

16. The Crime Reducing Effect of Improved Street Lighting: The Dudley Project

Kate Painter and David P. Farrington

EDITOR'S NOTE: Until recently, the received wisdom on improved street lighting was that it might reduce fear, but it has little effect on crime (Tien et al., 1979). This view is now changing, largely due to the work of Kate Painter in Britain. In the face of much skepticism (cf. Atkins et al., 1991; Ramsay and Newton, 1991), she has produced a series of studies suggesting that the crime prevention benefits of street lighting have been underestimated. Each of her studies has sought to improve on the methodology of earlier ones. In this latest study, written for this volume with David Farrington, she avoids the pitfalls of using official crime data by conducting victimization surveys in carefully matched experimental and control areas — two adjacent public housing estates in the town of Dudley. The study produces clear evidence that crime of all kinds decreased significantly in the re-lit estate compared with the control. Moreover, there was little sign of displacement of crime to the control estate. Young people who might have been responsible for much crime on the estates were not displaced to the control estate by the re-lighting. Rather, the improved lighting attracted more young people at night to the experimental area. This might have been expected to produce more crime except that the improved lighting probably increased the fear of apprehension. It is common to criticize situational measures for their harmful social effects, but this study

shows, once again, how situational measures can reduce crime and improve the quality of life — in this case by facilitating the congregation of young people.

Previous Research on Street Lighting and Crime

The main aim of this project is to investigate the effects of improved street lighting as a crime prevention technique. Modern interest in the relationship between street lighting and crime began in North America amidst the dramatic rise in crime which took place in the 1960s. Many towns and cities embarked upon major street lighting programs as a means of reducing crime and initial results were encouraging (Wright *et al.*, 1974). A review by Berla (1965) claimed that street lighting improvements caused a substantial reduction in crime in six cities, and other research in New York (Wheeler, 1967) and Newark (Tyrpak, 1975) found that improvements were particularly effective when introduced alongside an increase in police patrols. Following a major relighting program in Kansas City, night-time robbery and assaults decreased by half and property offenses also decreased, but non-significantly (Wright *et al.*, 1974). Also, a study of four high crime areas in the District of Columbia reported that night-time crime reduced between 30% to 54% following street lighting improvements (Hartley, 1974).

The proliferation of positive results across North America led to a detailed review of the effect of street lighting on crime by Tien *et al.* (1979) funded by the Law Enforcement Assistance Agency (LEAA). The final report describes how the 103 street lighting projects originally identified were eventually reduced to a final sample of only 15 that were considered by the review team to contain sufficiently rigorous evaluative information. With regard to the impact of street lighting on crime, the authors found that as many projects reported an increase or no change as a reduction in crime. However, each project was considered to be seriously flawed because of such problems as: weak project designs; misuse or complete absence of sound analytic techniques; inadequate measures of street lighting; poor measures of crime (all were based on police records); insufficient appreciation of the impact of lighting on different types of crime; and inadequate measures of public attitudes and behavior.

The Tien *et al.* (1979) review was undeniably painstaking but it may have been too negative and dismissive of the available evidence. Among the 15 projects studied in detail, it did not distinguish between those that were more rigorous methodologically and the remainder. The report concluded (Tien *et al.*, 1979:93) that "... the paucity of reliable and uniform data and the inadequacy of available evaluation studies preclude a definitive statement regarding the relationship between street lighting and crime." However, it is interesting that the most rigorous project, the only one with before and after measures of crime in experimental and control areas (Wright *et al.*, 1974, in Kansas City), found a significant reduction in violent crime. Perhaps the most important point made by Tien *et al.* (1979) was that improved street lighting could lead to increased reporting of crime to the police, and hence that the effects of improved street lighting should *not* be measured using police-recorded crime.

Logically, the Tien *et al.* (1979) review should have led to attempts to measure the effects of improved street lighting using alternative measures of crime, such as victim surveys, self-reports or systematic observation. Unfortunately, it was interpreted as showing that street lighting had no effect on crime, and it basically consigned the issue to

the wilderness in North America.

In the United Kingdom, very little research was carried out on street lighting and crime until the late 1980s, and two brief literature reviews bemoaned the "scant and elusive" nature of street lighting research (Mayhew *et al.*, 1976; Fleming and Burrows 1986). There was a resurgence of interest in the issue between 1988 and 1990, when three small scale street lighting projects were implemented and evaluated in different areas of London (in Edmonton, Tower Hamlets and Hammersmith/Fulham). Each project was focused on a poorly lit, essential street or walkway leading from residential accommodation to transport, leisure and shopping facilities. Pedestrians were interviewed about their experiences of crime and disorder six weeks before and six weeks after street lighting improvements were made and pedestrian street use was also monitored. In each location crime, disorder and fear of crime declined and pedestrian street use increased dramatically after the improvements (Painter, 1994). The third project (in Hammersmith and Fulham) also included a follow-up survey twelve months later with elderly people which found that the reduction in crime and disorder had been sustained (Painter, 1991a).

Similar results were obtained in a project using surveys twelve months before and twelve months after street lighting improvements on a local authority estate in the North West of England. Both survey-reported crime and police-recorded night-time crime reduced (Painter, 1991b). More mixed results were obtained when street lighting was upgraded across an inner city area of Birmingham. Survey-reported crime against households reduced but there was no discernible change in crime against pedestrians or commercial premises. Night-time pedestrian street use increased substantially (Bainbridge and Painter, 1993).

In contrast to these generally positive results, a major Home Office-funded evaluation in Wandsworth (Atkins *et al.*, 1991) concluded that improved street lighting had no effect on crime, and a Home Office review, published simultaneously, also asserted that "better lighting by itself has very little effect on crime" (Ramsay and Newton, 1991). The Atkins *et al.* (1991) evaluation appeared to be well designed, since it was based on before and after measures of police statistics and victimization reports in relit (experimental) and control areas. However, in analyzing police statistics, crimes were dubiously classified into those likely or unlikely to be affected by street lighting. Robbery and violence, which decreased significantly in the Wright *et al.* (1974) project, were thought *unlikely* to be affected by street lighting (Atkins *et al.*, 1991: 7). Interestingly, while the "likely" crimes decreased by only 3% after the improved lighting, the "unlikely" crimes decreased by 24% (Atkins *et al.* 1991:10). Unfortunately, the response rates in the victimization surveys were very low (37% before and 29% after). Only 39 crimes were reported in the before survey in the experimental area and only 13 in the control area, suggesting that the research had insufficient power to detect changes.

Other British evaluations of the effect of improved street lighting on crime have been carried out in Cardiff (Herbert and Moore, 1991), Hull (Davidson and Goodey, 1991), Leeds (Burden and Murphy, 1991) and Strathclyde (Ditton *et al.* 1993). These evaluations produced mixed results; for example, the number of crimes increased after relighting in the Cardiff study but decreased in the Strathclyde study. Unfortunately, these projects, along with all other published British street lighting evaluations except Atkins *et al.* (1991), involved simple before-after comparisons in a relit area, and lacked a control area. This simple one-group pre-test/post-test design has been described as "the least sophisticated approach to evaluation" (Lurigio and Rosenbaum 1986:34) because of the numerous

threats to internal validity which make it impossible unambiguously to attribute changes in an outcome measure (e.g. crime) to the effects of an intervention program (e.g. improved street lighting).

A control area is required to rule out the following major threats to internal validity (Cook and Campbell, 1979):

- a. *History*: changes in crime after lighting improvements might be attributable to events other than the improvements;
- b. *Maturation*: changes in crime after lighting improvements might be attributable to a continuation of pre-existing trends;
- c. *Testing*: changes in measured crime after lighting improvements may be attributable to the effects of the "before" measurement;
- d. *Instrumentation*: changes in crime after lighting improvements may be attributable to changes in methods of measurement;
- e. *Regression*: especially if lighting improvements are implemented in a high-crime area, decreases in crime after the improvements may reflect regression to the mean as a result of natural fluctuations over time; and
- f. *Mortality*: changes in reported crime after lighting improvements may be attributable to non-comparable samples caused by dropping out of respondents.

The main aim of the present research is to assess the effect of improved street lighting on crime, using before and after victimization surveys in experimental and control areas, with samples that are sufficiently large to detect likely effects of the intervention. To the extent that the areas are comparable, selection effects (changes in crime attributable to pre-existing differences between the samples) are controlled. Also, the study resembles a "double-blind" clinical trial, since neither respondents nor interviewers knew about its purpose. Also, it permits the investigation of displacement of crime from the experimental to the control area.

Theoretical Relationships between Street Lighting and Crime

There is no specific body of theory that relates street lighting to crime. Nonetheless, explanations of the way street lighting improvements could prevent crime can be found in "situational" approaches which focus on reducing opportunity and increasing perceived risk, through modification of the physical environment (Clarke, 1992); and in perspectives which have stressed the importance of strengthening informal social control and community cohesion through more effective street use (Jacobs, 1961; Angel, 1968) and investment in neighborhood conditions (Taub *et al.*, 1984; Fowler and Mangione, 1986; Lavrakas and Kushmuk, 1986; Taylor and Gottfredson, 1986).

Situational crime prevention suggests that crime can be prevented by environmental measures which directly affect offenders' perceptions of increased risks and decreased opportunities. Street lighting is likely to increase the visibility of offenders at night and hence to increase their perceived risks of being seen, recognized, reported, interrupted or caught. Certainly, the deterrent effect of visibility and potential surveillance has been a consistent theme to emerge when offenders have been interviewed about their motives,

methods and target selection (Bennett and Wright, 1984; Feeney, 1986; Nee and Taylor, 1988; Nugent *et al.*, 1989; Cromwell *et al.*, 1991; Light *et al.*, 1993), although there is no direct evidence that offenders are less willing to define a well-lit street as a criminal opportunity than a dark street. It might be thought that improved lighting might possibly increase criminal opportunities if it encourages more potential victims to go out at night. Against this, however, is the deterrent effect of having more people on the street.

Jane Jacobs drew attention to the role of good visibility combined with natural surveillance as a deterrent to crime. In particular, she emphasized the association between levels of crime and public street use and suggested that less crime would be committed in areas with an abundance of potential witnesses (Jacobs, 1961). Similarly, Angel (1968) observed that opportunities for street robbery were high on streets used by few people: a sufficient number to provide targets without too much waiting around, but not enough to operate as a deterrent. As pedestrian street use increased, streets became safer because of the proximity of potential guardians, an effect also stressed by routine activity theorists (Cohen and Felson, 1979). In a British context, Hillier (1987) found that areas with the highest public use experienced low burglary rates in comparison to areas with low pedestrian use. Hence, if good street lighting encourages more effective use of neighborhood streets at night, opportunities for natural surveillance and informal social control will be enhanced.

Street lighting improvements may also reduce crime and fear by eliminating areas where offenders can hide prior to committing an offense. Areas of concealment have been identified as a design characteristic affecting public surveillance and defensibility of space (Newman, 1972; Merry, 1981) and they have been shown to influence an offender's choice of crime locations (Phelan, 1977; Bennett and Wright, 1984). In addition, increased illumination and visibility could reduce crime if an increased range of vision alerts pedestrians to potential offenders sufficiently in advance to allow them to take evasive action. Feeney (1986) noted that a substantial number of robbery victims were aware that they were about to be robbed when they saw the aggressor approach, wait or pass them on the street. In turn, fears for personal safety, which dramatically undermine the quality of life in a neighborhood (Skogan, 1990), might reduce if the perceived risk of a surprise attack diminishes.

Other theoretical perspectives have emphasized the importance of investment to improve neighborhood conditions as a means of strengthening resident confidence, cohesion and social control (Wilson and Kelling, 1982; Taub *et al.*, 1984; Taylor and Gottfredson, 1986; Skogan, 1990; Foster and Hope, 1993). As a highly visible sign of positive investment, improved street lighting might reduce crime if it physically improves the environment and signals to residents that efforts are being made to invest in and restore their neighborhood. In turn, this may lead them to have a more positive image of their area and increased community pride, optimism and cohesion. It should be noted that this theoretical perspective predicts a reduction in both day-time and night-time crime. Consequently, attempts to measure the effects of improved lighting should not concentrate purely on night-time crime.

In addition to leading to a positive change in resident opinions and physically creating a brighter and safer environment, street lighting might also send a non-verbal message to offenders that the reputation of the area is improving, that there is more social control, order and surveillance and hence that crime at that location is riskier than elsewhere. Crime could

further reduce because offenders living in the area will be deterred from committing offenses there and potential offenders from outside the **area** will be dissuaded from entering it. Crime and fear in the area should diminish as perceived risks and personal knowledge of crime reduce. Furthermore, street lighting may also prevent crime by discouraging disorderly behavior, thereby preventing trivial offences from escalating into more serious crime (Wilson and Kelling, 1982).

In summary, the relationship between visibility, social surveillance and criminal opportunities is a consistently strong theme to emerge from the literature. A core assumption of both opportunity and informal social control models of prevention is that criminal opportunities and risks are influenced by environmental conditions in interaction with resident and offender characteristics. While street lighting is not a direct solution to crime, it can act as a catalyst to stimulate crime reduction through a change in the perceptions, attitudes and behavior of residents and offenders.

The Present Research

RESEARCH DESIGN

This evaluation employed a non-equivalent control group design with before and after measures of crime in experimental (relit) and control areas. Using a victim survey, the prevalence and incidence of crime were measured twelve months before and twelve months after the installation of improved street lighting in the experimental area and at the same times in a control area where the street lighting remained unchanged. The questions on crime were identical in all surveys. An adjacent area was selected as the control area because it was envisaged that the people living there would be similar in many respects to those in the experimental area, and also to facilitate the investigation of spatial and temporal displacement. Hence, demographic factors which might influence crime rates should be equivalent at the outset. It becomes more plausible, therefore, that any change in crime between the relit and non-relit areas can be attributed to the street lighting program rather than to pre-existing differences between the samples. This design controls for the major threats to internal validity outlined above.

RESEARCH HYPOTHESES

The primary hypothesis guiding the evaluation was that street lighting improvements would reduce crime in the relit area. A subsidiary hypothesis was that, if effective, street lighting might displace crime to the adjacent non-relit area. In order to investigate theoretical links between street lighting and crime prevention, other predictions concerning intervening causal mechanisms were tested. It was hypothesized that any decrease in crime would be mediated by measurable changes in attitudes, perceptions and behavior. Additional subsidiary hypotheses were that street lighting improvements might:

- a. reduce fear for personal safety after dark;
- b. reduce vicarious victimization (knowing someone who has been victimized);
- c. alleviate perceptions of neighborhood problems;
- d. enhance the perceived quality of neighborhood life;
- e. increase night-time pedestrian street use.

SELECTION AND DESCRIPTION OF THE PROJECT AREAS

The research was **carried** out in Dudley, West Midlands. The experimental area was selected for lighting by local authority engineers on the basis of need. The street lighting was in a bad state of repair and had been the subject of complaints from tenants. The adjacent control area was similar to the experimental area on basic demographic and design characteristics. Both **areas** were local authority housing estates which had been built at approximately the same time (mid-1930s) and had similar architectural design (semi-detached and terraced rows of four, low-rise dwellings with gardens front and back); a similar number of dwellings (approximately 1,200-1,300 on each estate); the same housing authority and similar housing allocation policies; and clear geographic boundaries (both backed on to a nature reserve and were separated and bounded by main arterial roads). The design and layout of the estates, and the type of dwellings, facilitated natural surveillance, which was potentially important for street lighting to be effective as a crime prevention strategy.

DESCRIPTION OF THE STREET LIGHTING PROGRAM

In a four-week period during February-March 1992, 129 high-pressure sodium (white) street lights were installed over 1,500 meters of roadway in the experimental area. The new lighting replaced the older type mercury lamps. The improvements were made only to residential roads: footpaths between the houses were not relit. The area was illuminated in accordance with category 3/2 of BS 5489, which gives an average **illuminance** of 6 lux and a minimum of 2.5 lux. The British Standard (BS 5489, Part 3) lists three categories of lighting levels corresponding to low, medium and high crime risk areas and levels of traffic and pedestrian usage. Given the improved technology and required lighting level, column spacing decreased from 40 meters before to 33 meters after. The new lighting scheme resulted in savings in energy and maintenance costs and the amount of useful light more than doubled. The pre-existing street lighting in the experimental area did not achieve the minimum **standard** of 3/3 laid down in British Standards. Consequently, the lighting upgrade constituted a noticeable alteration of the **night-time** environment.

THE BEFORE AND AFTER VICTIMIZATION SURVEYS

The timing of data collection was the same in both survey areas. The before survey was completed between February and the end of the **first** week in March 1992. The after survey was completed during the four week period mid-February to mid-March 1993. Both surveys enquired about events in the previous twelve months. Thus, the before survey period covered January 1991-January 1992 and the after survey period covered February 1992-February 1993, including the street lighting installation period which began in the third week of February 1992. Care was taken to ensure that nobody in the before survey was interviewed after the lights were improved.

The before and after surveys measured household victimization and respondents' perceptions, attitudes and behavior. The majority of questions on victimization, fear of crime and quality of life were **similar** to those used in successive British Crime Surveys (e.g. Mayhew *et al.*, 1993). Respondents were only asked about crimes which had occurred **on their estate** during the previous twelve months, and supplementary questions ensured that the same criminal event did not generate reports of two categories of crime. Additional questions on public reactions to the new lighting and travel behavior after dark were

included at the end of the after survey as part of a process **evaluation of** program implementation. Other crime prevention strategies, such as Neighborhood Watch and policing strategies, were monitored through closed and open-ended questions and interviewer fieldwork sheets, as were other possible extraneous historical influences which might have caused a change in outcomes within and between the survey areas.

SAMPLING PROCEDURES

The sampling frame was the electoral register which contained 2,560 addresses in the two areas. This was supplemented by field enumeration to identify missing properties. Statistical power analysis (Cohen, 1988) was carried out to estimate the minimum sample sizes needed to detect an effect of the intervention on crime in the experimental area. The analysis was based on the likely prevalence of victimization, which was assumed to be about 50%; any sample sizes that were sufficient to detect differences in prevalence would be likely also to detect differences in incidence (the number of crimes), since incidence is the more sensitive measure.

It was assumed that the sample sizes should be sufficient to detect a 10% decrease in the prevalence of victimization, from 50% to 40%. Setting the probability of a Type I error or alpha (the probability of falsely rejecting the null hypothesis) to the conventional level of .05, two-tailed, and the statistical power, or 1 minus beta (where beta is the probability of falsely accepting the null hypothesis) to the conventional level of .80 (Cohen, 1988:56), yielded a required sample size of 407 per group (Fleiss, 1981:273). Relaxing alpha to .05, one-tailed, reduced the required sample size to 325 per group; relaxing power to .75 reduced it to 362; and assuming a decrease in prevalence from 40% to 30% reduced it to 376. These sensitivity analyses suggested that samples of about 325-400 people before and after in both areas were required.

In the 1992 British Crime Survey, 10,059 interviews were achieved from 14,890 issued addresses (Mayhew *et al.*, 1993:154). Assuming the same success rate in the present survey suggested that about 600 addresses should be issued in each area in order to achieve about 400 interviews. Exactly 600 addresses were issued to interviewers in each area (essentially a 50% random sample drawn from the electoral register); 431 interviews were achieved in the experimental area and 448 in the control area. Excluding void properties (6% experimental, 3% control), the before response rate was 77% in each area, exactly the same as in the 1992 British Crime Survey. This response rate is likely to be an underestimate, because there was a high rate of non-contact (14% experimental, 16% control) and some of these properties should probably have been classified as void (empty or ineligible); 12% of issued addresses were classified as void in the 1992 British Crime Survey.

Only the addresses interviewed in the before survey were issued to interviewers for the after survey. The number of households re-interviewed was 372 (86% of the before sample) in the experimental area and 371 (83%) in the control area. Of those re-interviewed, 90% were the same respondent as in the before survey, 7% were the same household but a different respondent, and 3% were a different household at the same address. Unfortunately, it was not possible to **link** up before addresses with after addresses in order to carry out longitudinal analyses with each address acting as its own control. Hence, the before and after surveys had to be treated as repeated cross-sectional surveys.

INTERVIEWING PROCEDURES

The household face-to-face interviews took between 45 and 90 **minutes** depending on the extent of victimization. Prior to an interviewer calling, households were sent a leaflet which explained that a crime survey was taking place, but no mention was made of the proposed street lighting initiative. To minimize any unwitting interviewer bias, interviewers were *not* told about the true purpose of the survey and were, therefore, unaware of the lighting improvements that were to take place. They were also unaware that there were experimental and control areas. The same interviewing team, consisting of 17 interviewers, were employed in each of the study areas, before and after. For the after survey, every effort was made to match interviewers to their before respondents. The research was carried out by a company which had previous experience of undertaking community surveys. A 20% quality control check was undertaken. Each week the fieldwork supervisor visited 10% of respondents, and a further 10% were mailed a **self-completion** questionnaire which asked whether the interview had been conducted in a satisfactory manner.

The type of local authority dwelling ensured that only one household lived at each address. A "household" was defined as "people who are catered for by the same adult(s) and share the same meals." An individual over the age of 18 years was selected for interview by a random procedure, which involved the interviewer listing, in alphabetical order, the first names of household members. The individual for interview was selected on the basis of a pre-assigned random number between one and nine, depending on the number of persons living in the household. The initial cross-sectional target samples can therefore be considered as representative of people living in the areas.

In the before survey, interviewers were instructed to make unlimited call-backs to contact the selected individual and no substitution was allowed, in the after survey, interviewers were instructed to contact the same individual from the same household. After six call-backs, another member of the household could be selected for interview, using the same randomized procedures described above. New tenants who had moved in were interviewed in the after survey, but no attempt was made to trace individuals who had moved from one address on the estate to another.

Victimization surveys have many limitations. Respondents may experience memory decay, especially in relation to less important events which have occurred within the previous twelve months. "Telescoping" is also a possible distorting factor, in that respondents may recall events from outside the twelve-month period as occurring within it. However, the comparison of experimental and control areas, and before and after surveys, largely controls for these kinds of measurement limitations, which should be similar in all surveys.

Results

COMPARABILITY OF EXPERIMENTAL AND CONTROL AREAS IN THE BEFORE SURVEYS

Table I shows the extent to which the experimental and control areas were comparable in the before surveys. For example, 64.0% of experimental respondents were female, compared with 65.6% of control respondents, a non-significant difference on the 2x2 chi-squared test. Experimental and control respondents were overwhelmingly white, and most had lived on their estate for 20 or more years. However, more of the control respondents were aged 60 or over.

TABLE 1
COMPARABILITY OF EXPERIMENTAL AND CONTROL AREAS BEFORE

Variable	Experimental %(N=431)	Control %(N=448)	Chi- Squared
<i>Demographics</i>			
Female respondent.....	64.0	65.6	ns
White respondent.....	97.4	97.8	ns
Age 18-44 respondent.....	47.8	45.3	ns
Age 60+ respondent.....	25.3	35.0	**9.45
Age 13-17 in house.....	18.1	15.4	ns
Age over 60 in house.....	31.1	39.5	*6.45
On estate 20+ years.....	55.2	56.0	ns
Not employed.....	70.7	67.2	ns
Vehicle owner.....	43.2	46.6	ns
<i>Area Cohesion</i>			
Satisfied with estate.....	63.3	68.5	ns
Estate is friendly.....	88.7	90.9	ns
Know most neighbours.....	76.5	80.3	ns
Neighbours keep watch.....	60.7	65.2	ns
<i>Area Lighting</i>			
Estate well lit.....	51.4	55.0	ns
Lighting adequate.....	49.9	50.6	ns
Lighting bright.....	46.9	43.8	ns
<i>Fear of Crime</i>			
Crime is a problem.....	78.4	75.8	ns
Lot of crime on estate.....	70.5	63.8	*4.16
Estate safe after dark.....	45.2	45.2	ns
Won't go out alone if dark.....	43.2	46.6	ns
Risks for women after dark.....	69.6	68.3	ns
Risks for men after dark.....	29.7	36.8	*4.64
Feel unsafe in own home.....	28.6	34.8	ns
Experiment estate crime worse.....	17.9	48.4	**90.87
Control estate crime worse.....	9.7	2.7	**17.82
<i>Crime Prevalence</i>			
Burglary.....	11.6	8.3	ns
Outside theft/vandalism.....	20.9	19.4	ns
Vehicle crime.....	15.1	13.6	ns
Property crime.....	37.8	34.6	ns
Personal crime.....	13.5	8.9	*4.10
All crime.....	42.0	39.1	ns
% Committed in dark.....	69.2	73.1	ns
% Reported to police.....	49.7	51.6	ns
Saw police on estate last month.....	17.4	27.5	*12.04

Notes: * $p < .05$, ** $p < .01$ (two-tailed), ns = not significant.

For committed in dark, N=250 crimes (experimental), 193 crimes (control).

For reported to police, N=300 crimes (experimental), 246 crimes (control).

Experimental and control respondents were equally likely to say that they were satisfied with their estate, and about 90% thought that their estate was friendly. They did not differ in their opinion of the street lighting on their estate. More experimental respondents thought that there was a lot or a fair amount of crime on their estate, but more control respondents thought that there were risks for men after dark on their estate. However, experimental and control respondents were equally likely to say that crime was a problem on their estate, that their estate was safe after dark, or that they would not go out alone after dark. Control respondents thought that crime was worse on the experimental estate, while experimental respondents were significantly more likely to say that crime was worse on the control estate.

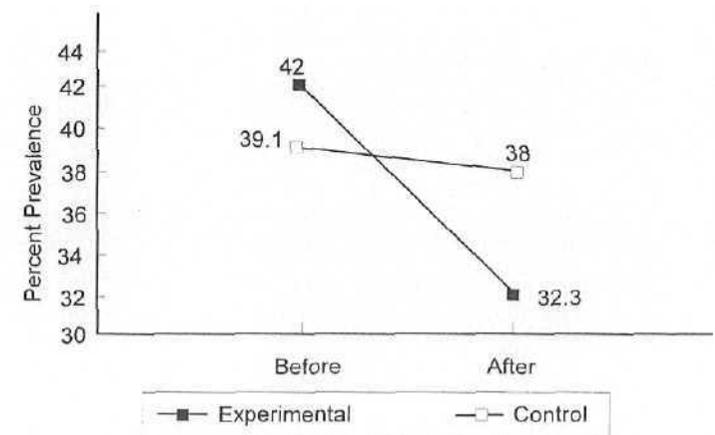
Crimes were divided into four types:

- burglary (including attempts);
- theft outside the home, vandalism of the home or bicycle theft;
- theft of or from vehicles or damage to vehicles; and
- personal crime: robbery, snatch theft, assault, threatening behavior, sexual assault or sexual pestering.

Categories (a), (b) and (c) together constitute property crime. Table 1 shows that the experimental and control estates were comparable on the prevalence of property crime, but there was a marginally significant ($p = .043$) tendency for the prevalence of personal crime to be higher on the experimental estate. About 70% of crimes on both estates were committed in the dark, and about half were reported to the police according to victims. One significant difference between the estates was that the control respondents were more likely to say that they had seen a police officer on the estate in the previous month.

On most variables, the experimental and control areas seemed closely comparable. If anything, however, the experimental area seemed slightly worse on crime.

FIGURE 1
PREVALENCE OF ALL CRIME



CHANGES IN THE PREVALENCE OF CRIME

Figure 1 shows that for all crime in the **experimental** area, prevalence decreased by 23% after the improved street lighting compared with before (from 42.0% victimized to 32.3%). In the control area, prevalence decreased by only 3% (39.1% victimized to 38.0%).¹ The changes in the experimental area were significantly greater than the changes in the control area for burglary, personal crime and all crime.

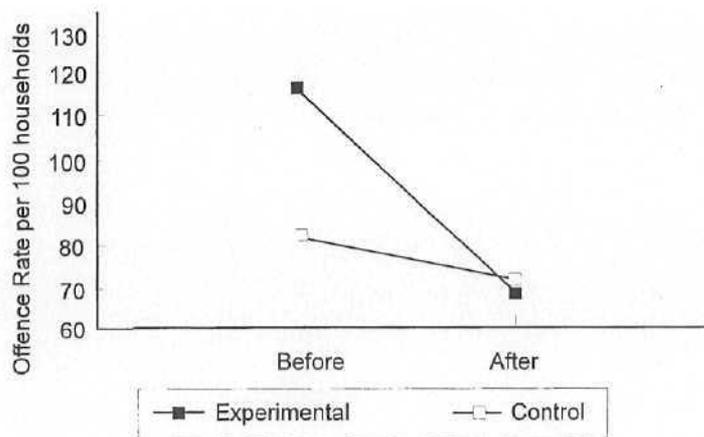
One problem of interpretation centers on the non-equivalence of the experimental and control areas before the intervention. The most important differences were that more of the control sample were aged 60 or over and more of the control sample said that they had seen a police officer on their estate in the previous month. Combining experimental and control samples before and after, persons aged 60 or over were significantly less likely than others to be victimized (for all crime, the prevalence was 23.5% of those aged 60 or over and 44.4% of the remainder: chi-squared = 63.02, $p < 0.001$). However, seeing a police officer on the estate in the previous month was not significantly related to the prevalence of victimization (3y.6% as opposed to 37.3%: chi-squared = 0.64, not significant).

To investigate whether these differences between experimental and control samples before the intervention might have influenced the results, the logistic regression for all crimes was repeated, entering age and seeing a police officer in the equation first and the interaction term last. However, the interaction term was unchanged (likelihood ratio chi-squared = 3.29, $p = .035$). Hence, the greater decrease in prevalence in the experimental area held independently of the prior non-equivalence of the experimental and control samples.

CHANGES IN THE INCIDENCE OF CRIME

Figure 2 shows changes in the incidence of crime (the average number of victimizations per 100 households, allowing a maximum of 10 per household) in the experimental and control areas. For all crime in the experimental area, incidence decreased by 41% after

FIGURE 2
INCIDENCE OF ALL CRIME



the improved **street** lighting compared with before (from 114.8 crimes per 100 households to 68.0). In the control area, incidence decreased by 15% (from 82.1 crimes per 100 households to 69.8). There was a highly significant decrease in the incidence of all crime in the experimental area after the improved lighting compared with before ($t = 3.73$, $p = .0001$). The decrease in the experimental area was significantly greater than in the control area, both according to an Ordinary Least Squares (OLS) multiple regression analysis (F change = 4.69, $p = .015$) and according to a Poisson regression (likelihood ratio chi-squared = 10.42, $p = .001$).

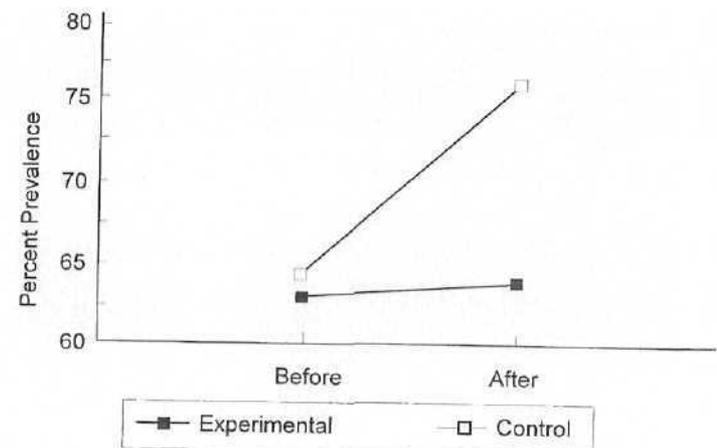
In the experimental area, there was a substantial and significant decrease in the incidence of all categories of crime after the improved street lighting. There were no significant changes in the control area. The changes in the experimental area were significantly greater than the changes in the control area for vehicle crime, properly crime, personal crime and all crime.

In order to investigate the effect of prior non-equivalence of the experimental and control samples on the results, the OLS multiple regression equation for all crime was repeated, entering age and seeing a police officer first and the interaction term last. The interaction term was still significant (F change = 4.28, $p = .019$), **showing** that the greater decrease in incidence in the experimental area held independently of this prior non-equivalence.

CHANGES IN THE PREVALENCE OF KNOWN VICTIMS

An additional, indirect measure of crime is a resident's personal knowledge of the victimization of other residents living on the estate (Foster and Hope, 1993). Hence, respondents were asked whether they, personally, knew anyone else from their estate who had experienced a number of specified crimes in the previous twelve months. Figure 3 shows changes in the prevalence of known victimization in the experimental and control

FIGURE 3
PREVALENCE OF KNOWN VICTIMS



areas. For all crime in the experimental **area**, there was no change in the percentage of respondents who said that they personally knew a victim (62.9% before, 63.7% after). However, in the control area, significantly more respondents said that they knew a victim after compared with before (76.0% compared with 64.3%; $Z=3.70$, $p=.0101$). The change in the control area was significantly different from the change in the experimental area ($Z=2.34$, $p=.010$).

With only one exception (personal crime), the prevalence of respondents who personally knew a victim significantly increased for all types of crimes in the control area. Similarly, with only one exception (vandalism of the home, which decreased), the prevalence of respondents did not change significantly for all types of crimes in the experimental area. With the exception of all crime, prevalence decreased in the experimental area and increased in the control area. As before, the changes in the experimental area were in a significantly more desirable direction than in the control area, even though changes in the prevalence of known victims were somewhat different from changes in (the prevalence and incidence of crime).

OTHER CHANGES ON THE ESTATES

Table 2, modeled on Table 1, shows differences between the estates after the improved street lighting in the experimental estate. The experimental sample were somewhat more satisfied with their estate after the intervention (65.6% versus 59.6%), but not significantly so. However, they had been somewhat less satisfied with their estate before the intervention (63.3% versus 68.5%; see Table 1). On the difference of difference of proportions test, the change in the experimental sample was significantly more favorable than the change in the control sample ($2=2.35$, $p=.009$). Therefore, it can be concluded that, compared with the control sample, the experimental sample became significantly more happy with their estate after (he improved street lighting. The vast majority of the experimental sample clearly noticed the improved street lighting. For example, just over 80% said that their estate was well lit.

On some measures, fear of crime was less in the experimental area than in the control area. The experimental sample were significantly less likely to say that crime was a problem or that there were risks for women going out after dark, and significantly more likely to say that their estate was safe after dark and that the quality of life had improved.

The crimes that were committed were just as likely to be committed after dark in the experimental area as in the control area, both before and after the improved street lighting. There was some tendency for the probability of reporting crimes to the police to be higher in the experimental area after the improved street lighting than in the control area, but none of the statistical tests were significant. Respondents on the experimental estate were significantly more likely to say that they had seen a police officer on the estate in the previous four weeks after the improved street lighting than those on the control estate, reversing the result shown in Table 1. However, as already explained, this difference did not affect the major results. Finally, control respondents were significantly more likely to say that burglary, vandalism and vehicle crime had got worse on their estate during the previous twelve months.

Statements from respondents in the experimental area suggested that they thought that the improved street lighting had decreased crime rates and improved their quality of life, especially because the improved illumination led to increased surveillance:

TABLE 2
DIFFERENCES BETWEEN EXPERIMENTAL AND CONTROL AREAS AFTER

Variable	Experimental %(N=372)	Control %(N=372)	Chi- squared
<i>Area Cohesion</i>			
Satisfied with estate.....	65.6	59.6	ns
Estate is friendly.....	88.5	87.6	ns
<i>Area Lighting</i>			
Estate well lit.....	83.2	44.7	**117.18
Lighting worse.....	9.9	48.8	**133.30
Lighting better.....	65.1	2.4	**322.89
<i>Fear of Crime</i>			
Crime is a problem.....	73.7	85.2	**14.38
Lot of crime on estate.....	62.1	66.0	ns
Estate safe after dark.....	45.9	35.1	**8.40
Won't go out alone if dark.....	54.3	60.4	ns
Risks for women after dark.....	66.9	76.0	**7.06
Risks for men after dark.....	28.0	34.2	ns
Feel unsafe in own home.....	24.5	26.1	ns
Experimental estate crime worse.....	17.7	39.6	**42.42
Control estate crime wor.se.....	12.4	6.2	**7.67
<i>Crime</i>			
% Committed in dark.....	68.8	70.8	ns
% Reported to police.....	50.6	46.4	ns
Saw police on estate last month.....	38.2	30.7	*4.23
Burglary worse.....	40.9	56.9	**18.43
Vandalism worse.....	40.1	49.1	*5.74
Vehicle crime worse.....	41.9	50.4	*5.03

* $p<.05$, ** $p<.01$ (two-tailed).

ns = not significant.

For committed in dark, N=144 crimes (experimental), 154 crimes (control).

For reported to police, N=164 crimes (experimental), 179 crimes (control).

Last year I told you [the interviewer] that my greenhouse [at the side, towards the back of the house] had been smashed twice at night and I was really frightened because the kids used to come into the garden and knock on my door and windows. I sleep on the ground floor since my husband died last year and I was scared to death in case they came in. It's all stopped now. They know they can be seen and my neighbor across the road has been out to them when they've got noisy.

The lighting is so much belter. It's champion up here now. We never feel frightened here at all now. We've got one just outside our window. It's great. There are no shadows or dark patches.

The street lighting has improved. It's 100% better. Bound to feel safer because you can see who's about. It's lighter on the roads and in the gardens. These lights spread the light all around. Feel so much better.

The villains have gone. They don't like lights when they're up to no good. Everything is brighter and quieter. There are less places for people to hide with it being so light.

You can see things so much more clearly. It puts the villains off. You can recognize people in the street now. Before you could not. There are a lot less burglaries.

Things have really improved on this estate with regard to crime since the lights were put in. You can't half see a long way with this new lighting.

The lighting has brought more people on to the streets at night. Especially young girls. It's so much brighter and you can see people. Before you could not see people at all. It's cut out all the shadows in the street. It even shows up the gardens and you can see better in previously dark alleyways.

Pedestrian street use increased in the experimental area after the improved street lighting. The number of pedestrians was counted in two streets in each area on Thursday and Saturday evenings between 7:00 p.m. and 10:00 p.m. during the first week of March in 1992 and 1993. In both years the weather conditions were similar (cold but dry). The number of male pedestrians increased by 22.1% (from 515 to 629) in the experimental area and by 15.0% (from 347 to 399) in the control area, a non-significant difference (chi-squared = 0.35, not significant). However, the number of female pedestrians increased by 27.7% (from 386 to 493) in the experimental area but decreased by 21.2% (from 312 to 246) in the control area, a highly significant difference (chi-squared = 19.20, $p < .0001$). Hence, it is plausible that the improved street **lighting** encouraged more women to use the streets after dark.

Unfortunately, it was not possible to compare changes in police-recorded crimes in the experimental and control areas before and after, because of changes in recording procedures and inadequacies of available data.

Summary and Conclusions

In the Dudley project, improved street lighting was followed by significant decreases in the prevalence and incidence of crime in the experimental area compared with the control area. The experimental sample noticed that the lighting had improved, became more satisfied with their estate, and had less fear of crime. After the improved lighting, the experimental sample were less likely to say that there were risks for women after dark, and there was a significant increase in the number of female pedestrians on the street after dark in the experimental area. However, crimes were no less likely to be committed after dark compared with during the daylight and no more likely to be reported to the police after the improved street lighting.

Most threats to internal validity are controlled by the experimental-control, before-after design. For example, mortality (the loss of subjects from the pre-test to the post-test) was the same in the experimental and control areas and therefore could not account for differential changes in reported crime between the areas. The major uncontrolled threats

were selection (the control sample were older) and history (apparently, police were seen more on the control estate before the intervention and more on the experimental estate after). However, the greater decrease in the prevalence and incidence of crime in the experimental estate held after controlling statistically for both these factors.

Extensive monitoring of other factors which might have contributed to the decreases in crime was undertaken through the questionnaire, interviewer **fieldwork** sheets and liaison with other agencies including the police and housing personnel. There were no changes to policing strategies or to Neighborhood Watch or to the history of the survey areas in any way which might account for the differential changes in crime.

The evaluation was designed to look for evidence of spatial, temporal and target displacement. The study found no evidence that any of these types of displacement occurred. Crime decreased in both estates, but decreased significantly more in the experimental estate. Also there was no evidence of displacement of crime from night to day. Of course, it is not possible to state conclusively that displacement **did** not take place because the phenomenon is so complex and can take so many different forms that no single research study can adequately encompass all possibilities (Barr and Pease, 1990; Ekblom and Pease, 1995). Notwithstanding this caveat, the findings from this study are **entirely** consistent with a growing body of research which shows that displacement is only a possible, not inevitable, outcome of effective crime prevention strategies. A collection of 22 studies presented by Clarke (1992) provided many examples of genuine reductions in crime with little or no displacement. Similarly, a review of 55 crime prevention projects, which specifically looked for evidence of displacement, found that in 22 cases no displacement occurred and that in 6 of these studies there was evidence of diffusion of benefits. Of the remaining 33 studies, some displacement was observed but it was limited in amount and scope (Hesseling, 1994).

An understanding as to why displacement did not occur in this study can be gleaned from a parallel, pre-post test household survey of 350 young people, aged 11-17 years carried out in the experimental and control areas. Results revealed that one reason crime was not displaced was because young people were not **deflected** from the re-lit area by the improved street lighting. On the contrary, they preferred to remain in the re-lit **area**; on both the adult and young people's surveys, respondents reported a highly significant increase in the number of young people in the re-lit **area** after dark ($p < .0001$). In theory, this could have led to an increase in crime, in practice it did not — presumably because the improved lighting not only made the re-lit areas more attractive to young people, but also increased the perceived risks of offending through enhanced natural surveillance of the streets.

The most plausible conclusion from this research is that the improved street lighting was responsible for the decrease in crime. Further research is needed to investigate how far these results can be replicated in different types of areas (external validity); it may be that improved lighting works better within certain contexts or "boundary conditions." Another key issue is the "dose-response" curve relating street lighting and crime; it may be that improved street lighting decreased crime in Dudley because the improvement was so dramatic. Further research is also needed to advance knowledge about the intervening processes, and interviews with potential offenders may be especially informative.

Our hypothesis is **that** more eyes and ears on the street and community improvements led residents to be more confident and optimistic and less afraid of crime. In turn, the increased community pride tended to exert increased informal social control on potential

offenders, inhibiting them from committing crimes both day and night. Hence, improved street lighting was the catalyst which physically altered the built environment and brought about changes in resident and offender behavior which reduced crime and improved the quality of life on the experimental estate.

It is not impossible that alternative physical interventions, for example closed circuit television or increased locks and bolts, might have achieved a similar effect on crime as did street lighting improvements. However, street lighting has some advantages over other situational measures which have been associated with the creeping privatization of public space, the exclusion of sections of the population and the move towards a "fortress" society (Bottoms, 1990). Street lighting benefits the whole neighborhood rather than particular individuals or households. It is not a physical barrier to crime, it has no adverse civil liberties implications and, as has been demonstrated, it can increase public safety and effective use of neighborhood streets at night. In short, improved street lighting has no negative effects and has demonstrated benefits for law-abiding citizens.

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Note

1. The difference of proportions test was used to investigate whether changes in prevalence were significant. For example, there was a significant decrease in the prevalence of all crime in the experimental area after the improved lighting compared with before ($Z=2.87$, $p=.002$). Two methods were used to contrast the change in prevalence of the experimental sample with the change in prevalence of the control sample. The difference of difference of proportions test produced a significant Z value for all crime of 1.80 ($p=.036$). The interaction term in a logistic regression equation produced a likelihood ratio chi-squared value of 3.29 ($p=.035$). Hence, both tests showed that the decrease in all crime in the experimental area was significantly greater than in the control area.