(315)	(386)	(181)
	$p < .90$		$p < .20$	
Employment				
Work full or part time	62	59	66	60
Other	38	41	34	40
	(542)	(315)	(387)	(181)
	$p < .50$		$p < .30$	
Length of Residence				
0-2 years	47	35	47	31
3-5 years	20	22	16	17
6-9 years	9	11	7	8
10+ years	24	32	30	49
	(543)	(315)	(389)	(181)
	$p < .01$*		$p < .01$*	

*Statistically significant at $p \leq .01$.
Note: Both columns for each area drawn from Wave 1 data.

TABLE 19

PROGRAM EXPOSURE:
PERCENTAGE OF RESIDENTIAL RESPONDENTS RECALLING
ASPECTS OF THE PROGRAM, PROGRAM AND COMPARISON AREAS

(Panel Respondents Only)

		Program Area (Golfcrest)				Comparison Area (Shady Acres)			
		Wave 1	Wave 2	Diff.	Sigf.	Wave 1	Wave 2	Diff.	Sigf.
Type of Exposure									
Recalled police came to door to ask about problems or provide information	[N]	4 [310]	15 [310]	+11	.001*	3 [180]	2 [180]	- 1	.40
Had seen police officer									
more than 1 week ago		36	20	-16		33	22	-11	
within past week		36	36	0	.001*	39	34	- 5	.001*
within past 24 hours		28	44	+ 6		29	44	+15	
	[N]	[315]	[315]			[181]	[181]		
Knew a police officer who worked in the area		15	22	+ 7	.01*	5	8	+ 3	.06
	[N]	[312]	[312]			[175]	[175]		

*Statistically significant at $p \leq .01$.
Chi-square tests of significance.

TABLE 20

DIFFERENCES IN WAVE 1-WAVE 2 OUTCOME SCORES FOR PANEL RESPONDENTS, PROGRAM AND COMPARISON AREAS

		Program Area (Golfcrest)				Comparison Area (Shady Acres)			
		Wave 1	Wave 2	Diff.	Sigf.	Wave 1	Wave 2	Diff.	Sigf.
Outcome Scale									
Fear of Personal Victimization in Area	(sd)	1.84 (.57)	1.65 (.58)	-.19	.001*	1.70 (.56)	1.65 (.58)	-.05	.12
	[N]	[314]				[181]			
Perceived Area Personal Crime Problems		1.51	1.31	-.20	.001*	1.40	1.33	-.07	.07
	(sd)	(.62)	(.49)	-.20	.001*	(.55)	(.51)		
	[N]	[303]				[169]			
Worry About Property Crime Victimization in Area		2.15	1.93	-.22	.01*	1.92	1.87	-.05	.19
	(sd)	(.65)	(.68)			(.66)	(.69)		
	[N]	[313]				[179]			
Perceived Area Property Crime Problems		1.83	1.51	-.32	.01*	1.56	1.50	-.06	.13
	(sd)	(.65)	(.60)			(.56)	(.57)		
	[N]	[307]				[171]			
Perceived Area Social Disorder Problems		1.47	1.30	-.17	.001*	1.38	1.38	.001	.50
	(sd)	(.52)	(.41)			(.47)	(.45)		
	[N]	[315]				[179]			

continued

*Statistically significant at $p \leq .01$.
One-tailed significance t-tests.

TABLE 20
(continued)

DIFFERENCES IN WAVE 1-WAVE 2 OUTCOME SCORES FOR PANEL RESPONDENTS, PROGRAM AND COMPARISON AREAS

		Program Area (Golfcrest)				Comparison Area (Shady Acres)			
		Wave 1	Wave 2	Diff.	Sigf.	Wave 1	Wave 2	Diff.	Sigf.
Outcome Scale									
Satisfaction with Area		2.43	2.68	+.25	.001*	2.48	2.54	+.06	.14
	(sd)	(.62)	(.60)			(.62)	(.58)		
	[N]	[315]				[181]			
Evaluations of Police Service		3.27	3.56	+.29	.001*	3.29	3.40	+.11	.25
	(sd)	(.73)	(.63)			(.69)	(.70)		
	[N]	[307]				[168]			
Perceived Police Aggressiveness		1.17	1.12	-.05	.03*	1.15	1.11	-.04	.15
	(sd)	(.45)	(.33)			(.40)	(.33)		
	[N]	[303]				[161]			
Defensive Behaviors to Avoid Personal Victimization		.51	.49	-.02	.16	.42	.48	+.06	.04
	(sd)	(.32)	(.36)			(.34)	(.35)		
	[N]	[315]				[179]			

*Statistically significant at $p \leq .01$.
One-tailed significance t-tests.

Panel Respondents: Program Effects

The preceding pooled, cross-sectional analysis of consequences for the neighborhood was based on two relatively independent surveys (about a 52 percent overlap) of the program and control areas, taken before and after the intervention. Those surveys were designed to be representative of the residents of the areas at those two points in time, and are our best descripton of the impact of the program on the neighborhood. Stronger tests of program effects can be made using data collected from the same individuals (a panel) at two points in time. These data permit tests of the effects of factors which may not be captured in the covariates used in the cross-sectional analysis but which might be represented by the pre-test scores for the outcome variables. Panel analysis can thus provide a more reliable test of the program impact, at least for the panel of individuals involved in the analysis.

Such data exist in the Fear Reduction surveys, since an effort was made to reinterview at Wave 2 each of the persons who was a respondent in Wave 1. For Golfcrest the resulting "panel" consists of 58 percent (N = 318) of the individuals who participated in the Wave 1 survey. For Shady Acres 46 percent (N = 181) of the Wave 1 sample were reinterviewed for the panel. The effects of the contact program on these panel members have been examined using a quasi-experimental form of analysis. It involves a regression-based model of analysis of covariance described below.

POSTTEST = a + b*PRETEST + b*TREAT + b*COVARIATES

Where:

POSTTEST = scale scores for an outcome measure;
a = intercept
PRETEST = scale scores for a pretest measure;
TREAT = residence in comparison (coded 0) or program (coded 1) area;
COVARIATES = indicators modeling differences between residents of the program and comparison areas which potentially are related to the outcome measures.

Treatment effect is estimated by the significance levels associated with the b's for TREATment area of residence. The COVARIATES (see page 66) control for a number of known correlates of the outcome measures which also may be related to area of residence. The PRETEST is a very important control for unmeasured covariates, and is the primary rationale for collecting panel data. The panel design also enables us to include as covariates pre-test measures of direct victimization (total, personal, and burglary) and vicarious victimization (knowing area crime victims), factors which in the cross-sectional analysis had to be excluded because they were potentially confounded with program effects.

The panel data provide important measures repeated over time among the same set of respondents. They present stronger evidence of true individual-level change. That change may or may not be related to the intervention--that is a research design issue. The change also may not be "true," but rather a reflection of measurement instability, a point we soon will discuss in greater detail.

Table 21 presents the results of the panel analysis. In this analysis we find living in the program (treatment) area to be positively and significantly ($p \leq .05$) associated with:

Satisfaction with Area, and
Evaluations of Police Service, and

negatively and significantly associated with:

Perceived Area Social Disorder, and
Property Crime Victimization.

Among panel respondents the program appears, then, to have had statistically significant effects on three out of eight attitudinal measures of impact and

TABLE 21

PROGRAM EFFECTS FOR PANEL SAMPLE RESPONDENTS:
REGRESSON COEFFICIENTS AND LEVELS OF SIGNIFICANCE

Outcome Scale	Regression Coefficient (b)	Level of Significance
Fear of Personal Victimization in Area	-.07	.16
Perceived Area Personal Crime Problems	-.08	.30
Worry About Area Property Crime Victimization	-.04	.48
Perceived Area Property Crime Problems	-.09	.10
Perceived Area Social Disorder Problems	-.13	.01*
Satisfaction with Area	+.15	.01*
Evaluations of Police Service	+.22	.01*
Perceived Police Aggressiveness	+.01	.59
Defensive Behaviors to Avoid Victimization	-.01	.74
Property Crime Victimization	-.11	.01*
Personal Crime Victimization	-.02	.60
(N)	(494)	

*Statistically significant at $p \leq .05$.

on two out of three measures of victimization. All other measures of effect were in the predicted direction but were not statistically significant.

Since the analysis for panel effects involved the same respondents at two points in time, the findings of impact are not subject to the question of whether there were differences in the characteristics of the Wave 1 and Wave 2 samples. There is, however, the possibility that differences may have developed over time within either the Golfcrest or Shady Acres panel (or in both); that is, people in either area may have experienced personal changes which would affect their responses to fear inducing or reducing stimuli. If, for example, more people in the Golfcrest panel married (or divorced) and became employed (or unemployed) during the year than was the case in Shady Acres, the Golfcrest panel might register lower fear levels in the Wave 2 survey for reasons independent of the contact strategy. Table 22 compares two potentially changeable characteristics (i.e. marital status and employment status) of the panel respondents in both areas at Wave 1 and Wave 2. There were no significant changes within either the Golfcrest or Shady Acres panels between Wave 1 and Wave 2.

Another possible explanation is that there were unmeasured personal differences in respondents that varied systematically by area and these differences are related to the tendency to experience or express fear. The pre-intervention, Wave 1 test scores were the principal means of controlling statistically for measurable sources of variation. However, differences between residents of the program areas not captured by the pretest or the

TABLE 22

POTENTIAL "TRUE" CHANGES IN PANEL COMPOSITION
BETWEEN WAVE 1 AND WAVE 2

	Program Area (Golfcrest)		Comparison Area (Shady Acres)	
Panel Characteristics	Wave 1	Wave 2	Wave 1	Wave 2
Marital Status				
Single	44	42	46	45
Married	56	58	54	55
	[315]	[315]	[181]	[181]
	$p < .30$		$p < .52$	
Employment Status				
Not Working	41	38	40	36
Working--Full or Part Time	59	62	60	64
	[315]	[315]	[181]	[181]
	$p < .17$		$p < .30$	

Note: Two-tailed paired sample t-test

other covariates examined here remain threats to the inference that the program "worked."

Additionally, there is a technical issue--that of a differential reliability of measurement--which can affect the otherwise straightforward nature of this type of analysis. Both the pretest and posttest measures of outcomes are fallible indicators of the true levels of fear, etc., of our survey respondents. This has two implications. One is that the statistical tests conducted above using multiple regression probably underestimate the true relationship between the pretest and post-scores which we controlled for--it would have been stronger, and we would have "taken out" more variation in the posttest score with the pretest score, if the measures were better. Second, if the pretest and posttest scores for an outcome are prone to different levels of error, then using the pretest to "adjust" the posttest for "how people stood before the program began" can produce biased results.

Nothing can be done about the first problem, for all indicators of hypothetical constraints are errorful. Two things can be done to deal with the second problem. The first is to examine whether or not there <u>is</u> differential reliability of measurement in the two waves of measures of outcomes and the second is to statistically adjust estimates of the pretest/posttest relationship for those reliabilities. In practical effect, this latter step only changes the results if the pretest and posttest reliabilities for a measure are substantially different. Appendix D presents a tabulation of the scale reliabilities for each outcome measure, for both the pre- and post-intervention surveys, for each area. It suggests

that the reliabilities of the scales were approximately the same for both pretest and posttest measures, alleviating in large part our second concern.

Perhaps the most troublesome alternative explanation of these findings is the possibility of regression toward the mean having occurred in the program area. (This problem was discussed in detail in a previous section dealing with the cross-sectional findings.) Similar to the situation with the cross-sectional respondents, we can see in Table 20 that the panel respondents in the program area had Wave 1 outcome scores which, on 6 out of 9 measures, were markedly higher (or lower) than the scores for respondents in the comparison area. On the two attitudinal outcomes (Satisfaction with the Area and Evaluations of Police Service) on which program and comparison respondents were close at Wave 1, the Wave 2 scores for the program area respondents are quite different from (better than) those of the comparison area respondents. Regression toward the mean over time probably would not explain why scores on these two outcomes moved well beyond the mean. However, there is no way to detremine whether these or other variables were subject to regression to the mean when we have data for only two periods from only one program area. The possible impact of regression toward the mean in the analysis of this program constitutes one of the arguments for replicating the strategy in a number of areas.

Finally, another alternative explanation is that some event or other activity impacted Golfcrest during the year of the contact strategy test in such a way as to lower levels of fear and concern. Apparent program effects might be due, then, to another program or condition rather than due to the contacts. There are no hard data which can be used to test this hypothesis.

However, this possibility was closely monitored by the evaluation observer and, as noted previously, she was able to identify no other event, program or condition, through interviews or through monitoring media coverage, which could have been expected to cause the reported outcomes in Golfcrest. However, there remains the possibility that something occurred which eluded documentation. As discussed previously, something may have happened (e.g., arrests) to reduce reported victimization which in turn could have affected the attitude changes in ways unrelated to the contact program.

Generalizability of Panel Findings

The significant regression coefficients reported in Table 21 provide evidence that the contact program had desirable impacts on perceptions of area disorder problems, satisfaction with the area, evaluations of police service, and victimization by property crimes.

To what extent are these findings generalizable--either to the Golfcrest area as a whole or to areas beyond Golfcrest? The first answer depends on the extent to which the characteristics of the panel sample match those of the larger populations. As we already have seen in Table 14, attrition* caused the panel samples in both areas to differ in some respects from the area-wide samples. In Golfcrest panel respondents were significantly more likely than cross-sectional respondents to be home owners and to have lived in the area for a longer period of time. In Shady Acres

*As a result of attrition, panel surveys inevitably are biased against (a) persons who move out of the area and are lost, (b) recent in-movers who could not have participated in the first wave of the survey, and (c) those who refuse to be reinterviewed.

the panel respondents were significantly older, more likely to own their own homes and to have lived in the area longer than the cross-sectional respondents. In both areas, these characteristics of the panel should predict, given the Wave 1 fear scores of these subgroups (See Appendix H), that the panels would tend to be more fearful than the cross-sectional respondents. However, the comparison in Table 23 of the Wave 1 fear scores for both the area (cross-sectional) and the panel samples indicates this was not the case; the differences between them were very small.

Despite their Wave 1 similarity, the area and panel analyses pointed to somewhat different effects of the contact program; and there were more significant effects for the pooled than for the panel analysis. We cannot determine whether these differences are due to the fact that the two data sets were subjected to different types of analyses, are due to the differential receptivity to the program on the part of respondents in the two types of samples, or are due to the effects of panel respondents having been interviewed twice in a year rather than only once (the case for the cross-sectional respondents).* Given the inability to distinguish among these possible explanations, it is simply safest to say two different ways of analyzing the data point to somewhat different results. We do, however, feel greater confidence in results that are duplicated in the two types of analysis.

*Although it appears not to be the case in this evaluation, (See Tables 14 and 21), it could be possible for an outcome to have the same size regression coefficient in both the pooled and panel analyses but to show different levels of significance as a results of different sample sizes. The same size coefficient would be less likely to be significant in the panel than in the pooled analysis.

TABLE 23

COMPARISON OF WAVE 1 AREA SAMPLE AND PANEL OUTCOME SCORES, FEAR AND PERCEPTIONS OF CRIME PROBLEMS PROGRAM AND COMPARISON AREAS

Outcome Scale	Program Area (Golfcrest)		Comparison Area (Shady Acres)	
	Wave 1 Area	Wave 1 Panel	Wave 1 Area	Wave 1 Panel
Fear of Personal Victimization in Area	1.81	1.84	1.69	1.70
Perceived Area Personal Crime Problems	1.54	1.51	1.44	1.40
Perceived Area Property Crime Problems	1.96	1.96	1.73	1.69

Extending the panel findings to other groups can be done only with caution. Being able to do so would depend on the other groups being similar to the panel and on their living in an area similar to Golfcrest, for that is the context in which effects were found. Similarly, the area-level findings are only generalizable to the extent that other neighborhoods are similar to Golfcrest as it was in 1983 and 1984. This is the reason attention was given in the beginning of this report to the nature of the Golfcrest area. Golfcrest was not an area where either crime or fear were extremely high. It was a neighborhood with only small pockets of physical deterioration but not one which appeared on the edge of imminent decay. It was not an area where police or outsiders had any sense of threat to their own safety. This was the setting in which the program appeared to work. We cannot say how it would fare in areas much better or much worse than Golfcrest. However, the strength of the Golfcrest findings, whether for the panel or the neighborhood, suggest that this program was sufficiently successful to deserve repeated tests in different kinds of settings, with populations of different types of individuals than were found in Golfcrest.

As a final comment on generalization, the obvious should perhaps be stated: these findings can, at best, be projected to implementations of the strategy which are at least as good as the Houston implementation. In this case the project was managed by one highly conscientious patrol officer while over 50 percent of the field work was done by another who was also productive.

Program Effects for Panel Members in Subgroups

Thus far, we have examined the impact of the program only for our neighborhood and panel samples as a whole. However, it is possible that a program like this could have a special impact upon selected subgroups of

the population, while having few--or different--consequences for others in the area. For example, this type of police operation might reduce the fear of people who generally are vulnerable to victimization and fear or who have had past experiences with crime, but not other groups. These are hypotheses about "treatment-covariate interaction." Such hypotheses imply that program contact (treatment) had special impact (an interaction effect) upon subgroups defined by particular factors (covariates).

Hypotheses about special impacts can be tested by including interaction measures in multiple regression analysis. Table 24 presents this type of analysis for seven population subgroups which are identified by measures of:

- age (the impact of the program upon older people),
- sex (the impact of the program upon females),
- victimization (the impact of the program upon victims identified by the Wave 1 survey),
- single family home (the impact of the program upon persons living in detached, one unit houses),
- ethnicity (the impact of the program upon hispanics and asians),
- race (the impact of the program upon blacks),
- renter (the impact of the program upon persons living in rented housing).

The table indicates the direction of the effect on the outcome measures of "being in a subgroup and living in the treatment area" (positive effect is +; negative effect is -) and the statistical significance of that effect. The coefficient indicators presented in Table 24 take into account the pretest score for each outcome, residence in the target or comparison area (our measure of program exposure), and the simple linear effect of being a group member. People who score high in the remaining interaction measure described here were (a) in the group, and (b) in the program area. (Construction of the outcomes scales is discussed in Appendix D. Values of the coefficients are presented in Appendix I.)

TABLE 24

PROGRAM EFFECTS FOR PANEL RESPONDENTS IN SUBGROUPS: COMBINED EFFECTS OF RESIDENCE IN THE PROGRAM AREA AND MEMBERSHIP IN THE SUBGROUP ON THE OUTCOME SCORES

(Panel Respondents)

	Subgroup: Age 65 and Over		Subgroup: Female	
Wave 2 Outcome	Effect	Sigf.	Effect	Sigf.
Fear of Personal Victimization in Area	+	.46	-	.30
Perceived Area Personal Crime Problems	-	.38	-	.95
Worry About Property Crime Victimization in Area	-	.92	+	.72
Perceived Area Property Crime Problems	-	.65	-	.50
Perceived Area Social Disorder Problems	-	.80	+	.40
Satisfaction with Area	+	.94	-	.22
Evaluation of Police Service	+	.49	-	.08
Total Victimization**	+	.31	0	.98

continued

* Statistically significant at $p \leq .05$.

** Dichotomy: victim or nonvictim.

Note: N is approximately 498 for all analyses.

TABLE 24
(continued)

PROGRAM EFFECTS FOR PANEL RESPONDENTS IN SUBGROUPS:
COMBINED EFFECTS OF RESIDENCE IN THE PROGRAM AREA AND
MEMBERSHIP IN THE SUBGROUP ON THE OUTCOME SCORES

(Panel Respondents)

	Subgroup: Wave 1 Victim		Subgroup: Asian/Hispanic	
Wave 2 Outcome	Effect	Sigf.	Effect	Sigf.
Fear of Personal Victimization in Area	-	.78	-	.46
Perceived Area Personal Crime Problems	+	.41	-	.20
Worry About Property Crime Victimization in Area	?	?	+	.98
Perceived Area Property Crime Problems	?	?	-	.48
Perceived Area Social Disorder Problems	+	.23	-	.04*
Satisfaction with Area	+	.95	+	.07
Evaluation of Police Service	+	.38	+	.42
Total Victimization**	***	***	-	.12

continued

* Statistically significant at $p \leq .05$.
** Dichotomy: victim or nonvictim.
***Cannot be determined since outcome is part of subgroup definition.
? Data missing from analysis.

Note: N is approximately 498 for all analyses.

TABLE 24
(continued)

PROGRAM EFFECTS FOR PANEL RESPONDENTS IN SUBGROUPS:
COMBINED EFFECTS OF RESIDENCE IN THE PROGRAM AREA AND
MEMBERSHIP IN THE SUBGROUP ON THE OUTCOME SCORES

(Panel Respondents)

	Subgroup: Black		Subgroup: Renter	
Wave 2 Outcome	Effect	Sigf.	Effect	Sigf.
Fear of Personal Victimization in Area	+	.22	+	.33
Perceived Area Personal Crime Problems	+	.03*	+	.05*
Worry About Property Crime Victimization in Area	+	.27	+	.40
Perceived Area Property Crime Problems	+	.06	+	.31
Perceived Area Social Disorder Problems	+	?	+	.34
Satisfaction with Area	-	.01*	-	.01*
Evaluation of Police Service	-	.01*	-	.01*
Total Victimization**	+	.03*	-	.40

continued

* Statistically significant at $p \leq .05$.
** Dichotomy: victim or nonvictim.
? Data missing from analysis.

Note: N is approximately 498 for all analyses.

TABLE 24
(continued)

PROGRAM EFFECTS FOR PANEL RESPONDENTS IN SUBGROUPS: COMBINED EFFECTS OF RESIDENCE IN THE PROGRAM AREA AND MEMBERSHIP IN THE SUBGROUP ON THE OUTCOME SCORES

(Panel Respondents)

	Subgroup: Single Family Home	
Wave 2 Outcome	Effect	Sigf.
Fear of Personal Victimization in Area	-	.41
Perceived Area Personal Crime Problems	-	.07
Worry About Property Crime Victimization in Area	+	.83
Perceived Area Property Crime Problems	-	.07
Perceived Area Social Disorder Problems	-	.71
Satisfaction with Area	+	.30
Evaluation of Police Service	+	.01*
Total Victimization**	-	.37

* Statistically significant at $p \leq .05$.

** Dichotomy: victim or nonvictim.

Note: N is approximately 498 for all analyses.

There were no program predictions which were specific to any of these subgroups. The Fear Reduction Task Force believed, from their reading of the literature on fear, that women, persons over the age of 65, persons living alone and persons who had previously been victimized were more likely to be fearful than persons who did not belong to these groups. However, there were no plans in this strategy to target any of these groups any differently than any other subgroup; in fact, the officers monitored their efforts to make sure they were contacting each major population subgroup in numbers proportionate to the subgroup's numbers in the Golfcrest population. Although they knew that some groups were reportedly more fearful, they did not know how each might react to the strategy. It was possible that fearful individuals might be more reluctant to open their doors and would, for that reason alone, be less affected by this strategy than might otherwise be expected given their initial levels of fear. And it isn't known, in general, whether more fearful persons are more or less amenable to any fear reduction techniques; they may be more fearful because they are more resistant to programs or information aimed at fear reduction. Without a basis for hypotheses, then, we merely present the subgroups' data for examination and discussion.

Age 65 and Over. The effects are inconsistent for this group, but none of them are statistically significant.

Female. The coefficients indicate some adverse program effects for women, but none of these is significant.

Wave 1 Victim. Effects for this group are generally as desired but, again, there is no statistical significance.

Single Family Home. Living in a single family home in Golfcrest was associated in all of the desirable ways with the program outcome measures, and the positive relationship with evaluations of police service is statistically significant ($p = .01$).

Asian/Hispanic. All of the apparent effects for these individuals were beneficial, although only the negative association with the tendency to perceive disorder as an area problem was statistically significant ($p = .04$).

Black. The effects of the program for black residents of Golfcrest need to be examined carefully. Judging from the regression analysis, the combined condition of living in Golfcrest and being black appear to be positively and significantly ($p = .03$) related to the tendency to perceive area personal crime as a problem, to be negatively and significantly associated ($p = .01$) with satisfaction with the area. It is negatively and significantly associated ($p = .01$) with evaluations of police service and positively and significantly associated ($p = .03$) with total victimization.
Each of these findings is contrary to desired program effects.

Renter. This is another group for which the program does not appear to have worked. Indeed, all of the relationships are in a direction opposite of what might be hoped, and three are statistically significant. Being a renter in the panel in Golfcrest is positively and significantly associated ($p = .05$) with the tendency to perceive area personal crime as a big problem. It is negatively and significantly associated ($p = .01$) with satisfaction with the area and is negatively and significantly associated ($p = .01$) with evaluation of police service.

Discussion of Effects for Blacks and Renters. That the regression results for blacks and renters should be similar is not surprising, since there is a considerable overlap in the program area between these two groups. But before considering why the program effects, as measured by the covariate analysis, appear undesirable for these groups, it is worth asking whether the apparent adverse effects are real. It is possible, given the nature of the regression analysis of covariates, that blacks, for example, could have registered improvements on the outcome scores from Wave 1 to Wave 2 but still be assigned a negative regression coefficient in an analysis which compares program impacts on them with program impacts on whites. Both groups could have positive changes over time, but if the change for whites was greater than the change for blacks, then the regression coefficient for the subgroup of blacks would be negative. The same could be true for renters if they experienced changes over time which were positive, but not as large, as the changes for home owners. Table 25 examines the Wave 1 and Wave 2 outcome scores for blacks in the panels in the program and comparison areas for those outcomes for which this group appear to have experienced adverse program impacts.

What we learn from this table is that the reality of program effects for blacks in the program area is very complex. Put most simply, on none of the outcomes for which a negative regression coefficient was found for them did blacks register a statistically significant difference of means over time which could suggest undesirable program impacts. There is no evidence, then, that blacks experienced adverse consequences of the program. At the same time, neither did they register significantly beneficial changes

TABLE 25

WAVE 1 AND WAVE 2 OUTCOME SCORES FOR SELECTED PROGRAM EFFECTS FOR RACIAL AND HOUSING SUBGROUPS IN PROGRAM AND COMPARISON AREAS

(Panel Respondents Only)

	Program Area (Golfcrest)					Comparison Area (Shady Acres)				
Outcome Scale and Subgroup	Wave 1	Wave 2	Diff.	Sigf.	[N]	Wave 1	Wave 2	Diff.	Sigf.	[N]
Perceived Area Personal Crime Problems										
Blacks	1.28	1.24	-.04	.35	[64]	1.32	1.13	-.19	.01*	[41]
Whites	1.54	1.29	-.25	.001*	[132]	1.42	1.37	-.05	.25	[90]
Hispanics	1.60	1.32	-.28	.001*	[102]	1.46	1.48	+.02	.45	[37]
Satisfaction with Area										
Blacks	2.62	2.58	-.04	.35	[66]	2.63	2.70	+.07	.30	[43]
Whites	2.36	2.69	+.33	.001*	[141]	2.42	2.50	+.08	.13	[99]
Hispanics	2.42	2.74	+.32	.001*	[103]	2.43	2.43	.00	.99	[37]
Evaluation of Police Service										
Blacks	3.14	3.28	+.14	.09	[66]	3.52	3.51	-.01	.50	[39]
Whites	3.40	3.74	+.34	.001*	[136]	3.30	3.43	+.13	.04	[95]
Hispanics	3.17	3.50	+.33	.001*	[100]	2.97	3.17	+.20	.09	[33]

continued

*Statistically significant at $p \leq .01$.
Significance test is one-tailed paired sample t-test of proportions.
Note: Race variable excludes "Other" category (N=7).

TABLE 25
(continued)

WAVE 1 AND WAVE 2 OUTCOME SCORES FOR SELECTED PROGRAM EFFECTS FOR RACIAL AND HOUSING SUBGROUPS IN PROGRAM AND COMPARISON AREAS

(Panel Respondents Only)

	Program Area (Golfcrest)					Comparison Area (Shady Acres)				
Outcome Scale and Subgroup	Wave 1	Wave 2	Diff.	Sigf.	[N]	Wave 1	Wave 2	Diff.	Sigf.	[N]
Victimization--All Types										
Blacks	56	48	- 8	.18	[66]	26	28	+ 2	.40	[43]
Whites	54	33	-21	.001*	[141]	38	42	+ 4	.50	[99]
Hispanics	43	36	- 7	.10*	[103]	43	51	+ 8	.25	[37]
Victimization--Property Crimes										
Blacks	45	27	-28	.005*		16	19	+ 3	.35	
Whites	37	18	-29	.001*		26	28	+ 2	.40	
Hispanics	36	20	-16	.001*		40	38	- 2	.40	
Victimization--Personal Crimes										
Blacks	27	29	+ 2	.50		12	14	+ 2	.40	
Whites	30	20	-10	.03		20	21	+ 1	.50	
Hispanics	26	22	- 4	.25		14	24	+10	.10	

*Statistically significant at $p \leq .01$.
Significance test is one-tailed paired sample t-test of proportions.
Note: Race variable excludes "Other" category (N=7).

over time, except in the case of property crime victimization in which blacks benefited as much as whites and hispanics. Otherwise, the program appears to have had little effect on this subgroup.

We can only speculate why the program may have had so little impact on blacks and renters. The data in Table 1 indicated that blacks were contacted in numbers proportionate to their numbers in the Golfcrest population, so the lack of program effect was not due to the fact that they were excluded from the program. And, yet, looking back at Table 9 through 11, we find that blacks were less likely than whites, and renters less likely than homeowners, to report that a police officer had come to their door. Blacks and renters also are less likely than whites, and owners to report knowing an officer who works in the area. However, hispanics have program awareness levels as low or lower than blacks but experience program effects similar to those of whites. The difference for blacks and hispanics may be tied to the fact that 40 percent of hispanic respondents are also homeowners while only 5 percent of black respondents are owners. If word-of-mouth is a phenomenon which helps create a desirable program effect, renters--who may be more transient--may not have enough contact with more permanent neighbors to be included in the neighborhood grapevine.

Program Effects for Panel Members Who Recall Meeting or Seeing Police

Table 19 reported the percentage of residents in the program and comparison areas who recalled that a police officer had been to their door and the percentage who recalled seeing a police officer in their neighborhood within the previous 24 hours.

Such responses can be taken as surrogate measures of exposure to the contact program. The variable "recall/not recall" can be used in the same type of regression analysis performed for all panel respondents, to take a more focused look at the impact of the program on individuals. The advantage of such an analysis is that by examining differences between recalling contact and not recalling contact with the program within the program area, we control for some of the differences between the treatment and control areas which have plagued earlier analyses.

However, one difficulty with this analysis is that it confounds measurement error with program involvement. That is, we cannot be sure that people's "yes" or "no" responses to program exposure measures truly reflect their contact with the program (they might forget, exaggerate, etc.) If this error is random, it will bias coefficients measuring the effect of the program downward, tending toward Type I error.

A different threat is that this recall error may be related to program contact; that is, people who were involved in some way with the program may be giving us a true "yes" response more often, while those who were not might be giving us "yes" or "no" responses for a variety of other reasons. This will bias the findings toward Type I error.

Alternatively (or, in addition), recall may be related to impact; that is, people who are affected by the program may be more likely to truly recall contact, while those whose lives were untouched by the program might forget such a contact more easily. This would bias the evaluation in the direction of finding a program effect, a Type I error.

In our experience, the second and third problems are more likely to be important than the first (that caused by random error). Thus, this correlational analysis could be biased in either direction and should, therefore, be viewed as suggestive rather than definitive.

All of this argues that the findings presented in Tables 26 and 27 should be interpreted very cautiously and that significant coefficients attached to these measures of program involvement are only weak evidence of program effect. Table 26 indicates that recalling that "...the police came to your door to ask about problems in the neighborhood or to give you information about crime" is significantly related to lower scores on Perceived Area Personal Crime Problems and Worry About Area Property Crime Victimization. Table 27 indicates that reports of more recent sighting of a police officer in the area* are significantly related to lower Fear of Personal Victimization, greater Satisfaction with Area as a place to live and higher Evaluation of Police Service. (Construction of these scales is discussed in Appendix D. Values of correlation coefficients are presented in Appendix J.)

It is interesting that Tables 26 and 27 indicate program impact on different outcome measures depending on whether the respondent recalled meeting an officer at the door or recalled having seen one in the area recently. These findings suggest that two conceptually distinct aspects of the strategy--the contact and the increased police presence in the area--may each have an impact and that the outcome measures affected by each element

*This measure is scored:
0 = Have not seen an officer in the past week
1 = Have seen an officer within the past week
2 = Have seen an officer within the past 24 hours

TABLE 26

PROGRAM EFFECTS FOR PANEL RESPONDENTS WHO RECALL POLICE CAME TO THE DOOR: RELATIONSHIP BETWEEN RECALL AND OUTCOME SCALES

(Panel Respondents)

Outcome Scale	Effect*	Sigf.
Fear of Personal Victimization in Area	-	.33
Perceived Area Personal Crime Problems	-	.03**
Worry About Area Property Crime Victimization	-	.01**
Perceived Area Property Crime Problems	-	.15
Perceived Area Social Disorder Problems	-	.24
Satisfaction with Area	+	.17
Evaluations of Police Service	+	.10
Police Aggressiveness	+	.65
Defensive Behaviors to Avoid Personal Crime	-	.11

*Controlling for the pretest scale score and indicators of: age, race, sex, income, education, length of residence, marital status, household organization and size, renter status, building size, personal victimization, knowledge of local crime victims. All control factors are measured using Wave 1 data.

**Statistically significant at $p \leq .05$.

Note: N is approximately 310 for all analyses.

TABLE 27

PROGRAM EFFECTS FOR PANEL RESPONDENTS WHO RECALL RECENT SIGHTING OF POLICE: RELATIONSHIP BETWEEN RECALL AND OUTCOME SCALES

(Panel Respondents)

Outcome Scale	Effect*	Sigf.
Fear of Personal Victimization in Area	-	.05*
Perceived Area Personal Crime Problems	-	.89
Worry About Property Crime Victimization in Area	-	.22
Perceived Area Property Crime Problems	+	.35
Perceived Area Social Disorder Problems	-	.93
Satisfaction with Area	+	.01**
Evaluations of Police Service	+	.001**
Police Aggressiveness	+	.81
Defensive Behaviors to Avoid Personal Crime	-	.53

*Controlling for the pretest scale score and indicators of: age, race, sex, income, education, length of residence, marital status, household organization and size, renter status, building size, personal victimization, knowledge of local crime victims. All control factors are measured using Wave 1 data.

**Statistically significant at $p \leq .05$.

Note: N is approximately 310 for all analyses.

may be different. Our ability to draw this conclusion from Tables 26 and 27 is limited by the fact that many of the people who give a positive response on one of the recall items may also give a positive response on the other.

Table 28 isolates those panel respondents who report a contact at the door but not a recent sighting of police and those who report a recent sighting but do not recall a contact at the door, using measures of each in a multiple regression analysis of outcome scores.

As in Tables 26 and 27, the analysis presented in Table 28 suggests that two aspects of the contact strategy--the contact itself and police visibility--may have some separable effects on the outcome measures. They also have some similar effects. Both are related to greater Satisfaction with Area and to higher Evaluation of Police Service. Only the recall of recent sighting of police is significantly related to reduction in Fear of Personal Victimization in Area and only the recall of a visit from an officer is significantly related to lower scores on Perceived Area Personal Crime Problems and Perceived Area Property Crime Problems. And a relationship not detected in the previous analysis is that between the recall of an officer visit and higher scores on Defensive Behaviors to Avoid Personal Victimization. Those respondents who recall a contact are more likely to avoid behaviors that might expose them to personal danger. This relationship may be the result of the contact and (or) of the newsletters which subsequently were mailed to persons who had been contacted; the newsletters contained several recommendations about protection of person and property. (See Appendix L for a content analysis of the newsletters and for a copy of one issue.)

TABLE 28

PROGRAM EFFECTS FOR PANEL RESPONDENTS WHO WERE AWARE OF PROGRAM
RELATIONSHIP BETWEEN TWO RECALL MEASURES OF PROGRAM EXPOSURE AND OUTCOME SCALES

(Golfcrest Panel Sample Only)

	Program Exposure: Recall Police Sighting		Recall Officer Visits			
Outcome Scale	Beta	Sigf.	Beta	Sigf.	R adj.	[N]
Fear of Personal Victimization in Area	-.11	.05**	-.04	.42	.17	[311]
Perceived Area Personal Crime Problems	-.01	.80	-.12	.03**	.13	[300]
Worry About Property Crime Victimization in Area	?	?	?	?	?	?
Perceived Area Property Crime Problems	?	?	?	?	?	?
Perceived Area Social Disorder Problems	-.01	.85	-.06	.24	.26	[312]
Satisfaction with Area	.15	.01**	.25	.001**	.11	[312]
Evaluation of Police Service	.19	.001**	.27	.001**	.25	[304]
Police Aggressiveness	.02	.77	.02	.67	.13	[287]
Defensive Behaviors to Avoid Personal Crime	-.02	.66	.21	.001**	.21	[312]

*Controlling for pretest score plus 16 measures of victimization, personal attributes and household organization.
**Statistically significant at $p \leq .05$.
? Data missing from analysis.

SUMMARY

Table 29 summarizes the findings of program effects for both the cross-sectional (area) sample and the panel sample. Three outcomes were documented in both samples; these included a reduction in perceived area social disorder problems, an increase in satisfaction with the area, and a reduction in property crime victimization. Three additional effects were measured for the cross-sectional sample--reduced fear of personal victimization, reduced perceptions of area personal crime problems and reduced perceptions of area property crime problems. One outcome--an increase in evaluations of police service--was found in the panel sample only. There were two hypothesized outcomes which were not found with either sample; these were reduced worry about area property crime and reduced defenisve behaviors taken to avoid personal victimization.

The analyses for subgroups found that the benefits were not equally distributed across groups. There was something about the program which caused its benefits not to be shared by blacks and renters. The explanation would not seem to lie simply in the greater fearfulness of these groups since, as can be seen in Appendix H, blacks were the least fearful of all the ethnic groups at Wave 1 and renters were no more fearful than owners. Females were more fearful then males, but their negative reactions to the program were neither as strong nor as consistent as those of blacks and renters. Both blacks and renters reported significantly lower levels of program awareness than did whites and homeowners. Hispanics who received program benefits similar to those measured for whites, also reported low program awareness. They were, however, significantly more likely than blacks to be homeowners. But these pieces of information are only slight clues to reasons for subgroup differences. The differential impacts of the program remain puzzling and deserving of further study.

TABLE 29

SUMMARY OF SIGNIFICANT PROGRAM EFFECTS
BY SAMPLE

Outcome Scale	Desirable Program Effect Measured in: Cross-Sectional Sample	Panel Sample
Fear of Personal Victimization in Area	X	
Perceived Area Personal Crime Problems	X	
Worry About Area Property Crime Victimization		
Perceived Area Property Crime Problems	X	
Perceived Area Social Disorder Problems	X	X
Satisfaction with Area	X	X
Evaluations of Police Service		X
Perceived Police Aggressiveness	X	
Defensive Behaviors to Avoid Victimization		
Property Crime Victimization	X	X
Personal Crime Victimization		

ANALYSIS AND RESULTS FOR NON-RESIDENTIAL RESPONDENTS

In Golfcrest, 155 non-residential establishments were listed on sample sheets prior to Wave 1; at the same time, 127 such establishments were listed in Shady Acres, the comparison area. Classification of these establishments by type as represented by the survey samples is provided in Table 30. It was expected that the contact officers would visit the non-residential locations as well as the residences and that the contacts would have similar effects on the non-residential as on the residential respondents. Additionally, it was hypothesized that the contacts would cause Golfcrest business people to believe that their employees and patrons were less concerned about crime and that these people would also report their business had improved over the project period. If area residents, and especially those who were patrons of the businesses, actually did feel less concern because of the contacts being made, they then might feel more inclined to shop in their area and business might improve as a result. Table 31 reports the percentage of non-residential respondents in the program and comparison areas who recall exposure to the contact program. Although there was an increase in the program area and a decrease in the comparison area in the number of respondents who recalled that the police had come to talk with them, the differences were not statistically significant in either area. In neither area were there significant Wave 1 - Wave 2 differences in the percentage of respondents reporting they had seen an officer in the area in the previous 24 hours or in the percentage who said they knew an officer who worked in the area.

TABLE 30

TYPES OF NON-RESIDENTIAL ESTABLISHMENTS SURVEYED IN PROGRAM AND COMPARISON AREAS AT WAVES ONE AND TWO*

	Program Area (Golfcrest)		Comparison Area (Shady Acres)	
Establishments Which Are:	Wave 1 (%)	Wave 2 (%)	Wave 1 (%)	Wave 2 (%)
Agricultural	2	2	3	0
Construction	3	2	13	14
Financial	0	0	0	2
Governmental	2	2	0	2
Manufacturing	7	10	10	9
Public Organizations	3	3	0	5
Retail	35	30	33	23
Services	21	13	23	30
Transportation	2	2	0	0
Wholesale	27	37	15	16
[N]	[68]	[67]	[39]	[44]

*See Appendix N for a more detailed listing of non-residential establishments in the program and comparison area samples at Wave 2.

TABLE 31

PROGRAM EXPOSURE:
PERCENTAGE OF NON-RESIDENTIAL RESPONDENTS RECALLING
ASPECTS OF THE PROGRAM, PROGRAM AND COMPARISON AREAS

(All Non-Residential Respondents)

		Program Area (Golfcrest)				Comparison Area (Shady Acres)			
		Wave 1	Wave 2	Diff.	Sigf.	Wave 1	Wave 2	Diff.	Sigf.
Type of Exposure									
Recalled police came to door to ask about problems or provide information		9	20	+11	.10	13	9	- 4	.70
	[N]	[67]	[66]			[38]	[44]		
Had seen police officer									
more than 1 week ago		19	19	0		13	23	+10	
within past week		22	21	- 1	.99	44	25	-19	.20
within past 24 hours		59	60	+ 1		44	52	+ 8	
	[N]	[68]	[67]			[39]	[44]		
Knew a police officer who worked in the area		32	34	+ 2	.90	28	36	+ 8	.50
	[N]	[68]	[67]			[39]	[44]		

Note: Statistical significance is $p \leq .01$.
Chi-square tests of significance.

Table 32 reports the differences over time on outcome scale scores for non-residential respondents in Golfcrest and Shady Acres. The construction of the non-residential scales is discussed in Appendix E. Scores for the individual items making up each scale are presented in Appendix K.

The analysis is based on a comparison of mean scores for program and comparison areas.

In neither the program nor the comparison areas were there any Wave 1 - Wave 2 differences which were statistically significant. The few changes in the direction of positive effect in the program area tended to be matched by similar movement in the comparison area. Thus, there is no reason to believe the contact strategy had any positive effects on the non-residential respondents in Golfcrest.

The finding of no effect seems at first surprising since there were statistically significant effects measured among residents in the program area. Furthermore, the contact officers reported having made 73 commercial contacts. This means they contacted a larger percentage (47%) of the non-residential establishments than of the housing units (37%). On this basis alone, one might have predicted a larger impact among non-residential respondents. However, there was never any reason to believe fear of crime had reached such levels in any of the Houston research areas that it created problems for area businesses. There was no reason to believe that residents were <u>not</u> going to their local stores because they were afraid to use the streets or because they were afraid a crime might be committed while they were in the business. Indeed, the research areas were not selected because

TABLE 32

NON-RESIDENTIAL SURVEY RESULTS:

DIFFERENCES IN WAVE 1- WAVE 2 OUTCOME SCORES
PROGRAM AND COMPARISON AREAS

		Program Area (Golfcrest)				Comparison Area (Shady Acres)			
		Wave 1	Wave 2	Diff.	Sigf.	Wave 1	Wave 2	Diff.	Sigf.
Outcome Scale									
Fear of Personal Victimization in Area		2.40	2.45	+.057	.051	2.12	2.19	+.074	.025
	(sd)	(.72)	(.73)			(.63)	(.65)		
	[N]	[68]	[67]			[39]	[44]		
Worry About Property Crime Victimization in Area		2.15	1.91	-.24	.03	2.13	1.92	-.21	.10
	(sd)	(.64)	(.61)			(.65)	(.60)		
	[N]	[68]	[67]			[39]	[44]		
Perceived Concern About Crime Among Employees and Patrons		2.25	2.19	-.06	.40	2.27	1.94	-.33	.05
	(sd)	(1.03)	(1.13)			(.90)	(.85)		
	[N]	[67]	[67]			[39]	[44]		
Perceived Area Social Disorder Problems		1.50	1.45	-.05	.40	1.33	1.42	+.09	.25
	(sd)	(.53)	(.51)			(.35)	(.39)		
	[N]	[68]	[67]			[39]	[44]		
Satisfaction with Area		2.41	2.54	+.13	.25	2.70	2.81	+.11	.25
	(sd)	(.61)	(.65)			(.57)	(.57)		
	[N]	[68]	[67]			[39]	[44]		

continued

NOTE: Statistically significant at $p \leq .01$.
One-tailed significance t-tests.

TABLE 32
continued

NON-RESIDENTIAL SURVEY RESULTS:

DIFFERENCES IN WAVE 1- WAVE 2 OUTCOME SCORES
PROGRAM AND COMPARISON AREAS

		Program Area (Golfcrest)				Comparison Area (Shady Acres)			
		Wave 1	Wave 2	Diff.	Sigf.	Wave 1	Wave 2	Diff.	Sigf.
Outcome Scale									
Changes in Business Conditions		1.77	1.97	+.20	.05	2.06	2.16	+10	.25
	(sd)	(.70)	(.69)			(.64)	(.62)		
	[N]	[66]	[67]			[39]	[43]		
Evaluation of Police Service		3.38	3.53	+.15	.25	3.46	3.22	-.24	.10
	(sd)	(.73)	(.81)			(.64)	(.81)		
	[N]	[65]	[67]			[38]	[44]		
Victimization--Robbery or Attempted Robbery		12	13	+ 1	.80	8	7	- 1	.90
	[N]	[68]	[67]			[39]	[44]		
Victimization--Burglary or Attempted Burglary		40	33	- 7	.50	44	50	+ 6	.70
	[N]	[68]	[67]			[39]	[44]		
Victimization--Vandalism		29	28	- 1	.90	15	20	+ 5	.70
	[N]	[68]	[67]			[39]	[44]		

NOTE: Statistical significance is $p \leq .01$.
One-tailed significance t-tests.

respondents were known to be highly fearful; the areas were matched on demographic characteristics and there is no reason to believe that fear of victimization was especially high in any of them. Even so, businesses might have noticed an improvement related to the less fearful attitudes among Golfcrest residents if the businesses were dependent on pedestrian traffic. But in Golfcrest, the businesses are not embedded in the residential neighborhoods as they are in some other areas of the city; instead they are on busy perimeter streets which might not appeal to pedestrian traffic. The overwhelming dependency of Houstonians on the automobile for even short trips may make businesses less vulnerable to the fear levels of their patrons (except of those who are fearful of having their cars stolen or vandalized).

Additionally, while there is reason to believe that most residents in Golfcrest who were contacted by the police in this program were receiving their first proactive, non-offense related visit by an officer, this is less likely to be true for the non-residential respondents, especially those who own businesses which officers might have frequented for a cup of coffee or meal while on their tour of duty in the area. Further, given their generally higher victimization rates, these businesses were more likely than residences to have had a reactive, crime-related contact with an officer. Because most of the non-residential establishments are on the perimeter streets, people who work in them will have been more accustomed to seeing police pass by than will people living on quieter, residential streets. Indeed, many of these establishments on the southern boundary street are within sight of the police district substation. In the Wave 1 survey, 59 percent of non-residential respondents but only 28 percent of residential

respondents in Golfcrest reported having seen a police officer in the previous twenty-four hours. This suggests that police presence was not the novel phenomenon for business managers or owners that it was for residents, and thus would not have been expected to have the same impact.

Although there were no measurable effects of the program for non-residential respondents, it is possible that under conditions different than those which existed in the Golfcrest area in 1983 and 1984, a contact program might have greater impact.

CONCLUSIONS

This evaluation of the Citizen Contact strategy which was conducted in Houston in 1983 and 1984 has concluded that the program had positive, statistically significant effects for both the area as a whole and for a panel of individuals who resided there before and after the program was conducted.

Area-level pooled cross-sectional analyses found that living in the program area and being interviewed after program implementation were negatively and significantly associated with:

- o fear of personal victimization in the area,
- o the perception that area personal crime is a big problem,
- o the perception that area property crime is a big problem,
- o the perception that area social disorder is a big problem,
- o the belief that the police are overly aggressive, and
- o property crime victimization.

These same conditions were positively and significantly associated with:

- o expressed satisfaction with the area as a place to live.

The panel analyses which were based on interviews with respondents who were the same at both waves of the survey provided a more reliable test of program effects, since it was possible to use pre-intervention scale scores to control statistically for other factors which might be related to measured changes. These analyses found for the entire group of individuals in the panel, statistically significant negative relationships between residence in the program area and scale scores which measured:

o the perception that area social disorder is a big problem, and

o property crime victimization.

There was a statistically significant positive relationship between program area residence and the scale scores which measured:

o satisfaction with the area as a place to live, and

o evaluations of police service.

Individuals in the panel who recalled exposure to the program, either a contact at their door by the police or the recent sighting of police, registered significant effects which may be attributable to the program. Those who recalled a contact were less likely to perceive area personal and area property crime as big problems than were respondents who did not recall a contact. Persons who recalled seeing an officer in the area in the last 24 hours were less likely to report fear of personal victimization and more likely to express satisfaction with the area and with police service than were people who had not recently seen an officer in the area.

Subgroups of residents within the panel did not all share the same reactions to living in the program area. When people were divided into groups on the basis of age, sex, prior victimization, race, home ownership, or single/multi family housing, significant beneficial effects were measured for people who were Asians or hispanics and also for people who live in single family dwellings. The program had almost no statistically significant effects on blacks and persons who rent their homes. The only significant benefit indicated for them was reduced property crime victimization which was reported by all groups of respondents. Also, women were less likely to benefit from the program than were men. There were no subgroup-specific components of the program, and the differential effect on subgroups is a matter for theorizing and additional research.

Non-residential respondents indicated no effects of the program. It is hypothesized that the lack of effect was due to the likelihood that the program did not change the level of exposure of commercial establishments to police in the same way it changed the exposure level of residents. For a number of reasons, non-residential respondents normally have more opportunity to have contact with the police than do residents.

DISCUSSION

There are two aspects of this project upon which we would like to focus additional discussion: (1) the nature of the contact program and (2) the relationship between the findings for this program and findings from tests of other patrol strategies.

The Nature of the Program.

While the face-to-face contact with citizens was the conceptual heart of the strategy, there were at least six elements of the program which may have created, or contributed to, the impacts documented by the evaluation. These were:

1. The contact in which the officer introduced him/herself, explained that he/she was the officer working in the neighborhood, asked whether the contacted person knew of any problems in the neighborhood which they wanted the police to know about, and frequently left a business card so that the citizen could contact the officer directly if they felt any reason to do so.

2. Increased perceived presence of police on the part of both ordinary citizens and potential offenders. This should have resulted from the exercise of beat integrity which confined the area officers to a smaller geographical space than they normally worked. Citizen awareness of police in the area may also have been enhanced by parking the patrol car at the curb and walking to a house.

3. New patrol tactics which may have increased public awareness of the police. These tactics included the more frequent patrolling of side streets en route to residential contacts, stopping pedestrians on streets and in parking lots in order to make contacts, and making traffic stops for the same purpose.

4. Accessibility of the officers to the citizens which occurred whenever the contacting officer left a business card with name and phone number on it. Citizens also may have become more accessible--both psychologically and physically-- to the police. They were psychologically more accessible because, after contacting them once, the police may have felt more comfortable approaching and talking to them a second time. They may have been physically more accessible as a result

of beat integrity which may have reduced police response time to calls for service.

5. Familiarity and identification with the area and its people on the part of the officers who did appear to identify more closely, over time, with the neighborhood and its problems. At the same time, residents had the opportunity to identify a small number of officers as "our police."

6. The newsletter which provided safety and crime prevention tips and gave examples of citizens and/or police working to reduce area crime. Most residents who were contacted received from one to four issues of the newsletter.

In combination, these elements appear to have had a number of positive consequences for the residents of Golfcrest. We are not able in this study to separate the individual elements which, alone, or in different combinations might have different effects. For example:

o Increased perceived presence alone might increase apprehension on the part of residents that more police in the area meant that something was wrong. Some unknown number of face-to-face contacts may be necessary to guarantee that word-of-mouth communication spreads the news that the police are only trying to get to know their area better.

o The contact alone, made by officers who did not work the area regularly, might have an effect similar to only increased perceived presence. A "strange" officer who was not seen regularly in the area, might be suspected of working a case and using the friendly contact as a good cover story. Leaving the business card with the telephone number may be critical to insuring the resident of the sincere intent of the contact.

o The contact alone, without the elements of familiarity and identification with the area on the part of the officers and the mutual accessibility of the police and residents might also have a different

effect. Unless the officer knows the area well and works it regularly, random bits of information picked up here and there may have very little practical utility. Knowing what response to make to a problem identified by a resident may depend on knowing the area well.

o Immediate physical access to the neighborhood by the officers working there--and the predictably shorter response times to calls--may have been important to feelings of reassurance on the part of residents.

The program elements of perceived presence, accessibility, familiarity and identification are all the result of beat integrity having been used in the implementation of this strategy. It is possible to imagine variations of this strategy which would not depend on beat integrity (e.g., officers could be asked occasionally to make a contact at an additional address in any area where they might happen to make a call). While this seems feasible, it is important to point out that we have no information about whether such an approach to citizen contacts would have any of the effects measured in Golfcrest. It is impossible in this study to separate the distinct contributions various program elements may have made to the total effect.

There is another condition, although not an element, of the Citizen Contact Program which should be considered in thinking about its effects. The neighborhood in which it was conducted was one in which residents were not accustomed to seeing police frequently or face-to-face. Were the program to continue there, residents--like the commercial respondents--might come to take for granted familiarity with, visibility of, and contact with their police. Given this possibility, another evaluation of the impact of the program conducted a year from now could provide information about the

durability of program effects. The results of this study can only speak to the effect of introducing the strategy in a neighborhood where there has not been much prior contact with the police and evaluating it after 10 months of operation.

The measured effects of the contact strategy as compared to the effects of other tested patrol strategies

The Kansas City Preventive Patrol Experiment (Kelling, Pate, Dieckman, and Brown, 1974) found that varied levels of motor patrol did not affect crime, service delivery or citizens' feelings of security. The major conclusion of this study was that officers normally had a large amount of uncommitted patrol time which could be used for other policing activities without threatening public welfare. The study of citizen contact patrol reported here suggests that getting out of the police vehicle may make a difference and that making citizen contacts is one of the ways which officers in squad cars could more usefully employ their time. There were no measured effects of increased patrol in Kansas City despite the fact that citizens in the area where patrol levels were increased were somewhat more aware of seeing police more often. This suggests that police presence alone may not be the key element in the Golfcrest results.

The Newark Foot Patrol Experiment (Police Foundation, 1981) found that increases in foot patrol in Newark produced greater citizen awareness of the police in their neighborhood than did increases in motor patrol in Kansas City. Additionally, although foot patrol did not have measured effects on reported crime and victimization, the authors concluded that in the areas

where foot patrol was added, "citizens' fear of typical street crimes seems to go down and generalized feelings of personal safety to go up." (p. 6) As in the Kansas City study of motor patrol, the Newark study of foot patrol found business people generally unaware of the change in patrol levels; both studies found the attitudes of commercial respondents to be unimproved as a result of the changes in levels of patrol.

Other research in the United States (Trojanowicz, et al., 1982) and in the Netherlands (Spickenheuer, 1983) also have suggested that foot patrol may have positive effects. Unfortunately, however, these effects were either combined with other program activities, were evaluated in problematic fashion or both, thus making the inferences from those studies questionable.

These studies taken together suggest that it isn't necessary to abandon the squad car (an impossibility in cities like Houston) in order to get an effect on resident attitudes and experiences of crime. Greater police presence alone may not be the key, however. The personal contact may be the factor that made motor patrol more effective in Houston than Kansas City. Obviously there are many differences among the cities involved in these studies and more differences among the programs and evaluations than can be addressed here. However, the suggestion of the contact as the critical element warrants researchers taking a closer comparative look at these studies.

RECOMMENDATIONS

We believe that citizen contact patrol may well have caused the substantial reductions in reported victimization and fear, as well as the other outcomes reported in this evaluation. It is not clear how long the changes will last, but they were major changes to produce in such a short time period.

These findings are based on a sample of only two areas. They would be much more convincing if they were based on 50 areas, since it would help to rule out pre-existing differences in the areas as a cause of the change. Even with this caution, however, the results are still quite impressive.

We recommend that police departments should adopt a citizen contact patrol in similar low-density neighborhoods. Special emphasis should be placed on home visits, since these comprised the bulk of the contacts in the Houston experiment.

We further recommend that any future efforts to implement citizen contact patrol be accompanied by training of the officers (which was not done in Houston, except for one officer's visits to other cities). Supervision and support of the program should be given consideration.

Replication. The findings warrant a careful replication with similarly detailed measurement, including measures of how many arrests result from these contacts. In the meantime, however, police departments can conduct their own replication with the following basic steps. We recommend this kind of pilot phase-in rather than city-wide overnight adoption of citizen contact patrol.

1. Select 50 patrol beats at random from all beats or all residential beats.

2. Choose 25 at random to receive citizen contact patrol.

3. Train all officers working or substituting on those 25 beats.

4. Have the beats supervised by sergeants who have been trained to manage the program.

5. Require citizen contact cards from household visits to be turned in daily.

6. After one year, compare arrests per officers (counted properly--see Police Foundation Report #2) to see if citizen contact patrol leads to more arrests.

7. Report your findings to the national police community. This can be done by writing an article for Police Chief or some of the academic journals; by sending a copy of your report to the National Criminal Justice Reference Service, or to Law Enforcement News; and by presenting the findings at professional meetings, such as those of the International Association of Chiefs of Police, National Sheriffs' Association, National Organization of Black Law Enforcement Officers, Police Executive Research Forum, City Managers' Association, U. S. Conference of Mayors, American Society of Criminology and the American Criminal Justice Society.

It is only with widespread and careful replication of this kind of test that the police field will be able to accumulate knowledge about how to patrol more effectively in a wide range of cities. But the Houston study alone refutes the way the Kansas City experiment has often been misread to say patrol has no impact. Police patrol perhaps can make a difference in citizen attitudes and victimization rates--depending upon how it is done.

A POSTSCRIPT

On Thursday, October 25, 1984, Officer Charlie Epperson was in a hardware store in the Golfcrest area. An older gentleman approached and addressed him, "Mr. Epperson...." The man proceeded to describe an abandoned vehicle in the area.

Officer Epperson ticketed the car twice and then arranged to have it towed the following Monday.

The Golfcrest resident reporting the problem had been contacted by Officer Epperson during the first month of the Citizen Contact Program; he had not been contacted again in the 14 months between the contact at his home and the meeting in the hardware store.

"Mr. Epperson," he said.

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CITIZEN CONTACT PATROL:

THE HOUSTON FIELD TEST

APPENDICES

APPENDIX A

THE FEAR REDUCTION PROGRAM

THE FEAR REDUCTION PROGRAM

The program described in this report was one of several strategies tested as part of a Fear Reduction Program which was carried out in Houston, Texas, and Newark, New Jersey, in 1983 and 1984. The police departments in these two cities were invited to design and implement strategies to reduce fear of crime. The Police Foundation with funding provided by the National Institute of Justice (NIJ) provided technical assistance to the departments during the planning phase of the program and conducted rigorous evaluations of the strategies which were developed. NIJ also supported a dissemination program, in which the National Conference of Mayors, the Police Executive Research Forum, the National Organization of Black Law Enforcement Executives, and the National Sheriffs' Association sent representatives to observe the strategies in action and report on them to their members. The questions they asked and the written observations they shared with the Houston and Newark departments provided constructive criticism of the program implementation process.

Program Objectives. The overall goal of the program was to find new ways to help citizens gain a realistic picture of the crime problems facing their neighborhoods, reduce excessive fear of crime, encourage greater positive police-citizen cooperation in crime prevention, spark increased awareness among people of the steps which they could take to reduce crime, and help restore their confidence in the police and faith in the future of their communities.

In each city a number of different strategies were developed which addressed these issues. Previous research has found crime to be only one of the causes of fear and declining community morale, so those strategies addressed a broad spectrum of issues. Some focused upon reducing physical disorder, including trash and litter, abandoned buildings, graffiti, and deterioration. Others targeted social disorder, including loitering, harassment, disorderly street behavior, and violations of rules of conudct on mass transit. A number were designed to increase the two-way flow of information between citizens and the police. From the police side this included developing new mechanisms to gather information about community problems often of a seemingly "nonpolice" nature, assisting citizens in organizing to address such problems, and testing new mechanisms to "spread the word" about community programs and the things that individual citizens could do to prevent crime.

Site Selection. Houston and Newark were selected as examples of two different types of American cities. Houston is a relatively young city, with low population density and a developing municipal infrastructure, while Newark is a mature city with high population density and no significant growth. Because they are so different, some of the strategies they developed for the Fear Reduction Project were unique, but most addressed the same underlying problems and many were surprisingly similar. The two cities were also selected because of the capacity of their police departments to design and manage a complex experimental program.

Within each city, "matched" neighborhoods were selected to serve as testing grounds for the strategies. Because Newark has a predominantly black population, five physically similar areas with a homogeneous racial composition were selected. The heterogeneous nature of Houston called for the selection of neighborhoods with a population mix more closely resembling that of the city as a whole. In both cities the selected areas were approximately one square mile in size, and physically separated from each other. Site selection was guided by the 1980 Census, observations of numerous potential sites, and extensive discussions with police crime analysts and district commanders in the cities.

The Task Force Planning Process. In both cities, the program planning process had to design programs which met two constraints: they could be carried out within a one-year time limit imposed by the National Institute of Justice, and they could be supported entirely by the departments--there was no special funding available for these projects.

The planning processes themselves took different forms in the two cities. In Houston, one patrol officer from each of the four participating police districts was assigned full time for two months to a planning Task Force, which was headed by a sergeant from the Planning and Research Division. A civilian member of the Planning and Research Division also served on the Task Force. During the planning period the group met regularly with staff members of the Police Foundation to discuss past research related to the project. They also read studies of the fear of crime, and visited other cities to examine projects which appeared relevant

to fear reduction. By April, 1983, the group had formulated a set of strategies which they believed could be implemented effectively in Houston and had the potential to reduce citizen fear.

Then, during April and May the plan was reviewed and approved by Houston's Chief of Police, the department's Director of Planning and Research, by a panel of consultants assembled by the Police Foundation, and by the Director of the National Institute of Justice.

In Newark, the Task Force included several members of the police department as well as representatives of the Mayor's office, the Board of Education, the New Jersey Administrative Office of the Courts, the Essex County Courts, the Newark Municipal Courts, the Essex County Probation Department and the Graduate School of Criminal Justice of Rutgers University. The group met once or twice a week for a month to discuss the general problems of fear, then broke into several committees to consider specific program possibilities. In April, 1983 the committees submitted lists of proposed programs to the entire task force for approval. These programs were reviewed by the panel of consultants, assembled by the Police Foundation and by the Director of the National Institute of Justice.

Technical Assistance by the Police Foundation. The Police Foundation provided the departments with technical assistance throughout the planning stages of the Fear Reduction Project. Its staff assisted the departments in locating potentially relevant projects operating in other cities, accumulated research on fear and its causes, arranged for members of the Task Forces to visit other departments, and identified consultants who

assisted the departments in program planning and implementation. This activity was supported by the National Institute of Justice.

Strategies Developed by the Task Force. In Houston, strategies were developed to foster a sense that Houston police officers were available to the public and cared about individual and neighborhood problems. Some of the strategies also were intended to encourage citizen involvement with the police and to increase participation in community affairs. The strategies included community organizing, door-to-door police visits, a police-community newsletter, recontacts with crime victims, and a police-community storefront office.

The Newark strategies were directed at the exchange of information and the reduction of social and physical disorder. The police strategies included door-to-door visits, newsletters, police-community storefronts, and the intensified enforcement and order maintenance. In association with the Board of Education, recreational alternatives to street-corner loitering were to be provided. With the cooperation of the courts system, juveniles were to be given community work sentences to clean up deteriorated areas; with the assistance of the municipal government, abandoned or deteriorated buildings were to be demolished and delivery of city services intensified.

Implementation of the Strategies. Responsibility for implementing the strategies in Houston was given to the planning Task Force, which then consisted of a sergeant, four patrol officers, and a civilian member of the department. Each of the patrol officers was directly responsible for the

execution of one of the strategies. They were joined by three additional officers; two from the Community Services Division were assigned to work on the community organizing strategy, and another was assigned to work on the door-to-door contact effort. During the implementation period, two more officers were assigned to the victim recontact program and another to the community organizing strategy.

During the nine-to-twelve month period that the strategies were operational, the original Task Force members assumed total responsibility for implementation. They conducted much of the operational work themselves and coordinated the few other officers from each patrol district who were involved in program implementation. When implementation problems required swift and unique solutions (a condition common during the start up period), the Task Force officers worked directly with the district captains and/or with the sergeant from Planning and Research who headed the Task Force. This sergeant would, in turn, take direct action or work with the Director of Planning and Research or with one of the Deputy Chiefs over the patrol districts and/or with the Assistant Chief in charge of Operations. The amount of responsibility placed on the task force members had some of the disadvantages which can exist when the traditional chain of command is circumvented, but it had the advantage that Task Force members felt ownership of, and pride in, the program they had designed.

In Newark, responsibility for implementing each program component was assigned to one or more officers, who in turn were monitored by the program coordinator and his assistant. Those officers working in particular patrol divisions--those in the community police center and those making door-to-

door contacts--reported formally to the division Captain and informally to the program coordinator, who, at the beginning of the program was still a Lieutenant. This somewhat ambiguous reporting structure created some delays, lack of coordination and misunderstanding during the early months of program implementation; these problems were largely overcome with the cooperative efforts of the parties involved. Officers who implemented the other programs reported directly to the program coordinator, a system which worked effectively throughout the program.

The Overall Evaluation Design. All of the strategies tested in Houston and Newark were to be evaluated as rigorously as possible. Two of them--the victim recontact program in Houston and police-community newsletters in both cities--were evaluated using true experiments, in which randomly selected groups of citizens were either contacted by the program or assigned to a noncontacted control group. The other strategies, including the one reported here, were area-wide in focus, and were evaluated using pre- and post-program area surveys. Surveys were also conducted in a comparison area, in which no new programs were implemented, in each city.

APPENDIX B:

INSTRUMENT FOR OBSERVATION OF CITIZEN CONTACT

Table B-1

PROJECT MONITOR'S FORM FOR REPORTING DIRECTED CONTACT OBSERVATIONS

Date of contact _____ Time _____ to _____

Officer making contact _____

Address _____

Type of location:

RESIDENCE ___
APARTMENT ___
BUSINESS ___
STREET OR
PARKING LOT ___
OTHER ___

Personal manner of the person contacted:

FRIENDLY 1 2 3 4 5 6 UNFRIENDLY

RELAXED 1 2 3 4 5 6 UNCOMFORTABLE

TRUSTING 1 2 3 4 5 6 SUSPICIOUS

COOPERATIVE 1 2 3 4 5 6 UNCOOPERATIVE

INTERESTED 1 2 3 4 5 6 DISINTERESTED

Personal manner of the officer:

FRIENDLY 1 2 3 4 5 6 UNFRIENDLY

RELAXED 1 2 3 4 5 6 UNCOMFORTABLE

INFORMATIVE 1 2 3 4 5 6 UNINFORMATIVE

INTERESTED 1 2 3 4 5 6 DISINTERESTED

Problems mentioned by person contacted:	Officer Response:
_____	_____
_____	_____
_____	_____
_____	_____

General opinion of the officer's performance: GOOD___ OK___ POOR___

Observer's opinion of the response of the person contacted to the contact:

Reaction was: POSITIVE___ NEUTRAL___ NEGATIVE___

Observer's comments, including sugestions for improving contacts:

APPENDIX C:

PROBLEMS IDENTIFIED DURING CONTACTS

Table C-1

Detailed Problem Codes

Domestic Disputes
- domestic disputes/disturbances
- assaults-domestic
- child neglect/abuse

Suspicion
- suspicious persons/circumstances

Vehicle Problems
- speeding vehicles/accidents
- reckless driving of school buses
- abandoned cars
- parked vehicles
- traffic violations
- drunk driving

Juvenile Problems
- juveniles causing problems
- gangs
- truancy

Disputes
- tresspassing
- tenant/landlord
- personal confrontations
- people saying insulting things/bothering
- obscene-threatening comments, calls, mail
- fighting in public
- neighborhood disputes

Disorders
- beggars/panhandlers
- disorderly conduct
- drinkig/public intoxication
- firearms carried/fired
- mentally disturbed persons
- sex in public
- noisey parties, neighbors, businesses
- peeping toms
- prostitutes

Environmental Decay
- animals running loose
- abandoned houses/buildings
- barking dogs
- dirty streets/sidewalks
- disrepair of public property
- grafitti
- inadequate public services
- physical hazards/nuisances
- street lights poor
- vacant lots
- illegial dumping (not on original list***)

Victimization of Vehicles
- burglary/theft from
- stolen vehicles
- vehicles vandalized

Burglary

Personal Crimes
- robbery
- rape
- assault

Vandalism

General Theft

Other

TABLE C-2

PROBLEMS REPORTED BY INDIVIDUALS
BY TYPE OF CONTACT

Problem Category	Proactive	Reactive	Vehicle	Pedestrian	Resident
Domestic Violence	1	20	3	6	7
Suspicion	3	12	-	4	6
Vehicle Problems	7	2	3	10	5
Juvenile Problems	2	3	5	4	2
Disputes	3	5	3	-	4
Disorders	8	11	13	6	9
Environment	6	2	-	6	5
Vehicle Crime	12	18	18	14	14
Burglary	10	16	8	6	13
Personal Crime	3	3	-	6	3
Vandalism	1	4	3	2	2
General Theft	2	5	3	4	3
Other	2	4	3	2	3
No Problem Mentioned	54	9	53	56	38
Total	114%	114%	115%	126%	114%
[N]	[307]	[116]	[38]	[52]	[335]

SOURCE: Gathered by police officers during directed patrol interviews in Houston 1983-84. Percentages are based upon total problems mentioned divided by the number of individuals contacted, so they may sum to more than 100%.

TABLE C-3

PROBLEMS REPORTED BY INDIVIDUALS
BY AGE

Problem Category	Under 26	26-32	33-44	50 and Over
Domestic Violence	10	9	4	2
Suspicion	5	6	3	6
Vehicle Problems	4	3	6	9
Juvenile Problems	1	6	2	2
Disputes	7	5	-	2
Disorders	4	6	12	14
Environment	4	1	6	6
Vehicle Crime	12	20	21	6
Burglary	6	8	10	20
Personal Crime	4	2	3	2
Vandalism	1	5	-	3
General Theft	4	2	2	3
Other	4	2	2	3
No Problem Mentioned	46	36	45	39
Total	112%	117%	117%	116%
[N]	[113]	[98]	[92]	[118]

SOURCE: Gathered by police officers during directed patrol interviews in Houston 1983-84. Percentages are based upon total problems mentioned divided by the number of individuals contacted, so they may sum to more than 100%.

TABLE C-4

PROBLEMS REPORTED BY INDIVIDUALS
BY GENDER

Problem Category	Males	Females
Domestic Violence	3	10
Suspicion	3	8
Vehicle Problems	8	3
Juvenile Problems	3	2
Disputes	2	5
Disorders	10	8
Environment	6	3
Vehicle Crime	16	12
Burglary	13	11
Personal Crime	3	3
Vandalism	2	2
General Theft	4	2
Other	2	3
No Problem Mentioned	44	38
Total	119%	110%
[N]	[215]	[211]

SOURCE: Gathered by police officers during directed patrol interviews in Houston 1983-84. Percentages are based upon total problems mentioned divided by the number of individuals contacted, so they may sum to more than 100%.

TABLE C-5

PROBLEMS REPORTED BY INDIVIDUALS
BY RACE

Problem Category	Blacks	Whites	Hispanics
Domestic Violence	3	3	6
Suspicion	2	7	6
Vehicle Problems	4	8	2
Juvenile Problems	1	3	2
Disputes	6	3	4
Disorders	6	7	12
Environment	5	6	1
Vehicle Crime	16	11	17
Burglary	9	16	7
Personal Crime	1	4	3
Vandalism	1	2	3
General Theft	-	4	3
Other	4	2	3
No Problem Mentioned	42	39	44
Total	110%	115%	113%
[N]	[105]	[210]	[107]

SOURCE: Gathered by police officers during directed patrol interviews in Houston 1983-84. Percentages are based upon total problems mentioned divided by the number of individuals contacted, so they may sum to more than 100%.

APPENDIX D:

SCALING THE RESIDENTIAL SURVEY DATA

SCALING THE RESIDENTIAL SURVEY DATA

This report describes how analytic scales were developed for the Fear Reduction Project Evaluation's panel sample surveys. These scales measure the central outcomes of interest in this project: perceptions and fear of crime, evaluations of the quality of police service, assessments of neighborhood problems, residential satisfaction, and crime related behaviors. Each measure is a composite of responses to two or more items which were included in the surveys to tap those dimensions. Such multiple-item scales yield more reliable, general, stable measurements of peoples attitudes and experiences than do responses to single survey questions.

CRITERIA

In each case the goal was to arrive at scales with the following properties:

1. Responses to each item should be consistent (all positively correlated). This was established by examining their intercorrelations, after some items were rescaled for directionality of scoring. A summary measure of the overall consistency of responses to a set of items is Cronbach's Alpha, which is an estimate of their joint reliability in producing a scale score for an individual.
2. Item responses should be homogeneous, or single-factored (indicating they all measure "the same thing"). This was established by a principle components factor analysis of the items hypothesized to represent a single dimension. The items were judged homogeneous when

they all loaded only on the first factor (their "principle component").

3. The items should share a substantial proportion of their variance with the hypothesized underlying dimension (perhaps precluding them from being significantly responsive to other conditions or events). This was demonstrated in two ways. Good items were those which evidenced a high correlation with others in the set. This was measured by their item-to-total correlation ("corrected" by excluding them from that particular total). Items were judged useful when, in a principal components factor analysys, the factor on which they fell accounted for a high proportion of their total variance (they had a high "communality").
4. The items on their face should seem related to a problem which is an object of one or more of the demonstration programs (suggesting they could be responsive to those interventions). Things which "scale together" based upon their naturally occurring covariation are not necessarily all useful, if they all should not be affected by the program of interest. The substantive utility of individual items cannot be statistically demonstrated; it is, rather, an argument.

The statistical analyses described above were done using SPSS-X. That system's RELIABILITY procedure generated inter-item correlations, calculated item-to-total correlations, and estimated a reliability coefficient (Cronbach's Alpha) for each set of item responses. FACTOR was used to extract the principal component from sets of items hypothesized to be unidimensional.

The scales were first developed using a random subset of the large Wave 1 survey data set. Then, all conclusions were confirmed and the scaling information presented below was calculated using the entire sample. The final scaling procedures then were duplicated separately for a number of subgroups, to examine whether or not things "went together" in the same fashion among those respondents. The scales were developed using unweighted data.

FEAR OF PERSONAL CRIME

Eight items were included in the survey to represent this general construct. Analysis of the first wave of the data indicated one should be dropped, and that the remaining set was two-factored.

The original items asked about the extent to which stranger assault, rape, and robbery were problems in the area, how worried the respondents were about being robbed, attacked, or being at home when someone broke in ("home invasion"), how safe they felt out alone in the area at night, and if there was a place nearby where they were afraid to walk.

An examination of correlations among these items indicated that worry about home invasion was only moderately correlated with the others, and excluding it from the group would improve the reliability of the resulting scale.

Excluding this item but using all of the others would yield an additive scale with a reliability of .78. However, a factor analysis of the remaining set suggested they were not unidimensional. Rather, three items asking about "how big a problem" specific personal crimes were <u>in the area</u> tapped a different dimension than those asking people how afraid they were and how worried they were about <u>personally</u> being victimized by the same types of crime. These

respondents seem to distinguish between personal risks and their general assessments of area problems. The two clusters of items loaded very distinctly on their unique factors, with high loadings.

Based upon this analysis, the following items were combined to form the "Fear of Personal Victimization in Area" measure:

Q34: How safe would you feel being outside alone in this area at night? (very safe to very unsafe)[1]

Q35: Is there any place in this areas where you would be afraid to go alone either during the day or at night? (yes or no).

Q43: [How worried are you that] someone will try to rob you or steal something from you while you are outside in this area? (very worried to not worried at all)

Q44: [How worried are you that] someone will try to attack you or beat you up while you are outside in this area? (very worried to not worried at all)

These items were added together to form a scale with a reliability of .72. The average item-total correlation of its components was .54, and the first factor explained 56 percent of the total variation in response to the items. Responses to Q35 were dichotomous, and as a result the item had only about two-thirds of the variance of Q43 and Q44, and one-half that of Q34. If such disparities are extreme, the items making up a simple additive scale will have a differential impact upon its apparent content. However, in this case there was no meaningful difference between the simple additive alpha and the alpha for a standardized scale score which equated the variances of its component parts. As a result, a simple additive scale score will be employed. A high score on this scale indicates respondents are fearful.

1. A few people who responded to Q34 that they "never go out" were rescored as "very unsafe" (see below).

The remaining items were combined to form the "Perceived Area Personal Crime Problems" scale:

[...please tell me whether you think it is a big problem, some problem, or no problem here in this area?]

Q114: People being attacked or beaten up by strangers?

Q117: People being robbed or having their money, purses or wallets taken?

Q121: Rape or other sexual assaults?

Because responses to these items all were measured on the same three-position set of response categories, the scale scores were generated by simply adding them together. As they had about the same mean and standard deviation (the rape question was somewhat lower on both), the items all contribute about equally to the total score for each individual. The factor lying behind these items accounted for 65 percent of their total variance. The reliability of the scale is .73. A high score on this issue indicates that these personal crimes were seen as "big problems in the area."

WORRY AND PERCEPTIONS ABOUT PROPERTY CRIME VICTIMIZATION IN AREA

There were five candidate items in this cluster. Three asked "how big a problem" burglary, auto theft, and auto vandalism were in the area, and two "how worried" respondents were about being victimized by burglary and auto theft or vandalism. Other research on concern about victimization or assessments of risk (see Baumer and Rosenbaum, 1981) indicates the distinction between personal and property crimes is a fundamental one, and that perceptions of the two are best gauged separately. (Auto vandalism was experimentally included among a set

of "disorder" items which included other vandalism activities, but empirically it belongs in this cluster of more serious crimes; (see below).

Although all five items clustered together, the following items were combined to for the "Worry About Property Crime Victimization in Area" scales:

Q45: [How worried are you that] someone will try to break into your home while no one is there? (Not worried at all to very worried)

Q47: [How worried are you that] someone will try to steal or damage your car in this area? (Not worried at all to very worried)

These two items were combined to form a scale. They were intercorrelated .43 and formed an additive scale with an Alpha of .60. Because the items employed similar three-category responses and they had about the same means and standard deviations, they were scaled by adding them together. A high score on this scale identifies respondents who are very worried about property crime.

The remaining three items were combined to form another scale, "Perceived Area Property Crime Problems" which, although highly correlated with the previously discussed "Worry about Property Crime" scale, omits, for theoreticial reasons, all emotive references such as "worry" or "fear." The average correlation among these items is .53; the Alpha was .77. The items were:

[...please tell me whether you think is a big problem, some problem, or no problem here in this area.]

Q68: People breaking in or sneaking into homes to steal things?

Q70: Cars being vandalized--things like windows or radio aerials being broken?

Q71: Cars being stolen?

PERCEIVED AREA SOCIAL DISORDER PROBLEMS

This is a concept introduced by Hunter (1978) (as "incivility"), and elaborated by Lewis and Salem (1981) and Skogan and Maxfield (1981). Many of its measures were first developed by Fowler and Mangione (1974). It has great currency in the research literature on the fear of crime. Recently, Wilson and Kelling (1982) have expanded its theoretical significance by linking disorders explicitly to the generation of other serious crimes, and lent it some controversy by recommending that disorders become the direct object of aggressive, neighborhood-based policing. The level of disorder has been shown to have direct consequences for aggregate levels of fear, community cohesion, and residential stability, in urban residential neighborhgoods and public housing projects (Skogan, 1983).

Seven candidate items were analyzed as part of the scale development process. They all focused upon deviant behaviors of varying illegality and seriousness, most of which take place in public locations. They were:

[...please tell me whether you think it is a big problem, some problem, or no problem at all.]

Q18: Groups of people hanging around on corners or in streets.

Q20: People saying insulting things or bothering people as they walk down the street?

Q24: People drinking in public places like on corners or in streets?

Q66: People breaking windows of buildings?

Q67: Graffiti, that is writing or painting on walls or windows?

Q113: Gangs?

Q120: Sale or use of drugs in public places?

Responses to these eight items were all positively intercorrelated (mean r=.40), and they had roughly similar means and variances. A scale "Perceived Area Social Disorder Problems," was formed by adding together responses to them. The principal component factor for these items explained 48 percent of their total variance. This scale has a reliability of .85. A high score on this scale points to areas in which these are seen as "big problems."

An additional six items included in the survey could have been included in a disorder scale. They were:

Q23: Truancy, that is, kids not being in school when they should be?

Q72: The wrong kind of people moving into the neighborhood?

Q119: Pornographic movie theaters or bookshops, massage parlors, topless bars?

Q116: Prostitutes?

Q19: Beggars or panhandlers?

Q115: Children being bothered on their way to and from school?

Responses to the these items were consistent with the others, but were excluded from the scale because they probed problems which were not explict foci of any program.

SATISFACTION WITH AREA

Satisfaction with the area was probed by two questions:

Q5: In general, since July of 1982, would you say this area has become a better place to live, gotten worse, or stayed about the same? (better, worse, or about the same)

Q14: On the whole, how do you feel about this area as a place to live? Are you... (very satisfied to very dissatisfied?)

Responses to these two questions were correlated .36, and had similar variances. Added together they formed a scale, "Satisfaction with Area," with a reliability of .50, good for a two-item measure. A high score on this scale identifies respondents who think their area is a good place to live, and has been getting better.

EVALUATIONS OF POLICE SERVICE AND AGGRESSIVENESS

A number of questions in the survey elicited evaluations of police service. Some items focused upon recent, specific police-citizen encounters which were identified in the survey, while others were "generic" and referenced more global opinions. Ten generic items were included in the questionnaire, and they revealed two distinct clusters of opinion: one referring to proactive, aggressive police action, and the other to the quality of services provided citizens and anticipated police demeanor in police-citizen encounters. A question referring to the strictness of traffic law enforcement was inconsistently correlated with most of the items, and had a low (about .10) correlation with the other measures of police aggressiveness; it was excluded completely.

Two general items consistently factored together, evidencing response patterns which differed from others focusing upon the police. Added together, they form a "Police Aggressiveness" measure. They are:

> [...please tell me whether you think it is a big problem, some problem, or no problem here in this area.]
>
> Q21: Police stopping too many people on the streets without good reason in this area?
>
> Q26: Police being too tough on people they stop?

These two items were correlated +.50, and when factor analyzed with the remaining set (see below) formed a significant second factor with loadings of .83 and .86, respectively. They had about the same mean and standard deviation, so they were scaled by adding them together. The scale has a reliability of .66, good for a two-item measure. A high score on this scale identifies people who think these are "big problems."

The remaining items also formed a distinct factor, and make up a second additive measure, "Evaluation of Police Service." They are:

Q50: How good a job do you think [police] are doing to prevent crime? (very good to very poor job)

Q51: How good a job do you think the police in this area are doing in helping people out after they have been victims of crime? (very good to very poor job)

Q52: How good a job are the police in this area doing in keeping order on the streets and sidewalks? (very good to very poor job)

Q57: In general, how polite are the police in this area when dealing with people? (very polite to very impolite)

Q58: In general, how helpful are the police in this area when dealing with people around here? (very helpful to not helpful at all)

Q59: In general, how fair are the police in this area in dealing with people around here? (very fair to very unfair)

The simple additive combination of these items has a reliability of .86, and they were correlated an average of .56. They were single factored, and their principal factor explained 60 percent of the total variation in the items. There was some variation in the response format for these items, but differences in the variances in the items were not great enough to preclude adding them together in simple fashion to form a scale. A high score on this measure points to a favorable evaluation of the police.

PERCEIVED AREA PHYSICAL DETERIORATION PROBLEMS

Itmes in this cluster refer to the prevalance of problems with trash, abandoned buildings, and dirty streets and sidewalks. These are interesting because their frequency presumably reflects the balance of two opposing forces: the pace at which people or businesses create these problems and the efficiency

with which the city deals with them. Identical conditions can result from differing mixes of either activity.

The questions were:

[...please tell me whether you think it is a big problem, some problem, or no problem here in this area?]

Q15: The first one is dirty streets and sidewalks in this area?

Q22: Abandoned houses or other empty buildings in this area?

Q65: Vacant lots filled with trash and junk?

Responses to these questions were moderately intercorrelated (an average of .36), but single-factored. That factor explained 57 percent of the variance in the items. They had similar means and standard deviations as well as sharing a response format, so they were scaled by adding them together. This measure has a reliability of .63. A high score on this scale indicates that physical deterioration is thought to be a problem in the area.

A related survey item (Q69) asking about problems with abandoned cars would scale with these, but that problem was not a target of the clean-up program in Newark.

CRIME PREVENTION EFFORTS

There are a series of anti-crime actions taken by city residents which might be relevant for this evaluation. Four questions in the surveys probed the extent to which respondents took <u>defensive behaviors to protect themselves</u> from personal victimization in public locations. They were asked:

The next questions are about some things people might do when they go out after dark. Now think about the last time you went out in this area after dark.

Q80: Did you go with someone else to avoid crime? (yes or no)

Q81: The last time you went out after dark in this area, did you stay away from certain streets or areas to avoid crime? (yes or no)

Q82: When you last went out after dark in this area, did you stay away from certain types of people to avoid crime? (yes or no)

Q86: In general, how often do you avoid going out after dark in this area because of crime? (never go out to never avoid)

In survey questions like these, a few respondents inevitably respond that they "never go out." With the exception of the disabled this is highly unlikely, and people who answer in this way frequently are fearful and score as high "avoiders" on the other measures. For analytic purposes it proves useful (see Skogan and Maxfield, 1981) to count them along with the others. The "message" they are communicating seems to be that "it's a dangerous place out there," so we have classed them as "precaution takers" and assigned them "yes" responses to these items.

Responses to these four items were very consistent. They were correlated an average of .41, and formed a simple additive scale "Defensive Behaviors" with a reliability of .74. The last item, Q86, was rescored so that its four response categories ranged in value betwen zero and one, like the others. The items then all had similar means and standard deviations. The resulting scale is a simple additive combination of the four.

A second set of behaviors measured in the survey referred to household crime prevention efforts. Several elements of the program were designed to increase the frequency with which people take such measures. Questions in the survey which tapped these activities included:

> The next few questions are about things that some people might do for protection from crime.
>
> Q74: Have any special locks been installed in this home for security reasons? (yes or no)
>
> Q75: Have any special outdoor lights been installed here to make it easier to see what's going on outside your home? (yes or no)
>
> Q76: Are there any timers for turning your lights on and off at night? (yes or no)
>
> Q77: Have any valuables here been marked with your name or some number? (yes or no)
>
> Q78: Have special windows or bars been installed for protection? (yes or no)
>
> Q85: Think about the last time when no one was home for at least a day or two. Did you ask a neighbor to watch your home? (yes or no)

Responses to these questions all were positively intercorrelated. The correlations often were low, however, probably due to the extremely skewed marginal distributions of many of them. For example, less than 20 percent reported having timers, marking their properly, and installing special security windows or bars. Nonparametric measures of association between these items--which are not affected by their skewed marginals--were more robust. Correlations between reports of the more normally distributed activities (39 percent have special locks, 30 percent outdoor lights, and 64 percent have neighbors watch their homes) were somewhat higher, averaging .20-.30. If added together, responses to these items would form a scale with a low reliability.

Also, a factor analysis of the entire set indicated they were not single-factored. Responses to Q75 and Q76, two questions about lighting, "went together" separately. So, in this evaluation analysis we simply added together the number of "yes" responses to the entire set of items, as a count of actions taken and, where relevant, analyzed the adoption of these measures separately.

DISTRIBUTION OF SCALE SCORES

Because they were to be used in multivariate regression analyses, it was important that the distribution of the scale scores described above meet the assumptions of regression. Also, one assumption in ANCOVA (carried out in this project using multiple regression) is that the relationship between pre- and post-test scores is linear, and this is also better determined if the scores themselves are fairly normally distributed. So, scale scores for both waves of each survey were examined for non-normality. Only one score for the Wave 1 panel survey was heavily skewed, (that for "Police Aggressiveness"), and it was logged for use in statistical analysis.

THE REPRODUCEABILITY OF SCALES AMONG SUBPOPULATIONS

Tables 1-3 summarize the reliability for the scales discussed above and present them for a variety of subgroups and area samples used in the evaluation. Table 1 presents the findings separately for Houston and Newark. Table 2 presents scale reliabilities for the major racial and ethnic groups surveyed in Houston--blacks, whites, and Hispanics. (In Newark, only largely black

neighborhoods were involved in the Fear Reduction Project.) Table 3 breaks the data down separately for the ten neighborhoods surveyed.

While the reliabilities presented here fluctuate from place-to-place and group-to-group, the generalizability of the scales used in the evaluation is evident. There is no evidence that special measures must be tailored for any particular group or area; rather, the various reports and analyses based upon these data can employ the same measures throughout.

A NOTE ON CALCULATING SCALE SCORES

There is a scattered amount of missing data for all of these items. There were substantially more missing data for questions dealing with the police than for generic questions about neighborhood conditions, probably reflecting many people's true ignorance of police affairs. Because a number of these scales summarize responses to several questions, if one missing element for a scale led to the complete exclusion of a respondent, the number of cases available for analysis would drop quite substantially. Because these items are single-factored and internally consistent, a better strategy is to let responses to components of a scale which <u>are</u> present "stand in" for occasional missing data. This was accomplished by basing each individual's calculated score on the sum of valid responses, standardized by the number of valid responses (scores = sum of response value/number of valid responses). Neither excluding respondents because of nonresponse nor fabricating data for them in the form of imputed values (such as means or "hot deck" values) is likely to be a superior strategy, in light of our scaling approach to measurement (cf. Kalton, 1983).

Table 1

Wave 1 Scale Reliabilities

All Respondents

Houston - Race Totals

Scale	Black	White	Hispanic
Fear of Personal Victimization in Area	.71	.71	.64
Perceived Area Personal Crime Problems	.76	.82	.79
Worry About Property Crime Victimization in Area	.63	.60	.69
Perceived Area Property Crime Problems	.79	.76	.79
Perceived Area Social Disorder Problems	.81	.82	.84
Satisfaction with Area	.51	.44	.39
Police Aggressiveness	.69	.60	.68
Evaluation of Police Service	.83	.84	.78
Perceived Area Physical Deterioration Problems	.60	.63	.61
Defensive Behaviors to Avoid Personal Crime	.69	.71	.66
(Cases)	(578)	(1091)	(443)

Table 2

Wave 1 Scale Reliabilities

All Respondents

City Totals

Scale	Total	Houston	Newark
Fear of Personal Victimization in Area	.72	.70	.74
Perceived Area Personal Crime Problems	.73	.80	.67
Worry About Property Crime Victimization in Area	.61	.62	.55
Perceived Area Property Crime Problems	.77	.77	.73
Perceived Area Social Disorder Problems	.84	.83	.77
Satisfaction with Area	.50	.44	.43
Police Aggressiveness	.66	.68	.64
Evaluation of Police Service	.86	.83	.84
Perceived Area Physical Deterioration Problems	.63	.62	.52
Defensive Behaviors to Avoid Personal Crime	.73	.69	.77
(Cases)	(4134)	(2178)	(1956)

Table 3

Wave 1 Scale Reliabilities

All Respondents

Area Totals

Scale	North line	Lang-wood	Wood Bayou	Golf Crest	Shady Acres	S-1	S-2	S-4	W-1	N-2
Fear of Personal Victimization in Area	.71	.69	.71	.68	.70	.74	.75	.74	.73	.72
Perceived Area Personal Crime Problems	.79	.80	.78	.83	.74	.68	.66	.57	.66	.72
Worry About Property Crime Victimization in Area	.65	.65	.56	.52	.67	.60	.69	.59	.63	.48
Perceived Area Property Crime Problems	.81	.78	.80	.71	.76	.77	.76	.72	.72	.74
Perceived Area Social Disorder Problems	.81	.81	.83	.84	.85	.73	.77	.77	.80	.74
Satisfaction with Area	.45	.48	.51	.42	.42			.44	.45	.45
Police Aggressiveness	.74	.66	.70	.65	.61	.71	.62	.71	.52	.60
Evaluation of Police Service	.86	.79	.83	.84	.80	.85	.82	.82	.85	.84
Perceived Area Physical Deterioration Problems	.67	.58	.62	.59	.57	.64	.52	.36	.56	.39
Defensive Behaviors to Avoid Personal Crime	.70	.67	.68	.71	.65	.73	.75	.78	.80	.76
(Cases)	(398)	(378)	(506)	(526)	(370)	(398)	(340)	(441)	(402)	(375)

APPENDIX E:

SCALING THE NON-RESIDENTIAL SURVEY DATA

SCALING THE NONRESIDENTIAL SURVEY DATA

This appendix describes how analytic scales were developed for the Fear Reduction Project Evaluation's nonresidential sample surveys. These scales measure the central outcomes of interest in this project: perceptions and fear of crime, evaluations of the quality of police service, assessments of neighborhood problems, and satisfaction with business conditions in the area. As in other components of this evaluation, outcomes were measured by a composite of responses to two or more items which were included in the surveys to tap those dimensions. The item combination which was finally used to represent each outcome was determined by examining responses to the first, pre-test, surveys conducted in all areas of Houston and Newark. Scaling decisions were then verified on the post-test surveys. The pre-intervention survey with 414 business establishments was used to determine the empirical relationship between responses to survey items. They were intercorrelated and factor analyzed, and the results of those analyses informed our final scaling decisions. However, the scales also were formed based upon past research, to maintain consistency with other surveys conducted as part of the Fear Reduction evaluation, and to maintain their conceptual unity. Always, the programmatic relevance of each item played an important role in determining whether or not it would be included in the final scales.

FEAR OF PERSONAL VICTIMIZATION IN AREA

A number of items were included in the survey to represent this general construct. After examining the pre-intervention data, three measures of various forms of fear of crime were developed. The following items were combined to form a measure of "Fear of Personal Victimization in Area:

Q26: How safe would you feel while working here alone during the day? (very safe to very unsafe)

Q27: How about while working here after dark? How safe would you feel if you were to work here after dark? (very safe to very unsafe)

Q28: How safe would you feel being outside alone in this area after dark? (very safe or very unsafe)

Q42: How worried are you that someone will try to rob you or steal something from you here in this establishment? (very worried or not very worried at all)

Q43: What about outside of this establishment? How worried are you that someone will try to rob you or steal something from you somewhere else in this area? (very worried or not very worried at all)

These items were added together to form a scale with a reliability of .84. The average item-total correlation of its components was .51, and the first factor explained 61 percent of the total variation in response to the items. There was no meaningful difference between the additive alpha and the alpha for a standardized scale score which equated the variances of its component parts (also .84). Therefore, a simple additive scale was employed. A high score on the measure indicates respondents were fearful of personal victimization in and around their establishments.

Two other items were combined to form a measure of the "Perceived Concern About Crime" expressed by employees and patrons of the establishments, as reported by our respondents. They were:

Q29: In the last month, how frequently have you heard employees express concern about their personal security in this area? (very frequently to never?)

Q30: In the last month, how frequently have you heard people who come here express concern about their personal security in this area? (very frequently to never)

Responses to these items all were measured on the same four-position set of response categories. As they had about the same mean and standard deviation, the items contribute about equally to the total score for each individual. The correlation between responses to the two items was .54, and the reliability of the resulting scale was .70. These items factored separately from the previous measure of personal fear.

Two survey questions were posed to measure "Worry About Property Crime in the Area;" they asked "how worried" respondents were about being victimized by burglary and vandalism. Other research on concern about victimization or assessments of risk (see Baumer and Rosenbaum, 1981) indicates the distinction between personal and property crimes is a fundamental one, and that perceptions of the two are best gauged separately.

Q44: [How worried are you that] someone will try to break into this place to steal something? (not worried at all to very worried)

Q45: [How worried are you that] someone will try to vandalize this place? (Not worried at all to very worried)

These two items were combined to form a multiple item scale; they were substantially intercorrelated (.72) and formed an additive scale with an Alpha of .84. A high score on this measure identifies respondents who are worried about area burglary and vandalism. Another question asked, "How big a problem"

burglary of business was in the area. Responses to this item are analyzed separately.

PERCEIVED AREA SOCIAL DISORDER PROBLEMS

Six candidate items for this cluster were analyzed as part of the scale development process. They all focused upon deviant behaviors of varying illegality and seriousness, most of which takes place in public locations. They were:

[...please tell me whether you think it is a big problem, some problem, or no problem at all.]

Q15: People saying insulting things or bothering people as they walk down the street?

Q18: People drinking in public places, like on corners or in streets?

Q19: People breaking windows of buildings?

Q16: Graffiti, that is, writing or painting on walls or windows?

Q14: Gangs?

Q25: Sale or use of drugs in public places?

Responses to these items were all positively intercorrelated (mean r=.39). They had roughly similar means and variances, so the scale was formed by adding together responses to them. The principal component factor for these items explained 50 percent of their total variance. This scale has a reliability of .80. A high score on this measure points to areas in which these are seen as "big problems."

In addition, several items included in the survey could have been included in a disorder scale. They were:

Q17: Truancy, that is, kids no being in school when they should be?

Q24: Prostitutes?

Q13: Beggars or panhandlers?

Responses to these items were consistent with the others, but were excluded from the scale because they probed problems which were not the explicit focus of any of the Fear Reduction programs.

Two items were combined to form a measure of "Perceived Area Physical Deterioration Problems." They were:

Q20: [How big a problem here in this area?] Abandoned stores or other empty buildings? (No problem to big problem)

Q23: [How big a problem here in this area?] Dirty streets and sidewalks? (no problem to big problem)

Responses to these two items were correlated .44, and combined they formed an additive scale with a reliability of .61, good for a two-item measure. A high score on this measure identifies respondents who thought that these forms of physical decay were "big problems" in their area.

SATISFACTION WITH AREA

Two measures of satisfaction with neighborhood conditions were developed. The first probed general satisfaction with the area:

Q7: On the whole, how do you feel about this area as a place for this establishment? Are you (very satisfied to very dissatisfied)

Q8: Since July of 1982, would you say this area has generally become a better place to be located, gotten worse, or stayed about the same?

Responses to these two questions were correlated .34, and had similar variances. Added together they formed a scale with a reliability of .48, only marginally acceptable. A high score on this measure identifies respondents who think their area is a good place to work, and has been getting to be a better place to be located.

A second measure points directly to perceived changes in the business environment in the recent past. Respondents were asked if, "since July of 1982" (the onset of the program):

Q9: ...has the number of people who come here increased, decreased, or stayed about the same?

Q12: What about the amount of business done here? Compared to last year, has that increased, decreased, or stayed about the same?

Responses to these items were correlated .58, and formed an additive scale with a reliability of .73, very high for a 2-item scale. These two items factored separately from the previous set measuring general perceptions of the area.

EVALUATION OF POLICE SERVICE

A number of questions in the survey gathered evaluations of police service. Some items focused upon recent, specific encounters between the police and those interviewed in the nonresidential survey, while others were "generic" and referenced more global opinions. Six generic items were included in the questionnaire, and they revealed one distinct cluster of opinion concerning the quality of services provided citizens and anticipated police demeanor in police-citizen encounters.

Q46: How good a job are the police in this area doing to prevent crime to businesses and other establishments? (very good to very poor job)

Q47: How good a job do you think the police are doing in helping busineses and other establishments out after they have been victims of crime? (very good to very poor job)

Q50: How good a job are the police in this area doing in keeping order on the streets and sidewalks? (very good to very poor job)

Q53: In general, how polite are the police in this area when dealing with people in businesses and other establishments? (very polite to very impolite)

Q54: In general, how helpful are the police in this area when dealing with people in business and other establishments? (very helpful to not helpful at all)

Q55: In general, how fair are the police in this area in dealing with people in business and other establishments? (very fair to very unfair)

The simple additive combination of these items has a reliability of .89, and they were correlated an average of .57. They were single factored. There was some variation in the wording of the response format for these items, but differences in the variances in the items were not great enough to preclude adding them together in simple fashion. A high score on this measure points to a favorable evaluation of the police.

THE REPRODUCEABILITY OF SCALES AMONG AREAS

Table 1 summarizes the reliabilities for the scales discussed above, and presents them for the area samples used in the evaluation. The non-residential survey samples for individual areas were quite small, so the reliabilities presented there fluctuate from place-to-place. However, the generalizability of the scales used in the evaluation is evident. The only notable exception is the general area satisfaction measure for the Langwood area in Houston, and the

two items which go into it will be analyzed separately for that area. There is no evidence in Table 1 that other special measures must be tailored for any particular area; rather, the various reports and analyses based upon this data can employ the same measures throughout.

A NOTE ON CALCULATING SCALE SCORES

There is a scattered amount of missing data for all of these items. There were substantially more missing data for questions dealing with the police than for generic questions about neighborhood conditions, probably reflecting many people's true ignorance of police affairs. Because a number of these scales summarize responses to several questions, if one missing element for a scale led to the complete exclusion of a respondent, the number of cases available for analysis would drop quite substantially. Because these items are single-factored and internally consistent, a better strategy is to let responses to components of a scale which <u>are</u> present "stand in" for occasional missing data. This was accomplished by basing each individual's calculated score on the sum of valid responses, standardized by the number of valid responses (score = sum of responses values/number of valid responses). Neither excluding respondents, because of nonresponse nor fabricating data for them in the form of imputed values (such as means or "hot deck" values) is likely to be a superior strategy, in light of our scaling approach to measurement (cf, Kalton, 1983).

SCALE RELIABILITY SUMMARY

Non-Residential Survey

	All Areas		South 1		West 1		South 4		Northline		Langwood		Golfcrest		Shady Acres	
Scale	Wave 1	Wave 2	Wave 1	Wave 2	Wave 1	Wave 2	Wave 1	Wave 2	Wave 1	Wave 2	Wave 1	Wave 2	Wave 1	Wave 2	Wave 1	Wave 2
Fear of Personal Victimization in Area	.84	.84	.83	.79	.80	.85	.86	.90	.81	.82	.80	.74	.84	.87	.85	.86
Evaluation of Police Service	.89	.86	.90	.86	.88	.87	.92	.91	.86	.89	.84	.80	.87	.84	.63	.86
Perceived Social Disorder Problems	.80	.79	.64	.78	.71	.79	.74	.65	.76	.55	.81	.51	.85	.83	.65	.71
Business Change	.73	.78	.61	.82	.68	.65	.33	.85	.80	.77	.76	.76	.82	.83	.54	.62
Satisfaction With Area	.48	.54	.57	.43	.69	.31	.67	.72	.54	.57	.00	.68	.44	.53	.35	.44
Worry About Property Crime	.84	.80	.97	.93	.88	.72	.92	.78	.76	.84	.86	.94	.84	.66	.90	.77
Employee-Patrol Concern	.70	.81	.82	.99	.66	.57	.84	.82	.68	.78	.54	.82	.67	.79	.56	.40
(N)*	(414)	(283)	(34)	(47)	(26)	(28)	(35)	(32)	(44)	(41)	(37)	(27)	(67)	(66)	(39)	(42)

* Ns vary slightly from scale to scale; figure here is for fear scale

APPENDIX F:

RESIDENTIAL AREA-LEVEL RESULTS

TABLE F-1

Wave One - Wave Two Outcome Measures

All Respondents

Fear of Personal Victimization

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	1.80	1.63	1.69	1.65
(sd)	(.57)	(.58)	(.56)	(.61)
[N]	[543]	[559]	[389]	[403]
Sigf.	$p < .001$		$p < .25$	
Q34 Unsafe Alone				
Mean	2.77	2.52	2.79	2.68
(sd)	(1.03)	(.99)	(1.04)	(1.12)
[N]	[543]	[559]	[387]	[396]
Sigf.	$p < .001$		$p < .10$	
Q35 Place Fear to Go				
Mean	.57	.52	.54	.60
(sd)	(.50)	(.50)	(.50)	(.49)
[N]	[528]	[552]	[376]	[394]
Sigf.	$p < .10$		$p < .05$	
Q43 Worry robbery				
Mean	1.98	1.50	1.78	1.73
(sd)	(.75)	(.75)	(.72)	(.79)
[N]	[540]	[559]	[385]	[401]
Sigf.	$p < .001$		$p < .25$	
Q44 Worry assault				
Mean	1.85	1.64	1.59	1.59
(sd)	(.77)	(.74)	(.71)	(.74)
[N]	[536]	[557]	[384]	[399]
Sigf.	$p < .001$		$p < .75$	

Note: One-tailed t-test

*rescored so high score indicates fear

TABLE F-2

Door to Door

Wave One - Wave Two Outcome Measures

All Respondents

Perceived Area Personal Crime Problem

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	1.54	1.32	1.44	1.38
(sd)	(.64)	(.51)	(.57)	(.55)
[N]	[529]	[554]	[372]	[394]
Sigf.	$p < .001$		$p < .10$	
Q114 Stranger Assault a big problem				
Mean	1.53	1.32	1.48	1.39
(sd)	(.72)	(.59)	(.70)	(.66)
[N]	[496]	[543]	[352]	[373]
Sigf.	$p < .001$		$p = < .05$	
Q117 Robbery a big problem				
Mean	1.61	1.42	1.54	1.48
(sd)	(.76)	(.65)	(.71)	(.72)
[N]	[505]	[543]	[353]	[377]
Sigf.	$p < .001$		$p < .25$	
Q121 Rape a big problem				
Mean	1.43	1.23	1.23	1.22
(sd)	(.70)	(.53)	(.54)	(.54)
[N]	[456)	[533]	[333]	[361]
Sigf.	$p < .001$		$p < .50$	

Note: One-tailed t-test

*Rescored so high score indicates fear

TABLE F-3

Wave One - Wave Two Outcome Measures

All Respondents

Concern About Area Property Crime

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	1.96	1.74	1.73	1.67
(sd)	(.55)	(.57)	(.55)	(.57)
[N]	[543]	[558]	[388]	[403]
Sigf.	$p < .001$		$p < .10$	
Q45 Burglary worry				
Mean	2.23	2.06	2.09	1.94
(sd)	(.77)	(.78)	(.76)	(.82)
[N]	[541]	[555]	[387]	[399]
Sigf.	$p < .01$		$p < .005$	
Q47 Auto theft worry				
Mean	2.08	1.89	1.76	1.75
(sd)	(.79)	(.82)	(.78)	(.82)
[N]	[505]	[532]	[364]	[355]
Sigf.	$p < .001$		$p < .50$	
Q68 Burglary problem				
Mean	2.03	1.64	1.82	1.71
(sd)	(.81)	(.75)	(.78)	(.76)
[N]	[515]	[545]	[361]	[384]
Sigf.	$p < .001$		$p < .05$	
Q70 Auto vandalism problem				
Mean	1.72	1.55	1.48	1.47
(sd)	(.81)	(.75)	(.69)	(.71)
[N]	[513]	[551]	[364]	[381]
Sigf.	$p < .001$		$p < .50$	
Q71 Auto theft problem				
Mean	1.74	1.58	1.48	1.44
(sd)	(.80)	(.78)	(.72)	(.72)
[N]	[498]	[547]	[356]	[380]
Sigf.	$p < .001$		$p < .25$	

Note: One-tailed t-test

TABLE F-4

Wave One - Wave Two Outcome Measures

All Respondents

Disorder Problems in Area

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	1.49	1.30	1.40	1.39
(sd)	(.51)	(.39)	(.46)	(.47)
[N]	[543]	[560]	[387]	[402]
Sigf.	$p < .001$		$p < .40$	
Q18 Groups hanging around on corners				
Mean	1.67	1.40	1.63	1.57
(sd)	(.82)	(.67)	(.80)	(.77)
[N]	[516]	[550]	[374]	[388]
Sigf.	$p < .001$		$p < .25$	
Q20 People saying insulting things				
Mean	1.29	1.18	1.27	1.25
(sd)	(.58)	(.47)	(.59)	(.60)
[N]	[520]	[556]	[375]	[385]
Sigf.	$p < .001$		$p < .40$	
Q24 Drinking in public place				
Mean	1.64	1.34	1.53	1.52
(sd)	(.82)	(.62)	(.73)	(.77)
[N]	[516]	[550]	[375]	[386]
Sigf.	$p < .001$		$p < .50$	
Q66 Breaking Windows				
Mean	1.45	1.31	1.39	1.41
(sd)	(.71)	(.58)	(.64)	(.68)
[N]	[520]	[548]	[363]	[388]
Sigf.	$p < .001$		$p < .40$	

TABLE F-4 continued

Wave One - Wave Two Outcome Measures

All Respondents

Disorder Problems in Area

	Program Area		Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Q67 Graffiti				
Mean	1.41	1.24	1.29	1.33
(sd)	(.69)	(.52)	(.57)	(.62)
[N]	[519]	[554]	[370]	[385]
Sigf.	$p < .001$		$p < .25$	
Q118 Gang				
Mean	1.38	1.23	1.29	1.21
(sd)	(.68)	(.53)	(.58)	(.50)
[N]	[493]	[525]	[355]	[380]
Sigf.	$p < .001$		$p < .025$	
Q120 Sale or use of drugs in public places				
Mean	1.60	1.39	1.48	1.39
(sd)	(.78)	(.66)	(.75)	(.69)
[N]	[452]	[506]	[321]	[353]
Sigf.	$p < .001$		$p < .10$	

Note: One-tailed t-test

TABLE F-5

Wave One - Wave Two Outcome Measures

All Respondents

Satisfaction With Area

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	2.42	2.64	2.51	2.60
(sd)	(.62)	(.59)	(.61)	(.60)
[N]	[543]	[558]	[389]	[403]
Sigf.	$p < .001$		$p < .025$	
Q5 Area getting better				
Mean	1.77	2.11	1.82	1.94
(sd)	(.64)	(.65)	(.60)	(.60)
[N]	[524]	[544]	[371]	[382]
Sigf.	$p < .001$		$p = < .005$	
Q14 Satisfied with the area				
Mean	3.03	3.14	3.14	3.22
(sd)	(.85)	(.76)	(.81)	(.77)
[N]	[541]	[554]	[385]	[398]
Sigf.	$p < .025$		$p < .10$	

Note: One-tailed t-test

TABLE F-6

Wave One - Wave Two Outcome Measures

All Respondents

Evaluations of Police Service

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	3.24	3.49	3.23	3.37
(sd)	(.71)	(.64)	(.63)	(.71)
[N]	[535]	[552]	[372]	[388]
Sigf.	$p < .001$		$p < .005$	
Q50 Good job at preventing crime				
Mean	3.22	3.69	3.29	3.56
(sd)	(1.07)	(.97)	(.96)	(1.01)
[N]	[493]	[539]	[348]	[365]
Sigf.	$p < .001$		$p = < .001$	
Q51 Good job of helping victims				
Mean	3.28	3.53	3.14	3.36
(sd)	(1.08)	(1.02)	(1.05)	(1.14)
[N]	[449]	[489]	[288]	[282]
Sigf.	$p < .001$		$p < .01$	
Q52 Good job keeping order on street				
Mean	3.40	3.76	3.46	3.63
(sd)	(1.00)	(.92)	(.88)	(.97)
[N]	[495)	[530]	[341]	[350]
Sigf.	$p < .001$		$p < .01$	

TABLE F-6 continued

Wave One - Wave Two Outcome Measures

All Respondents

Evaluations of Police Service

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Q57 Polite in dealing with people				
Mean	3.26	3.38	3.20	3.27
(sd)	(.77)	(.70)	(.78)	(.79)
[N]	[450]	[513]	[312]	[311]
Sigf.	$p < .01$		$p < .25$	
Q58 Helpful in dealing with people				
Mean	3.13	3.30	3.12	3.22
(sd)	(.78)	(.69)	(.74)	(.73)
[N]	[453]	[502]	[306]	[325]
Sigf.	$p < .001$		$p = < .05$	
Q59 Fair in dealing with people				
Mean	3.23	3.29	3.16	3.22
(sd)	(.67)	(.64)	(.64)	(.65)
[N]	[449]	[511]	[289]	[314]
Sigf.	$p < .10$		$p < .25$	

Note: One-tailed t-test

TABLE F-7

Wave One - Wave Two Outcome Measures

All Respondents

Police Aggressiveness

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	1.22	1.13	1.15	1.11
(sd)	(.48)	(.35)	(.40)	(.32)
[N]	[509]	[552]	[363]	[375]
Sigf.	$p < .001$		$p < .10$	
Q21 Stop too many without good reason				
Mean	1.20	1.13	1.10	1.09
(sd)	(.53)	(.41)	(.37)	(.35)
[N]	[480]	[548]	[340]	[359]
Sigf.	$p < .01$		$p = < .40$	
Q26 Too tough on people they stop				
Mean	1.25	1.13	1.23	1.14
(sd)	(.59)	(.38)	(.56)	(.44)
[N]	[444]	[529]	[293]	[337]
Sigf.	$p < .001$		$p < .025$	

Note: One-tailed t-test

TABLE F-8

Wave One - Wave Two Outcome Measures

All Respondents

Defensive Behavior

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	.49	.47	.44	.47
(sd)	(.34)	(.36)	(.34)	(.35)
[N]	*[542]	[560]	[387]	[403]
Sigf.	p < .25		p < .25	
Q80 Go with escort*				
Mean	.49	.44	.41	.43
(sd)	(.50)	(.50)	(.49)	(.49)
[N]	*[541]	[560]	[385]	[402]
Sigf.	p < .05		p < .40	
Q81 Avoid certain areas*				
Mean	.50	.49	.43	.49
(sd)	(.50)	(.50)	(.50)	(.50)
[N]	*[539]	[559]	[387]	[399]
Sigf.	p < .40		p < .05	
Q82 Avoid types of people				
Mean	.58	.55	.53	.58
(sd)	(.49)	(.50)	(.50)	(.49)
[N]	[538]	[559]	[385]	[400]
Sigf.	p < .25		p < .10	
Q86 Avoid going out after dark				
Mean	2.09	2.24	1.97	2.13
(sd)	(.86)	(1.25)	(.87)	(1.20)
[N]	[542]	[559]	[384]	[402]
Sigf.	p < .025		p < .025	

Note: One-tailed t-test

*Rescored so high score indicates taking precaution

APPENDIX G

RESIDENTIAL PANEL RESULTS

TABLE G-1

Relation Between Residence in Treatment or Control Areas
and Post-Intervention Outcome Measures
Controlling for the Pre-Test and Other Explanatory Factors

Explanatory Factors	Outcome Measures: Fear of Personal Victimization		Concern About Area Personal Crime	
	Beta	(Sigf.)	Beta	(Sigf.)
live in target area	-.07	.10	-.11	.02
pretest scale score	.32	.001	.21	.001
personal attributes				
age	.03	.61	.04	.60
sex-female	.20	.001	-.00	.99
black	-.08	.11	-.11	.05
hispanic/other	.02	.63	.01	.83
income category	.03	.54	.01	.88
education category	.02	.67	.03	.53
length of residence	.05	.41	-.07	.34
marital - single	-.07	.16	-.01	.87
adults in household	-.05	.33	-.04	.39
single family head	.01	.84	.05	.35
rent home	.03	.56	.04	.52
live in large apt.	.03	.46	.05	.29
victimization				
number of times victim	.15	.06	.30	.001
number personal victim	-.07	.21	-.14	.02
number burglary victim	-.05	.38	-.21	.001
know victims in area	.01	.87	.17	.001
R^2 =	.19		.13	
(N)	(489)		(468)	

Note: All independent variables were measured using the pre-intervention survey only.

TABLE G-1 continued

Relation Between Residence in Treatment or Control Areas
and Post-Intervention Outcome Measures
Controlling for the Pre-Test and Other Explanatory Factors

Explanatory Factors	Outcome Measures: Concern about Area Property Crime		Satisfaction with Area	
	Beta	(Sigf.)	Beta	(Sigf.)
live in target area	-.08	.08	.13	.01
pretest scale score	.31	.001	.25	.001
personal attributes				
age	-.10	.13	.22	.01
sex-female	.08	.06	-.02	.71
black	.00	.99	.02	.70
hispanic/other	.00	.95	.10	.06
income category	.04	.46	-.02	.76
education category	.02	.59	-.00	.99
length of residence	.07	.28	-.08	.24
marital - single	-.08	.11	.04	.40
adults in household	-.09	.10	.02	.70
single family head	-.03	.60	.00	.95
rent home	.06	.27	-.06	.30
live in large apt.	.02	.65	.04	.35
victimization				
number of times victim	.32	.001	-.09	.28
number personal victim	-.17	.01	.08	.19
number burglary victim	-.17	.01	.13	.05
know victims in area	.03	.49	.03	.49
R^2 =	.16		.08	
(N)	(478)		(490)	

Note: All independent variables were measured using the pre-intervention survey only.

TABLE G-1 continued

Relation Between Residence in Treatment or Control Areas
and Post-Intervention Outcome Measures
Controlling for the Pre-Test and Other Explanatory Factors

Explanatory Factors	Outcome Measures: Disorder Problems	
	Beta	(Sigf.)
live in target area	-.17	.001
pretest scale score	.42	.001
personal attributes		
age	-.15	.02
sex-female	.02	.56
black	-.05	.32
hispanic/other	-.02	.63
income category	-.02	.70
education category	.02	.73
length of residence	.04	.56
marital - single	-.09	.07
adults in household	-.06	.21
single family head	-.06	.25
rent home	.01	.84
live in large apt.	-.00	.94
victimization		
number of times victim	.18	.02
number personal victim	-.11	.05
number burglary victim	-.08	.20
know victims in area	-.01	.73
R^2 =	.22	
(N)	(489)	

Note: All independent variables were measured using the pre-intervention survey only.

TABLE G-1 continued

Relation Between Residence in Treatment or Control Areas
and Post-Intervention Outcome Measures
Controlling for the Pre-Test and Other Explanatory Factors

Explanatory Factors	Outcome Measures: Evaluation of Police Service		Police Aggressiveness*	
	Beta	(Sigf.)	Beta	(Sigf.)
live in target area	.17	.001	-.20	.001
pretest scale score	.33	.001	.03	.55
personal attributes				
age	.22	.001	.07	.35
sex-female	.02	.68	.07	.15
black	-.08	.16	-.06	.30
hispanic/other	-.02	.65	-.04	.50
income category	-.03	.47	-.01	.83
education category	.01	.87	.03	.57
length of residence	-.08	.22	-.03	.71
marital - single	.11	.03	-.05	.37
adults in household	-.09	.07	.04	.43
single family head	-.05	.28	.01	.92
rent home	-.06	.24	-.04	.55
live in large apt.	-.07	.11	-.06	.25
victimization				
number of times victim	.02	.79	-.17	.06
number personal victim	-.01	.83	-.01	.89
number burglary victim	-.04	.51	.08	.25
know victims in area	.00	.98	-.05	.32
R^2 =	.21		.08	
(N)	(472)		(447)	

Note: All independent variables were measured using the pre-intervention survey only.

* Logged to normalize distribution

TABLE G-2

Relationship Between Program Area of Residence
and Reports of Victimization

Panel Respondents Only

Type of Victimization	Correlation (and significance) with Program Area Residence: No Controls r	(sigf)	Control r	Pretest (sigf)	Control r	Pretest +13 Factors (sigf)	[N]
All types:	-.03	(.52)	-.06	(.16)	-.08	(.07)	[490]
Personal Victimization	.02	(.71)	.00	(.99)	-.03	(.56)	[490]
Property Victimization	-.05	(.29)	-.08	(.07)	-.09	(.05)	[490]

Notes:
- Correlation is Pearson's r;
- Victimization measure is count of incidents from zero to maximum
- "Pretest" is victimization during 6 months prior to Wave 1 study
- All correlations are for the same subset of respondents with complete data on all measures
- All control factors measured using Wave 1 survey

APPENDIX H:

WAVE 1 "FEAR OF PERSONAL VICTIMIZATION" SCORES BY DEMOGRAPHIC GROUP

APPENDIX H

"FEAR OF PERSONAL VICTIMIZATION" SCORES
AT WAVE 1 BY DEMOGRAPHIC GROUPS
PROGRAM AND COMPARISON AREAS

(All Respondents)

Demographic Group	Program Area (Golfcrest) Wave 1	Comparison Area (Shady Acres) Wave 1
Male	1.64	1.54
Female	1.94	1.84
Black	1.71	1.56
White	1.85	1.71
Hispanic	1.85	1.73
Asian-Pacific Islander	1.56	1.67
Owner	1.85	1.78
Renter	1.77	1.62
Not High School	1.84	1.79
High School Graduate	1.79	1.61
Under $15,000 Income	1.83	1.75
Over $15,000 Income	1.76	1.64
15-24 years	1.70	1.59
25-49 years	1.80	1.66
50-98 years	1.89	1.77
No Children at Home	1.78	1.68
One or More Children at Home	1.82	1.69
One Adult in Household	1.82	1.68
Two Adults	1.81	1.72
Three or More Adults	1.78	1.53
Single	1.76	1.68
Married	1.84	1.69
Work Full-Part Time	1.74	1.63
Other	1.91	1.79
Resident 0-2 years	1.73	1.63
3-5 years	1.70	1.66
6-9 years	1.94	1.72
10+ years	1.99	1.79

APPENDIX I:

DIFFERENTIAL IMPACT ANALYSIS RESULTS

TABLE I-1

Differential Impact Analysis

Regression Analysis of the Input of Program Area Residence Upon Subgroups

Panel Respondents Only

Wave 2 Outcome	Aged Subgroup Impact Variable	Beta	(Sigf)	Female Subgroup Impact Variable	Beta	(Sigf)
Fear of victimization	Interaction	.09	(.46)	Interaction	.08	(.30)
Personal crime problems	Interaction	-.11	(.38)	Interaction	-.01	(.95)
Property crime problems	Interaction	-.05	(.71)	Interaction	-.03	(.69)
Disorder problems	Interaction	-.03	(.80)	Interaction	.07	(.40)
Satisfaction with area	Interaction	.01	(.94)	Interaction	-.11	(.22)
Evaluations of police	Interaction	.08	(.49)	Interaction	-.15	(.08)
Total victimization	Interaction	.13	(.31)	Interaction	.00	(.98)

Note: "N" approximately 490 for all analyses

TABLE I-1
(continued)

Differential Impact Analysis

Regression Analysis of the Input of Program Area Residence Upon Subgroups

Panel Respondents Only

Wave 2 Outcome	Wave 1 Victim Impact Variable	Beta	(Sigf)	Black Subgroup Impact Variable	Beta	(Sigf)
Fear of victimization	Interaction	-.02	(.78)	Interaction	.08	(.22)
Personal crime problems	Interaction	.08	(.41)	Interaction	.16	(.03)
Property crime problems	Interaction	.03	(.75)	Interaction	.12	(.08)
Disorder problems	Interaction	.10	(.23)	Interaction	.09	(.18)
Satisfaction with area	Interaction	.01	(.95)	Interaction	-.20	(.01)
Evaluations of police	Interaction	.08	(.38)	Interaction	-.18	(.01)
Total victimization	Interaction	-.11	(.24)	Interaction	.16	(.03)

Note: "N" approximately 490 for all analyses

TABLE I-1
(continued)

Differential Impact Analysis

Regression Analysis of the Input of Program Area Residence Upon Subgroups

Panel Respondents Only

Wave 2 Outcome	Asian/Hispanic Subgroup Impact Variable	Beta	(Sigf)	Renter Subgroup Impact Variable	Beta	(Sigf)
Fear of victimization	Interaction	-.06	(.46)	Interaction	.07	(.33)
Personal crime problems	Interaction	-.11	(.20)	Interaction	.16	(.05)
Property crime problems	Interaction	-.05	(.54)	Interaction	.08	(.33)
Disorder problems	Interaction	-.16	(.04)	Interaction	.07	(.34)
Satisfaction with area	Interaction	.15	(.07)	Interaction	-.20	(.01)
Evaluations of police	Interaction	.07	(.42)	Interaction	-.24	(.01)
Total victimization	Interaction	-.14	(.12)	Interaction	.07	(.40)

Note: "N" approximately 490 for all analyses

TABLE I-1
(continued)

Differential Impact Analysis

Regression Analysis of the Input of Program Area Residence Upon Subgroups

Panel Respondents Only

Wave 2 Outcome	Single Family Home Subgroup Impact Variable	Beta	(Sigf)
Fear of victimization	Interaction	-.07	(.41)
Personal crime problems	Interaction	-.17	(.07)
Property crime problems	Interaction	-.10	(.25)
Disorder problems	Interaction	-.03	(.71)
Satisfaction with area	Interaction	.10	(.30)
Evaluations of police	Interaction	.23	(.01)
Total victimization	Interaction	-.08	(.37)

Note: "N" approximately 490 for all analyses

APPENDIX J:

RECALLED PROGRAM EXPOSURE EFFECT RESULTS

TABLE J-1

Relationship Between Self-Reported Program Exposure and Outcome Measures

Q60-61: Last Time Saw a Police Officer?

Panel Respondents in Golfcrest Program Area Only

Scale Score Outcome	Correlation (and significance level) between recall exposure measure and outcome scores controlling for other factors: Simple correlation only		Partial correlation controlling for pretest		Partial correlation controlling for sixteen factors**		[N]
	r	(sigf)	r	(sigf)	r	(sigf)	
Fear of Victimization	-.15	.01	-.15	.01	.05	.05	[311]
Perceived Area Personal Crime Problems	-.01	.90	-.00	.94	-.01	.89	[310]
Worry About Area Property Crime Victimization	-.08	.14	-.09	.12	-.07	.22	[310]
Perceived Area Property Crime Problems	.06	.30	.07	.22	.05	.35	[310]
Satisfaction With Area	.13	.03	.11	.06	.14	.01	[312]
Perceived Area Social Disorder Problems	-.02	.75	-.01	.88	-.01	.93	[312]
Evaluations of Police Service	.17	.01	.14	.01	.21	.001	[309]
Police Aggressiveness (Log)	-.03	.59	-.00	.96	.01	.81	[308]
Defensive Behaviors To Avoid Personal Crime	-.10	.08	-.06	.28	-.04	.53	[312]
Household Crime Prevention Measures	-.05	.42	-.01	.85	.03	.63	[312]

**includes indications of age, race, sex, income education, length of residence, marital status, household organization and size, renter status, building size, personal victimization, knowledge of local crime victims, and the pretest.

TABLE J-2

Relationship Between Self-Reported Program Exposure and Outcome Measures

Q100: Police Come to Your Door?

Panel Respondents in Golfcrest Program Area Only

Scale Score Outcome	Correlation (and significance level) between recall exposure measure and outcome scores controlling for other factors						[N]
	Simple correlation only		Partial correlation controlling for pretest		Partial correlation controlling for sixteen factors**		
	r	(sigf)	r	(sigf)	r	(sigf)	
Fear of Victimization	-.02	.71	-.05	.38	-.06	.33	[311]
Perceived Area Personal Crikme Problems	-.08	.14	-.12	.04	-.13	.03	[309]
Worry About Area Property Crime Problems	-.13	.02	-.13	.03	-.14	.01	[310]
Perceived Area Property Crime Problems	-.06	.28	-.08	.16	-.09	.15	[311]
Satisfaction With Area	.06	.32	.09	.12	.08	.17	[312]
Perceived Area Social Disorder Problems	-.01	.87	-.08	.18	-.07	.24	[312]
Evaluations of Police Service	.13	.02	.13	.03	.09	.10	[309]
Police Aggressiveness (Log)	-.02	.78	.04	.53	.03	.65	[308]
Defensive Behaviors To Avoid Personal Crime	-.07	.25	-.08	.15	-.09	.11	[312]
Household Crime Prevention Measures	.11	.05	.13	.02	.08	.19	[312]

**includes indications of age, race, sex, income education, length of residence, marital status, household organization and size, renter status, building size, personal victimization, knowledge of local crime victims, and the pretest.

APPENDIX K

NON-RESIDENTIAL RESULTS

TABLE K-1

Wave One - Wave Two Outcome Measures

Non Residential Survey

Fear of Personal Victimization

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	2.62	2.40	2.45	2.12
(sd)	(.72)	(.73)	(.63)	(.65)
[N]	[68]	[67]	[39]	[44]
Sigf.	$p < .05$		$p < .025$	
Q26 Fear working during the day				
Mean	2.18	1.94	1.79	1.41
(sd)	(1.12)	(.92)	(.77)	(.54)
[N]	[68]	[67]	[39]	[44]
Sigf.	$p < .10$		$p < .01$	
Q27 Fear Working at night				
Mean	3.04	2.92	2.92	2.44
(sd)	(1.04)	(1.06)	(.90)	(.98)
[N]	[67]	[66]	[39]	[43]
Sigf.	$p < .40$		$p < .025$	
Q28 Fear outside after dark				
Mean	3.40	3.18	3.18	3.00
(sd)	(.84)	(.97)	(.94)	(1.01)
[N]	[67]	[67]	[39]	[42]
Sigf.	$p < .10$		$p < .25$	
Q42 Worry about robbery in establishment				
Mean	2.26	1.98	2.18	1.86
(sd)	(.70)	(.75)	(.68)	(.76)
[N]	[68]	[67]	[39]	[44]
Sigf.	$p < .025$		$p < .05$	
Q43 Worry about robbery outside in area				
Mean	2.31	1.98	2.15	2.00
(sd)	(.72)	(.77)	(.67)	(.68)
[N]	[67]	[67]	[39]	[44]
	$p < .01$		$p < .25$	

TABLE K-2

Wave One - Wave Two Outcome Measures

Non Residential Survey

Concern About Property Crime

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	2.15	1.91	2.13	1.92
(sd)	(.64)	(.61)	(.65)	(.60)
[N]	[68]	[67]	[39]	[44]
Sigf.	$p < .025$		$p < .10$	
Q21 Burglary of establishments a problem				
Mean	2.15	1.83	1.95	1.75
(sd)	(.80)	(.80)	(.82)	(.84)
[N]	[67]	[65]	[39]	[44]
Sigf.	$p < .025$		$p < .25$	
Q44 Worry about burglary of establishment				
Mean	2.30	2.04	2.28	2.14
(sd)	(.74)	(.77)	(.65)	(.73)
[N]	[67]	[67]	[39]	[44]
Sigf.	$p < .025$		$p < .25$	
Q45 Worry about vandalism of establishment				
Mean	2.03	1.87	2.15	1.86
(sd)	(.83)	(.81)	(.71)	(.70)
[N]	[68]	[67]	[39]	[44]
Sigf.	$p < .25$		$p < .05$	

TABLE K-3

Wave One - Wave Two Outcome Measures

Non Residential Survey

Employee and Patrons Concern About Crime

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	2.25	2.19	2.27	1.94
(sd)	(1.03)	(1.13)	(.90)	(.85)
[N]	[67]	[67]	[39]	[44]
Sigf.	$p < .40$		$p < .05$	
Q29 Frequency employees express concern				
Mean	2.24	2.13	2.32	1.79
(sd)	(1.18)	(1.23)	(1.04)	(.95)
[N]	[62]	[67]	[38]	[42]
Sigf.	$p < .40$		$p < .025$	
Q30 Frequency patrons express concern				
Mean	2.18	2.24	2.36	2.05
(sd)	(1.11)	(1.27)	(1.03)	(1.07)
[N]	[67]	[67]	[39]	[43]
Sigf.	$p < .40$		$p < .10$	

TABLE K-4

Wave One - Wave Two Outcome Measures

Non Residential Establishments

Disorder Problems in Area

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	1.50	1.45	1.33	1.42
(sd)	(.53)	(.51)	(.35)	(.39)
[N]	[68]	[67]	[39]	[44]
Sigf.	$p < .40$		$p < .25$	
Q15 People saying insulting things				
Mean	1.41	1.37	1.29	1.37
(sd)	(.71)	(.72)	(.56)	(.66)
[N]	[64]	[65]	[38]	[41]
Sigf.	$p < .40$		$p < .40$	
Q18 Drinking in public place				
Mean	1.65	1.57	1.60	1.89
(sd)	(.75)	(.74)	(.79)	(.75)
[N]	[66]	[67]	[38]	[44]
Sigf.	$p < .40$		$p < .10$	
Q19 Breaking Windows				
Mean	1.79	1.64	1.58	1.43
(sd)	(.89)	(.77)	(.73)	(.62)
[N]	[67]	[67]	[36]	[44]
Sigf.	$p < .25$		$p < .25$	

TABLE K-4 continued

Wave One - Wave Two Outcome Measures

Non Residential Establishments

Disorder Problems in Area

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Q16 Graffiti				
Mean	1.34	1.36	1.23	1.27
(sd)	(.61)	(.69)	(.54)	(.50)
[N]	[68]	[67]	[39]	[44]
Sigf.	$p < .50$		$p < .40$	
Q14 Gangs				
Mean	1.32	1.24	1.10	1.18
(sd)	(.61)	(.46)	(.31)	(.45)
[N]	[68]	[67]	[38]	[44]
Sigf.	$p < .25$		$p < .25$	
Q25 Sale or use of drugs in public places				
Mean	1.49	1.50	1.22	1.31
(sd)	(.72)	(.77)	(.48)	(.66)
[N]	[63]	[66]	[37]	[39]
Sigf.	$p < .50$		$p < .40$	

Note: One-tailed t-test for small samples

TABLE K-5

Wave One - Wave Two Outcome Measures

Non Residential Survey

General Satisfaction with the Area

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	2.41	2.54	2.70	2.81
(sd)	(.61)	(.65)	(.57)	(.57)
[N]	[68]	[67]	[39]	[44]
Sigf.	$p < .25$		$p < .25$	
Q7 Satisfaction with area				
Mean	3.09	3.21	3.36	3.48
(sd)	(.83)	(.85)	(.90)	(.79)
[N]	[67]	[66]	[39]	[44]
Sigf.	$p < .25$		$p < .40$	
Q8 Area getting better in last year				
Mean	1.70	1.89	2.05	2.14
(sd)	(.61)	(.66)	(.51)	(.63)
[N]	[66]	[66]	[39]	[44]
Sigf.	$p < .05$		$p < .25$	

TABLE K-6

Wave One - Wave Two Outcome Measures

Non Residential Survey

Changes in Business Conditions

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	1.77	1.97	2.06	2.16
(sd)	(.70)	(.69)	(.64)	(.62)
[N]	[66]	[67]	[39]	[43]
Sigf.	$p < .05$		$p < .25$	
Q9 Number of people coming is increasing				
Mean	1.88	2.00	2.18	2.09
(sd)	(.73)	(.67)	(.64)	(.72)
[N]	[66]	[67]	[39]	[43]
Sigf.	$p < .25$		$p < .40$	
Q8 Amounts of business done here increasing				
Mean	1.67	1.94	1.95	2.23
(sd)	(.79)	(.82)	(.89)	(.75)
[N]	[66]	[66]	[38]	[43]
Sigf.	$p < .05$		$p < .10$	

TABLE K-7

Wave One - Wave Two Outcome Measures

Non Residential Establishments

Evaluations of Police Service

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Scale Score				
Mean	3.38	3.52	3.46	3.85
(sd)	(.73)	(.81)	(.64)	(1.02)
[N]	[65]	[67]	[38]	[44]
Sigf.	p < .25		p < .10	
Q46 Good job at preventing crime to business/ establishments				
Mean	3.26	3.54	3.60	3.22
(sd)	(1.12)	(1.07)	(.95)	(1.29)
[N]	[61]	[66]	[38]	[41]
Sigf.	p < .10		p = < .10	
Q47 Good job of helping business/ establishment victims				
Mean	3.13	3.40	3.19	3.05
(sd)	(1.28)	(1.25)	(1.09)	(1.28)
[N]	[60]	[62]	[36]	[40]
Sigf.	p < .25		p < .40	
Q50 Good job keeping order on street				
Mean	3.53	3.82	3.49	3.48
(sd)	(1.02)	(.97)	(.96)	(1.01)
[N]	[64]	[65]	[37]	[40]
Sigf.	p <.10		p < .50	

TABLE K-7 continued

Wave One - Wave Two Outcome Measures

Non Residential Establishments

Evaluations of Police Service

	Golfcrest Program Area		Shady Acres Control Area	
	Wave 1	Wave 2	Wave 1	Wave 2
Q53 Polite in dealing with establishments				
Mean	3.53	3.63	3.62	3.40
(sd)	(.65)	(.61)	(.49)	(.73)
[N]	[62]	[62]	[34]	[42]
Sigf.	$p < .25$		$p < .10$	
Q54 Helpful in dealing with establishments				
Mean	3.24	3.45	3.54	2.92
(sd)	(.88)	(.76)	(.51)	(.84)
[N]	[59]	[62]	[33]	[39]
Sigf.	$p < .10$		$p = < .001$	
Q55 Fair in dealing with establishments				
Mean	3.49	3.37	3.54	3.25
(sd)	(.65)	(.73)	(.51)	(.65)
[N]	[59]	[62]	[33]	[36]
Sigf.	$p < .25$		$p < .025$	

Note: One-tailed t-test for small samples

TABLE K-8

Victimization by Crimes in the Area

Non Residential Establishments

Percent Victimized in Past Six Months	Golfcrest Program Area Wave 1	Golfcrest Program Area Wave 2	Shady Acres Control Area Wave 1	Shady Acres Control Area Wave 2
Robbery or Attempted Robbery[1]				
No	88	87	92	93
Yes	12	13	8	7
[N]	[68]	[67]	[39]	[44]
	$p < .80$		$p < 90$	
Burglary or Attempted Burglary[2]				
No	60	67	56	50
Yes	40	33	44	50
[N]	[68]	[67]	[39]	[44]
	$p < .50$		$p < 70$	
Vandalism[3]				
No	71	72	85	80
Yes	29	28	15	20
[N]	[68]	[67]	[39]	[44]
	$p < .90$		$p < 70$	

1 Questions 67, 70
2 Questions 61, 64
3 Question 73

APPENDIX L

THE NEWSLETTER: DESCRIPTION AND SAMPLE COPY

NEWSLETTER DESCRIPTION

Size and Format. The newsletter included four pages, exclusive of crime statistics, which were printed on a single 11" by 14" sheet, which was folded to produce four 7" x 11" pages. There were two columns per page, and a variety of spatial arrangements were used for stories which might occupy one-third or more of a single column or take two columns on the top or bottom half of a page.

The title, "Community Policing Exchange," had a subheading, "Published by the Houston Police Officers Serving your Neighborhood." Print was black on off-white stock. A variety of type sizes and styles were used for story headings. Stories were separated horizontally by lines. The final appearance was a clean attractive one that tried to draw the reader's attention to items the Task Force wanted to emphasize.

Production. The Task Force worked as a group to identify general items of itnerest, sometimes finding them in newsletters from other cities, and writing others from local source materials. Officers Herb Armand, Epperson, Jackson, Kirk and Tomlinson would write the items about their patrol neighborhoods, and these were then edited into a consistent style by Sergeant Fowler, Officer Alan Tomlinson and Ms. Mara English.

Publication Dates. The original timetable for the evaluation of the newsletter called for the first newsletter to be published in June, 1983, with the evaluation coming in January, 1984, after the distribution of six issues. The stasrt-up for the newsletter took much longer than initially scheduled, with the first newsletter being mailed in mid-November, followed by issues in December, Janaury, February and March.

Table 1

Percentage Distribution of Houston Newsletter Content
(Based on Column Inches)

Type of Content	Percent of Content	
Good News (Successful Prevention)	8%	
Crime Prevention Advice		
Personal Crime	8%	
Property Crime	21%	29%
Personal and Property Crime	0%	
Departmental Information		
Related to Fear Reduction	12%	
Not Related to Fear Reduction	16%	21%
Advice or Information		
Related to Crime	16%	
Not Related to Crime	12%	24%
Safety advice	12%	
Encouraging people to get involved	1%	
Offering police services to citizens	0%	
Greetings	4%	
Total*	99%*	

*Does not equal 100% because of rounding.

Table 2

Recorded Crime Presented in Houston Newsletters

Issue	1	2	3	4	5
Date	Nov 1983	Dec 1983	Jan 1984	Feb 1984	March 1984
Period Covered (days)	August (31)	Sept-Oct (61)	Nov-Dec (61)	Jan-Feb 6 (37)	Feb 7-23 (16)
Personal Crimes	5	15	16	1	2
Property Crimes	20	24	29	29	7
Auto Theft	0	4	21	30	15
Total	25	43	66	60	24

COPY OF NEWSLETTER

Community Policing Exchange

PUBLISHED BY THE HOUSTON POLICE OFFICERS SERVING YOUR NEIGHBORHOOD

H.P.D. reaches out with Community Newsletter

Welcome to the first edition of the Houston Police Department's **COMMUNITY POLICING EXCHANGE.** Please take the time to read the information assembled in this newletter. It's for your benefit. This information has been gathered by police officers working in your neighborhood who want to keep you informed about crime activity occurring in your neighborhood, crime prevention tips, and neighborhood news.

The purpose for providing this type information is to give a clearer understanding of what is going on in your neighborhood. We hope that this information will assist you and your neighbors in deciding if you should become more actively involved in looking out for each other's well being. Remember by ourselves, police can only react to crime, we need an involved citizenry to prevent it.

A community that employs crime prevention techniques, is alert to suspicious behavior and circumstances, and reports this information to the police, will be a far safer place to live than one that does not. Alert and responsive citizens, who are willing to become involved, can maximize the efficiency and effectiveness of the police in preventing crime and apprehending criminals.

Living with success

The most effective action against crime is citizen action. The police, by themselves, can only have limited success in dealing with neighborhood problems that contribute to fear.

We are often unaware of the success stories that happen every day when citizens confront problems in their neighborhoods. Through this newsletter, we will tell you of these successes.

Take a young man living in the Golfcrest neighborhood. He noticed suspicious activity in a nearby backyard and strange comings and goings to the nearby house. He suspected that drug dealing was going on and notified his local beat officer. After investigation, it was found that drugs were being manufactured. Arrests were made and the problem eliminated.

This is but one of the success stories from neighborhoods all over the city. Citizen action can make a difference. Tell us about your success story so we can let others know what has happened. Call our special number or drop us a line. Sergeant Steve Fowler, 221-0711 or Community Policing Exchange, 33 Artesian Street, Houston, Texas 77002. We'll write about these in each issue.

Community Comments

Lee P. Brown, Chief of Police

Policing the community involves selection of options for action in a variety of complex urban situations. The police must select options for action, based on an understanding of community priorities. It is equally important for the police to clearly state those values and beliefs which lay the foundation for priority-setting.

Values are those standards and beliefs which guide the operation of the Police Department. The values set forth the philosophy of policing in Houston and the committments made by the Department to high standards of policing. For values to be meaningful they must be widely circulated so that all members of the community are aware of them. Department values must incorporate and reflect citizen's expectations, desires, and preferences. The community's contributions in expressing their values are subsequently manifested in the Department's administrative policies.

For the Houston Police Department, several values need to be carefully reflected throughout its operations. These values are as follows:

- Police must involve the community in all aspects of policing which directly impacts the quality of community life.
- The Police Department believes that it has a responsibility to react to criminal behavior in a way that emphasizes prevention and that is marked by vigorous law enforcement.
- The Police Department believes that it must deliver its services in a manner that preserves and advances democratic values.
- The Department is committed to delivering police services in a manner which will best reinforce the strengths of the city's neighborhoods.
- The Department is committed to allowing public input in the development of its policies which directly impacts neighborhood life.
- The Department is committed to understanding neighborhood crime problems from the community's perspective and collaborate with the community by developing strategies that deal with neighborhood crime.

Bicycle safety tips

Nearly half the entire population of the United States rides bicycles, whether for recreation, transportation, or keeping in shape. There are as many adult bike riders as children. Obeying traffic laws and safety rules will make bicycling safer, more enjoyable, and will prevent accidents.

- Always ride in the same direction as other traffic. Stay close to the right edge of the roadway, except when passing or making a left turn. Be careful when passing a standing vehicle or one proceeding in the same direction.
- Whenever a usable path for bicycles has been provided, bicycles must use the path and not the roadway.
- Bicycles should not be used to carry more persons at one time than the number for which it is designed and equipped, except that an adult may carry a child securely attached to his person in a backpack or sling.
- Use caution at intersections and railroad crossings.
- Keep at least one hand on the handlebars at all times. If you plan to carry books, packages, or other items, you should add a front or rear carrier to your bicycle. If you carry items, you must drive with both hands on the handlebars.
- A bike flag and a rearview mirror are added safety precautions.
- When operating a bicycle, you must never attach yourself or your bicycle to any vehicle on the roadway.
- You must always stop before reaching a school bus that has stopped to load or unload passengers.
- Weaving from one lane to another is both illegal and dangerous.
- Don't make a U-turn without first looking carefully to see if it is safe to do so. On some streets U-turns are not permitted.
- You must never drive at a speed faster than that which is reasonable and safe. Use hand signals.
- Wear light-colored clothing or apply reflective tape to your clothing or the bicycle handlebars, frame or fenders. It will help you to be seen and may keep you from getting hit. Some riders use arm and leg lights.
- Watch for people getting into and out of parked cars, and for cars pulling into traffic from a curb or driveway.

Parents should be aware of the responsibilities that they must assume when their children ride bicycles. These responsibilities range all the way from selection of a proper bicycle for the child to seeing that the child learns and obeys all the traffic laws.

Be alert to suspicious circumstances

Anything that seems even slightly out of place for your area, or for the time of day, may mean criminal activity. In your neighborhood or business complex, you are the expert. You know if there is someone in the area that doesn't belong.

Some of the most obvious things to watch for and report:

- A stranger entering your neighbor's house when it is unoccupied may be a burglar.
- A scream heard anywhere may mean robbery or rape.
- Offers of merchandise at ridiculously low prices could mean stolen property.
- Anyone removing accessories, license plates, or gasoline from a vehicle should be reported.
- Anyone peering into parked cars may be looking for a car to steal or for valuables left displayed in the car.
- The sound of breaking glass or loud explosive noises could mean an accident, housebreaking, or vandalizing.
- Persons loitering around schools, parks, secluded areas, or in the neighborhoods could be sex offenders.
- A person running, especially if carrying something of value, could be leaving the scene of a crime.
- The abandoned vehicle parked on your block may be a stolen car.
- Persons being forced into vehicles, especially if juveniles or female, may mean a possible kidnapping.
- Apparent business tranactions conducted from a vehicle, especially around schools or parks, with juveniles involved, could mean possible drug sales.

H.P.D. community program implemented

Golfcrest area...

Your neighborhood has been selected by the Houston Police Department to participate in a new pilot program designed to measure the effectiveness of new methods of policing.

The Department in conjunction with the National Institute of Justice and the Police Foundation, has initiated this new program which utilizes six separate policing programs in four different neighborhoods throughout the City. These programs are aimed at increasing the interaction between the Police and the Community.

Beat officers in the Golfcrest area will be contacting you and your neighbors and soliciting concerns about problems that affect your day-to-day life. The officers participating in this program are canvassing the residence and business places in an effort to contact everyone that lives and/or works in your area. This provides a perfect opportunity for you to meet and get to know the beat officers assigned to your neighborhood.

All the officers involved in the Golfcrest area are assigned to the Park Place Substation at 7414 Park Place. The beat officers that you will be talking to are Charlie Epperson, Phil Brooks, James Hyden, Tom Hayes, and Elizabeth Scardino. These officers can be reached through the Substation at 649-5529 if you have any questions or just want to talk. Be reminded, however, that any emergencies should be addressed to the Houston Police Dispatchers office at 222-3131.

Protecting a precious resource

The child trusts him. He buys the child candy, takes the child to movies, gives the child his time when no one else will. He is the child's special friend.

The child does not want to lose his friend. The child will do anything to keep him. Besides, he is a grown-up who knows what is right and what is wrong.

Child pornographers can destroy precious moments of childhood. When a camera is held by a pornographer, the child will be haunted by the experience for the remainder of his life.

According to the Texas Department of Human Resources, studies show that a majority of those who are sexually abused as children will become child molesters as adults. The wreckage of the life of a sexually abused child is devastating and society pays the price.

Anyone from a stranger to a close friend or family member can be a sexual abuser of children. The Crime Stoppers Advisory Council for the month of November is concentrating its efforts on the prevention and apprehension of child pornographers in Texas.

Parents, family members and friends are encouraged to become informed on ways to prevent children from becoming involved with the child pornographers and sexual abusers, and learn to recognize the symptoms of a child under a pornographer's influence.

Persons with information on child pornographers are asked to call their local Crime Stoppers program or the toll-free Texas Crime Stopper's hotline at 1-800-252-TIPS anytime, day or night.

Improving your neighborhood

The main purpose of City and governmental agencies is to serve the citizens. Those who work in agencies are willing and well prepared to help. A valuable resource to those who are working toward neighborhood improvement is the information and assistance that these bodies can provide.

Listed below are some of the City departments that are most directly involved in neighborhood - related activities. You will notice that some of these departments also provide speakers on topics of neighborhood interest.

The **Neighborhood Revitalization Division** of the City Planning Department assists neighborhood groups in efforts to improve their neighborhoods. The Division provides data and information to groups; develops information sharing workshops; maintains a resource file of persons, agencies, and programs available to assist groups; and helps groups to develop comprehensive plans and strategies for improving their neighborhoods.

The **Mayor's Citizen's Assistance Office** located in City Hall, distributes a booklet listing City services and information about each service. This information makes it easier for you to request these services by phone. The Mayor's Citizen's Assistance Office refers requests for service to the proper City division or department for you. The Mayor's Citizen's Assistance Office, after referring your complaint to the appropriate City department, will contact you later to let you know what action has been taken. It also arranges for speakers for community groups.

The Community Services Division of the **Police Department** provides speakers to talk on subjects related to police-community matters.

The Public Education Section of the **Fire Department** offers a program that includes films, slides, lectures, and demonstrations on life and fire safety. The Special Services Section offers fire safety and home inspections upon request.

The **Public Works Department** provides for and maintains roads, drainage, sewer disposal and water for the City of Houston as some of its duties. Additional functions include the overseeing of all construction on City properties and the Street Repair Division maintains city streets and cleans and recuts roadside ditches and mows street rights-of-way. Repairs for sewer lines are handled by the Water Quality Section.

The **Traffic and Transportation Department** installs and maintains traffic signals, traffic signs and street signs throughout the City. Blind intersections, signs and signals in need of maintenance and requests for new traffic controls should be reported to them.

The resources listed are just sampling of the resources available to neighborhood groups. In your search for assistance you are certain to uncover other resources as you go along. Special thanks to the Neighborhood Revitalization Division of City Planning Department for providing this information.

Citizens fight back

The key to minimizing crime in any community is citizen involvement. A community that employs crime prevention techniques, is alert to suspicious behavior and circumstances, and reports this information to the police, will be a far safer place to live than one that doesn't. Alert and responsive citizens, who are willing to become involved, can maximize the efficiency and effectiveness of the police in preventing crime and apprehending offenders.

In July of 1983, officers received a call to an apartment complex in your area. The complainant stated to the officers that he heard his front patio door open, looked out of his window, and saw an unknown person stealing property off his patio. The suspect then proceeded to another apartment and was attempting to commit the same offense. The complainant at this time stopped the suspect, preventing him from taking any property belonging to his neighbor. The involvement of a concerned citizen prevented a neighbor from becoming a victim and losing his personal belongings.

The Police Department recognizes that there are other incidents where a citizen has performed an act which was a deterrent to crime. If you know of any instances where the act of a citizen's involvement deterred a criminal act, please contact us and the article will be published in this Newsletter. We are asking for your assistance and support in acquiring this information for these success stories. Our office is located at 33 Artesian, Planning and Research Division, telephone number 221-0711, c/o Sergeant Steve Fowler.

Crime prevention tips

After reviewing the crime reports for your area, we were able to determine which crime prevention tips would be most helpful to you as residents and business owners. A number of thefts occurring in your area involve "Pigeon Dropping." This type of theft is often performed by a "Con Artist," a smooth-talking criminal whose aim is to separate you from your money through trickery and deceit. The Pigeon Drop is an old and well-known confidence game, perpetrated mainly on elderly, trusting and unsuspecting citizens. They may stop you on the street, call you on the phone, or ring your door bell. They may pretend to be repairmen, building inspectors, bank examiners or any other identity. There are many different kinds of confidence games; they can occur at any time of the year and can be avoided if the intended victim (pigeon) recognizes the confidence game and refused to participate.

- Beware of friendly strangers offering goods or services at low rates.
- Be suspicious of telephone calls from persons claiming to be bank officials who ask you to withdraw money from your account for any reason. Legitimate banks communicate in writing on business transactions.

Protect your car

A million cars were stolen in the United States last year. Millions more were burglarized or vandalized. Before you become one of the statistics, learn how to fight back.

According to the FBI, most cars are stolen by "amateurs."–And they are stolen because they are easy to steal!

Your first defense against auto theft is to lock your car and protect your keys. Did you know that most cars are stolen because they were left unlocked or the keys were still in the ignition?

Although you can't make your car impossible to steal (a professional thief can get it if he really wants it), you can make it tough.

Take these tips:

- Store spare keys in your wallet, not in the car.
- Replace standard door lock buttons with the slim, tapered kind.
- In the driveway, park your car with the front toward the street, so anyone tampering with the engine can be seen more easily.

HOUSTON POLICE DEPARTMENT

OFFICE OF THE CHIEF OF POLICE
61 RIESNER STREET
HOUSTON, TEXAS 77002

APPENDIX M

MISSING DATA ANALYSIS

Table M

A Comparison of Including All Cases Versus Excluding Missing Value Cases

b (and sigf.) For Area-Treatment Interaction

	Signs of Crime				Citizen Contact Patrol			
	All Cases		Exclude Missing Value		All Cases		Exclude Missing Value	
	b	Sigf.	b	Sigf.	b	Sigf.	b	Sigf.
Fear of Personal Victimization in Area	.03	.61	-.01	.91	-.12	.02	-.12	.03
Perceived Area Personal Crime Problems	.15	.01+	.12	.05	-.14	.01	-.14	.01
Worry About Property Crime Victimization in Area	-.11	.08	-.12	.09	-.11	.10	-.11	.10
Perceived Area Property Crime Problems	-.04	.47	-.04	.51	-.21	.01+	-.21	.01
Perceived Area Social Disorder Problems	-.06	.22	-.05	.35	-.15	.01+	-.14	.01
Satisfaction with Area	-.17	.01+	-.20	.01+	.13	.02	.11	.05
Evaluation of Police Service	.00	.96	.01	.87	.09	.13	.06	.32
Police Aggressiveness	-.06	.92	-.04	.09	-.04	.04	-.03	.13
Perceived Area Physical Deterioration Problems	.06	.27	.04	.51	-.09	-.08	-.10	.06
Defensive Behaviors to Avoid Personal Crime	-.02	.48	-.04	.20	-.03	.32	-.04	.26
Household Crime Prevention Measures	.52	.01+	.45	.01+	-.19	.10	-.29	.02
Total Victimization	.08	.08	.07	.19	-.15	.01+	-.15	.01+
Property Victimization	.04	.35	.05	.32	-.15	.01+	-.16	.01+
Personal Victimization	.08	.04	.07	.10	-.06	.08	-.06	.11
[N]	[1711]		[1457]		[1893]		[1718]	

Note: Controls for 18 covariates; panel analysis also controls for pretest and pre-intervention victimization. Missing data coded to medians and mid-range values.

APPENDIX N

NON-RESIDENTIAL ESTABLISHMENTS IN THE PROGRAM AND COMPARISON AREAS, WAVE 2

APPENDIX N-1

NON-RESIDENTIAL ESTABLISHMENTS IN THE PROGRAM AREA, WAVE 2

The 67 establishments in the non-residential sample in the program area included the following:

Adult book store	1
Alarm company	1
Appliance/business machines sales and service	2
Automotive repair	2
Automotive parts/tires	7
Bakery	1
Bar	3
Beauty/barber shop	3
Church	1
Clothes/furniture sales	3
Commercial deliveries	1
Day care center	1
Dry cleaners	1
Engravers/printers	2
Food supplier	1
Florist	1
General constructor	1
Glass company	3
Grocery	2
Hospital	1
Industrial equipment and tools, sales and service	10
Industrial supplies	6
Liquor store	1
Lumber yard	1
Manufacturer	1
Marine sales	1
Plumber	1
Public housing	1
Restaurant	2
Service station	2
Telephone answering service	1
Trailer/van rental	1
Veternarian	1

APPENDIX N-2

NON-RESIDENTIAL ESTABLISHMENTS IN THE COMPARISON AREA, WAVE 2

The 44 establishments in the non-residential sample in the comparison area included the following:

Automotive equipment warehouse	1
Automobile/truck repair	5
Carpet cleaners	1
Church	1
Computing service	1
Construction contracting	4
Electrical contracting	1
Engravers/printers	2
Florist	1
Furniture sales	1
Graphic arts equipment	1
Grocery store	1
Heating and air conditioning sales and service	1
Industrial field services	1
Janitorial service	1
Landscape architect	1
Laundry self-service	2
Lubrication equipment	1
Machine shop	1
Mobile home sales	1
Plumbing contractors	2
Property Management	1
Retail sales (general household merchandise)	1
Restaurant	1
Saw sharpening	1
Service station	2
Sheet metal construction	1
Steel storage equipment	1
Tool and die	1
Truck rental	1
Union office	1
Used car sales	1