Effectiveness of Street Lighting in Preventing Crime in Public Places

An Updated Systematic Review and Meta-Analysis

Brandon C. Welsh, David P. Farrington, Stephen Douglas

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The Swedish National
Council for Crime Prevention

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The Swedish National Council for Crime Prevention (Brottsförebyggande rådet – Brå) works to reduce crime and improve levels of safety in society by producing data and disseminating knowledge on crime and crime prevention work and the justice system's responses to crime.

Authors: Brandon C. Welsh, David P. Farrington and Stephen Douglas

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Cover Illustration: Helena Halvarsson

Swedish National Council for Crime Prevention, Box 1386, SE-111 93 Stockholm, Sweden
+46 (0)8–527 58 400, e-mail info@bra.se, www.bra.se

Foreword

Darkness in built up areas can contribute to feelings of personal insecurity, and can produce favorable conditions for vandalism and theft, including bicycle thefts and thefts from vehicles, offences which are very common. The crime preventive effects of improved lighting in public places are therefore often discussed, and measures to improve lighting are often implemented as a means of combating crime. But does improved lighting reduce levels of crime? What do the strongest evaluations tell us? These questions are answered in this systematic review, which examines the strongest available research to date.

There are never sufficient resources to conduct rigorous evaluations of all the crime prevention measures employed in an individual country such as Sweden. Nor are there resources to conduct scientific studies of all the possible effects produced by different measures against crime and unsafety. For these reasons, the Swedish National Council for Crime Prevention (Brå) has commissioned distinguished researchers, led by Professor David P. Farrington at Cambridge University, to conduct a series of international reviews of the research published in various fields. In 2007, Brå published a systematic review of this kind on the effects of improved lighting. The publication was based on the 13 studies available at the time whose methodology was sufficiently rigorous to meet the inclusion criteria for a systematic review. This report comprises an updated review, which now includes a total of 21 studies. As in the review published in 2007, the study follows the rigorous methodological requirements of a systematic review and statistical meta-analysis. The analysis combines the results from studies that are considered to satisfy a list of empirical criteria for measuring effects as reliably as possible. Even though important questions remain unanswered, the study provides a vital and far-reaching overview of the available evidence on the preventive effects of improved street lighting.

The study has been conducted, and the report written, by Professor Brandon C. Welsh at Northeastern University (USA), Professor David P. Farrington at the University of Cambridge (UK), and Doctoral student Stephen Douglas, also at Northeastern University (USA).

Stockholm, December 2021

Kristina Svartz
Director-General

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Summary

In the first update of our systematic review on the effects of street lighting interventions on crime, we reported that research on the topic "appears to have come to a standstill" (Farrington and Welsh, 2007, p. 25). This is no longer the case. As we find in this current update of the review, there has been a renewed interest in evaluation research on street lighting and crime. Eight new studies met the review's inclusion criteria, and seven of them were reported since 2015. In addition, many more evaluations of street lighting interventions were identified that did not meet the inclusion criteria. It seems that this uptick in evaluation research is closely tied to new developments in and applications of street lighting technology. Based on a total of 21 high-quality evaluation studies, this report presents the results of a new update of the systematic review.

Studies were included in this systematic review if street lighting was the main intervention, if it was implemented in a public place, if there was an outcome measure of crime, if the evaluation design was of high-quality methodologically, and if the total number of crimes in experimental and control areas before the intervention was at least 20. (Any study with less than 20 crimes before would have insufficient statistical power to detect changes in crime.)

Six search strategies were used to locate studies meeting the criteria for inclusion: searches of electronic bibliographic databases; searches of literature reviews on the effectiveness of street lighting interventions on crime; searches of bibliographies of evaluation reports of lighting studies; forward citation searches using Google Scholar; searches of the scholarly networking site ResearchGate; and contacts with leading researchers. Twenty-one studies met the criteria for inclusion in the review, representing four countries: 12 from the United States; seven from the United Kingdom; and one each from Brazil and South Korea.

It is concluded that street lighting continues to be an effective intervention in preventing crime in public places. A narrative synthesis of effects of the eight new studies and a meta-analysis of 17 studies confirmed this conclusion. In pooling the effects of 17 studies that could be included in the meta-analysis, it was found that street lighting led to a significant 14% decrease in crime in experimental areas compared with comparable control areas. While not every

study was effective in preventing crime, desirable effects of street lighting interventions were greater in studies that measured both night and day crimes than in studies that only measured night crimes. This translates to a significant 18% decrease in crime for night/day studies compared to a non-significant 3% decrease in crime for night only studies. These findings suggest 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 or deterrence.

It was also possible to use meta-analytic techniques to investigate the effects of street lighting interventions on violent and property crimes, the two main types of crimes that were measured in the studies. Street lighting interventions were followed by a significant decrease in property crimes (12%), but not in violent crimes. Importantly, this effect on property crimes closely approximates the overall effect of street lighting interventions on total crime (14%), suggesting that the overall effect on crime may be largely a function of street lighting's impact on property crimes. Cost-benefit analyses of improved lighting suggested that the benefits (in terms of fewer crimes) outweighed the costs, but similar analyses of part-night lighting also suggested that this had benefits.

Many of the recommendations that we made 14 years ago for research on the effects of street lighting on crime are still needed today. For example, future evaluation studies should include several experimental areas and several adjacent and comparable non-adjacent control areas. Adjacent areas are needed to investigate crime displacement and diffusion of crime prevention benefits. In addition, a long time series of crimes should be studied to investigate pre-existing crime trends, as well as the extent to which any effects of street lighting interventions persist or wear off over time. It would also be highly desirable for researchers to carry out more cost-benefit analyses of street lighting interventions. This would allow for a number of key policy questions to be addressed, such as: Do the monetary benefits to society from decreased crime rates outweigh the monetary costs of implementing and maintaining street lighting projects? To whom do the monetary benefits (or costs) accrue?

Compared to years past, it would seem that an even stronger case can be made today for street lighting interventions to be part of national and local crime prevention policy. A larger body of high-quality evaluation research, implemented in a range of high-crime public places, some evidence of value

for money, and the enduring impact on crime, especially property crimes, all point to the policy significance of street lighting interventions.

Acknowledgements

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We also wish to extend our sincere appreciation to the following individuals for putting up with our endless questions and providing us with data from their studies: Professor Aaron Chalfin (University of Pennsylvania) and Professors Cristina Mihale-Wilson, Patrick Felka, and Oliver Hinz (all at Goethe University Frankfurt).

Brandon C. Welsh, David P. Farrington and Stephen Douglas

Introduction

The main aim of this report is to present the results of a second update of our systematic review and meta-analysis on the effects of street lighting on crime in public places. Several points are noteworthy. First, this report represents a 14-year update (covering the period of January 2007 to March 2021) since the first update, which was also funded and published by the Swedish National Council for Crime Prevention (Farrington and Welsh, 2007). Six years elapsed between the initial review (up to December 2000), which was funded and published by the British Home Office (Farrington and Welsh, 2002a; see also Farrington and Welsh, 2002b), and the first update (January 2001 to December 2006). Second, our review is no longer focused solely on improved street lighting, with "improved" often characterized as an increase in the brightness of illumination (i.e., by 2, 3, or more times). In the intervening years since the first update of our review, cities and towns in many parts of the world have begun to implement a range of new lighting technologies (e.g., light-emitting diode [LED], smart lights) and applications (e.g., temporary or permanent switching-off, temporary lighting towers), and many have been the subject of evaluations to test their impact on crime. Third, unlike the first update, which did not identify one new study that met the review's criteria for inclusion, the current update identified eight new studies that met the inclusion criteria. This means that the results presented here are based on a total of 21 studies.

Street lighting serves many purposes, one of them being the prevention of crime in public places. Street lighting is not always implemented with the express aim of preventing crime; traffic and pedestrian safety may be viewed as more important goals (see Beyer and Ker, 2009). Although 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 potential criminals and potential victims.

Explanations of the way street lighting interventions could prevent crime can be grouped into two main perspectives: (a) 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); and (b) 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 et al., 1984; Taylor and Gottfredson, 1986).

The situational approach to crime prevention suggests that crime can be prevented by measures that directly affect offenders' perceptions of increased risks and efforts and decreased rewards, provocations, and excuses (Cornish and Clarke, 2003). This approach is also supported by theories that 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.

Improvements in street lighting, for instance, may encourage increased street usage, which intensifies natural surveillance. The change in routine activity patterns works to reduce crime because it increases the flow of potentially capable guardians who can intervene to prevent crime (Cohen and Felson, 1979). From a potential offender's perspective, the proximity of other pedestrians acts as a deterrent because the risks of being recognized or interrupted when attacking personal or property targets are increased. From a potential victim's perspective, the perceived risks and fears of crime are reduced.

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 and Coles, 1996; Skogan, 1990; Wilson and Kelling, 1982). As a highly visible sign of positive investment, street lighting interventions might reduce crime if they 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 residents to have a more positive image of the area and to have increased community pride, optimism, and cohesion. This might even encourage residents to take more actions to prevent crime. It should be noted that this theoretical perspective predicts a reduction in both daytime and nighttime crime. Consequently, attempts to measure the effects of street lighting should not concentrate purely on nighttime crime.

The relationship among visibility, social surveillance, and criminal opportunity 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 street lighting interventions could, in certain circumstances, increase opportunities for crime. They may bring greater numbers of potential victims and potential offenders into the same physical space. The increased visibility of potential victims may allow better judgments of their vulnerability and attractiveness (e.g., in terms of possession of valuables). Increased social activity outside the home may increase the number of unoccupied homes available for burglary. Increased illumination, for example, may make it easier to commit crimes and to escape.

The effects of street lighting interventions are likely to vary in different conditions. In particular, they are likely to be greater if the existing lighting is poor, unreliable, or non-existent and if the lighting improvements are considerable. Effects 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 street lighting interventions may also interact with other crime prevention measures, such as video surveillance cameras or security patrols.

It is important to note that not all of the different types of street lighting interventions under consideration in the current review may be fully supported by the aforementioned theoretical positions. For example, the dimming or turning off of street lights during certain times of the night (to reduce public expenditure) does not signal an investment in neighborhood conditions or lead to improved informal social control and community cohesion. On the other hand, the replacement of incandescent lights with LEDs (to improve energy efficiency) might signal an investment in neighborhood conditions, but may not lead to greater informal social control

and community cohesion (owing to concerns about decreased visibility from a reduction in brightness).

This report is divided into five parts. The second part provides some background on the use of street lighting interventions to prevent crime in public places. The third part, on research methods, describes the criteria for inclusion of evaluation studies, strategies used to search for new studies, the process of coding new studies and the protocol for carrying out the review, the meta-analytic techniques used, and the review's investigation of the displacement of crime and the diffusion of crime prevention benefits. The fourth part reports on the key characteristics of the included studies and the findings of the meta-analysis. The final part provides some concluding comments and explores the implications of the review's findings for research and policy.

Background

Contemporary interest in the effect of street lighting on crime dates back to the 1960s in the United States, a time when crime rates were increasing dramatically.[1] Many cities and towns across the country embarked upon major street lighting projects as a means of reducing crime, and initial results were encouraging (Wright et al., 1974).

The proliferation of projects across the U.S. led to a detailed review of the effects of street lighting in preventing crime by Tien and colleagues (1979), as part of the National Evaluation Program of the Law Enforcement Assistance Administration (LEAA). Their report described how 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 et al. (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 design, misuse or absence of sound analytic techniques, inadequate measures of street lighting, poor measures of crime (all were based on police

[1] The earliest recorded use of public street lighting to aid in the reduction of crime dates back to 1667 in France during the reign of Louis XIV. In later years, the king placed the responsibility for lighting under the control of street police (Zahm, 2004, p. 536).

records), and insufficient appreciation of the impact of lighting on different types of crime.

The review should have led to attempts to evaluate the effects of 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 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. for a long time.

In the United Kingdom, little research on street lighting and crime was conducted until the late 1980s (Fleming and Burrows, 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: Edmonton, Tower Hamlets, and Hammersmith/Fulham (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 desirable results, a major British 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, p. 24). However, as further evidence accumulated, there were more signs that street lighting could have an effect in reducing crime. A narrative review by Pease (1999, p. 68) considered that "the capacity of street lighting to influence crime has now been satisfactorily settled." 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?" (p. 72).

Shortly following these pronouncements, the Home Office commissioned the first systematic review of the effects of improved street lighting on crime (Farrington and Welsh, 2002a; see also Farrington and Welsh, 2002b). From a meta-analysis of the studies included in the review (n = 13), it was found that street lighting produced a significant and rather sizeable (20%)

reduction in crime.[2] The results were even more impressive for the five British studies included in the review, with a 30% reduction in crime. (The other eight studies were implemented in the U.S.) Little changed in the U.K. following the release of these findings by the government in the summer of 2002; that is, no part of the government's crime prevention policy included street lighting improvement measures and, to our knowledge, no public funds were expended on research in this area. In contrast, despite our conclusion that CCTV reduced crime by only 4% on average (Welsh and Farrington, 2002), there was massive UK investment in CCTV and the Home Office funded a large-scale national, multi-site evaluation of CCTV (see Farrington et al., 2007).

In the last two decades, there have been some notable developments in street lighting and crime prevention, as well as a renewed interest in evaluating street lighting projects. However, the first part of this period of time was anything but promising. As we found in the first update of our systematic review (Farrington and Welsh, 2007), there was a complete lack of high-quality evaluations of street lighting projects; this extended from 2001 through 2014. In fairness, evaluation research on street lighting continued during this time, but the methodological rigor of evaluations fell short by not including comparable control areas (e.g., Coumarelos, 2001; Evensen, 2010).

At the same time, new developments in and applications of street lighting technology were starting to generate policy interest, especially in the U.S. and U.K. (Welsh and Farrington, 2009), and this continues to present day. One example is the replacement of incandescent lighting with more energy-efficient LEDs (Walter, 2011). Closely tied to some of these changes in technology and application has been a renewed interest in evaluating street lighting projects, with new evaluations reported since 2015. However, of the seven high-quality evaluations of street lighting reported between 2015 and 2021 (each included in the current systematic review), not one focuses on traditional improved lighting. It is also important to note that some countries have begun spending on street lighting as part of crime prevention initiatives. In 2020, the British government, as part of its Safer Streets plan, allocated £25 million to street lighting and other situational crime prevention measures

[2] This percentage change is more accurately expressed as a reduction in crimes in experimental areas compared to comparable control areas.

to reduce burglary and theft in high crime neighborhoods (Telegraph, January 26, 2020).

Methods

This report presents the results of a second update of our systematic review on the effects of street lighting on crime, and it closely follows the methodology of this review technique. Systematic reviews utilize rigorous methods for locating, appraising, and synthesizing evidence from existing evaluation studies, and they are reported with the same level of detail that characterizes high-quality reports of original research. Systematic reviews "essentially take an epidemiological look at the methodology and results sections of a specific population of studies to reach a research-based consensus on a given study topic" (Johnson et al., 2000, p. 35). They have explicit objectives, explicit criteria for including or excluding studies, extensive searches for eligible evaluation studies from all over the world, careful extraction and coding of key features of studies, and a structured and detailed report of the methods and conclusions of the review. All of this contributes greatly to the ease of their interpretation and replication by other researchers. It is beyond the scope of this report to discuss all of the features of systematic reviews, but interested readers should consult key volumes on the topic (see Petticrew and Roberts, 2006; Welsh and Farrington, 2006).

Criteria for Inclusion of Evaluation Studies

In selecting evaluations for inclusion in this review, the following criteria were used:

1. Street lighting was the focus of the intervention. This included improved street lighting (e.g., increasing brightness) and a range of lighting technologies (e.g., LEDs, smart lights) and applications (e.g., switch-off lighting schemes). For evaluations involving one or more other interventions, only those evaluations in which lighting was the main intervention were included. The determination of what was the main intervention was based on the author identifying it as such or, if the author did not do this, the importance that the report gave to the lighting intervention relative to other interventions.

2. The intervention was implemented in a public place. By public place, we mean those places that individuals can make use of or visit in a free and unencumbered way. Typical public places include city and town centers,

parking lots or car parks that are available for public use, public housing communities, residential neighborhoods, and commercial areas. For residential neighborhoods and commercial areas, it is important to note that lighting schemes that are operated in private space (e.g., exterior lighting censors on homes or businesses) are not included.

3. There was an outcome measure of crime. The most relevant crime outcomes were property and violent crimes.

4. The evaluation design was of high-quality methodologically, with the minimum design involving before-and-after measures of crime in experimental and comparable control areas.

5. The total number of crimes in each area before the intervention was at least 20. The main measure of effect size was based on changes in numbers of crimes between the before and after time periods. It was considered that a measure of change based on a 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 were reluctant to exclude studies unless their numbers were clearly inadequate.

Search Strategies

In order to locate studies meeting the inclusion criteria, six search strategies were used:

1. Searches of electronic bibliographic databases: Criminal Justice Abstracts; National Criminal Justice Reference Service (NCJRS) Abstracts; Sociological Abstracts; Educational Resources Information Clearinghouse (ERIC); Government Publications Office Monthly Catalogue (GPO Monthly); Psychology Information (PsychInfo); HeinOnline; Dissertation Abstracts; Social, Pyschological, Educational, and Criminological Trials Register (C2-SPECTR); Google Scholar; and Medline. These 10 electronic databases were selected on the basis of the most comprehensive coverage of criminological, criminal justice, and social and behavioral science literatures. They are also among the top databases recommended by the Campbell Collaboration Crime and Justice Group. The following terms were used to search the 10 databases: street lighting, lighting, illumination, and natural surveillance. When applicable, "crime" and "crime prevention" were added to each of these terms (e.g., street lighting and crime) to narrow the search parameters.

2. Searches of reviews of the literature on the effects of street lighting on crime. Three new literature reviews were identified and assessed: Beyer and Ker (2009), Lester (2010), and Struyf (2020).

3. Searches of bibliographies of evaluation reports of street lighting studies.

4. Forward citation searches using Google Scholar. We used the "cited by" function in Google Scholar to conduct forward citation searches of all of the studies that met our inclusion criteria. We also conducted forward citation searches of the earlier systematic reviews on street lighting.

5. Searches of the scholarly networking site ResearchGate. ResearchGate is a scientific collaboration platform that allows researchers to promote and share their published and unpublished works.

6. Contacts with leading researchers (see Acknowledgements).

Both published and unpublished reports were considered in these searches. Furthermore, the searches were international in scope and were not limited to the English language. These searches were completed from March to April 2021, and reflect material reported over a 14-year period, between January 2007 and March 2021.

Coding and Protocol

The following key features of the included studies were coded: author and date, outlet (published or not), location, context of intervention, type of intervention (and any secondary interventions), sample size, outcome measure and data source, and evaluation design (with before and after periods of time).

A coding protocol was established by the research team. The first step involved the researchers confirming the criteria for inclusion of studies and the measures to be coded. Next, studies were collected and the coding was carried out by one of the researchers. The researchers met and communicated periodically to discuss the coding of all of the studies and resolve any questions.

Effect Sizes and Meta-Analyses

In an evaluation of the effects of street lighting on crime, the most basic information that is available in most evaluations is as follows:

	Before	After
Experimental	a	b
Control	c	d

where a, b, c, d are numbers of crimes in the experimental area (which is relit after but not before) and the control area.

In their first review of this topic, Farrington and Welsh (2002a) proposed the odds ratio [OR = (a * d)/(b * c)] as an intuitively appealing and meaningful measure of the effects of street lighting on crime. They stated that:

The odds ratio has a very simple and meaningful interpretation. It indicates the proportional change in crime in the control area compared with the experimental area. An odds ratio greater than 1.0 indicates a desirable effect of improved lighting, while an odds ratio less than 1.0 indicates an undesirable effect. In this example, the odds ratio of 1.39 [in Atlanta] indicates that crime increased by 39 per cent in the control area compared with the experimental area. An odds ratio of 1.39 could also indicate that crime decreased by 28 per cent in the experimental area compared with the control area, since the change in the experimental area compared with the control area is the inverse of the odds ratio, or (1/1.39) here. (Farrington and Welsh, 2002a, p. 28)

The variance of the OR is obtained from the following equation:

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

where LOR = Ln(OR)

Unfortunately, with numbers of crimes rather than numbers of individuals, this equation may underestimate the true variance. Farrington and Welsh (2006, pp. 213-214) noted the following:

This estimate of the variance is based on the assumption that total numbers of crimes (a, b, c, d) have a Poisson distribution. Thirty years of mathematical models of criminal careers (see e.g. Blumstein et al., 1986; Piquero et al., 2003) have been dominated by the assumption that the commission of crimes can be accurately modelled by a Poisson process. If the number of crimes has a Poisson distribution, its variance should be the same as its mean. However, the large number of changing extraneous factors may cause over-dispersion; that is, where the variance of the number of crimes

VAR exceeds the number of crimes N. [The equation] D = VAR/N specifies the over-dispersion factor. Where there is over-dispersion, VAR(LOR) should be multiplied by D. Our best estimate was that the true value of D was about 2. Hence, VAR(LOR) calculated from the usual formula above was doubled in all cases.

Farrington and Welsh (2004) offer a more detailed discussion of the variance in this case.

In their previous report for the Swedish National Council for Crime Prevention on the effects of street lighting on crime, Farrington and Welsh (2007) used an over-dispersion equation derived by Farrington et al. (2007) in their analyses of the effects of closed-circuit television (CCTV) on crime:

D = .0008 * N + 1.2

This was based on the mean and variance of 70 sets of monthly crime figures in the UK. Farrington and Welsh (2007, p. 18) concluded that the mean value of D was about 1.56.

In their Campbell Collaboration review, Welsh and Farrington (2008) used the same equation for D, but now referred to the "Relative Effect Size" (RES) rather than to the OR. We will use RES rather than OR in the current report.

As a current example, monthly crime numbers were available in the evaluation by Davies and Farrington (2020). They compared two areas of Essex (U.K.) in 2006 and 2008: Maldon (where many street lights were switched off in 2007) and Braintree (where there were no changes in street lighting during this time period). They analyzed police data on criminal damage, vehicle crime, burglary, and violence. The D values (measures of over-dispersion based on the ratio of the monthly variance to the monthly mean) were 1.37 for Maldon in 2006, 1.85 for Maldon in 2008, 2.97 for Braintree in 2006, and 2.40 for Braintree in 2008, for an average of 2.15. These D values are overestimates because the monthly variance is inflated by seasonal variations. Nevertheless, in the interests of drawing conservative conclusions, we assumed a D value of 2 in estimating the variance of each effect size.

In deriving weighted mean estimates of the effect of street lighting from several studies (e.g., in the U.K.), we used the Multiplicative Variance Adjustment (MVA) model, described by Farrington and Welsh (2013). The

most commonly used meta-analytic methods are the fixed effects and random effects models, but both have serious flaws. The fixed effects model weights each effect size by the inverse of its variance (thereby appropriately giving more weight to larger studies), but it assumes that each observed effect size is normally distributed about the true effect size. Unfortunately, based on the Q statistic for heterogeneity, it is commonly found that the effect sizes are significantly more heterogeneous than this. In order to decrease heterogeneity, the random effects model adds a constant to the variance of each effect size. But, unfortunately, this often tends to give a similar weight to each effect size in calculating the weighted mean effect size, which is undesirable.

The MVA model overcomes both of these problems by multiplying each VAR(LOR) by Q/df (where df is the number of degrees of freedom, or the number of studies minus 1). The weighted mean effect size is the same as in the fixed effects model, but its variance is greater, because the MVA model exactly adjusts for heterogeneity (yielding Q = df). It also overcomes the problem of adjusting for over-dispersion, because it does not matter whether D is 1.5, 2, or any other value up to Q/df. This is because the weighted mean effect size and its variance will be unchanged. Thus, the MVA model yields the most valid measures of the weighted mean effect size and its variance.

Crime Displacement and Diffusion of Crime Prevention Benefits

This review also reports on displacement of crime and diffusion of crime prevention benefits. Displacement is often defined as the unintended increase in targeted crimes in other locations following from the introduction of a crime reduction scheme.[3] Reppetto (1976) identified five different forms of displacement: temporal (change in time), tactical (change in method), target (change in victim), territorial (change in place), and functional (change in type of crime). Diffusion of benefits is defined as the unintended decrease in crimes following from a crime reduction scheme, or the "complete reverse" of displacement (Clarke and Weisburd, 1994; see also Johnson et al., 2014).

In order to investigate these topics, the minimum research design should involve one experimental area, one adjacent area, and one non-adjacent comparable control area. If crime decreased in the experimental area, increased in the adjacent area, and stayed constant in the control area, this

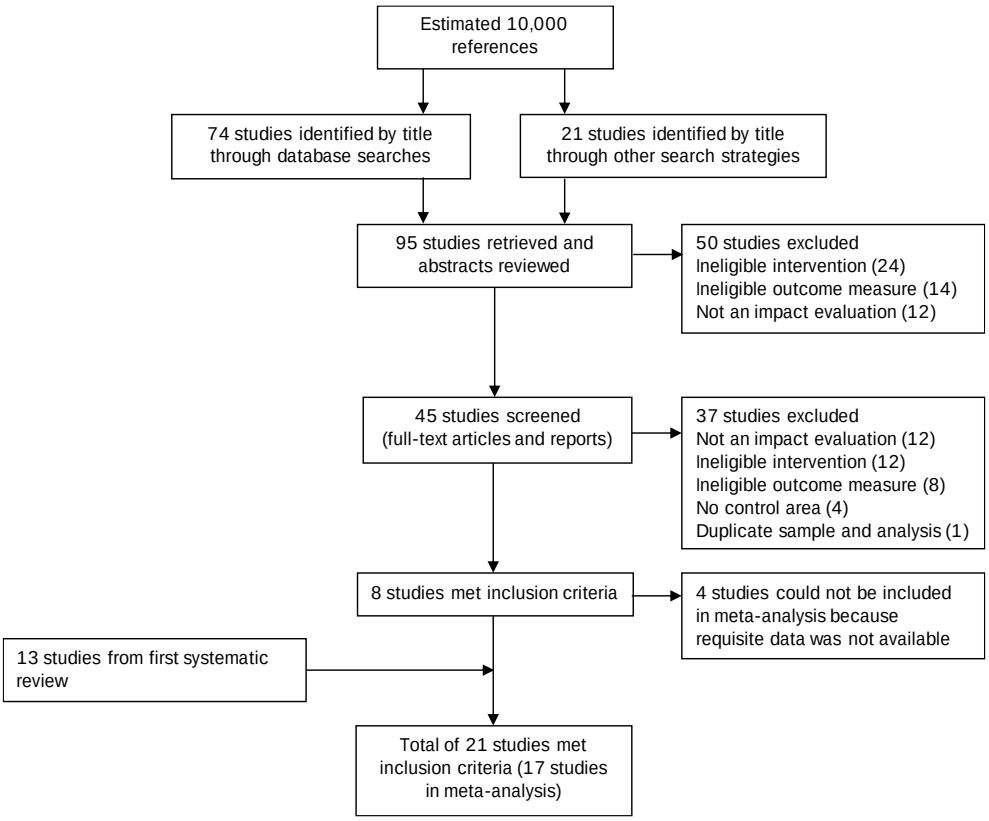
[3] See Barr and Pease (1990) for a discussion of "benign" or desirable effects of displacement.

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.

Findings

Figure 1 summarizes the process of identifying, collecting, and screening new studies that met the criteria for inclusion in the systematic review. The search strategies yielded an estimated 10,000 references. Based on a review of titles, 95 studies (74 from electronic bibliographic databases and 21 from other search strategies) were identified as potentially relevant. Upon retrieval and review of abstracts, more than one-half of the studies (n = 50) were excluded, because of an ineligible intervention (n = 24) or outcome measure (n = 14) or because of no impact evaluation (n = 12). The next stage entailed full-text screening of the remaining 45 studies. From these studies, eight met the inclusion criteria. The other 37 studies were excluded for the following reasons: (a) not an impact evaluation (n = 12); (b) ineligible intervention (n = 12); (c) ineligible outcome measure (n = 8); (d) no control area (n = 4); and (e) duplicate sample and analysis (n = 1). With the addition of 13 studies from the first systematic review, the current review reports on the findings of a total of 21 studies.

Figure 1, Flowchart for selection of studies



Details of the Included Studies

Table 1 summarizes key characteristics of the 21 included studies. The studies originated in four countries (U.S., U.K., Brazil, and South Korea), with the vast majority taking place in the U.S. (n = 12) and the U.K. (n = 7). A full two-thirds of the studies (n =14) are more than 20 years old, with the other seven reported in the last six years. There was much variability in the context of intervention, with most of the studies implemented in either residential areas (n = 6) or city centers (n = 6). On the matter of the type of intervention, two-thirds of the studies (n = 14) focused on improved lighting, but only eight specified the degree of improvement in the lighting: by seven times in Milwaukee, five times in Stoke-on-Trent, four times in Atlanta and Chicago (Morrow and Hutton, 2000), three times in Fort Worth, and two times in Portland, Bristol, and Dudley. One study assessed the implementation of 'smart' lighting with adaptive brightness capabilities on city street corners (in San Diego), and another evaluated the impact of

improved access to electricity in rural municipalities (across Brazil). Three studies assessed the absence of lighting through the evaluation of switch-off lighting interventions (n = 2) or street lighting outages (n = 1). Only two of the 21 studies involved other interventions, which are considered secondary to the street lighting intervention.

Table 1, Summary of street lighting evaluations

Author, Publication Date, Location	Context of Intervention	Type of Intervention (Other Interventions)	Sample Size	Outcome Measure and Data Source	Research Design	Results and Displacement/ Diffusion
Atlanta Regional Commission (1974), Atlanta (GA), USA	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	Desirable effect; no displacement
Department of Intergovernmental Fiscal Liaison (1974), Milwaukee (WI), USA	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	Desirable effect; some displacement
Inskeep and Goff (1974), Portland (OR), USA	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	Null effect; displacement and diffusion did not occur
Wright et al. (1974), Kansas City (MO), USA	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	Desirable effect (for violence); some displacement
Harrisburg Police Department (1976), Harrisburg (PA), USA	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	Null effect; no displacement

Author, Publication Date, Location	Context of Intervention	Type of Intervention (Other Interventions)	Sample Size	Outcome Measure and Data Source	Research Design	Results and Displacement/ Diffusion
Sternhell (1977), New Orleans (LA), USA	Residential and commercial areas	Improved street lighting (none)	E=2 high crime areas; C=2 adjacent areas	Crime (burglary, vehicle theft, and assault); police records	Before-after, experimental-control; before period = 51 months, after period = 29 months	Null effect; no displacement
Lewis and Sullivan (1979), Fort Worth (TX), USA	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	Desirable effect; possible displacement
Poyner (1991), Dover, UK	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	Desirable effect (for theft of vehicles); no displacement
Shaftoe (1994), Bristol, UK	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	Desirable effect; displacement and diffusion not measured
Poyner and Webb (1997), Birmingham, UK	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)	Desirable effect; no displacement and some diffusion
Painter and Farrington (1997), Dudley, UK	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	Desirable effect; no displacement

Author, Publication Date, Location	Context of Intervention	Type of Intervention (Other Interventions)	Sample Size	Outcome Measure and Data Source	Research Design	Results and Displacement/ Diffusion
Quinet and Nunn (1998), Indianapolis (IN), USA	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	Null effect; no displacement
Painter and Farrington (1999), Stoke-on-Trent, UK	Local authority housing estate	Improved (5x) street lighting (none)	E=1 housing estate; A=2 adjacent estates; C=2 non-adjacent estates	Crime (total and types of offenses); victim survey	Before-after, experimental-control and statistical analyses; before and after periods = 12 months	Desirable effect; diffusion, no displacement
Morrow and Hutton (2000), Chicago (IL), USA	City center (high crime)	Improved (4x) lighting in alleys (none)	E=1 multi-block area; C=1 non-adjacent multi-block area	Crime (total and types of offenses); police records	Before-after, experimental-control; before and after periods = 6 months	No effect; displacement and diffusion not measured
Perkins et al. (2015), UK	Local authority areas in England and Wales	Switch-off lighting (off permanently), part-night lighting (off 12am-6am), white lights/LEDs, and dimming lights (none)	62 local authorities (Middle Super Output Areas, approx. 3200 households each)	Crime (burglary, theft of or from a vehicle, robbery, and violence); police records and victim survey data	Controlled-interrupted time series analysis; study period = 36 months	Mixed effects (white light and dimming weakly associated with reductions in total crime); displacement and diffusion not measured
Arvate et al. (2018), Brazil	Local municipalities	Improved access to electricity in municipalities with less than 85% coverage (none)	5,457 municipalities (approx. 20,000 residents each)	Homicide (homicides per 100,000 residents); hospital records	Panel design with instrumental variables (compares homicide rates in 2000 to rates in 2010 across eligible and non-eligible municipalities)	Desirable effect; displacement and diffusion not measured

Author, Publication Date, Location	Context of Intervention	Type of Intervention (Other Interventions)	Sample Size	Outcome Measure and Data Source	Research Design	Results and Displacement/ Diffusion
Kang and Yeom (2019), Seoul, South Korea	Single autonomous city district	Installation of LED street lights (none)	E=7 multi-block areas; C=7 adjacent multi-block areas	Crime (theft, violence, and sexual violence); police records	Before-after, experimental-control; before and after periods = 12 months	Desirable effect; some displacement
Mihale-Wilson et al. (2019), San Diego (CA), USA	City center	Smart lights installed at experimental street corners (none)	E=14 street corners; C=78 street corners	Crime (total and types of offenses); police records	Before-after, experimental-control; before and after periods = 6 months	Desirable effect; displacement and diffusion not measured
Davies and Farrington (2020), Essex, UK	Local council districts	Part-night lighting (off 11.30 pm to 5.30 am) (none)	E=1 district (Maldon); C=1 district (Braintree)	Crime (burglary, criminal damage, vehicle crime, and violence); police records	Before-after, experimental-control; before and after periods = 36 months	Desirable effect (for burglary and vehicle crime); displacement and diffusion not measured
Chalfin et al. (2021a), New York City (NY), USA	Residential neighborhoods (high crime)	397 temporary lighting towers installed in experimental areas (none)	E=40 public housing developments; C=40 non-adjacent public housing developments	Crime (murder, robbery, felony assault, burglary, grand larceny, motor vehicle theft); police records	Randomized controlled experiment; before period = 24 months, after period = 6 months	Desirable effect; displacement and diffusion did not occur
Chalfin et al. (2021b), Chicago (IL), USA	City streets with lighting outages	Minor outages (1-2 lights out) and major outages (more than 2 lights out) (none)	368,000 outages at ~50,000 street segments over 2,808 days (8 years)	Crime (violent crimes, property crimes, robbery, assault, and motor vehicle theft); police records	Natural experiment comparing crime before and after repair of lighting outage; before period = up to 7 days, after period = 4	No effect; some displacement, no diffusion

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

There was a fair amount of variability in sample size and the (geographical or spatial) unit of analysis, which included high crime areas, police beats, housing estates or public housing developments, and municipalities. Nearly

all of the included studies used police records as a measure of crime (n = 17). Three studies used victim surveys as the measure of crime, one in combination with police records (Perkins et al., 2015) and another with self-reported delinquency (Painter and Farrington, 2001a). Arvate et al. (2018) used hospital records as a measure of homicide. All of the studies used high-quality designs to evaluate the impact of street lighting on crime, and among the eight new studies was the first randomized controlled experiment of street lighting (Chalfin et al., 2021a).

The time periods during which the effects of street lighting were investigated were usually quite short. Only four of the studies had time periods greater than 12 months before and after the intervention: Sternhell (1977) in New Orleans studied 51 months before and 29 months after; Poyner (1991) in Dover studied 24 months before and 24 months after; and Perkins et al. (2015) in the UK and Davies and Farrington (2020) in Essex studied 36 months before and 36 months after. Only Davies and Farrington (2020) reported the effects in different time periods. Comparing 12 months before with 12 months after, switching off street lighting overnight led to increases in burglary and vehicle crime but to decreases in violence. Comparing 36 months before with 36 months after, switching off street lighting still led to increases in vehicle crime, but the changes in other types of crime were no longer significant. More research is clearly needed on whether the effects of street lighting persist or wear off over longer time periods.

Narrative Synthesis of Effects of the Eight New Studies

As shown in Table 1, five of the eight new studies found that that street lighting interventions were effective in preventing crime (Brazil, Seoul, San Diego, Essex, and New York City), two studies found that lighting had no significant effect on crime (both Chicago studies), and the other study reported mixed effects across a wide range of crimes (United Kingdom).
The first Chicago study (Morrow and Hutton, 2000) evaluated the effects of improved lighting in alleys on multiple crime types. Crime counts were compared across two multi-block areas (one experimental and one control) six months before and six months after lighting had been improved in alleys within the experimental area. Violent crimes increased by 31.6% (57 to 75) in the experimental area and by 1.3% (75 to 76) in the control area. Property crimes increased in both conditions: by 76.9% (13 to 23) in the experimental area and by 37.5% (16 to 22) in the control area. Crimes during the night-time hours increased by 40.0% in the experimental area (205 to 287) and by

19.3% in the control area (166 to 198), while crimes during the daylight hours decreased by 20.8% (240 to 190) in the experimental area and by 23.0% (365 to 281) in the control area. Overall, improved lighting in alleys in Chicago did not lead to a reduction in crimes, and increases in crimes in the experimental area compared to the control area were not significant.

In the United Kingdom study (Perkins et al., 2015), which was carried out across England and Wales, reduced street lighting was found to have an overall non-significant effect on crime. Three of the four lighting adaptations (permanent switching-off lighting, part-night lighting, and dimmed lighting) reduced lighting on road segments, and the other improved lighting through the installation of LEDs (Light-Emitting Diodes or white light). An interrupted controlled time series design was used to estimate the association between the proportion of total road/street coverage of each lighting intervention (in kilometers) and crime in 62 local authorities over the 36-month study period.

Based on rate ratios (RR) and 95% confidence intervals (CI), which indicate the expected change in crime if 100% of roads in an area had one of the four lighting adaptations, the authors found that dimmed lighting (RR = 0.84, CI: 0.70-1.02) and white light (RR = 0.89, CI: 0.77-1.03) were weakly associated with reductions in total crime. Switching-off (RR = 0.11, CI: 0.01-2.75) and part-night lighting (RR = 0.96, CI: 0.86-1.06) were found to be not associated with changes in total crime. There was some evidence that part-night lighting may increase robberies (RR = 1.48, CI: 0.99-2.21) and dimmed lighting may decrease total violent crimes (RR = 0.78, CI: 0.60-1.01).

The study in Brazil (Arvate et al., 2018) evaluated the impact of an electrification program designed to improve street lighting coverage in rural municipalities on homicide rates in those communities. Using the program's eligibility criterion of municipalities with less than 85% household electricity access (in the year 2000) to approximate the prevalence of street lighting, electricity coverage and homicide rates (in the years 2000 and 2010) were compared across eligible and non-eligible municipalities (5,457 municipalities were included in the sample). On average, electricity coverage increased from 86% to 97%.

The use of instrumental variables and a regression discontinuity design allowed an assessment of the effects of the electrification program on homicide rates (measured using hospital records). After dividing the country into five macro-regions (North, Northeast, Southeast, South, and Midwest), the electrification program was found to be primarily concentrated in municipalities in the North and Northeast regions, where coverage had been lowest in 2000 and increased the most between 2000 and 2010. On average, the eligible municipalities experienced a reduction of 91.76 homicides on public streets and 17.61 homicides in hospitals (per 100,000 inhabitants). The authors suggest that electrification may have a greater desirable impact on homicide rates in municipalities with large rural populations, such as those in the Northeast region, than in those that had high levels of electrification prior to 2000.

The study in Seoul, South Korea (Kang and Yeom, 2019), compared crime counts across 14 multi-block residential areas (seven experimental and seven control) 12 months before and 12 months after the installation of LEDs in the experimental areas. The authors found that night-time crimes decreased by 4.3% (299 to 286) in the experimental areas and increased by 9.1% (471 to 514) in the control areas. Regarding specific crime types, thefts decreased by 33.6% (128 to 85) in experimental areas and increased by 6.6% (167 to 178) in control areas; conversely, violent crimes increased in both conditions: by 25.5% (145 to 182) in experimental areas and by 29.0% (245 to 316) in control areas. It is important to note that only the decrease in thefts reached statistical significance. The authors also found that some crime was displaced from the centers of experimental areas (where most of the lighting interventions were implemented) toward their boundaries.

In the San Diego study (Mihale-Wilson et al., 2019), smart lights with adaptive brightness capabilities were found to produce a significant reduction in crimes. Smart lights were installed on 14 street corners within a single multi-block area in downtown San Diego, representing the experimental condition. The remainder of the street corners in this area (n = 78) maintained their regular lighting and served as the control condition. Total crimes and specific crime types (including property and violent crimes) at experimental and control corners were compared six months before and six months after the implementation of the smart lights. It was found that total crimes (night and day) decreased by 52.8% (163 to 77) at experimental corners, with a smaller decrease of 2.5% (595 to 580) at control corners.

Night-time crimes decreased by 23.8% (42 to 32) at experimental corners and increased by 57.6% (217 to 342) at control corners. Property crimes (occurring at night) decreased by 20.0% (10 to 8) in experimental areas and increased by 79.2% (53 to 95) in control areas. Violent crimes decreased in both experimental and control areas.

In the study in Essex, England (Davies and Farrington, 2020), street lights were switched off in the experimental area and left on in the control area. Considered a part-night lighting intervention, lights were turned off in the experimental area between 11.30 p.m. and 5.30 a.m. Monthly counts of several crime types were compared across two adjacent council districts (one experimental and one control) 36 months before and 36 months after the implementation of the intervention. The results showed that switching off the street lights had undesirable effects on burglary and vehicle crimes but desirable effects on violent crimes. For example, based on the mean per 1,000 residents, the authors found that vehicle crimes decreased by 2.7% (26.0 to 25.3) in the experimental area, but decreased by 29.0% (28.6 to 20.3) in the control area. Another analysis focused on the period of 12 months before and 12 months after the intervention, finding that burglaries decreased by 12.8% (19.5 to 17.0) in the experimental area, but decreased by 30.0% (28.4 to 20.0) in the control area. However, violence decreased by 14.7% (58.5 to 49.9) in the experimental area, but increased by 7.4% (60.9 to 65.4) in the control area. Davies and Farrington (2020) suggested that the switching off of street lighting might have deterred people from going out at night, resulting in fewer violent incidents. The authors found no evidence of crime displacement or diffusion of crime prevention benefits.

The New York City study (Chalfin et al., 2021a) represents the first randomized controlled experiment of a street lighting intervention. A total of 80 public housing developments that were identified as high-priority by the New York City Housing Authority were randomly assigned to experimental and control conditions. Temporary lighting towers (a minimum of two per housing development) were installed in the experimental areas between February and March of 2016, and the towers remained illuminated during all night-time hours for six subsequent months.

In order to evaluate the effects of the lighting intervention on crime, the average count of Index[4] crimes occurring between March and August in 2011 to 2015 (a total of 24 months before implementation) were compared with the period of March to August in 2016 (a total of six months after implementation). It was found that the experimental area experienced a 35% reduction in outdoor night-time crimes, which equated to a reduction of approximately 4% in total Index crimes in the housing developments. The authors also found no evidence of crime displacement.

The most recent study in Chicago (Chalfin et al., 2021b) evaluated the short-term impact of street light outages on crime at city street segments. Based on the knowledge that the repair and maintenance of street lights are a municipal responsibility, and that they tend to be repaired very quickly in most cities, the study examined changes in reported crimes at street segments for up to seven days before and four days after a street light is repaired. Using crime records and 311 call data from the Chicago Police Department over a period of eight years, it was found that street segments experienced a small and non-significant increase in total crimes (2%), violent crimes (1%), and property crimes (1%). The authors found some evidence of crime displacement.

Meta-Analysis

Table 2 summarizes the results of the 17 studies that provided the requisite data for inclusion in the meta-analysis. This shows the relative effect size (RES) for total crime in each study plus its 90% confidence interval and statistical significance. It can be seen that only five studies (Portland, New Orleans, Indianapolis, Chicago [Morrow and Hutton, 2000], and Essex) had a RES less than 1.0, meaning that street lighting was followed by an increase in crime, and in no case was this increase significant. The other 12 studies had a RES greater than 1.0, meaning that street lighting was followed by a decrease in crime, and in seven studies this decrease was significant (or nearly so). For example, in San Diego, there were 163 crimes in the experimental area before the street lighting intervention and 77 after, while there were 595 crimes in the control area before and 580 after. These numbers yielded RES = 2.06 (CI: 1.46-2.93, p = .0003), indicating a significant and highly desirable effect of street lighting on crime.

[4] Part I Index crimes include murder and non-negligent manslaughter, robbery, felony assault, burglary, grand larceny, and motor vehicle theft.

Table 2, Effects of street lighting on total crime

	RES	90% Confidence Interval	Z Score	P Value
US/SK N Studies				
Portland	0.94	0.76-1.16	-0.47	n.s.
Kansas City	1.24	0.90-1.72	1.10	n.s.
Harrisburg	1.02	0.71-1.48	0.10	n.s.
New Orleans	0.99	0.85-1.15	-0.14	n.s.
Seoul	1.15	0.90-1.47	0.95	n.s.
US ND Studies				
Atlanta	1.39	0.98-1.96	1.57	.058
Milwaukee	1.37	1.01-1.86	1.67	.047
Fort Worth	1.38	0.91-2.11	1.27	n.s.
Indianapolis	0.75	0.43-1.29	-0.88	n.s.
Chicago (2000)	0.99	0.80-1.23	-0.04	n.s.
San Diego	2.06	1.46-2.93	3.41	.0003
UK ND Studies				
Dover	1.14	0.56-2.33	0.30	n.s.
Bristol	1.35	1.21-1.50	4.67	.0001
Birmingham	3.82	2.07-7.05	3.60	.0002
Dudley	1.44	1.11-1.86	2.30	.011
Stoke-on-Trent	1.71	1.07-2.75	1.89	.029
Essex	0.98	0.88-1.10	-0.23	n.s.
Summary Results				
5 US/SK N studies	1.03	0.95-1.11	0.59	n.s.
6 US ND studies	1.25	1.02-1.53	1.81	.035
6 UK ND studies	1.21	1.03-1.42	1.98	.024
10 US studies	1.10	0.98-1.23	1.29	.099
12 ND studies	1.22	1.09-1.37	2.85	.002
All 17 studies	1.16	1.06-1.27	2.78	.003

Notes: Chicago (2000) is by Morrow and Hutton (2000); US = United States; SK = South Korea; UK = United Kingdom; N = only night crimes measured; ND = night and day crimes measured; RES = relative effect size; n.s. = non-significant; p values are one-tailed.

It is important to note that most values of RES are based on similar calculations. However, in Essex, street lights were switched off in the experimental area. Therefore, the lights were on in the before period and off in the after period. There were 1359 crimes in the experimental area before and 1111 after, compared with 3489 crimes in the control area before and 2897 after. In order to measure the effect of this intervention the usual

formula was inverted, so that RES = (1111 * 3489)/(1359 * 2897) = 0.98 (CI: 0.88-1.10, n.s.), indicating no overall effects on crime of the switching off of street lights. It is also noteworthy that all numbers of crimes in Table 2 were based on police records, except in the Dudley and Stoke-on-Trent studies, where they were based on victim reports.

Table 2 also shows the overall effect of street lighting interventions on crime. In pooling the effects of the 17 studies, it was found that street lighting had a significant desirable effect on total crime, with a weighted mean RES = 1.16 (CI: 1.06-1.17, p = .003). This means that crimes increased by 16% after street lighting in control areas compared with experimental areas or, conversely, crimes decreased by 14% (1/1.16) in experimental areas compared with control areas. Compared with our prior systematic review (Farrington and Welsh, 2007), this represents a sizeable reduction (from 21% to 14%) in the effectiveness of street lighting in preventing crime. This is largely accounted for by variability in street lighting interventions (improved street lighting was the focus of all 13 studies in the prior review) and a more conservative meta-analytic approach used in the present review. As in the prior review, this review found that the desirable effects of street lighting interventions were greater in studies that measured both night and day crimes than in studies that only measured night crimes (see Table 2). For the 12 night/day studies, RES = 1.22 (CI: 1.09-1.37, p = .002), meaning that crimes decreased by 18% (1/1.22) in experimental areas compared with control areas. In contrast, for the five night only studies, RES = 1.03 (CI: 0.95-1.11, n.s.), indicating a non-significant 3% (1/1.03) decrease in 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 or deterrence.

For the two main countries in which street lighting interventions have been evaluated, it was found that desirable effects were greater in U.K. studies than in U.S. studies (see Table 2). For the six U.K. studies, RES = 1.21 (CI: 1.03-1.42, p = .024). For the 10 U.S. studies, RES = 1.10 (CI: 0.98-1.23, p = .099). However, a like-with-like comparison of the night/day studies for the two countries (n = 6 for each country) shows quite similar desirable effects of street lighting interventions on crime: U.K. RES = 1.21 (CI: 1.03-1.42, p = .024) versus U.S. RES = 1.25 (CI: 1.02-1.53, p = .035).

It was also possible to use meta-analytic techniques to investigate the effects of street lighting interventions on violent and property crimes, the two main types of crimes that were measured in the studies. Violent crimes were measured in 13 studies, and property crimes were measured in 15 studies. Table 3 shows that street lighting interventions were followed by a significant reduction in property crimes (RES = 1.14, CI: 1.03-1.27, p = .018), but not in violent crimes (RES = 0.99, CI: 0.87-1.13, n.s.). Two points are noteworthy about this effect on property crimes. This significant desirable effect closely approximates the overall effect of street lighting interventions on total crime (RES = 1.16, CI: 1.06-1.27, p = .003; see Table 2), suggesting that the overall effect on crime may be largely a function of street lighting's impact on property crimes. Also, as shown in Table 3, the effects on property crimes does not change considerably when one study (Birmingham; RES = 3.82, CI: 2.07-7.05, p = .0002), which could be considered an outlier, is excluded from the analysis. Here, RES = 1.12 (CI: 1.03-1.21, p = .011) for the effects of the other 14 studies on property crimes.

Table 3, Effects of street lighting on violent and property crimes

	RES	90% Confidence Interval	Z Score	P Value
Violent Crime				
Portland	1.04	0.78-1.37	0.22	n.s.
Kansas City	1.79	1.12-2.86	2.04	.021
Harrisburg	0.81	0.44-1.50	-0.56	n.s.
New Orleans	0.86	0.64-1.16	-0.82	n.s.
Seoul	0.98	0.72-1.34	-0.09	n.s.
Atlanta	1.20	0.80-1.80	0.74	n.s.
Milwaukee	1.09	0.42-2.83	0.15	n.s.
Chicago (2000)	1.08	0.73-1.59	0.31	n.s.
San Diego	2.01	1.00-4.05	1.64	.050
Bristol	0.48	0.20-1.16	-1.36	.087
Dudley	1.77	1.09-2.89	1.93	.027
Stoke-on-Trent	1.89	0.49-7.31	0.77	n.s.
Essex	0.79	0.65-0.95	-2.05	.020
All 13 studies	0.99	0.87-1.13	-0.11	n.s.
Property Crime				
Portland	0.83	0.60-1.14	-0.97	n.s.
Kansas City	0.88	0.56-1.39	-0.46	n.s.
Harrisburg	1.14	0.71-1.82	0.45	n.s.
New Orleans	1.03	0.87-1.23	0.31	n.s.
Seoul	1.61	1.06-2.42	1.89	.029
Atlanta	1.47	0.76-2.84	0.96	n.s.
Milwaukee	1.03	0.69-1.52	0.11	n.s.
Chicago (2000)	0.68	0.35-1.32	-0.96	n.s.
San Diego	1.42	0.72-2.80	0.85	n.s.
Bristol	1.57	1.06-2.35	1.87	.031
Dudley	1.33	0.97-1.81	1.50	.067
Stoke-on-Trent	1.59	0.97-2.61	1.55	.061
Essex	1.11	0.97-1.27	1.26	n.s.
Dover	1.14	0.56-2.33	0.30	n.s.
Birmingham	3.82	2.07-7.05	3.60	.0002
All 15 studies	1.14	1.03-1.27	2.10	.018
(minus Birmingham)	1.12	1.03-1.21	2.29	.011

Notes: Chicago (2000) is by Morrow and Hutton (2000); RES = relative effect size; n.s. = non-significant; p values are one-tailed.

Crime Displacement and Diffusion of Crime Prevention Benefits

As shown in Table 1, 15 of the 21 included studies investigated for potential crime displacement, diffusion of crime prevention benefits, or both. In each case, the focus was on geographical or spatial displacement or diffusion, with

3 studies (Seoul, New York City, and Chicago [Chalfin et al., 2021b]) also examining temporal displacement, diffusion, or both. Of these 15 studies, 10 reported that displacement did not occur, and five reported that it was evident to some extent. Five of these studies assessed the potential for diffusion as well as displacement, with two studies (Birmingham and Stoke-on-Trent) reporting some evidence for diffusion of crime prevention benefits. Disappointingly, only three of the 15 studies (Portland, Stoke-on-Trent, and New York City) used the minimum design to investigate these topics—by including one experimental area, one adjacent area, and one non-adjacent comparable control area. Each of these studies reported no evidence of crime displacement.

Cost-Benefit Analyses

Five of the studies carried out cost-benefit analyses. Painter and Farrington (2001b) estimated that, in Dudley, the cost savings from reduced crimes after one year totaled £339,186, while the extra cost of the improved lighting was £54,815. This yielded a benefit-to-cost ratio of 6.2 to 1. More reasonably, assuming that the capital cost of the improved lighting was paid off over 20 years, the benefit-to-cost ratio was 74 to 1. In the similar study in Stoke-on-Trent, the cost savings from reduced crimes after one year totaled £188,170, while the extra cost of the improved lighting was £78,173. This yielded a benefit-to-cost ratio of 2.4 to 1. More reasonably, assuming that the capital cost of the improved lighting was paid off over 20 years, the benefit-to-cost ratio was 24 to 1. Chalfin et al. (2019) in New York City also carried out a cost-benefit analysis and estimated that, over a 10-year period, the benefit-to-cost ratio of improved lighting (based on reduced crimes) was 4 to 1.

The other two studies carried out a cost-benefit analysis of the effects of part-night lighting, but their results are somewhat inconsistent. In their quasi-experimental analysis, Davies and Farrington (2020) found that switching off lighting from 11.30 p.m. to 5.30 a.m. led to an increase in burglary and vehicle crimes but to a decrease in violence. Because the cost of violence is much greater than the cost of burglary and vehicle crimes, they estimated that part-night lighting saved £43,685 per year in crime costs, as well as £70,000 per year in energy costs. In their interrupted time series analysis, Perkins et al. (2015) concluded in their abstract that "Overall, there was no evidence that reduced street lighting was associated with crime". However, they found that switching off street lighting from 12.00 p.m. to 6.00 a.m. led to decreases in road collisions, burglary, vehicle crimes, and carbon

emissions, but to increases in robbery and violence. They estimated that, over five years, part-night lighting cost £74,754 more than maintaining the street lighting, but had total benefits of £2,029,519, yielding a benefit-to-cost ratio of 27 to 1.

In summary, the three evaluations of the effects of improved street lighting show that its benefits (in terms of reduced crimes) outweigh its costs. The two studies of part-night lighting are inconsistent in finding different effects on different types of crimes, but overall they suggest that the benefits of part-night lighting outweigh its costs. Clearly, more cost-benefit analyses of street lighting are needed.

Discussion and Conclusions

In the first update of our systematic review, representing a full 6 years (from January 2001 to December 2006; Farrington and Welsh, 2007), we reported that research on the effects of street lighting on crime "appears to have come to a standstill" (p. 25). This was based on two factors: (a) not finding any new study that met the criteria for inclusion in the systematic review; and (b) only finding a total of two evaluations of street lighting during this period of time (both were excluded for not meeting one criterion or another).

This is no longer the case. As we find in the current update of the systematic review, there has been a renewed interest in evaluation research on street lighting and crime. Of the eight new studies that met our inclusion criteria, seven were reported since 2015. In addition, many more evaluations of street lighting interventions were identified that did not meet the inclusion criteria and thus were excluded (covering the full 14-year period of the current review). It seems that this uptick in evaluation research is closely tied to new developments in and applications of street lighting technology.

Based on the full complement of studies that met our inclusion criteria (n = 21), street lighting continues to be an effective intervention in preventing crime in public places. In pooling the effects of the 17 studies that could be included in the meta-analysis, we find that street lighting led to a significant 14% decrease in crime in experimental areas compared with comparable control areas. While not every study was effective in preventing crime, desirable effects of street lighting interventions were greater in studies that measured both night and day crimes than in studies that only measured night crimes. This translates to a significant 18% decrease in crime for night/day studies compared to a non-significant 3% decrease in crime for night only studies. These findings suggest 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 or deterrence.

It was also possible to investigate the effects of street lighting interventions on violent and property crimes, the two main types of crimes that were measured in the studies. Street lighting interventions were followed by a significant decrease in property crimes (12%), but not in violent crimes. This effect on property crimes closely approximates the overall effect of street

lighting interventions on total crimes (14%), suggesting that the overall effect on crime may be largely a function of street lighting's impact on property crimes. Also important was that a large proportion of the studies (15 of 21) investigated potential crime displacement, diffusion of crime prevention benefits, or both, with the finding that displacement was somewhat infrequent. Finally, cost-benefit analyses of improved lighting suggested that the benefits (in terms of fewer crimes) outweighed the costs, but similar analyses of part-night lighting also suggested that this had benefits.

Directions for Research

Many of the recommendations that we made 14 years ago for research on the effects of street lighting on crime (Farrington and Welsh, 2007) are still needed today. For example, future evaluation studies should include several experimental areas and several adjacent and non-adjacent comparable control areas. Adjacent areas are needed to investigate crime displacement and diffusion of crime prevention benefits. The use of several areas would make it more possible to establish boundary conditions under which street lighting interventions had greater or lesser effects. Ideally, a large-scale national, multi-site evaluation of the effects of street lighting on crime is needed, perhaps similar to the Home Office evaluation of CCTV (Farrington et al., 2007). In addition, a long time series of crimes should be studied to investigate pre-existing crime trends, as well as the extent to which any effects of street lighting interventions persist or wear off over time. Several of the new studies included in the current review made an effort to address one or both of these needs (Perkins et al., 2015; Arvate et al., 2018; Davies and Farrington, 2020; Chalfin et al., 2021a), but there is yet little evidence of longer-term desirable effects of street lighting interventions on crime.

It would also be highly desirable for researchers to carry out benefit-cost analyses of street lighting interventions. This would allow for a number of key policy questions to be addressed, such as: Do the monetary benefits to society from decreased crime rates outweigh the monetary costs of implementing and maintaining street lighting projects? To whom do the monetary benefits (or costs) accrue? Previous research (Welsh et al., 2015) has shown that situational crime prevention in general is an economically efficient strategy in preventing crime.

The one area where there has been a noticeable improvement in research on street lighting (especially in the last half-dozen years) is in the use of high-

quality evaluation designs. This includes the first randomized controlled experiment of street lighting (Chalfin et al., 2021a), a natural experiment drawing on lighting outages and repairs (Chalfin et al., 2021b), and other high-quality quasi-experimental designs. We hope that this represents the beginning of a trend that continues for many years to come.

Directions for Policy

Compared to past years, it would seem that an even stronger case can be made today for street lighting interventions to be part of national and local crime prevention policy. A larger body of high-quality evaluation research, implemented in a range of high-crime public places, some evidence of value for money, and the enduring impact on crime, especially property crimes, all point to the policy significance of street lighting interventions.

It is important to note that this conclusion is based on street lighting as a stand-alone intervention. In the current review, only two of the 21 studies involved other or secondary interventions. Other research demonstrates the effectiveness of street lighting as a secondary intervention (e.g., combined with CCTV cameras) (Piza et al., 2019) or as part of a larger package of situational crime prevention measures (Eck and Guerette, 2014).

Yet another key factor that needs to be weighed in policy decisions about the use of street lighting for crime prevention is concerns about social costs. Research on the social costs of the different types of situational crime prevention that perform a surveillance function (e.g., CCTV, defensible space, place managers, street lighting) identifies street lighting as the least intrusive and exclusionary (Welsh and Farrington, 2009; Welsh et al., 2015). It does not violate anyone's privacy, infringe on civil liberties, or contribute to the social exclusion of groups. It would seem that street lighting for crime prevention has much to offer to law-abiding citizens, policymakers, and legislators.

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Darkness in built up areas can contribute to feelings of personal insecurity, and can produce favorable conditions for vandalism and theft, including bicycle thefts and thefts from vehicles. The crime preventive effects of improved lighting in public places are therefore often discussed, and measures to improve lighting are often implemented as a means of combating crime. But does improved lighting reduce levels of crime? What do the strongest evaluations tell us? These questions are answered in this systematic review, which examines the strongest available research to date.

The Swedish National Council for Crime Prevention (Brå) has commissioned distinguished researchers, led by Professor David Farrington at Cambridge University, to conduct a series of international reviews of the research published.

In 2007, Brå published a systematic review on the effects of improved lighting. The publication was based on the 13 studies available at the time whose methodology was sufficiently rigorous to meet the inclusion criteria for a systematic review. This report comprises an updated review, which now includes a total of 21 studies. The study follows the rigorous methodological requirements of a systematic review and statistical meta-analysis. Even though important questions remain unanswered, the study provides a vital and far-reaching overview of the available evidence on the preventive effects of improved street lighting.

Brandon C. Welsh is Professor of Criminology and Criminal Justice at Northeastern University (USA)

David P. Farrington is Emeritus Professor of Psychological Criminology at Cambridge University (UK)

Stephen Douglas is Doctoral Student at Northeastern University (USA)

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Brottsförebyggande rådet/National Council for Crime Prevention

Box 1386/Tegnérgatan 23, SE-111 93 STOCKHOLM
Tel +46 (0) 8 527 58 400, info@bra.se, www.bra.se

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