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Executive Summary

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## CHAPTER ONE: NEIGHBOURHOOD WATCH IN LONDON

### Background

The history of Neighbourhood Watch (NW) in London begins in October 1982 with the appointment of Sir Kenneth Newman as Commissioner of the Metropolitan Police. On his first day in office, The Times published an article outlining the new Commissioner's interest in NW as part of an overall plan to promote proactive policing (The Times, 1982). On 6 September, 1983, less than a year following the Commissioner's appointment, NW was launched in London on a forcewide basis.

An important event in the development of NW in London was the publication early in 1983 of a report by Superintendent Turner and Detective Inspector Barker who had recently returned from a study tour of the United States of America to investigate the workings of NW programmes in selected parts of the country.<sup>1</sup> The report represented the first major policy document issued by the Met. and included, along with a descriptive summary of schemes in Washington, D.C., New York, Detroit, Seattle and Orlando police departments, a proposal for the implementation of NW in London.

The recommendations of this report were broadly accepted and formed the basis of the Force Instructions which were circulated by the Assistant Commissioner of 'A' Department on 13 June 1983 (Assistant Commissioner 'A' Department, 1983). The Force Instructions and the 'A' Department Memorandum were sent to all divisional chief superintendents along with a request to make plans for the implementation of NW schemes in their areas in preparation for the official forcewide launch later that year. The Force

Instructions provided a summary of the main elements of NW and broad guidelines on methods of implementation. Details of the official launch were announced internally on 2 September in Police Order 24 and the official public launch took place at a press conference on 6 September, 1983.

What is Neighbourhood Watch?

One method of describing NW in London is by outlining the principles and principal elements of the programme as conceived by policy makers within the Met.

The primary sources of information concerning the principles of NW are the Turner and Barker report (Turner and Barker, 1983), the 'A' Department Memorandum including the Force Instructions (Assistant Commissioner 'A' Department, 1983) and the official guide to NW (Russell, undated). The three documents are broadly in agreement about both the principles and the principal elements.

The 'A' Department Memorandum introduced the concept of NW as, '...primarily a network of public spirited members of the community, who become the eyes and ears of the police' (1983, p.1). The concept of the public becoming the 'eyes and ears' of the police is noted in each of the three policy documents and has been picked up by the press and by crime prevention departments as a convenient catch phrase to summarise the essence of NW. The Force Instructions elaborate this idea further. The key section of the Instructions is reproduced below:

Neighbourhood Watch is primarily a network of public spirited

members of the community, who observe what is going on in their own neighbourhood and report suspicious activity to the police. In simple terms the citizen becomes the 'eyes and ears' of the police, looking out for the usual and unusual to protect their own home and that of their neighbour, thereby reducing opportunities for criminal activity, (1983, p.1)

NW in London is conceived as a comprehensive package. The Force Instructions describe the package as comprising four elements: (1) Neighbourhood Watch - as described above; (2) Property Marking Schemes - which involves participants in the programme marking property visibly or invisibly with a house number or first two letters of the house name along with the post code; (3) Home Security Surveys - whereby the police provide a free home security survey to advise participants on minimum levels of protection and low cost solutions; and (4) Community Crime Prevention and Environmental Awareness - which is described as the promotion of crime prevention and community campaigns to address particular local environmental issues.

The official guidelines add to this list three more elements: (1) Information - attempts to encourage the flow of information traditionally supplied to the police force by the public; (2) Community Spirit - attempts to encourage people to meet together to create an atmosphere in which neighbours are known to one another and are prepared to look after each other's property; and (3) Police-Public Contact - attempts to keep participants aware of local crime trends and to provide advice and other relevant information. The guidelines also describe more clearly than the Force Instructions what is meant by the fourth element 'Environmental Awareness'. The guidelines refer to Environmental Awareness as an attempt: 'to encourage members to consider their surroundings and put forward suggestions regarding alterations and improvements that could remove the opportunity for criminal nuisance e.g.: (1) the addition or re-positioning

of street lighting may reduce instances of criminal damage and rowdyism; (2) the re-designing of certain areas may prevent corridors and walkways being used as general thoroughfares and lead to residents noticing strangers' (Russell, undated, p.2). The section is concluded with the caveat: 'It is acknowledged that this is a long term objective and should be linked to a multi-agency involvement'.

NW in London can also be described by looking at the intended structure and operation of the schemes. In such a short review, it is only necessary to note some of the most important features of the programme.

From the outset it was envisaged that schemes would be both police initiated and public initiated. Initially, a high proportion of the programmes were police initiated, although now, due to public demand and pressures on police time, most schemes are public initiated. It is clear from the Force Instructions that not all areas are suitable targets for police initiation or are likely to result in a public initiated scheme. The original instructions for police initiated schemes were to target areas with high levels of residential burglary, to divide these areas into manageable sized groups, to canvass the target area by distributing a questionnaire to residents and to hold a public meeting during which the aims of the scheme were announced. Public initiated schemes were to be encouraged if a core group of 10-15 neighbours willing to support the scheme could be established. It was believed that a large number of these requests would come from members of resident associations and other community groups. During the early period of NW programmes of varying sizes were implemented ranging from just a few households to in excess of 3,000. It is now considered impracticable to manage very small and very large schemes and the optimal size sought is around 300-500 dwellings.

A new NW scheme begins with a public launch meeting. Originally, it was intended that two public meetings should be held within two weeks of one another, although it is now standard practice to hold just one meeting. The guidelines propose that the aims of the launch meeting are to explain the principles of NW, to indicate how the programme operates (supported by a video presentation), to explain property marking and home security surveys, to encourage residents to note and report suspicious incidents to the police, to disseminate information on recorded crime in the area and to explain the roles of the area and street co-ordinators. Once established, providing there is evidence of acceptable levels of participation, street signs are erected at all entrances to the NW site. Residents are encouraged by their area and street co-ordinators to display window stickers identifying themselves as participants in the scheme.

Apart from the initial launch meeting, it was envisaged that participants would continue to hold regular informal and formal meetings. The formal meetings were to provide an opportunity for all residents in the scheme to meet and discuss progress. The informal meetings were to provide an opportunity for additional training of street co-ordinators and were to be held at one of the member's homes. In fact, very few schemes hold regular formal meetings for the benefit of all residents participating in the scheme. Instead, most hold only informal meetings comprising area and street co-ordinators and their invited guests. The aim of these meetings is not, as was originally intended, the short-term training of co-ordinators, but often to create an opportunity for interaction between a selected minority of the programme participants and the police. Unfortunately, the potential benefits of these meetings are not enjoyed by the majority of participants in the scheme.



In addition to holding informal meetings either the police alone or in collaboration with the area and street co-ordinators prepare regular newsletters. The newsletter contains information on local crime trends, security and other advice, and news of local events relevant to the area and is usually distributed by the street co-ordinators to residents within the NW area. Not all schemes, however, prepare newsletters and not all schemes which do prepare them publish them regularly.

#### **What are the aims of NW?**

The primary aim of NW as outline in the Force Instructions is to reduce crime and the fear of crime (Assistant Commissioner 'A' Department, 1983, p.1). More specifically, NW is seen as a weapon against 'opportunistic crime' (the definition of this is still unclear, see Bennett and Wright, 1984) and in particular residential burglary. In addition, the Force Instructions identify street robbery, vehicle crime and criminal damage *as* offences that might also be reduced as a result of the successful implementation of NW.

Additional aims are summarised in the guidelines. The full list of aims include: (1) a reduction in crime levels; (2) heightened public awareness of the need to safeguard property; (3) greater contact between neighbours; (4) closer liaison between police and public; (5) a reduction in the fear of crime; and (6) greater participation by members of the public thereby reducing the demand upon the services of police (Russell, undated, p.1).

**How does NW achieve its aims?**

It is not absolutely clear from the available policy and publicity material available how NW is supposed to achieve these aims. Some processes, however, have been articulated in the literature.

The main mechanism that can be found in reports cited above which links NW to decreases in crime is 'opportunity reduction'. The most frequently recorded process by which NW is supposed to reduce opportunities is as a result of residents looking out for suspicious activities and reporting these to the police. The logical link between reporting and crime reduction is not elaborated. It might be argued, however, that reporting suspicious incidents to the police will deter offenders as they will be aware of the local residents' propensity to report suspicious behaviour and perceive the probability of getting caught as a result as unacceptably high. It might also be argued that increasing information flow from the public to the police will improve arrest and conviction rates and (when a custodial sentence is passed) decrease the number of active offenders in the area and the number of crimes committed.

Another mechanism which is not mentioned in these reports, but is occasionally mentioned in the publicity material, is the creation of signs of occupancy. It is interesting that the London version of NW, which although based to a large extent on the 'Community Crime Prevention Program' in Seattle, has not stressed in the same way the importance of signs of occupancy. Some of the methods that might be used are discussed in Cirel et al. (1977) and in Smith (1984) in the guidelines issued by the Home Office and include removing newspapers and milk from outside neighbours' homes when they are away. It could be argued that the mechanism

which links signs of occupancy to crime reduction is the effect that such cues have on the perceptions and assessments of potential offenders. Potential offenders might perceive the probability of getting caught in an apparently occupied dwelling as excessive and, as a result, refrain from offending.

The methods by which the other components of the NW package reduce crime is spelt out more fully. The Force Instructions argue that property marking enhances levels of detection and conviction and makes criminal disposal of property more difficult. Presumably, the former would have the effect of decreasing the number of active offenders through increasing arrest rates which, as a result, would decrease crime, and the latter would deter offenders due to an increase in their perceptions of the probability of getting caught. In addition, it is argued that the property marking sticker alone might act as a deterrent to potential offenders, again, presumably, because they would perceive the risks of getting caught in a dwelling with a window sticker displayed or with marked goods as unacceptably high. The mechanism defined- in the literature as linking home security to crime reduction is its preventive effect in relation to 'opportunistic crime'. This could mean one of two things: either the improvement in security is such that open windows or doors no longer motivate the previously unmotivated offender or the improvements in security prevent any but the most highly motivated and experienced offender from entering. Environmental awareness, the fourth item in the package, is reported as reducing crime again through reductions in opportunities. The guidelines note that repositioning street lighting and redesigning public access routes, for example, might reduce the opportunities for certain kinds of offence largely, it might be assumed, as a result of increasing offenders' estimation of risks of getting caught.

## **The structure of the report**

This report comprises an executive summary of a Home Office funded evaluation conducted at the Institute of Criminology, University of Cambridge of two NW schemes in London. The evaluation is one of the first independent research projects conducted in Britain on the effectiveness of NW using crime surveys. The primary aim of the project was to examine the impact of two new NW schemes on crime rates, fear of crime and other public attitudes as identified in the policy and publicity material discussed above.

Chapter Two describes the research design and the method of evaluating the effectiveness of the schemes. Chapter Three outlines the types of areas investigated and the nature of the specific programmes operating in these areas. Chapter Four presents the major findings relating to the impact of the programme on levels of crime in the areas. Chapter Five presents the major findings relating to the impact on public attitudes and perceptions including the fear of crime. Chapter Six summarises and interprets the research results. Chapter Seven provides an overall conclusion and discusses the implication of the study for future developments in NW.

## NOTES

[1] The single term 'Neighbourhood Watch' is used in this report for clarity although it in fact encompasses a wide variety of programmes such as: home watch, community watch, block watch and apartment watch.

## CHAPTER TWO: RESEARCH DESIGN

### Evaluative methodology

Ideally, social research that involves the evaluation of some kind of 'treatment' should be based on random allocation of individuals to treatment and non-treatment groups. When allocation of subjects is not random, as would be the case in the implementation of NW in selected areas, the research method is usually referred to as quasi-experimental (see Cook and Campbell, 1979; Shadish and Kenny, 1981).

The application of quasi-experimental design to the study of the effectiveness of crime prevention measures has not been widely adopted in this country. The approach has a much longer history, however, in the United States of America. Two common designs are, using the terminology of Cook and Campbell, 'The One-Group Design with Separate Pretest and Posttest Samples' and 'The Untreated Control Group Design with Separate Pretest and Posttest Samples'. Both designs were used in the current research. The former comprises a before and after cross-sectional survey of areas receiving the treatment (i.e. the crime prevention measure) and the success of the measure is determined by a comparison of the pretest and posttest scores. The latter is a stronger design and includes a before and after cross-sectional survey of treatment and non-treatment (control) areas. The success of the measure is determined by a comparison of the pretest and posttest scores of both treatment and control areas.

The choice of research design was influenced strongly by existing evaluations of NW. At the time of planning the research the most impressive

published evaluation was of the 'Community Crime Prevention Program' in Seattle (Cirel et al., 1977). One feature of this research, which seemed of crucial importance, was the use of crime surveys, rather than police crime reports, as a measure of crime. The benefits of using survey methods and the weaknesses of police recorded crime were also clearly demonstrated at the time in the publication of the first British Crime Survey (Hough and Mayhew, 1983).

### **The study areas**

The remit of the research was to examine just one or two schemes in detail rather than attempt a global evaluation of NW in London. In order to survey areas large enough to collect a sufficient number of crimes, it was estimated that the sites should comprise at least 500 households. This meant that no more than two NW areas and two non-treatment areas could be evaluated from the funds available for the survey work. The design chosen was a survey of four areas comprising two experimental evaluation areas (areas in which NW schemes were about to be implemented), one area adjacent to an experimental area (used to monitor the presence of possible displacement effects) and a standard control area (an area apart from the experimental areas used to monitor general changes).

Before beginning the process of selecting the experimental areas, it was necessary to think about what type of NW scheme to evaluate. Because of the wide variation in the size and nature of programmes in London, there appeared little to be gained from selecting a typical or representative scheme. This might have resulted in a very small scheme or an area with little crime. Instead, it was felt more useful to evaluate a programme which was clearly and unambiguously a NW scheme and which matched

professional and popular consensus about what constituted NW. It was decided that the schemes evaluated should match as closely as possible the concept or ideal type of NW. In addition, it was felt that the schemes chosen should have the best possible chance of success. If the most promising schemes were later shown to have little effect on crime, it would not be expected that less promising schemes would be any more successful.

The next task was to choose two areas in which suitable NW schemes were about to be implemented. The search for suitable areas began by identifying divisions which had implemented the kinds of schemes in which we were interested in the past. The experimental areas were selected with the assistance of Metropolitan Police Crime Prevention Branch (A7) who compiled a list of all watch schemes launched in London since 1983. Senior officers from divisions which had implemented comprehensive and large schemes in the past were contacted to determine whether they intended implementing similar schemes in the near future.

The two sites chosen, one in Acton and one in Wimbledon, matched our selection criteria most closely. These criteria related to: the size of watch schemes implemented in the division in the past, the comprehensiveness of programmes implemented, the level of crime in the study area over the previous two years (a consistently high rate was looked for), social composition and stability, existence of actual or symbolic boundaries to the area, enthusiasm of potential area co-ordinators, enthusiasm of senior officers, quality of officers directly responsible for administering the programme and, in the case of at least one area, an adjacent area with no NW programme in existence or planned.

The single displacement area was less difficult to choose. The important

criteria were: **the area should adjoin an experimental area along part of its boundary, it should be of similar social and architectural structure, it should have an actual or symbolic boundary, it should be of similar size to the experimental area and there should be no plans to launch a watch scheme in the area in the near future.** These criteria could only be met in relation to the **Wimbledon** experimental site.

The control site was chosen to match the Wimbledon experimental site as this **had a suitable** displacement area. This allowed one experimental site to **be evaluated using both** displacement and control sites. The Acton and Wimbledon sites were sufficiently similar, however, to be able to utilise the control as a comparison site for both areas. The control site was selected by **matching the Wimbledon** experimental area with a randomly chosen area of similar social composition and of similar distance from the centre of London. Investigations were then made to determine crime rates, the number of households and whether or not a watch scheme was planned for or existed in the area. The area which best matched the selection criteria was in Redbridge.

### The **crime surveys**

The main method of data collection used in the research was a crime survey or, more precisely, a crime census. In order to obtain sufficient numbers of reported crimes to conduct a meaningful statistical analysis, it was necessary to aim to interview a 100% sample of residents on each site. A list of households' was drawn from the electoral register and the missing addresses were filled in from ratings lists. Every dwelling in the area at the time of the most recent compilation of the registers was included in the initial sampling frame. The contract for the survey work went to NOP



who completed the first round of surveys in June 1985 and the second round of surveys in July 1986.

### The questionnaire

The questionnaire used was a modified version of the second British Crime Survey questionnaire (Hough and Mayhew, 1985). A small number of new questions were added concerning NW and the relationship between the public and the police. Similar questionnaires were administered in both pretest and posttest surveys.

The questionnaire was in five distinct parts and took between half an hour and one hour to administer depending on the number of victimisations reported. The first part was an address record sheet which was used to record details about contacts made. The second part was the main questionnaire comprising about 12 pages of questions concerning residents' perceptions of their area, sense of community, fear of crime, perceived probability of victimisation and victimisation experience over the previous 12 months. The third part was a victim form which was used to collect details on each victimisation in order to define it as a bona fide offence. The fourth part was a demographic questionnaire and was used to collect basic demographic and personal information about respondents and households. The fifth part was a follow-up questionnaire and included questions about police-public contact and NW.

### The selection of respondents

The interviewer selected from each eligible household a respondent aged 16 years or over using a random grid method. Households were considered

eligible if the members had lived in the area for at least one year prior to the interview. The final sample from each area thus comprised a random selection of members of householders who have lived in the area for at least a year aged 16 years or over.

### The samples

Details of the contact rate for each area and the total number of interviews achieved is presented in Appendix A, Table A.I. The overall contact rate was 62% in the pretest and 64% in the posttest surveys. The most common reasons for non-contact was 'refusal' and 'no reply after 4-calls'. These rates are slightly lower than have been achieved in other recent crimes surveys which might be a result of attempting to obtain a 100% sample in a small area over a short period of time.

### CHAPTER THREE: THE NW AREAS AND PROGRAMMES

This chapter outlines the nature of the 'treatment' received in the programme areas and includes a description of the areas, a brief history of the launch and development of the Nw schemes, and a summary of residents' participation in the schemes.

#### **The areas**

The exact location of the NW sites is well known to members of the Home Office and Research and Planning Unit and to the Met. Police who helped with the research. It has been the policy of the research, however, to keep the exact location of these sites confidential as it was foreseen that publications of the evaluation would include personal information about a large proportion of residents living in quite small areas.

The Acton experimental area is situated in the Ealing Police Division and covers nine roads. The area adjoins a major trunk road on its western side and is within walking distance of a major route road on its southern side. Residential roads adjoin it on three of its four sides. The area is not often used as a throughroute by pedestrians or vehicles, but it is used by outsiders as a place to park vehicles. The housing stock comprises large Edwardian and Victorian houses, many of which have been converted into flats or maisonnettes.

The Wimbledon experimental area is in the Merton Police Division and covers 13 full roads and 2 half roads. The area adjoins a major route road on its western side and is bounded by a railway line on its northern side. A busy

main road cuts across the southern boundary. On its eastern edge is the displacement area of similar housing stock. The area is not often used as a through route by pedestrians or vehicles.

The Wimbledon displacement area is very similar to the experimental area. It comprises 6 full roads and two half roads and is located on the eastern edge of the experimental area and is bounded by the same railway line at the north and stream at the east. It differs from the experimental area in that the southern edge meets other similar residential roads.

The Redbridge control area covers 2 full roads and 5 half roads. It is bounded by major roads at the north and east. At the southern edge of the area are similar residences which stretch down to a major high street. The western edge of the area comprises residences and a park. The area is dominantly semi-detached and terraced houses.

The programmes

The NW scheme in Acton was launched in June 1985 with a public meeting which was held in the evening in a church hall within the scheme area. It was attended by two home beat officers, a home beat sergeant, two crime prevention officers, a NW co-ordinator and a chief inspector and about 100 residents, including the area co-ordinator and some street co-ordinators who had already volunteered.

The first co-ordinators<sup>1</sup> meeting was held 10 days after the launch, of the programme at the home of the area co-ordinator and was attended by all street co-ordinators, a home beat officer and a crime prevention officer. Co-ordinators' meetings were then held at approximately monthly intervals

throughout the year. Later meetings sometimes were attended by outside speakers who spoke to the group after the business of the meeting. There were no formal meetings of all residents in the area during the period of the research. In preparation for the co-ordinators' meetings, the area co-ordinator, the home beat officer and the crime prevention officer collaborated in producing a monthly newsletter. The newsletter reported on crimes in the area over the preceding month and any other points about crime or the area that either the co-ordinators or the police wished to draw to the attention of the local residents. The newsletters were brought to the co-ordinators<sup>1</sup> meeting and distributed among the street co-ordinators who later delivered them by hand to participants in their streets.

It is often not possible, or desirable, to erect street signs immediately upon the launch of a NW scheme. The Met. Police were willing, however, to erect signs in the two experimental areas as quickly as possible to assist the evaluation. These were clamped to lamp-posts close to the entrance of every street in the area approximately 3 months after the launch meeting.

The NW scheme in Wimbledon was launched in July 1985 (less than 2 weeks following the Acton launch) at a meeting in a church hall within the experimental area. It was attended by a home beat officer, a crime prevention officer, a NW co-ordinator, a superintendent and about 100 local residents.

The first co-ordinators' meeting was held approximately 3 months after the launch in the home of the area co-ordinator. The meeting was attended by the home beat officer, the area co-ordinator and the street co-ordinators. It was decided by those in attendance that co-ordinators' meetings should

be held once every three months. In fact, only three meetings were held during the first year of the scheme. As in Acton, there were no further formal meetings of all residents in the area after the launch meeting during the period of the research. Two newsletters were produced over the year by the home beat officer in collaboration with the area co-ordinator. The topics covered: trends in crime in the area, successful arrests, the importance of displaying NW window stickers and the procedure for obtaining police advice on property marking.

Street signs were erected in the area about 3 months after the launch and were positioned on lamp-posts at the entrance of every road leading into the area.

#### Levels of participation

In the second round of surveys, respondents in the two experimental areas were asked a number of questions about the NW scheme: their knowledge of it, their participation in it and their attitudes towards it. Their responses provided an overall picture of the impact and nature of the schemes in the two areas.

A number of measures were used to determine participation rates in the programmes. Residents were first asked whether they were aware that a NW watch scheme had been operating in their area over the previous year. Almost all residents interviewed said that they were aware of the scheme and about half could estimate to within one month how long the scheme had been running. Residents were also asked whether they thought of themselves as participants in the scheme. Sixty-two per cent of respondents in Acton and 55% of those in Wimbledon said that they perceived themselves to be a

participant. Approximately, the same percentage of respondents (both participants and non-participants) in each area reported displaying a NW sticker at some time over the last 12 months. The proportion of households reporting that they had marked their property over the last 12 months was low in both areas (16% in Acton and 13% in Wimbledon) and the proportion who said that they had done this as a result of the NW scheme was even lower (8% in Acton and 6% in Wimbledon). All residents were asked if they had been visited by a police crime prevention officer, or any other police officer, over the last 12 months concerning a security survey of their home. Only 5% of residents in Acton and 3% in Wimbledon said that they had received such a visit.

One of the key mechanisms by which NW is supposed to reduce crime is through the active surveillance of residents and their reporting of suspicious incidents to the police. Residents were asked if over the last 12 months they had deliberately looked out for anything suspicious in their area. Forty-seven per cent of respondents in Acton and 40% in Wimbledon said that they had done so. They were then asked if they had seen anything suspicious. Twenty-three per cent of interviewees in Acton and 18% in Wimbledon said that they had seen something. When asked if they had reported what they had seen to somebody, about half of the residents who had seen something in Acton and about one-third of those in Wimbledon said that they had done so. Most respondents who had reported something suspicious reported it to the police (73% of reporters in Acton and 75% in Wimbledon). Overall, 39% of residents in Acton and 27% of residents in Wimbledon who saw something suspicious over the last 12 months reported it to the police.

Just over half of the residents in Acton, who were not themselves street

co-ordinators, knew, either by name or by sight, who their street co-ordinator was. Less than one-quarter of residents in Wimbledon said that they knew who their street co-ordinator was. The proportion of residents who knew their home beat officer was quite low in both areas. Only 28% interviewees in Acton and 15% of respondents in Wimbledon reported that they knew the officer's name or appearance and only 18% of residents in Acton and 13% of residents in Wimbledon said that they had seen him at least once during the previous 12 months. Only 14% of respondents in Acton and 9% in Wimbledon reported that over the last 12 months they had spoken to their home beat officer.

As noted earlier, the two NW areas were selected as they appeared to represent potentially successful sites in terms of both likely participation and potential success. The results described above show that even in these most promising areas, the actual levels of participation and the comprehensiveness of the programme were still not great. The issue of whether these schemes are, in fact, the best examples of NW in the Met. is distinct, however, from the issue of whether they represent some kind of treatment capable of being evaluated. It would seem fair to argue that any crime prevention measure implemented by between 44% and 62% of residents (the proportion of households in each area claiming to be participants in the scheme) should be regarded as a considerable achievement. Clearly, some kind of treatment has been implemented in the two areas during the research period and, as such, can be evaluated. Whether or not the treatment constitutes a NW scheme, or an adequate version of one, is another matter which will be discussed in detail in later chapters.



## CHAPTER FOUR: THE IMPACT OF NW ON CRIME

One of the main aims of NW is crime prevention. This chapter presents the major findings of the research concerning the impact of NW on crime and its effect on reporting rates and crime detection.

The main research findings on the relationship between NW and crime derive from the results of the crime surveys. Before presenting these findings it is important that the research definition of crime and the procedure for selecting crimes for inclusion in the survey is made explicit.

### **Offences included in the surveys**

The main questionnaire contained screening questions relating to 17 categories of offence. Details about each offence (up to a maximum of 8 per household) were recorded on a victim form and were later coded by trained staff in accordance with Home Office counting rules. The codes used were the same as those used in the second British Crime Survey, although offences falling outside the survey's coverage and offences which were ambiguous were excluded from the analysis. A list of offences and the offence codes used in the survey is presented in Appendix A, Table A.2.

### Method of counting offences

Victim forms were completed for Up to 8 single or series incidents. Series offences were crimes which were committed in a similar way, against the same victim or household, and possibly by the same person. Series offences were recorded on a single victims form, but, for the purpose of analysis,

were counted as separate offences up to an arbitrary maximum of 5.

Offences were included only if they were committed within the boundaries of the survey area and a question to determine this was added to each victim form. It was also necessary to determine the exact month, or exact quarter, in which the offences were committed in order to place them correctly within or without the experimental (either pretest or posttest) period. Offences falling outside of the specified periods were excluded from the analysis. It was also necessary to exclude all offences which were not dated. Offences were included as falling within the pretest period if they were committed between July 1984 and June 1985 and within the posttest period if they were committed between July 1985 and June 1986. Series offences were dated according to the date specified for each offence within the series (rather than by weighting on the most recent incident). Offences within the series falling inside the experimental periods have been included in the analysis and those falling outside (or when the data is unknown) have been excluded.

#### Method of determining a programme effect

The effectiveness of NW was assessed by using multivariate analysis. The choice of method was affected by the nature of the dependent variable (total victimisations). An important assumption of parametric multiple regression analyses is that the error distribution of scores for the independent variable approaches normality (a normal distribution or 'bell-shaped' curve). As the error distribution curve of the total number of victimisations (TOTOFF) was not normally distributed, it was decided that the assumptions of parametric multiple regression tests could not be met. An alternative programme was chosen which allowed multivariate

analysis and which accommodated variables based on 'counts' with non-normal error structures- The package on which most of the following analysis is based is the GLIM (Generalised Linear Interactive Modelling) programme developed by a Working Party of the Royal Statistical Society (Payne, 1986).

The analysis required creating a number of pooled datasets comprising data for an experimental and control area for the pretest and the posttest surveys. Two dummy variables were added to the data: one to register whether the area was a NW area (EXPCON - 'Experimental or CONTROL); and the other to identify whether the period was the pretest or the posttest (WAVE - Wave 1 or Wave 2). The procedure adopted was to add first all the 'covariates' to the model (in GLIM terminology the 'continuous' independent variables) and to remove them one at a time to determine whether their removal significantly increased the scaled deviance. If the variables contributed significantly to the model, they were replaced, if not, they were removed. The 'factors' (in GLIM terminology 'categorical' independent variables) were then added to the model as a block and removed or refitted depending on their overall contribution to the model. Finally, the interaction term was added.

Before proceeding, it might be helpful to explain the importance of the interaction term. The test of a NW effect cannot be determined by looking solely at differences between the pretest and posttest (WAVE) or differences between the experimental and control areas (EXPCON). In the former, total crimes for Wave 1 for both experimental and control areas are compared with total crimes for Wave 2 for both experimental and control areas. This comparison masks the effect of being a resident in an experimental or a control area. In the latter, total crimes for the

experimental area for Wave 1 and Wave 2 combined are compared with total crimes for the control area for Wave 1 and Wave 2 combined. This comparison masks the effect of being a respondent in the Wave 1 or Wave 2 surveys. In order to observe a programme effect it is necessary to look simultaneously at WAVE and EXPCON. This is done by creating an interaction term which is the product of the two terms and is usually written as WAVE\*TREATMENT (in this case WAVE\*EXPCON).

The test for a programme effect was, therefore, whether the removal of the interaction term increased the scaled deviance significantly and whether the sign associated with the scale parameter indicated that this effect was positive. A programme effect was defined as occurring when the interaction terms added significantly to the model and a 'successful' programme effect was defined as occurring when the interaction term was positive (i.e. when victimisation rates reduced by a greater amount or increased by a lesser amount in the experimental area than in the control area).

The covariates and factors used in the pooled-dataset analysis were those most frequently used as independent variables in the analysis of crime reporting. Ideally, covariates should be unrelated to the treatment, but related to the outcome measure. It was hypothesised that the demographic variables chosen would not be affected by the treatment (the NW programme), but would affect the likelihood of victimisation.

#### Changes in survey reported crime

The first calculation involved dichotomising the dependent variable (total number of victimisations) into 'none' and 'one or more' offences to provide a measure of change in prevalence of victimisation (the number of

households victimised at least once). The error structure was defined as 'binomial' for the purpose of the GLIM analysis. The results of the comparisons are presented in Appendix A, Table A.3- The results showed that removing the interaction term had no significant effect in terms of explaining the prevalence of household victimisation for the Wimbledon sample. In other words, there was no apparent programme effect in relation to the proportion of households victimised. The effect of removing the interaction term was significant, however, for the Acton sample, but, NOT in the direction hypothesised. The negative interaction effect indicated that the proportion of households victimised increased more rapidly in the experimental area than in the control.

The second calculation involved using the full version of TOTOFF comprising the total number of victimisations recorded per household. The error structure was defined as 'poisson' for the GLIM analysis. The results of the comparisons are presented in Appendix A, Table A.4. For each comparison of an experimental with the control area the effect of removing the interaction term was shown to be statistically significant, but again, NOT in the direction hypothesised. For each comparison, total recorded victimisations increased more rapidly in the experimental areas than in the control area. In the Wimbledon area, therefore, the increase in victimisations was brought about by an increase in the rate of victimisation (i.e. an increase in multiple victimisations) rather than by an increase in prevalence. In the Acton area, the increase in victimisation resulted from an increase in both prevalence and incidence.

The third calculation would have concerned the displacement of crime. The displacement area was included in the research design to assess whether any reduction in crime in the experimental area resulted in increases in crime

in the adjacent area as a result of crime being displaced. As there was no significant reduction in either the prevalence or the incidence of crime in the Wimbledon experimental area the displacement comparison becomes redundant.

A fourth calculation was conducted to determine whether there was a differential programme effect among sample sub-groups. It is possible that, while the programme was not effective for the population as a whole, it was effective for these specific subgroups. The bivariate analysis showed that there were some reductions (albeit non significant) in the total victimisations for single persons and for renters in the Wimbledon area. In order to test for such an effect, data was selected for these subgroups and comparisons were made between Wimbledon experimental area and Redbridge control areas using GLIM analysis. The results showed that the interaction term was not significant for either comparison.

#### Changes in reporting rates

One of the aims of NW in London is to improve communication between the police and the public (i.e. the public becoming the 'eyes and ears' of the police). An important element of this communication is the reporting of crime. To a greater or lesser extent the police encourage residents in NW areas to report victimisations. The widely held belief that reporting rates increase in NW areas is often used as an explanation of poor reductions, or even increases, in crime following the launch of NW schemes. The use of crime surveys enables an assessment of changes in reporting rates.

A question was included on each victim form enquiring whether the offence was reported to the police. As only one victim form was used to record

information about series offences (relating to the most recent offence in the series) it was necessary to weight these responses. (No more than 5 offences per series were included in the analysis.) As the dependent variable (REPORT) was dichotomous and a nominal scale factor GIM analysis was used specifying a binomial error structure. Pooled data sets were created using Wave 1 and Wave 2 data for combinations of experimental and non-experimental areas. A new variable was created which measured the distribution of offence types (OFFTYP). The results of the analysis are presented in Appendix A, Table A.5.

None of the comparisons shows a WAVEEXPCON interaction effect. Reporting rates increased only in Acton and this was by a small amount. After controlling for differences in the samples and differences in the distribution of offence types, there is no evidence from this data of a favourable treatment effect in terms of reporting rates.

#### Changes in clear-up rates

It was not possible in the time available to collect details on offences cleared-up which were committed in the experimental areas. It was possible, however, to obtain data on the clear-up rate for the subdivisions as a whole for the pretest and posttest periods. In both experimental areas, NW schemes were being launched at a regular rate. Consequently, the proportion of residents living in a NW scheme area was increasing over time. If NW improved detection, it would be expected that this would be reflected in the clear-up rates for the subdivisions as a whole. The clear-up rates for a package of four offence types over the pretest and posttest period is shown in Appendix A, Table A.6.

In fact, the clear-up rate for all four offences decreased from the pretest to the posttest period in both areas. In addition, there was no single offence category in either area which experienced an improvement in clear-up rate during the experimental period.

### **Explaining the changes as a programme effect**

The preceding analysis has failed to show any positive changes associated with NW and its impact on crime, its reporting or its detection. The analysis has shown, however, one negative finding concerning an increase in prevalence of victimisation in the Acton area and two negative findings concerning an increase in total victimisations in both NW areas. It should be considered, therefore, whether the negative results could be explained by the implementation of the programmes.

It is possible that offenders who seek to steal goods (burglars or car thieves) are attracted to NW areas because the scheme suggests to them that there is something in the area worth taking. This is a familiar argument and has been applied to other preventive efforts such as the installation of burglar alarms and the fitting of additional security locks. The extent to which such visual cues attract criminals is unknown. Another possible explanation is that residents become complaisant following the introduction of NW which derive from a belief that others are now looking after the problem of crime and, as a result, they need no longer make an individual effort. There is no convincing evidence for either explanation from the criminological literature or from the results of informal discussions with residents in the areas. It is prudent, therefore, to consider other possible explanations.



### Alternative explanations

Some of the problems of validity associated with quasi-experimental survey research designs are outlined by Cook and Campbell (1979). They divide the problems into statistical conclusion validity, internal validity and external validity. The main issues which concern the question of the cause of the increase in crime relate to the former two threats to validity. The first concerns whether the statistical methods used are sensitive enough and the second whether it is reasonable to identify one of the variables as the cause of the other.

It is well known that the parametric multiple regression methods are more powerful than their non-parametric equivalents. It could be argued that the GLIM statistical package, which although is not non-parametric will allow a broader range of data to be analysed, is not the most powerful package that can be used. Comparisons between GLIM and other packages, however, do not find it substantially inferior to its parametric counterparts (Farrington and Tarling, 1985).

Assuming that there is a statistical association between NW and crime, what are the dangers in drawing the conclusion that the schemes caused the increase in total victimisations? The two main threats to internal validity which could affect the interpretation of the results are statistical regression and history.

Statistical regression means that crime rates returned from an untypically extreme level (either high or low) in the pretest period to a more typical average level in the posttest period. In order to apply this to the current findings, it would have to be shown that crime rates in the two

experimental areas were abnormally low in the pretest period and returned (or regressed) to a more typical higher level in the posttest. What evidence is there for this? As only the one pretest survey was conducted, there is no additional survey data to compare the pretest survey recorded crime rates with earlier rates. It is reasonable, however, to draw upon any other data that might shed light on trends in crime over longer periods. The most obvious alternative source is police recorded crime. In Wimbledon, there is no evidence that crime rates in the pretest period were abnormally low. In Acton, there was some evidence of an unusually low rate of crime in the pretest period. There are problems associated with making assumptions about crime trends based on police recorded crime and based on small numbers of crime. It is possible, however, that, in Acton at least, the increase in survey reported crime in the posttest period can, in part, be explained by regression to the mean.

It is also possible that the control area digressed from a mean level in the pretest period to a below mean level in the posttest. What evidence is there that crime rates in the control area fell to an untypically low level during the posttest period? Evidence from police recorded crimes over a three year period prior to the launch suggests that this could have occurred in relation to the Redbridge site. Such a movement would have affected both Acton and Wimbledon comparisons creating greater differentials between the experimental and control areas than might normally have been observed.

The increase in crime could also be explained in terms of differences in history between the areas (apart from the introduction of NW). Efforts were made in the design of the evaluation to reduce differences between areas as much as possible so that, ideally, only the effects of the programme were

being measured. It was not possible, of course, to control all of the differences in the histories of the areas over the experimental period. Attempts were made to avoid particular changes such as ensuring: that no new police initiatives were launched in the areas during the research period, that the home beat officers remained in the areas for the entire 12 month period, that the boundaries of the NW areas were not altered over the period of the experiment and that no other schemes or streets were grafted on to the experimental programmes. Nevertheless, other changes might have occurred in the areas which could not be controlled. What evidence is there that anything changed which might have affected crime rates either in the experimental or control areas? The areas were monitored closely during the course of the research to determine whether such changes occurred. One of the areas had a problem with commuter parking and for some years the local council had tackled this problem by blocking nearby street entrances with gates, but there was no evidence during the research period that any changes had been made to traffic flow or vehicular parking. Another issue relating to history is the existence of and movement of active criminals. There was no evidence that in either area particularly prolific offenders had been arrested or released from custody during the research period. There was also no evidence of changes in pedestrian routes through or away from the experimental areas.

Finally, outcome differences might have resulted from differences in the experimental and control samples which were not accounted for by the factors entered into the multivariate analysis. There may be other differences between the groups which have not been controlled and these differences may relate to victimisation. The extent to which this can explain the significant increases in crime in the experimental areas and the reductions in crime in the control areas is uncertain. It is difficult

to imagine that differences in the characteristics of the population in Acton could alone explain the increase in crime over the experimental period.

#### **Concluding comment**

This chapter has examined the impact of NW on crime, reporting rates and crime detection. There is no evidence of beneficial effects in any of these areas. The negative finding that victimisations rates increased more frequently in the NW areas compared with the non-NW areas is most plausibly explained in terms of regression of crime levels to the mean. In one experimental area crime levels were lower than normal in the pretest period and in the control area crime levels were higher than normal in the posttest period. Overall, the strongest conclusion that can be **drawn** on the basis of the available evidence (taking into account all the problems noted) is that NW had no discernable impact on crime, its reporting or its detection.

## CHAPTER FIVE: THE IMPACT OF NW ON PUBLIC ATTITUDES, BEHAVIOUR AND PERCEPTIONS

The survey was designed to collect information not only on the victimisation experiences of the residents in the NW areas, but also on possible qualitative changes that might have occurred in these communities. This chapter examines residents' responses to questions contained in the pretest and posttest surveys which aimed to tap some of the proposed qualitative benefits of NW. In addition, an examination is made of changes in the number and nature of station telephone calls and emergency ('999') calls made by the public to the police.

### Method of analysis

The choice of qualitative changes to be investigated derived from the claims of the NW publicity material and academic discussion on the likely impact of these programmes. The main areas identified and selected for investigation were: (1) fear of crime; (2) perceived probability of victimisation; (3) satisfaction with living in the area; (4) social cohesion of the area; (5) security behaviour (6) evaluation of police service; and (7) contact with the police.

As often more than one question was used to evaluate each topic, it was necessary to combine the responses into a single measure. This involved devising analytic scales based on the combined responses of the relevant questions. The method adopted of selecting and scaling items was similar to that used by Wycoff et al. (1985) and Rosenbaum et al. (1985). Both authors used 'principal component factor analysis' and the 'reliability' procedure within the SPSS-X package (SPSS Inc., 1986) to construct scales from an

initial list of eligible items. The main purpose of using these techniques was to ensure that the scales were single factored or • unidimensional<sup>1</sup> (measuring one thing) and internally consistent (the items 'hung together'<sup>1</sup>). The scales were developed using the full set of Wave 1 and Wave 2 data (combined) for the two experimental areas. The scales generally 'hung together' well with Alpha coefficients ranging from .72 to .83.

Strictly speaking, the scales (the dependent variables in the analyses) do not meet all of the requirements of parametric tests. Variables based on strength of attitudes or on the perceived certainty of something happening do not constitute interval scales as their intervals might not represent the same degree or same strength. In this chapter, the GLIM (Generalised Linear Interactive Modelling) package has been used to conduct all multivariate analyses as it can accommodate non-interval scale dependent variables (Payne, 1986).

The experimental design used in the analysis of public attitudes, behaviour and perceptions comprised a single group design with separate pretest and posttest samples. The effectiveness of the programme was assessed, therefore, by comparing respondents' scores in the pretest and the posttest. This design is not as strong as the untreated control group design as the absence of a control group leads to difficulties in interpreting the results. Nevertheless, the design is common in experimental research and can provide interpretable findings. It is important, however, that the additional threats to internal validity are considered in arriving at a conclusion.

The method of determining a programme effect was essentially the same as that described in the previous chapter. The dependent variables were the

scale scores for each of the factors being investigated. A pooled dataset was created comprising data for one experimental area for the pretest and posttest surveys. A dummy variable (WAVE) was created which identified the period as the pretest or posttest. Covariates were added to the model in a block and removed one at a time to determine whether their contribution was significant. Significant covariates were replaced and non-significant ones removed. The factors were then added and the procedure repeated. The programme was defined as being effective if the removal of the variable WAVE (whether pretest or posttest) increased the scaled deviance significantly and the direction of change was favourable.

Changes in attitudes, behaviour and perceptions

The significance of WAVE in relation to each of the factors investigated is shown in Appendix A, Tables A.7 and A.8. In Acton, two factors changed significantly in a favourable direction from the pretest to the posttest and one changed significantly in an unfavourable direction. In Wimbledon, one of the factors changed significantly in a favourable direction from the pretest to the posttest and one changed significantly in an unfavourable direction.

The most encouraging result is the significant reduction in the fear of household (property) crime among residents in Acton. Another encouraging finding was a significant increase in social cohesion in the Acton area. Finally, significant improvements were recorded in the involvement of others in looking after unoccupied dwellings in the Wimbledon area.

The less encouraging results include those which showed no significant change. In neither area did residents in the posttest surveys report

significantly lower levels of fear of personal crime or significantly lower estimates of the likelihood of being victimised. In neither area were residents more likely in the posttest surveys to report being satisfied with the area in which they lived or more likely to evaluate the police highly. One explanation for this absence of change in the last two categories is that residents generally reported that they were satisfied with their area and with the police at the pretest stage of the surveys. It is obviously more difficult to improve on an already highly favourable attitude.

The most troubling finding was the significant reduction in both the Acton and Wimbledon sites in the recency of observation of police in the area. This is particularly hard to explain as the local home beat officers had clearly put considerable effort into the schemes. It is possible, however, that the implementation of the NW scheme resulted in the officers directing more of their attention to the area and street co-ordinators, whom the home beat officers saw regularly, and less time to the other residents in the area. It is also possible that the residents in the area had become more observant, as a result of NW, and more capable of accurately estimating the last time they saw an officer in their area.

The impact of NW might differ between subgroups of the population. The analysis revealed some additional positive associations between the variable WAVE and particular factor scales for specific subgroups in the Wimbledon area. Significant reductions in fear of personal crime were found for females and significant reductions in the fear of household crime among females and low income groups. No additional significant positive associations were found in either area.



**Changes in telephone contacts from the public to the police**

The purpose of investigating telephone messages by the public to the police was to determine the extent to which NW affected police-community relations in the sense of increasing formal contacts between the two and also the extent to which it affected the quantity and quality of information made available to the police which might be useful in the detection of crime.

Telephone messages to the police comprise direct calls to the local station (usually redirected from area switching centres) and '999' or emergency calls. The small number of calls made directly to individual officers has not been included in the analysis. Telephone messages were recorded by the police by hand on station message pads or recorded in the form of printer output and were normally kept by the local station in files for a period of six months before destruction. The stations were instructed not to destroy any messages as soon as the areas were selected. Information on both kinds of telephone message was collected for a period before the launch of the NW schemes (limited to 7 months before as more distant messages had been destroyed) and for the entire posttest period. In order to simplify comparison, however, only those messages received during the 7 month period before and the same 7 month period after the launch are presented here.

A summary of the number of calls received over the pretest and posttest periods is presented in Appendix A, Table A.9. The total number of station messages received for the seven month period before the launch of the NW scheme and the same seven month period following its launch decreased in both experimental areas. As station messages include crime reports, it is possible that the total number of station messages is related to the total number of crimes committed which might obscure other underlying trends.

Excluding crime reports from the analysis in fact accelerates the reduction. During the same period the number of station messages received from the Wimbledon displacement area actually increased by a small amount.

The total number of emergency calls received over the same pretest and posttest periods decreased from the Wimbledon experimental area by one-half. In the Acton area, the number of emergency calls increased by just over one-third. It should be noted, however, that the numbers on which these percentages are based are small.

It might be assumed that the closer relationship between the police and the public would lead to more direct calls to the station and fewer emergency '999' calls. The proportion of '999' calls to direct messages to the station decreased in both the Wimbledon experimental and displacement areas. In neither case was the reduction statistically significant. In Acton the proportion of '999' calls increased. There is no clear evidence, therefore, that NW has shifted the method of police contact.

It could be argued that it is the quality not the quantity of information that is received by the police which is important. Although the number of calls made to the police did not increase, it is possible that the quality of the calls received improved. It is difficult to decide what is useful information for the police and discussions with police officers suggested that almost any kind of message can be useful. Nevertheless, the police accept that information relating to suspicious persons or incidents is particularly valuable to them as it can lead directly to an arrest. In addition, the police emphasise reporting suspicious behaviour in the NW publicity material and at the launch meetings of individual schemes. A simple qualitative classification of messages can be constructed,

therefore, by dividing messages into those relating to suspicious persons or incidents and other messages.

Overall, there was no increase in the number of station messages relating to suspicious persons or behaviour in either Acton or Wimbledon. There was an increase in the number of emergency calls relating to suspicious events in Acton, although, the numbers involved were small (just 6 messages more in the posttest than in the pretest period). In Wimbledon, the number of emergency calls relating to suspicious circumstances decreased from the pretest to the posttest.

#### **Explaining the changes as a programme effect**

The analysis has revealed that fear of household crime and social cohesion changed significantly in a favourable direction in the Acton NW area and involvement with others in home protection changed significantly in a favourable direction in the Wimbledon area. It was also found that the public were significantly less likely to report a recent sighting of the police in both the Acton and Wimbledon areas. Is it possible that these changes could have been the result of the NW programme?

There is little explanation in the publicity material of the process by which NW is supposed to create beneficial attitudinal, perceptual and behavioural effects in a community. It might be speculated, however, that fear of household crime might be reduced as a result of residents having the opportunity to share their fears with other residents. The broad message of neighbourhood watch is that residents should help one another in the fight against crime which some members might find consoling. The formal involvement of the local police in the community may suggest to the

residents that help is now closer at hand than before.

Was the significant increase in social cohesion in Acton the product of NW? Interviews with street co-ordinators conducted independently of the social surveys revealed a strong belief that cohesion had improved and that this was the result of the NW scheme. The absence of any significant increase in social cohesion in Wimbledon might be the result of a difference in the level of implementation of the scheme or simply a difference in the style of the scheme.

Was the improvement in the use of others in the protection of unoccupied dwellings in Wimbledon a result of the NW programme? This change is probably one of the most expected as householders are actively encouraged at the launch meeting and in the publicity material to share with the police in the protection of the community. It is hard to explain, however, why this change should be significant in Wimbledon but not in Acton.

#### **Alternative explanations**

Cook and Campbell (1979) propose that the major threats to **internal validity in quasi-experimental design** are **statistical regression, history,** and maturation.

**Statistical regression** occurs when a score achieved in the **pretest** is **abnormally high or abnormally low** so that, all other things being equal, it would be expected that scores would tend to move towards the mean. To apply this to the positive, significant findings it would have not be argued that the significant-reductions in unfavourable responses in the posttest were achieved as a result of abnormally high levels of these responses in the

pretest. What grounds are there to suspect that any of these measures were in such a state during the first round of surveys?

One way to assess this is to examine differences between the two experimental areas. It could be hypothesised that if regression were occurring, the pretest scores for the area experiencing a significant effect should be higher (less favourable) than the pretest scores for the area not experiencing a significant effect. In fact, the mean pretest scores for the two experimental areas were similar. The only reliable way to rule out statistical regression, however, is to analyse data covering a period prior to the pretest surveys. Such data cannot normally be collected within the usual budgetary constraints of survey research.

History can affect outcomes when some external event not measured in the survey influences the dependent variable. It is unfortunate that a full control group design could not be adopted as this would have helped (but not entirely overcome) the problem of disentangling programme effect from external event effect. It would have to be argued that an external event influenced factor scores in either the Wave 1 or Wave 2 surveys (i.e.: the event was present or absent, or weaker or stronger, in Wave 1 or Wave 2). What evidence is there that the changes in attitudes, perceptions and behaviours identified resulted from local history?

Apart from the greater involvement of the community with the police resulting directly from the implementation of the NW programme, there was no known local event which could have affected attitudes and perceptions in these small communities. All developments in the area were monitored as closely as possible over the pretest and posttest periods and the only major change noted in the areas was the implementation of NW. Conceivably ,

national events could have affected the area. During a period of rising recorded crime rates, however, and corresponding press reports declaring these increases, it seems unlikely that national crime rates could have positively affected fear of crime and perceptions of crime. The only other national event that might directly affect perceptions of crime is a national crime prevention initiative. Over the period of the research the major crime prevention initiative (apart from the MAGPIE campaign) was the implementation of NW.

Maturation refers to the independent growth rate of the experimental subjects (or areas) which might simulate or distort a treatment effect. It must be considered, therefore, whether the favourable changes measured in the surveys could have resulted from natural and independent growth rates in the areas investigated. The residential areas in Acton and Wimbledon had been gradually improving over a period of years before the implementation of NW. The Wimbledon site was at one time a run down area and was threatened with demolition to make way for a motorway. Once the threat had passed, the price of homes in the area gradually rose and began, once again, to attract purchasers. Over the last ten years, houses in both areas have undergone home improvements of various kinds. It is conceivable, therefore, that, in such generally improving areas, attitudes and perceptions relating to the area would also improve.

The problem of maturation in relation to evaluating NW is closely linked to the problem of causal order. This problem is especially acute when the schemes are resident rather than police initiated. The most effective way of overcoming this problem is by random allocation of NW schemes. Another possible solution is multiple pretest- over quite long periods of time, although such an approach would be expensive and would probably be

impracticable. In the absence of objective evidence of maturation it is necessary to draw on anecdotal and other kinds of 'soft' data to help in determining the impact of maturation. Conversation with police officers who have known these areas for some time suggest that both physical and attitudinal improvements in the area have taken place. Whether such improvements could progress at a rate that would be noticeable over a one year period is not known. The influence of maturation on the significant findings shown in the research cannot be ruled out, however, and it almost certainly played some part in creating the measured improvements.

#### Concluding comment

This chapter has examined the impact of NW on public attitudes, behaviour and perceptions. The most encouraging findings were the reduction in fear of household crime and increase in social cohesion in one area and the increase in the involvement of others in home protection in the other area. Less encouraging findings were the absence of any significant impact of NW on fear of personal crime, perceived probability of crime, satisfaction with the area or police evaluation. The most troubling result was the significant reduction in the recency of observation of police in the area. Similarly, the analysis of public telephone calls to the police showed no overall improvement in either their number or quality.

## CHAPTER SIX: SUMMARY AND DISCUSSION

The findings of the evaluation were not very encouraging in terms of the impact of the schemes on crime. The research has shown that the prevalence of crime (the number of households victimised at least once) did not change significantly over the experimental period in one area and increased in the other and the incidence of crime (the total number of victimisations) increased in both areas. Reporting rates and clear-up rates changed little from the pretest to the posttest periods. The findings relating to public attitudes, behaviour and perceptions were more favourable, although the overall level of improvements was not great. One of the NW areas showed a decrease in the fear of household crime and an increase in social cohesion. The other NW area showed an improvement in residents' involvement with others in home protection and a reduction in fear of crime in relation to particular subgroups (but not for the sample as a whole).

The general impression given by these results is that the measured changes were less favourable than might have been expected. The results showed few positive effects, a large number of non-effects and some negative effects. Some readers might find the few positive effects sufficient in themselves and consider that these constitute an overall programme success. Nevertheless, the evidence from this evaluation suggests only a limited programme effect. Why were these schemes limited in their effectiveness?

There are a number of reasons why an evaluation fails to observe the effects hypothesised. Rosenbaum (1986) has grouped these reasons into three broad categories: (1) 'measurement failure'; (2) 'programme failure'; and (3) 'theory failure'. 'Measurement failure' or conclusion validity failure



occurs when a programme is measured as being unsuccessful when in fact is successful. This is also referred to as a Type II error (concluding that a treatment effect does not exist when it does). Measurement failure can occur as a result of non-existent or inappropriate statistical tests of significance or as a result of weak or inappropriate research designs. 'Programme failure' can occur when the scheme fails to operationalise the theoretical construct being tested. This might result from implementation failure or inappropriate implementation. The danger of programme failure is that the lack of success of the programme might be attributed to the theoretical construct rather than to the particular scheme being investigated. 'Theory failure' can occur when the absence of change resulting from a programme is a product of a weakness in the theoretical formulation of the treatment implemented. Lack of success might be the result of a defect in the theory itself rather than in the method by which it was implemented. In order to assess the most likely reasons for the limited impact of the NW schemes investigated, it is necessary to examine each of these potential sources of failure.

#### Measurement failure

Measurement failure might generate a Type II error as a result of inappropriate research design or failure to consider threats to internal validity, and inappropriate methods of analysis.

It is widely accepted that even the best quasi-experimental design generates problems of internal validity. As the allocation of respondents to treatment and non-treatment groups is not random, they will be different in ways apart from the experience of the experimental programme. The implementation of NW in London, for example, has not been conducted on a

random basis as the launch of police initiated schemes is guided by the suitability of the area and the launch of public initiated programmes is reliant on the enthusiasm of active residents. Inherent differences between treatment and control groups lead to problems of interpretation and determining whether any changes are a result of the treatment or other differences between the groups or areas. These inherent differences between the groups generate problems which are commonly referred to as threats to internal validity.

The extent to which threats to internal validity might have affected the current research findings have already been discussed in detail in the concluding sections of both of the main results chapters. The most influential of the threats identified was the problem of natural regression to the mean. It was accepted that the recorded increases in both the incidence and prevalence of victimisation in the Acton area was almost certainly due to a regression from a unusually low level of crime to a more typical higher level. In addition, the results of both NW area comparisons with the Redbridge control area were affected by the untypically low level of crime reported in the control area during the posttest period. It was a result of this threat that it was decided that the strongest conclusion that could be drawn was that NW had no significant effect on victimisation rate.

Financial and other restraints often demand, utilising less than perfect research designs. The strongest quasi-experimental method is the untreated control group design with pretest and posttest using the same pretest and posttest samples. The advantage of this method is that unmeasured differences between the groups can be controlled, to some extent, by using pretest scores as a covariate. The design used in the evaluation of the

impact of NW on crime comprised a untreated control group design using separate pretest and posttest samples. The choice of method was influenced by the known problems associated with survey designs which require reinterviewing the same respondents in the posttest period. Research using this design has reported as much a 50% attrition rate between the first and second surveys. The design used, however, has the drawback that pretest and posttest samples are separate which creates an additional threat to internal validity. It is unlikely, however, that using separate rather than the same samples could alone account for the failure to observe a crime reduction effect. This could only have occurred if the posttest samples in both areas were significantly less victim-prone than the pretest samples and that this victim-proneness was of a kind that could not be measured by known demographic factors.

The design used in the evaluation of the impact of NW on public attitudes, behaviour and perceptions was also a weaker quasi-experimental. In order to attract market research companies to conduct the research for the sum of money available, it was necessary to cut down on the interviewing time. It was decided to do this by asking questions relating to public attitudes, behaviour and perceptions only in the experimental areas. The final research design thus comprised a simple before and after comparison using separate samples. The main additional problem that this design creates is that it is unknown whether any changes observed in the treatment groups would have happened anyway. The improvements might have happened anyway if the improvements occurred nationwide or if they occurred as a result of a specific process of maturation. It seems unlikely that the changes observed in reductions in fear of crime, increases in social cohesion and improvements in home protection behaviour were at the time occurring nationally or in similar areas up and down the country. The problem of

whether they would have occurred anyway because of maturation (the area becoming gradually less fearful or more cohesive as a result of natural growth) would exist for any kind of experimental design and comprised just one of the many threats to internal validity. The only way in which this design could have masked more impressive improvements in public attitudes and perceptions is if they worsened elsewhere. There is little reason to suppose that such attitudes worsened in similar areas over the one year period of the research.

Measurement failure can also occur as a result of the type of analysis used. The main problems are whether the statistical tests are appropriate for the data and whether they are powerful enough. The choice of test has been discussed at length elsewhere in the report. There is no reason to believe that the method of analysis employed was inappropriate for the type of data used. It has been argued that the GLIM package is not as powerful in its ability to guard against Type I and Type II errors as parametric multiple regression tests. It has also been argued that this difference in power is probably not substantial. It is unlikely, therefore, that the method of analysis has masked any stronger treatment effect.

#### Programme failure

Another possible explanation of the limited success of schemes is programme failure. Programme failure or implementation failure might result from the implementation of a weak version of the intended programme or a wrong version of the intended programme.

The first problem occurs when a weak version of the right kind of programme is implemented. It has to be decided whether the weak programme represents

what Rosenbaum (1986) calls the 'minimum dosage' or minimum treatment required to bring about the desired effect. It would not be expected that a particular scheme would show evidence of success if the actual scheme implemented was in some way less than that necessary to bring about the desired effect. The second problem occurs when a strong scheme is implemented which does not accurately operationalise the principles of the programme intended. This problem is sometimes referred to as a problem of construct validity and occurs when the programme implemented falls to match the underlying theoretical concept being tested. In this case, the lack of a treatment effect would say little about the effectiveness of the theoretical formulation which had not been evaluated by the programme.

To what extent can the limited effects of the two programmes investigated be explained in terms of weak programme implementation? There are no generally agreed minimum dosages of the particular elements of NW which can be used to assess the level of implementation of the current programmes. Nevertheless, it would be expected that there was some evidence of each of the key elements of the programme. It would have to be shown, therefore, that there was evidence of window stickers and street signs to identify the programme, that there was evidence that residents looked out for suspicious incidents and reported these to the police, that there was evidence that residents met and discussed the running of the scheme, and that there was evidence of the other elements of a comprehensive NW package such as property marking and home security surveys. The programmes investigated showed some evidence of each of these elements. It is more difficult to assess whether these elements were implemented to a sufficient degree.

The assessment of whether the dosages of each of these elements was high enough must be to some extent subjective. It would seem that the

identification of the two areas as NW sites was adequate.- Street signs were erected on lamp-posts on every road entering the area and a high proportion of residents displayed NW window stickers. It is less certain whether the number and nature of meetings between residents represented an adequate level of implementation. Unlike many North American schemes, residents in the area who were not area or street co-ordinators were not expected to attend formal meetings. Apart from the initial public launch meeting, the majority of residents took no further part in other meetings or organised discussions concerning the running of the programme. As a result, the contribution of the majority of residents in the area was limited to displaying a window sticker and looking out for anything suspicious. Over half of respondents said that they had looked out for suspicious activity during the last twelve months which, although could be improved, was perhaps sufficient to implement the surveillance element of the programme. The proportion of households reporting marking their property or receiving a security survey was low and almost certainly would not comprise a meaningful crime prevention element of the programme.

Were the schemes implemented examples of NW or some other kind of programme? The nature of a NW schemes is poorly specified in the literature which makes it difficult to know what is and what is not NW. It would appear, however, that the schemes implemented approximated the theoretical formulation of NW more closely than any other kind of crime prevention measure. It is argued, therefore, that these schemes more accurately reflected weak versions of NW than strong versions of some other programme.

### **Theory failure**

The limited success of the schemes might also be explained in terms of a defect in the theory or principles of NW as a crime prevention or fear reduction measure. Few attempts have been made, however, to specify the theory or principles of NW.

The theory of NW can be extrapolated from what is known about its structure, its aims and the social processes by which it is supposed to achieve its aims. Some of the elements of NW have been discussed already in Chapter One. NW in London has been described as a network of public spirited members of the community who become the eyes and ears of the police. It should be noted, however, that not all versions of NW are conceived in this way and some commentators have placed much greater emphasis on the ability of communities to police themselves. The aims of NW invariably include crime reduction and almost invariably include reduction in fear of crime and improved community involvement and satisfaction. The social process by which NW supposedly achieves these aims is through opportunity reduction as a result of neighbours looking out for suspicious incidents and reporting them to the police. North American versions of NW also note the importance of creating signs of occupancy as a means of deterring potential offenders.

The issue of theory failure concerns whether the process described above can reasonably be expected to bring about the beneficial effects that the programmes claim. Until the theory or *key* elements of the theory have been tested it is not possible to conclude that it is or is not a failure. Nevertheless, on the basis of what is known, there are at least some problems associated with this conceptualisation. The effectiveness of

public observation in small communities is probably very limited. Many household are unoccupied for most of the day; many dwellings are poorly situated to facilitate effective observation; many communities have a sufficiently high turnover of residents to make identification of strangers difficult; and many communities are populated by outsiders who enter the area for legitimate purposes. In addition, there are some doubts about whether offenders can be deterred from offending by the knowledge that the residents look out for suspicious activities and report these to the police. It is also uncertain whether monthly or quarterly meetings of residents to discuss crime or the publication of a regular newsletter reporting latest crime figures could possibly lead to a reduction in fear of crime. Conceivably, such events could just as easily increase fear of crime.



## CHAPTER SEVEN: CONCLUDING COMMENT

The previous chapter outlined some of the possible reasons for the limited effect of the NW schemes investigated in this report. In this final chapter, the weight of the evidence is assessed in order to provide an overall conclusion.

There were a number of problems with the research design and method of measurement which need to be taken into account when assessing the findings. Nevertheless, it is unlikely that the failings of the research have masked what in reality was a much stronger NW success. There are also problems associated with the theory of NW. It is unconvincing to argue, however, that the broadest versions of the theoretical formulation of NW are so impoverished that without further testing they can be dismissed as a theory failure. This leaves programme failure as the most likely cause of the schemes' limited success.

In the context of the current research the issue of whether the schemes evaluated were weaker than they should be is double edged. It has to be decided whether the specific programmes are good versions of NW in London and it also has to be decided whether the schemes being implemented in London are good examples of NW in general.

It is argued that the two schemes investigated were good or at least average examples of NW in London. Regular monitoring of the progress of the experimental programmes indicated that they were enthusiastically and actively implemented by all of the key public and police personnel responsible for their development. One of the areas, for example, held an

anniversary street party to celebrate the first year of the scheme at which over one hundred local residents attended. It most respects the key actors in the programme did **all** that was expected of them within the framework of **NW** in the **MPD**. **It would** appear, therefore, that the major problem of implementation did not lie in the specific efforts of residents and police responsible for the **two** programmes investigated, but in the general design and administration of NW in London.

There are a number of general problems of implementation of NW in the MPD. Very few schemes hold regular formal meetings involving all residents in the area and little attempt has been made to encourage such meetings. The majority of residents often do no more than display a window sticker and look out for suspicious activities. Another problem concerns the flexible nature of these programmes. The divisional chief superintendents were instructed to fit NW to the needs of the individual communities and were given only broad guidelines on the kinds of schemes to establish. Consequently, there has been little guidance on the specific details of the programme and little indication of acceptable minimum levels of implementation of the individual elements. As a result, many schemes have no formal organisational structure, hold no informal or formal meetings and produce no newsletter.

A major problem with programme implementation in the MPD is that the police are finding it difficult to deliver the goods. Crime prevention officers can provide home security surveys for only a small proportion of households in any one year. Similarly, they can provide only a limited personal property marking service and most of the time can do no more than give advice on where to buy DIY property marking equipment. Home beat officers also find it difficult to deliver their share of the agreement. The

increasing pressures on home beat officers to launch and administer NW schemes, to otherwise patrol their beats and to be on call for other, often public order, duties at short notice means that the amount of time that they can spend in any single scheme area is limited.

There are a number of reasons, some of which have been discussed above, why the implementation of NW schemes in the MPO falls short of the ideal specified in the theoretical formulation of NW. It is possible that the limited impact of the two schemes evaluated can to some extent be explained in terms of a forcewide problem of implementation. This conclusion is not intended to be a criticism of the Met. but an indication of the direction in which those responsible for future developments of NW in London might choose to turn.

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APPENDIX A

Table A.I

RESPONSE RATES FOR THE PRETEST AND POSTTEST SURVEYS FOR THE FOUR RESEARCH AREAS

	Act. Exp.	Wimb. Exp.	Wimb. Disp.	Red. Con.
<b>Pretest</b>				
total h.h.'s	639	711	540	495
total ineligible [1]	115	73	74	53
total eligible	524	638	466	442
total interviewed	306	353	323	306
response rate [2]	58%	55%	69%	69%
<b>Posttest</b>				
total h.h.'s	710	702	570	557
total ineligible	95	179	87	44
total eligible	615	523	483	513
total interviewed	400	309	332	330
response rate	65%	59%	69%	64%

[1] Addresses are ineligible if they are vacant, demolished or non-residential. Households are ineligible if residents have not lived in the areas for the whole of the experimental period.

[2] Response rate is the total number of interviews divided by the total number of eligible households.

Table A.2

## CLASSIFICATIONS OF OFFENCES USED IN THE ANALYSIS

Household Offences	Code	Description
Theft of motor veh.	60	Theft of car/van
	62	Theft of motorbike/scooter/moped
Theft from motor veh,	61	Theft from car/van
	63	Theft from motorbike/scooter/moped
Bicycle theft	64	Theft of pedal cycle
Burglary	50	Attempted burglary to non-connected domestic garage/outhouse
	51	Burglary in a dwelling (nothing taken)
	52	Burglary in a dwelling (something taken)
	53	Attempted burglary in a dwelling
	57	Burglary from non-connected domestic garage/outhouse (nothing taken)
	58	Burglary from non-connected domestic garage/outhouse (something taken)
Theft in a dwelling	55	Theft in a dwelling
Other h.h. theft	65	Theft from outside dwelling (excluding theft of milk bottles)
	66	Theft of milk bottles from outside dwelling
	56	Theft from meter
Criminal damage	80	Arson
	81	Criminal damage to a motor vehicle (£20 or under)
	82	Criminal damage to a motor vehicle (over £20)
	83	Criminal damage to the home (£20 or under)
	84	Criminal damage to the home (over £20)
	85	Other criminal damage (£20 or under)
	86	Other criminal damage (over £20)
	88	Attempted criminal damage

(Continued)



Table A.2 (continued)

## CLASSIFICATIONS OF OFFENCES USED IN THE ANALYSIS

Personal Offences	Code	Description
Assault	11	Serious wounding
	12	Other wounding
	13	Common assault
	21	Attempted assault
Theft from person	43	Snatch theft from person
	44	Other theft from person
	45	Attempted theft from person
Robbery	41	Robbery
	42	Attempted robbery
Sexual offences	31	Rape
	32	Serious wounding with a sexual motive
	33	Other wounding with- sexual motive
	34	Attempted rape
	35	Indecent assault
Threats	91	Threats to kill/assault
	92	Sexual threat
	93	Other threat or intimidation
	94	Threats against respondent made to someone else

Table A.3

**MULTIVARIATE (GUM) ANALYSIS OF THE RELATIONSHIP BETWEEN THE WAVE.TREATMENT  
INTERACTION TERM AND THE PREVALENCE OF VICTIMISATION**

Comparison	Change In scaled deviance	Change in d.f.	Significance of change
Acton experimental versus Redbridge control	9.04	1	<.001
Wimbledon experimental versus Redbridge control	2.62	1	n.s.

Note: Chi square test of significance: n.s.= not significant.

Note: The following covariates and factors were entered initially into each equation: age, income, age completed full-time educ., number of adults in h.h. , number of children in h.h., s.e.g., gender, marital status, tenure, race, employment status, car ownership.

Table A.4

**MULTIVARIATE (GLIM) ANALYSIS OF THE RELATIONSHIP BETWEEN THE WAVE. TREATMENT  
INTERACTION TERM AND THE INCIDENCE OF VICTIMISATION**

Comparison	Change in scaled deviance	Change in d.f.	Significance of change
Acton experimental versus Redbridge control	15.7	1	<.001
Wimbledon experimental versus Redbridge control	9.7	1	<-001

Note: Chi square test of significance.

Note: The following covariates and factors were entered initially into each equation: age, income, age completed full-time educ, number of adults in h.h., number of children in h.h., s.e.g., gender, marital status, tenure, race, employment status, car ownership.

Table A.5

**MULTIVARIATE (GLIM) ANALYSIS OF THE RELATIONSHIP BETWEEN THE WAVE-TREATMENT INTERACTION TERM AND REPORTING RATES**

Comparison	Change in scaled deviance	Change in d.f.	Significance of change
Acton experimental versus Redbridge control	1.4	1	n.s.
Wimbledon experimental versus Redbridge control	0.7	1	n.s.

Note: Chi square test of significance: n.s.= not significant.

Note: The following covariates and factors were entered initially into each equation: age, income, age completed full-time educ, number of adults in h.h. , number of children in h.h., s.e.g., gender, marital status, tenure, race, employment status, car ownership.

Table A.6

**CLEAR-UP RATES IN THE PRETEST AND POSTTEST PERIODS FOR FOUR OFFENCE TYPES IN THE ACTON AND WIMBLEDON SUBDIVISIONS.**

Percentages

	<u>Acton subdivision</u>		<u>Wimbledon subdivision</u>	
	Pretest	Posttest	Pretest	Posttest
Theft of motor veh. [1]	5.8	5.8	9.0	8.0
Theft from vehicle	4.1	3.7	7.8	4.3
Burglary Dwelling	7.1	4.4	11.1	6.3
Criminal Damage	10.4	8.0	6.9	5.6
Total for all offs.	7.1	5.7	8.3	5.7

[1] Includes theft and unauthorised taking of motor vehicles

Table A.7

**MULTIVARIATE (GLIM) ANALYSIS OF THE RELATIONSHIP BETWEEN WAVE (WHETHER PRETEST OR POSTTEST) AND VARIOUS PUBLIC ATTITUDES, BEHAVIOURS AND PERCEPTIONS**

ACTON EXPERIMENTAL AREA

Factor	Change in scaled deviance	Change in d.f.	Significance of change
Fear of personal crime	0.1	1	n.s.
Fear of household (property) crime	7.0	1	<.01
Perceived probability of personal crime	0.2	1	n.s.
Perceived probability of household (property) crime	0.4	1	n.s.
Satisfaction with area	2.5	1	n.s.
Social cohesion	5.2	1	<.05
Involvement of others in home protection	0.2	1	n.s.
Evaluation of police performance	0.6	1	n.s.
Police contact	32.0	1	<.001

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Note: Chi square test of significance: n.s.= not significant.

Note: The following covariates and factors were entered initially into each equation: age, income, age completed full-time educ., number of adults in h.h. , number of children in h.h., victimisation rate over last 12 months, s.e.g., gender , marital status, tenure, race, employment status, car ownership.

Table A.8

**MULTIVARIATE (GLIH) ANALYSIS OF THE RELATIONSHIP BETWEEN WAVE (WCTHER PRETEST OR POSTTEST) AND VARIOUS PUBLIC ATTITUDES, BEHAVIOURS AND PERCEPTIONS**

WIMBLEDON EXPERIMENTAL AREA

Factor	Change in scaled deviance	Change in d.f.	Significance of change
Fear of personal crime	0.5	1	n.s.
Fear of household (property) crime	1.2	1	n.s.
Perceived probability of personal crime	0.4	1	n.s.
Perceived probability of household (property) crime	0.2	1	n.s.
Satisfaction with area	3.2	1	n.s.
Social cohesion	0.8	1	n.s.
Involvement of others in home protection	7.4	1	<.01
Evaluation of police performance	0.1	I	n.s.
Police contact		1	<.001

**Note:** Chi square test of significance: n.s.= not significant.

**Note:** The following covariates and factors were entered initially into each equation: age, income, age completed full-time educ. , number of adults in h.h., number of children in h.h. , victimisation rate over last 12 months, s.e.g. , gender, marital status, tenure, race, employment status, car ownership.

Table A.9

**TELEPHONE MESSAGES RECEIVED BY THE LOCAL POLICE STATION FOR A SEVEN MONTH PERIOD  
BEFORE THE LAUNCH AND FOR THE SAME SEVEN MONTH PERIOD AFTER THE LAUNCH OF THE  
NEIGHBOURHOOD WATCH SCHEME IN ACTON EXPERIMENTAL AREA AND FOR WIMBLEDON  
EXPERIMENTAL AND DISPLACEMENT AREAS**

	Act. Exp.			Wimb. Exp.			Wimb. Disp.		
	[1] Pre	Post	%chg.	Pre	Post	%chg.	Pre	Post	%chg.
<b>Station Messages</b>									
Crime Report	15	19		9	20		11	16	
Susp. Pers./Inc.	11	8		11	7		10	1	
Disturb./Dispute	11	5		6	7		11	7	
Requ. for Assist.	12	10		13	3		7	6	
Ace./Haz./Other	15	9		6	5		10	13	
<b>Total</b>	<b>64</b>	<b>51</b>	<b>-20</b>	<b>45</b>	<b>42</b>	<b>-7</b>	<b>49</b>	<b>51</b>	<b>+4</b>
<b>Emergency '999' calls</b>									
Crime Report	8	3		18	10		9	8	
Susp. Pers./Inc.	4	10		6	3		0	4	
Disturb./Dispute	8	10		10	9		13	5	
Requ. for Assist.	0	4		7	1		2	1	
Ace./Haz./Other	6	3		13	4		8	7	
<b>Total</b>	<b>26</b>	<b>35</b>	<b>+35</b>	<b>54</b>	<b>27</b>	<b>-50</b>	<b>32</b>	<b>25</b>	<b>-ZZ</b>
<b>All Calls</b>									
Crime Report	23	22		27	30		20	24	
Susp. Pers./Inc.	15	18		17	10		10	11	
Disturb./Dispute	19	15		16	16		24	12	
Requ. for Assist.	12	14		20	4		9	9	
Ace./Haz./Other	21	17		19	9		18	20	
<b>Total</b>	<b>90</b>	<b>86</b>	<b>-4</b>	<b>89</b>	<b>69</b>	<b>-22</b>	<b>81</b>	<b>76</b>	<b>•6</b>

[1] Pretest and posttest periods are from December to the following In June.  
Note: Station messages for a 5 day period in the posttest period were missing in Acton.. Emergency calls for a 13 day period in the pretest and a 9 day period In the posttest periods were missing in Wimbledon. As the 5 day period in Acton and the 4 day difference in Wimbledon represents not more than 1 message, no adjustment has been made.