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Effects of Improved Street Lighting on Crime

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Cover sheet

Title

Effects of Improved Street Lighting on Crime

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Text of Review

Synopsis

Improved street lighting serves many functions and is used in both public and private settings. The prevention of personal and property crime is one of its objectives in public space, which is the main focus of this review. There are two main theories of why improved street lighting may cause a reduction in crime. The first suggests that improved lighting leads to increased surveillance of potential offenders (both by improving visibility and by increasing the number of people on the street) and hence to increased deterrence of potential offenders. The second suggests that improved lighting signals community investment in the area and that the area is improving, leading to increased community pride, community cohesiveness, and informal social control. The first theory predicts decreases in crime especially during the hours of darkness, while the second theory predicts decreases in crime during both daytime and nighttime. Results of this review indicate that improved street lighting significantly reduces crime. This lends support for the continued use of improved street lighting to prevent crime in public space. The review also found that nighttime crimes did not decrease more than daytime crimes. This suggests that a theory of street lighting focusing on its role in increasing community pride and informal social control may be more plausible than a theory focusing on increased surveillance and increased deterrence. Future research should be designed to test the main theories of the effects of improved street lighting more explicitly, and future lighting schemes should employ high quality evaluation designs with long-term follow-ups.

Abstract

Background

Improved street lighting is intended to serve many purposes, one of them being the prevention of crime. While street lighting improvements may not often be implemented with the expressed aim of preventing crime – pedestrian safety and traffic safety may be viewed as more important aims – and the notion of lighting streets to deter lurking criminals may be too simplistic, its relevance to the prevention of crime has been suggested in urban centers, residential areas, and other places frequented by criminals and potential victims.

Objectives

The main objective of this review is to assess the available research evidence on the effects of improved street lighting on crime in public space. In addition to assessing the overall impact of improved street lighting on crime, this review will also investigate in which settings, against which crimes, and under what conditions it is most effective.

Search strategy

Four search strategies were employed to identify studies meeting the criteria for inclusion in this review: (1) searches of electronic bibliographic databases; (2) searches of literature reviews on the effectiveness of improved street lighting in preventing crime; (3) searches of bibliographies of street lighting studies; and (4) contacts with leading researchers. Both published and unpublished reports were considered in the searches. Searches were international in scope and were not limited to the English language.

Selection criteria

Studies that investigated the effects of improved street lighting on crime were included. For studies involving one or more other interventions, only those studies in which improved street lighting was the main intervention were included. Studies were included if they had, at a minimum, an evaluation design that involved before-and-after measures of crime in experimental and control areas. There needed to be at least one experimental area and one reasonably comparable control area.

Data collection & analysis

Narrative findings are reported for the 13 studies included in this review. A meta-analysis of all 13 of these studies was carried out. The “relative effect size” or RES (which can be interpreted as an incident rate ratio) was used to measure effect size. Results are reported for total crime and, where possible, property and violent crime categories using (mostly) official data. In the case of studies that measure the impact of improved street lighting programs on crime at multiple points in time, similar time periods before and after are compared (as far as possible). The review also addresses displacement of crime and diffusion of crime prevention benefits.

Main results

The studies included in this systematic review indicate that improved street lighting significantly reduces crime, is more effective in reducing crime in the United Kingdom than in the United States, and that nighttime crimes do not decrease more than daytime crimes.

Reviewers’ conclusions

We conclude that improved street lighting should continue to be used to prevent crime in public areas. It has few negative effects and clear benefits for law-abiding citizens.

Background

Improved street lighting is intended to serve many purposes, one of them being the prevention of crime. While street lighting improvements may not often be implemented with the expressed aim of preventing crime – pedestrian safety and traffic safety may be viewed as more important aims – and the notion of lighting streets to deter lurking criminals may be too simplistic, its relevance to the prevention of crime has been

suggested in urban centers, residential areas, and other places frequented by criminals and potential victims.

Explanations of the way street lighting improvements could prevent crime can be grouped into two main perspectives:

1. As a situational crime prevention measure that focuses on reducing opportunity and increasing perceived risk through modification of the physical environment (Clarke 1995), such as Crime Prevention Through Environmental Design (Jeffery 1977).
2. As a method of strengthening informal social control and community cohesion through more effective street use (Angel 1968; Jacobs 1961) and investment in neighborhood conditions (Taub 1984; Taylor 1986).

The situational approach to crime prevention suggests that crime can be prevented by environmental measures, which directly affect offenders' perceptions of increased risks and decreased rewards. This approach is also supported by theories, which emphasize natural, informal surveillance as a key to crime prevention. For example, Jacobs (1961) drew attention to the role of good visibility combined with natural surveillance as a deterrent to crime. She emphasized the association between levels of crime and public street use, suggesting that less crime would be committed in areas with an abundance of potential witnesses.

Other theoretical perspectives have emphasized the importance of investment to improve neighborhood conditions as a means of strengthening community confidence, cohesion, and social control (Kelling 1996; Skogan 1990; Wilson 1982). Sampson (1997) argued that a low degree of "collective efficacy" in a neighborhood (a low degree of informal social control) caused high crime rates. As a highly visible sign of positive investment, improved street lighting might reduce crime if it physically improved the environment and signaled to residents that efforts were being made to invest in and improve their neighborhood. In turn, this might lead them to have a more positive image of the area and to have increased community pride, optimism, and cohesion. It should be noted that this theoretical perspective predicts a reduction in both daytime and nighttime crime. Consequently, attempts to measure the effects of improved lighting should not concentrate purely on nighttime crime.

The relationship among 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. Street lighting is a tangible alteration of the built environment, but it does not constitute a physical barrier to crime. However, it can act as a catalyst to stimulate crime reduction through a change in the perceptions, attitudes, and behavior of residents and potential offenders.

It is also feasible that improved street lighting could, in certain circumstances, increase opportunities for crime. It may bring greater numbers of potential victims and potential offenders into the same physical space. Increased visibility of potential victims may allow better judgments of their vulnerability and attractiveness (e.g., in terms of valuables). Increased social activity outside the home may increase the number of unoccupied homes available for burglary. Increased illumination may make it easier to commit crimes and to escape.

The effects of improved street lighting are likely to vary in different conditions. In particular, they are likely to be greater if the existing lighting is poor and if the improvement in lighting is considerable. They may vary according to characteristics of the area or the residents, the design of the area, the design of the lighting, and the places that are illuminated. For example, improved lighting may increase community confidence only in relatively stable homogeneous communities, not in areas with a heterogeneous population mix and high residential mobility. The effects of improved lighting may also interact with other environmental improvements, such as closed circuit television (CCTV) cameras or security patrols.

Contemporary interest in the effect of improved street lighting on crime began in the U.S. during the dramatic rise in crime 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 1974). This proliferation of projects led to a detailed review of the effects of street lighting on crime by Tien (1979), as part of the National Evaluation Program of Law Enforcement Assistance Agency (LEAA) funding. Their report described 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, Tien (1979) found that the results were mixed and generally inconclusive. 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); and insufficient appreciation of the impact of lighting on different types of crime.

The review by Tien (1979) should have led to attempts to evaluate the effects of improved street lighting using more adequate designs and alternative measures of crime, such as victim surveys, self-reports, or systematic observation. It should also have stimulated efforts to determine in what circumstances improved street lighting might lead to reductions in crime. Unfortunately, it was interpreted as showing that street lighting had no effect on crime and effectively ended research on the topic in the U.S.

In the U.K., very little research was carried out on street lighting and crime until the late 1980s (Fleming 1986). There was a resurgence of interest between 1988 and 1990, when three small-scale street lighting projects were implemented and evaluated in different areas of London (Painter 1994). In each location crime, disorder, and fear of crime declined and pedestrian street use increased dramatically after the lighting improvements.

In contrast to these generally positive results, a major British Home Office-funded evaluation in Wandsworth (Atkins 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 1991:24). However, as further evidence has accumulated, there have been more signs that improved street lighting could have an effect in reducing crime. In a recent narrative review by Pease (1999), he considered that “the capacity of street lighting to influence crime has now been satisfactorily settled” (68). He also recommended that the debate should be moved from the sterile “does it work or doesn’t it?” to the more productive “how can I flexibly and imaginatively incorporate lighting in crime reduction strategy and tactics?” (72).

Objectives

The main objective of this review is to assess the available research evidence on the effects of improved street lighting on crime in public space. In addition to assessing the overall impact of improved street lighting on crime, this review will also investigate in which settings, against which crimes, and under what conditions it is most effective.

Criteria for considering studies for this review

Types of studies

Studies were included if they had, at a minimum, an evaluation design that involved before-and-after measures of crime in experimental and control areas. There needed to be at least one experimental area and one reasonably comparable control area. The unit of interest is the area.

Types of interventions

Improved street lighting (or improved lighting) is the focus of the intervention. For studies involving one or more other interventions, only those studies in which street lighting was the main intervention were included. The determination of the main intervention was based on the study author identifying it as such or, if the author did not do this, the importance of street lighting relative to the other interventions.

Types of outcome measures

Studies had to include at least one outcome measure of crime. Where applicable, crime outcome data is reported separately for two main categories: official records (police reports) and unofficial measures (victim survey or self-report survey).

The total number of crimes in each area before the intervention needed to be at least 20. The main measure of effect size (see below) is based on changes in numbers of crimes between the before and after time periods. A minimum of 20 crimes in the before period was set because it was considered that a measure of change based on an *N* below 20 was

potentially misleading. Also, any study with less than 20 crimes before would have insufficient statistical power to detect changes in crime. (The criterion of 20 is probably too low, but we are reluctant to exclude studies unless their numbers are clearly inadequate.)

Search strategy for identification of studies

Four search strategies were employed to identify studies meeting the criteria for inclusion in this review: (1) searches of electronic bibliographic databases; (2) searches of literature reviews on the effectiveness of improved street lighting in preventing crime; (3) searches of bibliographies of street lighting studies; and (4) contacts with leading researchers.

Both published and unpublished reports were considered in the searches. Searches were international in scope and were not limited to the English language.

The search strategies were carried out in two waves. In the first wave, search strategies (1) to (4) were completed in January 2001 and reflect material published or known up to December 31, 2000. In the second wave, search strategies (1) to (4) were completed in March 2007 and reflect material published or known between January 2001 and December 2006.

In the first wave, the following electronic bibliographic databases were searched:

- Criminal Justice Abstracts
- NCJRS (National Criminal Justice Reference Service) Abstracts
- Sociological Abstracts
- SocialSciAbs (Social Science Abstracts)
- ERIC (Educational Resources Information Clearinghouse)
- GPO Monthly (Government Printing Office Monthly)
- PsychInfo (Psychology Information)
- PAIS International (Public Affairs Information Service)
- Dissertation Abstracts
- CINCH (Australian Criminology Database)
- C2-SPECTR (Campbell Collaboration Social, Psychological, Educational & Criminological Trials Register)

In the second wave, the following electronic bibliographic databases were searched:

- Criminal Justice Abstracts
- NCJRS (National Criminal Justice Reference Service) Abstracts
- Sociological Abstracts
- ERIC (Educational Resources Information Clearinghouse)
- GPO Monthly (Government Printing Office Monthly)
- PsychInfo (Psychology Information)
- Dissertation Abstracts

C2-SPECTR (Campbell Collaboration Social, Psychological, Educational & Criminological Trials Register)

Google Scholar

Medline

In the second wave, three databases, Social Science Abstracts (SocialSciAbs), Public Affairs Information Service (PAIS) International, and the Australian Criminology Database (CINCH), which were used in the first wave, were not used because they were no longer available to the researchers. In their place, two new electronic databases were searched: Google Scholar and Medline.

In both waves the following terms were used to search the databases: 'street lighting', 'lighting', 'illumination', and 'natural surveillance'. When applicable, 'crime' was added to each of these terms (e.g., 'street lighting and crime') to narrow the search parameters.

The following literature reviews on the effectiveness of improved street lighting in preventing crime were consulted: Beyer (2005), Cozens (2003), Eck (1997; 2002), Fleming (1986), Painter (1996), Pease (1999), Poyner (1993), Ramsay (1991), and Tien (1979).

The exhaustive review by Tien (1979) identified 103 street lighting projects carried out in the 1970s but only considered that 15 (listed on pp. 51-54) met their minimum methodological standards. Attempts were made to obtain 11 of these 15 evaluation reports that had an experimental-control comparison. For the other four studies (conducted in Baltimore; Chicago; Richmond, Virginia; and Washington, DC), Tien (1979) could not determine from the report that there was any kind of experimental-control comparison. Hence, attempts were not made to obtain and screen every possible study on street lighting and crime reported in Tien (1979), only studies that conceivably might meet our criteria for inclusion.

Method of the review

SELECTION OF EVALUATION STUDIES

The search strategies resulted in the collection of 13 improved street lighting evaluations that met the criteria for inclusion in this review. Another 19 street lighting evaluations were identified that did not meet the inclusion criteria and thus were excluded. Another group of identified street lighting evaluations (4 in total), which may or may not meet the criteria for inclusion, could not be obtained. Repeated attempts were made to obtain these studies.

These four evaluations were identified by Tien (1979): Department of Public Works (1976); Denver Anti-Crime Council (1977); Newark High Impact Evaluation Staff (1975); and Tucson Department of Human and Community Development (1971). It is not known if these reports would meet the inclusion criteria. According to Tien (1979), the control area was the city for the Miami and Newark projects, so they would likely be

excluded. However, according to Tien (1979), Denver had an adjacent control area and Tucson had randomly selected experimental and control areas, so they might be evaluation projects that could be included.

Table 1 lists the 19 evaluations that did not meet the criteria for inclusion in this review, summarizes their key features, and identifies the reasons for exclusion. The reasons for discussing these evaluations here are two-fold: first, it conforms with the widely-held practice in systematic reviews of listing excluded studies and second, it allows readers to judge for themselves the strength of observed effects in excluded evaluations compared with those included.

As shown in Table 1, 14 evaluations were excluded because they did not include a comparable control area in assessing the impact of the improved street lighting intervention. Three other evaluations were excluded because they did not include crime as an outcome measure, and two were excluded for both of these reasons. Three of the 19 excluded evaluations included other interventions, making it difficult to disentangle the effects of the improved street lighting from the effects of other interventions. Most suggested that improved street lighting was followed by a decrease in crime, but the low level of internal validity of these studies (together with other methodological problems) means that we cannot have confidence in their results.

ASSESSMENT OF METHODOLOGICAL QUALITY

For each study, we assessed methodological quality against one main characteristic: the presence of a reasonably comparable control area. In addition, the study had to report the number of crimes before and after in experimental and control areas.

DATA SYNTHESIS

The following characteristics of the 13 included studies were retrieved and retained for examination as potential moderators of study outcomes and are listed in tables of included studies (Tables 2 and 3):

- 1) author, publication date, and location: the authors and dates of relevant evaluation reports and the location of the program
- 2) context of intervention: this is defined as the physical setting in which the improved street lighting intervention took place
- 3) lighting: the degree of improvement in brightness
- 4) duration of intervention: the length of time the program was in operation, when reported
- 5) sample size: the number and any special features of the experimental and control areas
- 6) other interventions: interventions other than street lighting that were employed at the time of the program
- 7) outcome measure and data source: crime is the main outcome measure of interest to the review. The specific crime types, the data source of the outcome measure (e.g. police records, victim survey), and the measurement of crimes during daytime and/or nighttime are identified. Other (secondary) outcomes are also examined if reported

8) research design: the type of evaluation design used to assess the program's impact on crime is identified. If matching or other statistical analysis techniques are used as part of the evaluation of program effects, these are noted

9) before-after time period: the before and after time periods of the evaluation

As noted above, the main outcome measure of interest to this review is crime, specifically, property (e.g. burglary, theft of vehicles) and violent (e.g. assault, robbery) crimes. In summarizing results, the focus is on the main outcome of interest to this review and comparisons between experimental and control areas (see below for more details). Results are reported for total crime and, where possible, property and violent crime categories. In the case of studies that measure the impact of improved street lighting programs on crime at multiple points in time, similar time periods before and after (e.g. 12 months) are compared (as far as possible).

The review also reports on displacement of crime and diffusion of crime prevention benefits. Displacement is often defined as the unintended increase in crimes in other locations following from the introduction of a crime reduction scheme. Six different forms of displacement have been identified: temporal (change in time), tactical (change in method), target (change in victim), territorial (change in place), functional (change in type of crime), and perpetrator (Repetto 1976; Barr 1990). Diffusion of benefits is often defined as the unintended decrease in crimes in other locations following from a crime reduction scheme, or the "complete reverse" of displacement (Clarke 1994). In order to investigate territorial displacement and diffusion of benefits, the minimum design involves one experimental area, one adjacent area, and one non-adjacent control area. If crime decreased in the experimental area, increased in the adjacent area, and stayed constant in the control area, this might be evidence of displacement. If crime decreased in the experimental and adjacent areas and stayed constant or increased in the control area, this might be evidence of diffusion of benefits.

DATA ANALYSIS

A meta-analysis is carried out in order to estimate the average effect size in evaluations of the effects of improved street lighting on crime. In order to complete a meta-analysis, a comparable effect size is needed in each evaluation, together with its variance. This has to be based on the number of crimes in experimental and control areas in time periods (most commonly of 12 months) before and after the intervention, because this is the only information that is regularly provided in all the evaluations.

While studies based on police records can present time series data, studies based on victim surveys usually have data only for one time period before the intervention and one time period after. Because of the problem that the intervention may cause more reporting to police and recording by police, it is important to analyze both police and victim survey data.

The "relative effect size" or RES (which can be interpreted as an incident rate ratio) is used to measure effect size. The RES is calculated from the following table:

	Before	After
Experimental	a	b
Control	c	d

Where a, b, c, d are numbers of crimes

$$RES = a*d/b*c$$

In calculating the weighted mean effect size for all or a subset of the studies, the effect size is inversely weighted according to the variance of each study, as specified in Lipsey and Wilson (Lipsey 2001). Also, in calculating an average effect size for all or a subset of the studies, statistical tests are carried out to assess if the individual effect sizes were randomly distributed around the average effect size (or if there is heterogeneity). Moderators that predict effect sizes are investigated (where available).

The RES is intuitively meaningful because it indicates the relative change in crimes in the control area compared with the experimental area. $RES = 2$ indicates that d/c (control after/control before) is twice as great as b/a (experimental after/experimental before). This value could be obtained, for example, if crimes doubled in the control area and stayed constant in the experimental area, or if crimes decreased by half in the experimental area and stayed constant in the control area, or in numerous other ways.

The variance of the OR is usually calculated from its natural logarithm LOR:

$$VAR(LOR) = 1/a + 1/b + 1/c + 1/d$$

In this review, we use LRES, the natural logarithm of RES, and refer to VAR (LRES). This calculation of VAR (LRES) is based on the assumption that crimes occur at random, according to a Poisson process. This assumption is plausible because 30 years of mathematical models of criminal careers have been dominated by the assumption that crimes can be accurately modeled by a Poisson process (Piquero 2003). In a Poisson process, the variance of the number of crimes is the same as the number of crimes. However, the large number of changing extraneous factors that influence the number of crimes may cause overdispersion; that is, where the variance of the number of crimes VAR exceeds the number of crimes N.

$$D = VAR/N$$

specifies the overdispersion factor. Where there is overdispersion, V(LRES) should be multiplied by D. Farrington (2007a) estimated VAR from monthly numbers of crimes and found the following equation:

$$D = .0008 \times N + 1.2$$

D increased linearly with N and was correlated .77 with N. The mean number of crimes in an area in the lighting studies was about 445, suggesting that the mean value of D was about 1.56. However, this is an overestimate because the monthly variance is inflated by seasonal variations, which do not apply to N and VAR. Nevertheless, in order to obtain a conservative estimate, V(LRES) calculated from the usual formula above was multiplied by D (calculated from the above equation) in all cases. Specifically,

$$V(LRES) = Va/a^2 + Vb/b^2 + Vc/c^2 + Vd/d^2$$

where $Va/a = .0008 \times a + 1.2$

This is our best available estimate of the degree of overdispersion in area-based crime prevention studies. This adjustment corrects for overdispersion within studies but not for heterogeneity between studies.

Description of studies

The 13 improved street lighting evaluations included in this review were carried out in two countries: the United States and the United Kingdom.

U.S. studies

Of the 13 improved street lighting evaluations included in this review, 8 were carried out in the U.S. For the most part, residential neighborhoods was the setting for the intervention. Only 4 of the 8 evaluations specified the degree of improvement in the lighting: by 7 times in Milwaukee, 4 times in Atlanta, 3 times in Fort Worth, and 2 times in Portland (see Table 2). However, the description of the lighting in other cases (e.g. “high intensity street lighting” in Harrisburg and New Orleans) suggests that there was a marked improvement in the degree of illumination. Only in Indianapolis was the improved street lighting confounded with another concurrent intervention, and it was sometimes possible to disentangle this.

The control area was often adjacent to the experimental area. Hence, similar decreases in crime in experimental and control areas could reflect diffusion of benefits rather than no effects of improved lighting. In most cases, the reports noted that the control area was similar to the experimental area in sociodemographic factors or crime rates. However, none of the evaluations attempted to control for prior noncomparability of experimental and control areas. Only one evaluation (Portland) included an adjacent area and a comparable non-adjacent control area.

The outcome measure of crime was always based on police records before and after the improved street lighting. The Indianapolis evaluation was based on calls for service to the police, many of which did not clearly involve crimes (e.g. calls for “disturbance”). Only the Atlanta and Milwaukee studies provided total, nighttime, and daytime crimes.

The Portland, Kansas City, Harrisburg, and New Orleans studies measured only nighttime crimes, and the Fort Worth and Indianapolis studies reported only total crimes.

U.K. studies

The 5 U.K. lighting studies were carried out in a variety of settings, including a parking garage and a market, as well as residential neighborhoods (see Table 3). The market study involved improved lighting rather than improved street lighting. Three of the evaluations specified the degree of improvement in lighting: by 5 times in Stoke-on-Trent and by 2 times in Bristol (approximately) and Dudley. Control areas were usually located close to experimental areas. Only one evaluation (Stoke-on-Trent) included an adjacent area and a comparable non-adjacent control area. The outcome measure of crime was based on police records for 3 studies and on victim surveys in the other 2 cases (in Dudley and Stoke-on-Trent). Uniquely, the Dudley project also evaluated the impact of improved street lighting using self-reported delinquency surveys of young people. This project also included self-reports of victimization of young people and measures of fear of crime (Painter 2001).

Methodological quality of included studies

ASSESSMENT OF METHODOLOGICAL QUALITY

1. Did the investigators report on the presence of a reasonably comparable control area?

In each evaluation study included in this review, the control area needed to be at minimum reasonably comparable to the area in which the intervention was implemented (experimental area). The term ‘reasonably’ is used because in some cases investigators did not provide sufficient detail to allow for a determination that the experimental and control areas were comparable on the most important dimensions (e.g. crime rates, age of population, unemployment rates, poverty rates), but there was enough information to conclude that the two areas were somewhat comparable (beyond the investigators saying so without providing data to support their assertion). In practice, we included similarly-sized areas as comparable and only excluded much larger areas (e.g. the rest of the city). As noted above, 14 evaluation studies were excluded because they did not employ the use of a reasonably comparable control area.

The control area could take the form of an adjacent or nonadjacent area, but ideally it would not be adjacent to the experimental area. This is because of the potential for program contamination, from the experimental area to the adjacent area. In the two studies that reported multiple control areas, the nonadjacent area was used. In a few of the evaluation studies, statistical analyses were used to equate the experimental and control areas.

Results

NARRATIVE FINDINGS FROM THE STUDIES

U.S. studies

As shown in Table 4, in 4 evaluations improved street lighting was considered to be effective in reducing crime (Atlanta, Milwaukee, Fort Worth, and – for violence – Kansas City). In the other 4 evaluations, the improved street lighting was considered to be ineffective (Portland, Harrisburg, New Orleans, and Indianapolis).

Improved street lighting was most clearly effective in reducing crimes in the Fort Worth evaluation. Crimes decreased by 21.5% in the experimental area and increased by 8.8% in the control area (Lewis 1979:75). Since crime in the whole city stayed constant (a decrease of 1.1%), it might be argued that some crime had been displaced from the experimental to the adjacent control area. In the experimental area, property crime decreased but violent crime did not. Information about types of crime was not provided for the control area, and information was not provided about nighttime as opposed to daytime crime.

Improved street lighting was followed by a decrease in robberies and burglaries in Atlanta, whereas the incidence of these crimes increased in the control area (Atlanta Regional Commission 1974:11-12). There was an increase in assaults in the experimental area, but the numbers were relatively small (from 11 to 57). Overall, daytime crime decreased by 16.4% in the experimental area after the improved lighting, in comparison with an increase of 33.3% in the control area. Nighttime crime increased considerably in both areas.

In Milwaukee, information about total crimes was only available for 7 months before and after the improved lighting. Impressively, crimes decreased by 5.6% in the experimental area and increased by 29.2% in the control area (Department of Intergovernmental Fiscal Liaison 1973:6). Similar results were obtained for nighttime crime (15.3% decrease in experimental area, 20.0% increase in control area). There was also a big effect on daytime crime (2.2% increase in experimental area, 37.0% increase in control area). However, the effects were much less in a later report (Department of Intergovernmental Fiscal Liaison 1974:3) on 12 months before and after for nighttime crimes only (5.9% decrease in experimental area, 1.7% decrease in control area).

In Kansas City, improved street lighting was effective in reducing nighttime violent crimes (robbery and assault) but not nighttime property crimes (larceny and motor vehicle theft). Violent crimes decreased by 51.9% in the experimental area, compared with 7.2% in the control area (Wright 1974:49). However, property crimes decreased more in the control area (32.0%) than in the experimental area (22.6%). These results were statistically significant for violent crimes but not for property crimes.

In Indianapolis, the results were difficult to interpret. When the dates of special police initiatives were excluded, crimes increased more in the experimental area than in the control area (Quinet 1998:759, 763; their experimental areas A and C are included in our analyses). Area A and Control Area A2 were followed up for 7 months before and after

the improved lighting, whereas Area C and Control Area C2 were followed up for 10 months before and after. The data on types of crimes, which did not exclude these dates, showed that violent crimes increased more in the control area (Quinet 1998:769, 773). However, it is difficult to draw any conclusions from the data on types of crimes, because improved lighting was confounded with special police initiatives.

In Portland, there was little evidence that improved street lighting had led to any reduction in nighttime crime. The analysis of this project was complicated by the fact that one set of experimental, adjacent and control areas was followed up for 11 months before and after, while another set was followed up for 6 months before and after (Inskeep 1974:10). In the interests of calculating a single effect size, the figures are combined in Table 4. In general, changes in crime in the experimental areas were similar to and not more desirable than changes in crime in the other areas.

Finally, in Harrisburg nighttime crime increased similarly in experimental and control areas (Harrisburg Police Department 1976:Tables 1 and 2); and in New Orleans nighttime crime decreased similarly in experimental and control areas (Sternhell 1977:13-15). In New Orleans, numbers of crimes were estimated for 29 months before and 29 months after the improved lighting, based on the monthly rates reported.

Why was improved street lighting effective in reducing crime in some studies but not in others? There was no clear tendency for some types of crimes (e.g. violence) to decrease more than others. One clear difference was that both daytime and nighttime crimes were measured in the “effective” evaluations (Atlanta, Milwaukee, and Fort Worth), whereas only nighttime crimes were measured in the “ineffective” evaluations (Portland, Harrisburg, and New Orleans). However, both daytime and nighttime crimes were measured in the “ineffective” Indianapolis evaluation; and only nighttime crimes were measured in Kansas City, where there were reductions in violence. “Nighttime” was not defined in Portland or New Orleans; in Harrisburg, it was defined as the hours between 8:00 p.m. and 4:00 a.m., so this would exclude some crimes committed during the hours of darkness.

U.K. studies

Table 5, shows that improved lighting was considered to be effective in reducing crime in four studies (Bristol, Birmingham, Dudley, and Stoke). In the fifth (Dover) study, the improved lighting was confounded with other improvements, including fencing to restrict access to the parking garage and the construction of an office near the main entrance. The officials considered that the crime prevention measures were successful because the reduced costs of damage and graffiti paid for the improvements within one year. On the basis of police records, Poyner (1991) concluded that the intervention had reduced thefts of vehicles but not thefts from vehicles.

It was difficult to interpret the Bristol evaluation because the street lighting was gradually improved in different places over a period of 28 months. Information about crime was provided for 9 successive 6-month periods overlapping this time period (Shaftoe

1994:75). The first 12-month period before the improved lighting (January-December 1986) was compared with the 12-month period after the improved lighting (July 1989-June 1990). Table 5 shows the results of this comparison. For total, nighttime and daytime crime, crimes decreased in the experimental area after the intervention and increased in the control area. Therefore, notwithstanding the contrary result for robbery (a decrease from 18 to 13 crimes), it was concluded that improved street lighting was effective in reducing crime.

In the Birmingham study of city center markets, two 6-month periods before the improved lighting were compared with two 6-month periods after. There were interventions in one of the control markets that could have led to reductions in crime. Nevertheless, the reductions in thefts from the person in the experimental market after the improved lighting were far greater than in the control markets. The experimental market was large and covered, and its lighting was markedly improved. Poyner (1997:89) concluded that, "increased levels of illumination appear to have deterred would-be thieves."

In the Dudley study, crime was measured using before and after victim surveys in experimental and control areas. Large samples were interviewed; 431 in the experimental area and 448 in the control area. The response rate was 77% in both areas before and 84% after (of those interviewed before). Crime decreased more in the experimental area than in the control area (Painter 1997:221), as shown by the interaction term in a regression equation. Furthermore, this interaction term was still significant after controlling for other predictors of crime rates including the age of the respondent (Painter 1997:221).

The Dudley study also evaluated the impact of improved street lighting using a self-reported delinquency survey completed by young people living on the experimental and control estates. Altogether, 307 young people were interviewed in the before survey and 334 in the after survey (Painter 2001:271). The self-reported delinquency results were surprisingly similar to the victim survey results. Table 5 shows that crime decreased in the experimental area by 40.8% according to the victim survey and by 35.0% according to self-reports; crime decreased in the control area by 15.0% according to the victim survey and by 14.0% according to self-reports. Our measure of effect size in Dudley was based on the victim survey because we considered that this yielded the most valid measure of crime.

The Stoke study included both adjacent and non-adjacent control areas to investigate displacement and diffusion of benefits. Again, victim surveys were used, with an 84% response rate before and an 89% response rate after (of those interviewed). The incidence of crime decreased by 42.9% in the experimental area, by 45.4% in the adjacent area, and by only 2.0% in the control area (Painter 1999a:97). According to interaction terms in regression equations, the changes in experimental and adjacent areas were significantly greater than in the control area. Police records also showed a decrease in crime of only 2% in the larger police area containing all the project areas. It was concluded that improved street lighting had caused a decrease in crime in the experimental area and that

there had been a diffusion of benefits to the adjacent area, which was not clearly delimited from it. It was suggested that improved street lighting might have caused increased community pride, community cohesion and informal social control, which deterred potential offenders.

META-ANALYSIS

Pooled results

Table 6 shows the results of the meta-analysis. From the 13 evaluations, it was concluded that improved street lighting had a significant desirable effect on crime, with a weighted mean RES of 1.27 (95% confidence interval 1.09 – 1.47, $p = .0008$). This means that crimes increased by 27% in control areas compared with experimental areas, or conversely crimes decreased by 21% in experimental areas compared with control areas. Because the 13 effect sizes were significantly heterogeneous ($Q = 37.14$, 12 df, $p = .0002$), a random effects model was used here. Fixed effects models were used when the heterogeneity was not significant. The fixed and random effects models, and the other models used by Jones (2005), all produced similar weighted mean effect sizes.

Figure 1 summarizes the results of all 13 studies in a forest graph. It shows the RES for total crime measured in each study plus its 95% confidence interval. The studies are ordered according to their magnitudes of their RESs. It can be seen that only 3 studies (Portland, New Orleans, and Indianapolis) had RESs less than 1, meaning that improved street lighting was followed by an increase in crime, and in no case was this increase significant. The other 10 studies had RESs greater than 1, meaning that improved street lighting was followed by a decrease in crime, and in 6 cases this decrease was significant (or nearly so, in the case of Atlanta). Therefore, the hypothesis that more lighting causes more crime can be firmly rejected.

As shown in Table 6, both nighttime and daytime crimes were measured in all 5 British studies and 4 of the 8 U.S. studies. The 9 night/day studies also showed a significant desirable effect of improved lighting on crime (RES = 1.43, CI = 1.19 – 1.71, $p < .0001$).

U.S. studies

As shown in Table 6, improved street lighting was considered to have a desirable effect on crime in 4 evaluations: Atlanta, Milwaukee, Fort Worth, and Kansas City. In all 4 cases, the RES was 1.24 or greater. In the other 4 evaluations, the improved street lighting was considered to have no effect on crime. The results of the meta-analysis of the 8 U.S. studies confirm these conclusions. The average effect size was a RES of 1.08, which was not significant. Overall, crime increased by 8% in control areas compared with experimental areas, or conversely crime decreased by 7% in experimental areas compared with control areas.

The key dimension on which the 8 effect sizes differed seemed to be whether they were based on data for both night and day (Atlanta, Milwaukee, Fort Worth, and Indianapolis)

or for night only (the other 4 studies). For the four night/day studies, the average effect size was a significant RES of 1.28 (CI = 1.06 – 1.53, $p = .010$), meaning that crime increased by 28% in control areas compared with experimental areas, or decreased by 22% in experimental areas compared with control areas. For the 4 night only studies, the RES was 1.01 (n.s.), indicating no effect on crime. Therefore, the 8 U.S. studies could be divided into 2 blocks of 4, one block showing that crime reduced after improved street lighting and the other block showing that it did not. Surprisingly, evidence of a reduction in crime was only obtained when both daytime and nighttime crimes were measured, although this feature may be a proxy for some other aspect of the different evaluation studies.

U.K. studies

As noted above, improved street lighting was considered to be effective in reducing crime in 4 studies (Bristol, Birmingham, Dudley, and Stoke-on-Trent). In the fifth study (Dover), the improved lighting was confounded with other improvements, including fencing to restrict access to the parking garage and the construction of an office near the main entrance. On the basis of police records, Poyner (1991) concluded that the intervention had reduced thefts *of* vehicles but not theft *from* vehicles.

Results of the meta-analysis of the 5 British studies confirm these conclusions. Total crimes reduced significantly after improved lighting in Bristol, Birmingham, Dudley, and Stoke-on-Trent. When the RESs from the 5 studies were combined, crimes increased by 62% after improved street lighting in control areas compared with experimental areas, or conversely crimes decreased by 38% in experimental areas compared with control areas (RES = 1.62, CI = 1.22 – 2.15, $p = .0008$).

In conclusion, these more recent British studies agree in showing that improved lighting reduces crime. They did not find that nighttime crimes decreased more than daytime crimes, suggesting that a “community pride” theory may be more applicable than a “deterrence/ surveillance” theory.

Table 7 shows the results of meta-analyses for violent and property crimes separately. Briefly, improved lighting was followed by a significant reduction in property crime (RES = 1.20, CI = 1.02 – 1.41, $p = .024$) but not in violent crime (RES = 1.10, CI = 0.91 – 1.34, n.s.). Random effects models were used.

Discussion

Eight U.S. evaluation studies met the criteria for inclusion in the review, and their results were mixed. Four studies found that improved street lighting was effective in reducing crime, while the other 4 found that it was not effective. Why the studies produced different results was not obvious, although there was a tendency for effective studies to measure both daytime and nighttime crimes and for ineffective studies to measure only nighttime crimes. However, all except one of these U.S. evaluations date from the 1970s.

Five more recent U.K. evaluation studies showed that improved lighting led to decreases in crime. Furthermore, in 2 studies (Dudley and Stoke-on-Trent), the financial savings from reduced crimes greatly exceeded the financial costs of the improved street lighting (Painter 2001). Since these studies did not find that nighttime crimes decreased more than daytime crimes, a theory of street lighting focusing on its role in increasing community pride and informal social control may be more plausible than a theory focusing on increased surveillance and increased deterrence. The results did not contradict the hypothesis that improved street lighting was most effective in reducing crime in stable homogeneous communities. (While lack of systematic information on residential mobility made it difficult to draw clear conclusions about whether improved street lighting was more effective in reducing crime in stable homogeneous communities than in unstable heterogeneous communities, not one of the 10 studies that could be included in this analysis clearly contradicted this hypothesis, and 4 studies (Dudley, Stoke-on-Trent, Harrisburg, and Fort Worth) were clearly concordant with it. The 3 studies that could not be included in this analysis were: Indianapolis, Dover, and Birmingham.)

An alternative hypothesis is that increased community pride comes first, causing improved street lighting on the one hand and reduced crime on the other, with no causal effect of improved lighting on crime. It is difficult to exclude this hypothesis on the basis of most published evaluation reports. However, it can be excluded in the two evaluations (Dudley and Stoke-on-Trent) in which one of us (DPF) was involved.

In Dudley, there had been no marked changes on the experimental estate for many years. The tenants on this and other local authority housing estates had complained about the poor lighting for some time, and this was why the local authority decided to improve the lighting on the experimental estate. The improvement in lighting was very obvious, and tenants thought that their quality of life had been improved (Painter 1997). This stimulated the Tenants' Association on the experimental estate to obtain £10 million (approximately \$20 million) from the Department of the Environment for a program of neighborhood improvements in the next few years. The improvement in lighting on the experimental estate also stimulated the Tenants' Association on the control estate to petition the local authority to improve their lighting.

In Dudley, it was clear that the improved lighting occurred first, led to increased community pride, and acted as a catalyst for further environmental improvements. A similar chain of events happened in Stoke-on-Trent. While we cannot be sure that the same causal ordering occurred in all other street lighting evaluations, it might be concluded that in at least some studies improved lighting caused increased community pride and decreased crime.

Sensitivity Analyses

Our aim was to count the total number of crimes (including all types of crimes committed during day and night if possible) for the same time periods (as long as possible, but in most cases about 12 months) before and after improved lighting in experimental and

control areas. We wanted a single effect size measure in each evaluation. Where an evaluation had experimental, adjacent, and control areas, our measure of effect size was calculated by comparing experimental and control areas. We only included evaluations if they had a comparable control area and if there were at least 20 crimes in the control area before the improved lighting. In all cases, these rules unambiguously specified which evaluations were included and how many crimes were counted in each evaluation.

Nevertheless, it is perhaps useful to investigate the effects of variations in these assumptions. In 8 evaluations, no alternative counts of crimes are possible, except by not combining areas. For example, in Portland and Indianapolis, results in two experimental areas were combined, and also results in two control areas, in the interests of comparing a total experimental area with a total control area. As mentioned, we combined areas because we wanted to have only one effect size for each evaluation. In New Orleans, analyzing 51 months before the improved lighting (rather than 29 months) had no effect on the weighted mean RES.

In Bristol, we compared the first 12-month period before the improved lighting with the last 12-month period after. We had no alternative in choosing the 12-month period after, but there were in fact 18 months of data before the lighting began to be improved. When we compared this 18-month before period with the 12-month after period, the RES decreased from 1.35 to 1.23 (Table 8).

In Birmingham, theft figures were given for the 6-month periods March-August in 1982, 1983, 1984, and 1985. The lighting was improved towards the end of 1983. Therefore, we compared the data for 12 months before in 1982 and 1983 with data for 12 months after in 1984 and 1985. However, there was intensive police action in the control markets in 1982 that could have displaced crimes to the experimental market (Poyner 1997: 87). Therefore, we investigated the effects of comparing only 1983 (before) with 1984 and 1985 (after). The RES decreased from 3.82 to 2.19. Both of these modifications together reduced the weighted mean RES to 1.21 (Table 8).

We excluded the evaluations in Wandsworth (Atkins 1991) and Hastings (Ramsay 1991) because of small numbers of crimes and other serious flaws. If these evaluations had been included in the analysis (together with the Bristol and Birmingham modifications), the weighted mean RES would have reduced to 1.19.

Finally, in the worst-case scenario for lighting effects, we could have amalgamated the adjacent and control areas in the Portland and Stoke-on-Trent evaluations, although this would have contradicted the aim of investigating displacement and diffusion effects. This had no effect on the results in Portland, but it reduced the RES for Stoke-on-Trent from 1.71 to 1.11. With all these modifications, the weighted mean RES became 1.16 (CI 1.05 – 1.29, $p = .005$). Therefore, even in this worst-case scenario, improved lighting still had significant effects in reducing crime.

Other modifications in the analysis would lead to an increase in the weighted mean RES. In the Dudley victim survey, a limit of 10 was set on the number of crimes reported by

each household in each survey, in order to minimize the effect of outliers. No such limit was set in the Stoke-on-Trent survey. When the Dudley crime data were reanalyzed with no limit, the RES increased from 1.44 to 1.49 (Table 8). The weighted mean RES remained at 1.27.

One other excluded study, conducted in Portland by Griswold (1984), had a control area which was not comparable to the experimental area (the remainder of the city) and improved lighting which was confounded with other security measures. This evaluation could conceivably have been included. Table 8 (Portland-G) shows that its RES was 1.33 (CI 0.98 – 1.82, $p = .07$, almost significant). Including it would have increased the weighted mean RES to 1.28.

Finally, Jones (2005) compared six methods of meta-analysis (the familiar fixed and random effects models, a multiplicative variance adjustment-MVA method, logistic regression, a quasi-binomial approach, and a general linear mixed model fitted using the penalized quasi-likelihood approach). Apart from the random effects model, the other five models yielded almost identical results for the weighted mean RES. Table 8 shows the results for the MVA model, which multiplies the variance of each LOR to exactly fit the data. Thus, the MVA model exactly corrects for overdispersion and heterogeneity. With this model, the weighted mean RES was 1.22 (CI 1.07 – 1.40, $p = .004$).

We conclude that the significant effect of improved lighting in reducing crime holds up in the face of various other assumptions. We believe that the analyses in Table 6 are the most defensible.

Objections by the “Campaign for Dark Skies”

The “Campaign for Dark Skies”, as the name suggests, campaigns for less street lighting (“light pollution”) so that astronomers (professional and amateur) can see the stars more clearly. Dr. Marchant of Leeds Metropolitan University is a statistician who financially supports this Campaign, and he has criticized our research (Marchant 2004, 2005). His most important criticisms and our answers (Farrington 2004, 2006) are listed below:

- 1. Overdispersion:** Marchant (2004) argued that the variance of the LRES in our analyses (based on numbers of crimes) is greater than in the more usual use of the RES (based on numbers of persons). We have increased this variance according to our empirically-derived equation $VAR/N = .0008 * N + 1.2$ (see above). Also, the MVA method exactly adjusts for both overdispersion and heterogeneity.
- 2. Regression to the Mean:** Marchant (2005) argued that improved lighting has usually been applied to high-crime areas and that, because areas are not randomly assigned to experimental and control conditions, experimental areas will tend to be worse beforehand than control areas. Therefore, because of “regression to the mean”, experimental areas will tend to decrease more than control areas. Farrington (2004, 2006) carried out various analyses of “regression to the mean” and concluded that it could cause a 4% year-

on-year decrease in crimes in experimental areas. However, this was much less than the overall 21% decrease in crime found in our meta-analysis.

3. The Bristol Study: Marchant (2004: 442-444) argued that the time series data from this study did not show any effect of improved lighting on crime. Farrington (2004: 459-460) carried out a time series analysis of this study and showed that there was a significant reductive effect of improved lighting on crime.

4. The Birmingham Study: Marchant (2004: 444) objected to our analyses of this study on the basis of changes between 1982 and 1983 (see above discussion). When we excluded 1982 data from our analyses, this had little effect on the weighted mean RES. With the 1982 data, the weighted mean RES = 1.28 (CI 1.11-1.48). Without the 1982 data, the weighted mean RES = 1.25 (1.08 –1.44).

5. The Dudley Study: Based on arguments about overdispersion, Marchant (2004: 444-445) also objected to Painter's (1997) analyses of the Dudley study. Farrington (2004: 450-458) carried out numerous new analyses of the Dudley data and concluded that the RES in Dudley and the weighted mean RES were still significant in various scenarios. Farrington (2004: 458) concluded:

What if the lower bound of the RES was less than 1?... This might be converted into an investment offer for a local council: if you invest £4,611 on improved street lighting, you have a minute chance of losing a further £12,156, but on average you will gain £364,668, and your benefits could be as high as £612,642. Should a local council accept such an offer, or should they reject it on the grounds that the evidence of the effectiveness of improved street lighting was unconvincing because the confidence interval for the RES included 1.0?

We invite scholars to read the exchanges between Dr. Marchant and ourselves and to make up their own minds about whether existing evaluation studies show that improved street lighting causes a significant decrease in crime.

Limitations

It is difficult to test for publication bias among the 13 included studies. This is because it is confounded with the difference in effects between the U.K. and U.S. studies; that is, U.K. studies were more likely to be published. It is important to note, however, that only 3 of the studies (Fort Worth, Indianapolis, and Dover) were published in peer-reviewed journals.

Studies were included in this review if they had, at a minimum, an evaluation design that involved before-and-after measures of crime in experimental and (reasonably) comparable control areas. Most of the 13 included studies used a control area that was comparable to the experimental area. According to Cook and Campbell (Cook 1979) and Shadish, Cook, and Campbell (Shadish 2002), this is the minimum design that is interpretable. This design can rule out many threats to internal validity, including history,

maturation/trends, instrumentation, testing effects, and differential attrition. The main problems with it center on selection effects and regression to the mean (because of the non-equivalence of the experimental and control areas).

The randomized controlled experiment is considered the “gold standard” in evaluation research designs. It is the most convincing method of evaluating crime prevention programs (Farrington 2006). There have been many area-based studies that have employed randomized experimental designs (e.g., on hot spots policing; Braga 2005), but no experiment has yet been conducted to investigate the effects of lighting on crime.

Reviewers’ conclusions

Implications for practice

The policy implications of research on improved street lighting have been well articulated by Pease (1999). He pointed out that situational crime prevention involved the modification of environments so that crime needed more effort, more risk, and lower rewards. The first step in any crime reduction program required a careful analysis of situations and how they affected potential offenders and potential victims. The second step involved implementing crime reduction interventions. Whether improved street lighting was likely to be effective in reducing crime would depend on characteristics of situations and on other concurrent situational interventions. Efforts to reduce crime should take account of the fact that crime tends to be concentrated among certain people and in certain locations, rather than being evenly distributed throughout a community.

The U.K. studies included in this review show that improved lighting can be effective in reducing crime in some circumstances. Exactly what are the optimal circumstances is not clear at present, and this needs to be established by future evaluation research. However, improved street lighting should be considered as a potential strategy in any crime reduction program in coordination with other intervention strategies. Depending on the analysis of the crime problem, improved street lighting could often be implemented as a feasible, inexpensive, and effective method of reducing crime.

Street lighting has some advantages over other situational measures that 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 it can increase public safety and effective use of neighborhood streets at night. In short, improved street lighting has few negative effects and clear benefits for law-abiding citizens.

Implications for research

Future research should be designed to test the main theories of the effects of improved street lighting (i.e. community pride versus surveillance/deterrence) more explicitly.

Surveys of youth in experimental and control areas could be carried out, to investigate their offending, their opinions of the area, their street use patterns, and factors that might inhibit them from offending (e.g., informal social control by older residents, increased surveillance after dark). Household surveys of adults could also be carried out, focusing on perceptions of improvements in the community, community pride, informal social control of young people, street use, and surveillance after dark.

Ideally, future research should measure crime using police records, victim surveys, and self-reports of offending. It is possible that one effect of improved street lighting may be to facilitate or encourage reporting of crimes to the police; for example, if victims get a better view of offenders. Therefore, police records may be misleading. Surveys of potential victims and potential offenders are necessary for testing key hypotheses about the effects of improved lighting. It is unfortunate that most existing evaluations are based only on police records.

Future research should ideally include several experimental areas and several comparable adjacent and control areas. Adjacent areas are needed to test hypotheses about displacement and diffusion of benefits. The comparability of experimental, adjacent, and control areas should be investigated. The use of several areas would make it more possible to establish boundary conditions under which improved lighting had greater or lesser effects. The numbers of crimes recorded in each area in the before period should be sufficient to detect changes reliably. Ideally, large numbers of potential victims and potential offenders should be surveyed. It is unfortunate that in many existing evaluations the control area was adjacent to the experimental area.

Crimes should be measured before and after the intervention in experimental, adjacent, and control areas. Ideally, a long time series of crimes should be studied to investigate pre-existing crime trends and also how far any effects of street lighting persist or wear off over time. Time series analyses should be carried out. Different types of crimes should be measured, and also crimes committed during daytime and the hours of darkness. The improvement in lighting in different areas should be carefully measured, including vertical and horizontal levels of illumination. Cost-benefit analyses of the impact of improved street lighting should be carried out (only 2 of the 13 studies conducted a cost-benefit analysis). Our previous work (Welsh 1999; 2000) has shown that situational crime prevention is an economically efficient strategy in preventing crime.

In testing hypotheses, it would be useful to investigate the effects of street lighting in conjunction with other crime prevention interventions. To the extent that community pride is important, this could be enhanced by other environmental improvements. To the extent that surveillance is important, this could be enhanced by other interventions, such as CCTV cameras. For example, one experimental area could have both improved street lighting and CCTV, a second could have only improved street lighting, and a third could have only CCTV. This kind of planned evaluation of interactions of crime prevention initiatives has rarely been attempted.

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Potential conflict of interest

There is no conflict of interest on the part of either author. It is important to note that the second author (Farrington) was involved in 2 of the included evaluations (Dudley and Stoke-on-Trent). Farrington served as an independent researcher on these evaluations and both were published prior to the initiation of this systematic review (see Painter 1997, 1999a).

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Notes

Published notes

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Table 1
Improved Street Lighting Evaluations Not Meeting Inclusion Criteria

Author, Publication Date, and Location	Reason for Not Including Program	Other Interventions	Sample Size	Follow-up and Results
Hack 1974, Norfolk, Virginia, US	Crime not measured (fear of crime measured)	None	n/a	n/a
Siemon 1974, Dade County, FL, US	No control area used	None	1 public housing project (Larchmont Gardens)	9 months; class I crimes: -22.9% (245 to 189); class II crimes: - 51.4% (72 to 35)
Krause 1977, New Orleans, LA, US	No control area used	None	1 commercial area	9 months; commercial nighttime burglary (mean monthly difference): - 1.4
Griswold 1984, Portland, OR, US	Non-comparable control area (rest of city)	Multiple (e.g. security surveys, clean-up day, bus shelters)	1 commercial strip and adjacent neighborhoods	34 months; commercial burglary decreased, other crimes no change (time series analysis)
Bachner 1985, Camillus, NY, US	No control area used	None	1 parking lot of shopping mall	<1 year; vehicle break-ins: “virtually eliminated”
Davidson 1991, Hull, UK	No control area used	None	1 residential area (Dukeries)	6 weeks; percentage of victimizations: +9.5% (63% to 69%)
Vamplew 1991, Cleveland, UK	No control area used and crime not measured (public perception and fear of crime measured)	None	4 residential areas	12 months; n/a
Vrij 1991, Enkhuizen, The Netherlands	Crime not measured (fear of crime and perceived risk of victimization measured) ^a	None	n/a	n/a
Atkins 1991, Wandsworth, UK	Numbers of crimes too small. Victim survey response rate before = 37%	None	1 relit area, 1 adjacent non-relit area	VS: 7 weeks: relit crimes - 35.9% (39 decrease to 25); control crimes - 69.2% (13 to 4).

Ramsey 1991, Hastings, UK		Number of crimes too small	None	1 relit area, 1 control area	7 months: recorded crime in relit area +40.0% (15 to 21); control crimes +30.6% (85 to 111).
Challinger 1992, South Australia and Northern Territory, Australia		No control area used	Multiple (e.g., target hardening, security staff)	35,000 public pay phones	3 years; ^b vandalism: -19.0% (1,373 to 1,112)
Nair 1993, Glasgow, UK		No control area used and crime not measured (fear of crime measured)	Multiple (e.g., paths widened, entry phones)	n/a	n/a
Tilley 1993, Salford, UK		No control area used	None	3 businesses	12 months; total crimes: - 72.4% (29 to 8)
Ditton 1994, Glasgow and High Blantyre, UK		No control area used	None	1 residential area in both sites	3 months; 2 sites combined: total personal victimization: - 50.0% (12 to 6); total vehicle victimization: - 95.7% (23 to 1); total police-recorded crime: - 14.0% (57 to 49)
La Vigne 1994, Austin, TX, US		No control area used	None	38 convenience stores	n/a.; thefts of gasoline: - 65%
Painter 1994, 3 areas in London, UK	Edmonton	No control area used (for all 3 sites)	None (for all 3 sites)	1 street and 1 pedestrian footpath	6 weeks; total crime (at night): - 85.7% (21 to 3)
	Tower Hamlets			1 street	6 weeks; total crime (at night): - 77.8% (18 to 4)
	Hammer-smith and Fulham			1 street	12 months; total crime (at night): 2 to 0
Nair 1997, Glasgow, UK		No control area used	None	1 carriage-way	2 years; pestering/following: - 48.2% (112 to 58); sexual proposition: - 54.2% (24 to 11); assault/mugging: 3 to 1; sexual assault: 1 to 0 (all at night)

Tseng 2004, Columbus, Ohio, US	No control area used	Improved visibility, change in exit and entrance locations, re-design of stairways	2 parking garages on university campus	2 years; violent crime: -20.6% (34 to 27) property crime: -2.1% (1,362 to 1,333) total crime: -2.6% (1,396 to 1,360)
Willis 2005, 3 shire counties in UK (Bedfordshire, North Yorkshire, and Wiltshire)	Crime not measured	None	residential areas	n/a

^a Respectively, the questions asked were: ““To what extent do you feel safe here?”” and ““How likely do you think it is that you could be molested here?”” (Vrij 1991:211). ^b Follow-up period not specified for street lighting intervention. Notes: n/a. = not available or not applicable. VS = Victim Survey.

Table 2
Improved Street Lighting Evaluations in the United States

Author, Publication Date, Location	Context of Intervention	Type of Intervention (Other Interventions)	Sample Size	Outcome Measure and Data Source	Research Design
Atlanta Regional Commission 1974, Atlanta, GA	City center (high robbery)	Improved (4x) street lighting (none)	E=selected streets in census tract 27, C=rest of streets in census tract 27	Crime (robbery, assault, and burglary); police records	Before-after, experimental-control; before and after periods = 12 months
Department of Fiscal Liaison 1974, Milwaukee, WI	Residential and commercial area (older residents)	Improved (7x) street lighting (none)	E=1 area (3.5 miles of streets), C=1 adjacent area	Crime (property and person categories); police records	Before-after, experimental-control; before and after periods = 12 months
Inskeep 1974, Portland, OR	Residential neighborhood (high crime)	Improved (2x) street lighting (none)	E=2 areas, A=2 areas, C=surrounding areas	Crime (robbery, assault, and burglary); police records	Before-after, experimental-control; before and after periods = 6 or 11 months
Wright 1974, Kansas City, MO	Residential and commercial areas (high crime)	Improved street lighting (none)	E=129 relit blocks in 4 relit areas, C=600 non-relit blocks in same areas	Crime (violent and property offenses); police records	Before-after, experimental-control; before and after periods = 12 months
Harrisburg Police Department 1976, Harrisburg, PA	Residential neighborhood	Improved street lighting (none)	E=1 high crime area, C=1 adjacent area	Crime (violent and property offenses); police records	Before-after, experimental-control; before and after periods = 12 months
Sternhell 1977, New	Residential and	Improved street	E=2 high crime areas,	Crime (burglary,	Before-after, experimental-

Orleans, LA	commercial areas	lighting (none)	C=2 adjacent areas	vehicle theft, and assault); police records	control; before period = 51 months, after period = 29 months
Lewis 1979, Fort Worth, TX	Residential neighborhood	Improved (3x) street lighting (none)	E=1 high crime area, C=1 adjacent area	Crime (total); police records	Before-after, experimental-control; before and after periods = 12 months
Quinet 1998, Indianapolis, IN	Residential neighborhood	Improved street lighting (police initiatives)	E=2 multi-block areas, C= 2 areas with no new lights	Calls for service (violent and property crime); police records	Before-after, experimental-control; before and after periods = 7-10 months

Notes: E = experimental; C = control; A = adjacent; x = times increase in lighting.

Table 3
Improved Street Lighting Evaluations in the United Kingdom

Author, Publication Date, Location	Context of Intervention	Type of Intervention (Other Interventions)	Sample Size	Outcome Measure and Data Source	Research Design
Poyner 1991, Dover	Parking garage (in town center)	Improved lighting (at main entrance/ exit) (fencing, office constructed)	E=1 parking garage, C=2 open parking lots close to E	Crime (total and theft of and from vehicles); police records	Before-after, experimental-control; before and after periods = 24 months
Shaftoe 1994, Bristol	Residential neighborhood	Improved (2x) street lighting (none)	E=2 police beats, C=2 adjacent police beats	Crime (total); police records	Before-after, experimental-control; before and after periods = 12 months
Poyner 1997, Birmingham	City center market	Improved lighting (none)	E=1 market, C=2 markets	Thefts; police records	Before-after, experimental-control; before and after periods = 12 months (6 months in each of 2 years)
Painter 1997, Dudley	Local authority housing estate	Improved (2x) street lighting (none)	E=1 housing estate, C=1 adjacent estate	Crime (total and types of offenses); victim survey and self-reports	Before-after, experimental-control and statistical analyses; before and after periods = 12 months
Painter 1999, Stoke-on-Trent	Local authority housing	Improved (5x) street lighting	E=1 housing estate, A=2 adjacent estates, C=2	Crime (total and types of offenses);	Before-after, experimental-control and statistical

	estate	(none)	non-adjacent estates	victim survey	analyses; before and after periods = 12 months
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Notes: E = experimental; A = adjacent; C = control; x = times increase in lighting.

Table 4
Results of Improved Street Lighting Evaluations in the United States

Study	All Crimes (E Before, C Before)	Types of Crimes	Results and Displacement / Diffusion
Atlanta	T: E +34.2%, C +76.5% N: E +88.7%, C +121.5% D: E -16.4%, C +33.3% (114, 247)	Rob: E -8.1%, C +23.6% Aslt: E +418.2%, C +319.6% Burg: E -9.8%, C +32.8%	Effective No displacement occurred
Milwaukee	T (7m): E -5.6%, C +29.2% N (12m): E -5.9%, C -1.7% D (7m): E +2.2%, C +37.0% (161, 370)	Prop (N): E -5.8%, C -3.3% Viol (N): E -6.3%, C +2.0%	Effective Some displacement occurred
Portland	N: E -6.5%, A -11.8%, C -12.0% (340, 1,011; A Before = 365)	Rob (N): E -31.5%, A -36.6%, C -30.3% Aslt (N): E -11.3%, A -22.1%, C -5.6% Burg (N): E +11.9%, A +11.6%, C -7.3%	Not effective No displacement or diffusion occurred
Kansas City	N: E -36.7%, C -21.2% (188, 386)	Rob (N): E -52.2%, C -16.9% Aslt (N): E -40.5%, C +3.8% Larc (N): E -39.2%, C -28.9% MVT (N): E +3.0%, C -34.1%	Effective for violence Some displacement occurred
Harrisburg	N: E +14.4%, C +17.1% (201, 117)	Rob (N): E -8.7%, C +7.1% Aslt (N): E +9.4%, C -24.2% Burg (N): E +32.9%, C +46.0% MVT (N): E +2.4%, C +20.0%	Not effective No displacement occurred
New Orleans	N: E -25.4%, C -26.3% (863, 1,579)	Aslt (N): E -18.8%, C -30.1% Burg (N): E -25.8%, C -28.8% MVT (N): E -29.0%, C -22.6%	Not effective No displacement occurred
Fort Worth	E -21.5%, C +8.8% (261, 80)	Not available	Effective Possible displacement

			occurred
Indianapolis	E +39.0%, C +4.1% (excluding police actions) (118, 49)	Viol: E +39.2%, C +81.6% Prop: E -13.8%, C -18.2% (including police actions)	Not effective No displacement occurred

Notes: T =total; N = night; D = day; E = experimental; A = adjacent; C = control; Rob = robbery; Aslt = assault; Burg = burglary; Prop = Property; Viol = violence; Larc = larceny; MVT = motor vehicle theft; E Before = no. of crimes in experimental area before; C Before = no. of crimes in control area before.

Table 5
Results of Improved Street Lighting Evaluations in the United Kingdom

Study	All Crimes (E Before, C Before)	Types of Crimes	Results and Displacement / Diffusion
Dover	E -49.0%, C -41.9% (96, 43)	TFV: E -21.4%, C -50.0%; TOV: E -81.6%, C -47.1%	Effective (theft of vehicles) No displacement occurred
Bristol	T: E -5.3%, C +27.8% N: E -5.8%, C +19.3% D: E -4.9%, C +33.3% (2,931, 1,315)	Rob (N): E +50.8%, C -27.8%; TFV (N): E -29.6%, C +10.8%	Effective Displacement / diffusion not measured
Birmingham	(136, 81)	Theft (D): E -78.7%, C -18.5%	Effective No displacement occurred; some diffusion occurred
Dudley	VS: E -40.8%, C -15.0% SR: E -35.0%, C -14.0% N: E -31.9%, C -2.0% D: E -38.7%, C -26.0% (VS: 495, 368) (SR: 480, 499)	VS Burg: E -37.7%, C -13.4% Veh: E -49.1%, C -15.7% Viol: E -40.8%, C +4.9% SR Viol: E -39.6%, C -25.6% Vand: E -18.2%, C +10.9% Dish: E -7.1%, C +60.0%	Effective No displacement occurred
Stoke-on-Trent	VS: E -42.9%, A -45.4%, C -2.0% (551, 61: A before = 243)	VS Burg: E -15.1%, A -20.3%, C +0.6% Veh: E -46.4%, A -47.7%, C -34.7% Viol: E -68.0%, A -66.3%, C -39.2%	Effective No displacement occurred; diffusion occurred

Notes: T =total; N = night; D = day; E = experimental; A = adjacent; C = control; VS = victim survey; SR = self-reports; Rob = robbery; Burg = burglary; TFV = theft from vehicle; TOV = theft of vehicle; Viol = violence; Veh = vehicle crime; Vand =

vandalism; Dish = dishonesty; E Before = no. of crimes in experimental area before; C Before = no. of crimes in control area before.

Table 6
 Meta-Analysis of Improved Street Lighting Evaluations

	Relative Effect Size	Confidence Interval / Z Value		P Value
US N Studies				
Portland	0.94	0.75 - 1.18	-0.53	n.s.
Kansas City	1.24	0.90 - 1.71	1.34	n.s.
Harrisburg	1.02	0.72 - 1.46	0.13	n.s.
New Orleans	0.99	0.83 - 1.18	-0.14	n.s.
US ND Studies				
Atlanta	1.39	0.99 - 1.94	1.91	.055
Milwaukee	1.37	1.01 - 1.86	2.01	.044
Fort Worth	1.38	0.92 - 2.07	1.58	n.s.
Indianapolis	0.75	0.45 - 1.25	-1.10	n.s.
UK ND Studies				
Dover	1.14	0.58 - 2.22	0.38	n.s.
Bristol	1.35	1.16 - 1.56	3.98	.0001
Birmingham	3.82	2.15 - 6.80	4.56	.0001
Dudley	1.44	1.10 - 1.87	2.67	.008
Stoke-on-Trent	1.71	1.10 - 2.67	2.38	.017
Summary Results				
4 US N Studies	1.01	0.90 - 1.14	0.16	n.s.
4 US ND Studies	1.28	1.06 - 1.53	2.59	.010
5 UK ND Studies*	1.62	1.22 - 2.15	3.37	.0008
8 US Studies	1.08	0.98 - 1.20	1.55	n.s.
9 ND Studies*	1.43	1.19 - 1.71	3.87	.0001
All 13 Studies*	1.27	1.09 - 1.47	3.09	.0008

Notes: N = only night crimes measured; ND = night and day crimes measured;

* = random effects model used (fixed effects model used in other cases).

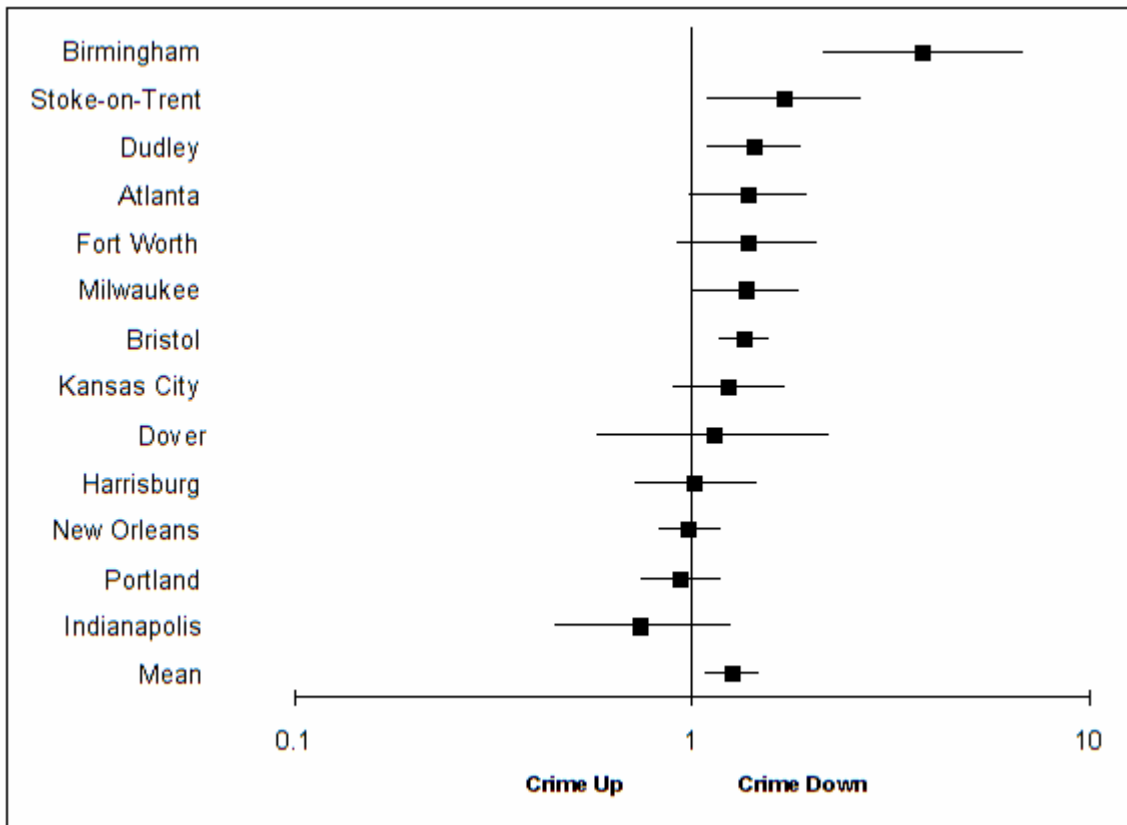
Table 7
 Meta-Analysis of Violent and Property Crimes

	Relative Effect Size	Confidence Interval	z	P value
Violent				
Portland	0.83	0.60 -1.14	-1.17	n.s.
Kansas City	1.79	1.14 – 2.79	2.55	.011
Harrisburg	0.81	0.46 – 1.44	-0.70	n.s.
New Orleans	0.86	0.64 – 1.16	-0.99	n.s.
Atlanta	1.20	0.81 – 1.78	0.92	n.s.
Milwaukee	1.09	0.45 – 2.65	0.19	n.s.
Bristol	0.48	0.21 – 1.09	-1.75	n.s.
Dudley	1.77	1.09 – 2.88	2.31	.021
Stoke	1.89	0.45 – 7.90	0.87	n.s.
Total	1.10	0.91 – 1.34	0.98	n.s.
Property				
Portland	0.83	0.60 – 1.14	-1.17	n.s.
Kansas City	0.88	0.57 – 1.36	-0.58	n.s.
Harrisburg	1.14	0.73 – 1.78	0.56	n.s.
New Orleans	1.03	0.85 – 1.26	0.33	n.s.
Atlanta	1.47	0.79 – 2.73	1.23	n.s.
Milwaukee	1.03	0.70 – 1.50	0.13	n.s.
Dover	1.14	0.58 – 2.22	0.38	n .s.
Bristol	1.57	1.07 – 2.31	2.31	.021
Birmingham	3.82	2.15 – 6.80	4.56	.0001
Dudley	1.33	0.98 – 1.80	1.81	n.s.
Stoke	1.59	0.99 – 2.56	1.93	.054
Total	1.20	1.02 – 1.41	2.25	.024

Table 8
Sensitivity Analyses

	Relative Effect Size	Confidence Interval	Z	P
(A) Total Effect	1.27	1.09 - 1.47	3.09	.002
1. Bristol – 18m	1.23	1.06 – 1.41	2.83	.005
Birmingham – 1983	2.19	1.16 – 4.16	2.41	.016
Total Effect	1.21	1.07 – 1.36	3.11	.002
2. Wandsworth	0.48	0.14 – 1.66	-1.16	n.s.
Hastings	0.93	0.41 – 2.10	-0.17	n.s.
Total Effect	1.19	1.06 – 1.34	2.96	.003
3. Stoke – Adjacent	1.11	0.86 – 1.42	0.78	n.s.
Total Effect	1.16	1.05 – 1.29	2.82	.005
(B) Total Effect	1.27	1.09 – 1.47	3.09	.002
1. Dudley – No Limit	1.49	1.16 – 1.92	3.10	.002
Total Effect	1.27	1.09 – 1.48	3.12	.002
2. Portland – G	1.33	0.98 – 1.82	1.81	.070
Total Effect	1.28	1.11 – 1.47	3.38	.0007
(C) Total Effect (MVA)	1.22	1.07 – 1.40	2.90	.004

Figure 1
 RESs and 95% Confidence Intervals for Total Crime by Study



Note: RESs on logarithmic scale.