

OUTDOOR LIGHTING AND CRIME, PART 1: LITTLE OR NO BENEFIT

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ABSTRACT

Scientific studies support common experience that light tends to allay the fear of crime at night. It is widely believed that outdoor lighting also helps prevent actual crime at night, but experiments have given equivocal results. Thorough scientific reviews published in 1977 and 1997 concluded that the effects were unknown. Recent work in the UK suggests that lighting does have a crime reducing effect by day as well as at night. This work appears to be flawed in ways that favour a crime-reducing result. While it seems reasonable to expect that social effects of outdoor lighting at night might have some influence on daytime crime, so far there appears to be no reliable evidence for any net crime-preventing effect, day or night. It even appears possible that lighting might increase crime, a topic investigated in Part 2 of this work.

CCTV competes with outdoor lighting for crime-prevention funding. The available evidence indicates that CCTV is not an effective alternative. Until the lighting and crime issue is better understood, no more security lighting or other lighting for crime-prevention should be installed and the funding should be redirected to rectification of existing overbright and glary outdoor lighting.

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EXECUTIVE SUMMARY

It is common experience that artificial light at night tends to allay the fear of crime, and this has been confirmed by scientific studies. It is also commonly believed that outdoor lighting helps to prevent crime at night but the evidence is equivocal. Crime-reducing, nil, uncertain, and increasing effects have variously been reported for relatively short-term field studies of lighting and actual crime. Thorough scientific reviews published in 1977 and 1997 in the USA concluded that the effects of lighting on crime were unknown. Claims that more outdoor artificial light reliably reduces crime largely originate from a relatively small number of experiments in the UK. Government authorities there and elsewhere have been increasingly inclined to install more and brighter lighting as a supposed crime prevention measure. Street crime in the UK rose by 28% in the year ending April 2002. Consistent with this, it now it seems that the experimental and analytical results in question are unreliable.

Some researchers claim that increased lighting at night can bring about social changes that influence crime by day as well as at night. Others deny this. In principle, the issue can be tested by making a distinction between direct effects, which are immediate, and indirect effects, which generally take time to develop. Lighting is defined as having a direct effect on crime if the light physically aids or hinders criminal acts at night. Indirect effects presumably depend on intervening social processes. Their development time results in the possibility for indirect effects to act by day as well as at night.

Some existing accounts of lighting and crime experiments present only nighttime crime data but this is no guarantee that the effects claimed are only direct. Other experiments produce day and night data, separately or combined. Direct and indirect effects often appear to be mixed indiscriminately in analyses of changes accompanying the lighting treatment. This could explain some of the discrepant results reported.

Another source of difficulty is that field experiments tend to be set up on an opportunistic basis, utilising municipal relighting programs determined by local authorities. Brighter lighting often appears to be installed as a response to localised crime concentrations. Over time, crime in these 'hotspots' tends to regress naturally to the mean, encouraging the erroneous conclusion that the lighting treatment has had a beneficial effect. This reinforces the use of lighting for crime prevention, regardless of the facts.

Researchers have been cautioned over the years to describe the photometric changes of the lighting treatments in detail. This has typically been ignored, leading to imprecision in results. A worse outcome is that researchers have seemingly been unaware when the results claimed have been improbably large for the lighting increments involved.

Attempts to define a precise relationship between typical lighting increments and measures of crime changes by pooling available results may actually mislead. The apparently improved precision of the weighted average generally does not compensate for systematic bias towards a beneficial effect that appears to be common to many of the individual experiments. Some of this bias is likely to be an outcome of the common practice of experiments being funded by stakeholder organisations.

On the basis of the studies reviewed, no reliable effect can be claimed for outdoor lighting increments as a means of preventing or reducing actual crime. It is possible that lighting could even be counterproductive, a topic taken up in Part 2 of this work. Governments should ensure that resources are not wasted by the installation of any more security lighting or other outdoor lighting at all where the justification includes or implies crime prevention.

National lighting standards should not contain any statement or implication that outdoor lighting will prevent or deter crime.

The evidence is examined for the effectiveness of CCTV as an alternative to lighting for crime prevention. As with lighting experiments, CCTV field studies have typically been done with inadequate attention to the photometric situation at night. If there are any non-zero effects of lighting on crime, lighting changes introduced for the cameras or for other reasons and ignored by the experimenters may have confounded the results. The best available estimate of the effect of CCTV as a crime deterrent is only 4%, and even that may be an overestimate. Funds earmarked for crime-prevention lighting should not be diverted to new CCTV installations, but used instead for rectification of existing overbright and glary lighting.

PREFACE TO PART 1

The original version of this document and its companion Part 2 was a public submission in May 2000 to a parliamentary committee on drugs and crime in the state of Victoria, Australia. It drew attention to the uncertain effects of outdoor lighting on crime. It was then recast as general guidance on outdoor lighting and crime within Australia, and posted on the website of the Astronomical Society of Victoria. This led to postings on several overseas websites. The need to expand the work into two parts only became apparent during a revision that started in January 2002.

This part deals with existing experimental and analytical work on outdoor lighting for crime prevention. Part 2 presents evidence that growth in crime is linked to growth in outdoor ambient artificial light.

Some references to Australian Standards and local lighting issues have been retained as illustrating general problems. The Australian spelling conventions used tend to follow UK practice, but quotations retain the original forms. Dates are given in the format recommended by the International Standards Organisation (ISO 2014-1976).

Revised versions of this document may be issued without notice as new information becomes available. Readers are advised to check the facts for themselves and to seek independent expert advice before taking any actions that could adversely affect visibility, safety, commerce or insurance cover, or might increase vulnerability to crime.

Contents

Abstract	1
EXECUTIVE SUMMARY	2
Preface to Part 1	3
1 INTRODUCTION	5
2 ESTABLISHING THE FACTS	5
2.1 ASPECTS OF SCIENTIFIC METHOD	5
2.2 RESEARCHERS AND BIAS	7
3 OVERVIEW OF LIGHT AND CRIME STUDIES	8
3.1 RESULTS FROM THE USA	8
3.2 RESULTS FROM THE UK	11
3.3 DIRECT AND INDIRECT EFFECTS	13
3.4 NEW JERSEY QUASI-EXPERIMENTS	14
3.5 HOTSPOTS AND CRIME DISPLACEMENT	16
4 SPECIFIC PROBLEMS WITH THE EVIDENCE	16
4.1 PAINTER AND FARRINGTON (1997)	17
4.2 PAINTER AND FARRINGTON (2001A)	23
4.3 PAINTER AND FARRINGTON (1999A)	25
4.4 PAINTER AND FARRINGTON (1999B, 2001B)	27
4.5 PEASE (1998, 1999)	28
4.6 QUINET AND NUNN (1998)	31
5 THE FARRINGTON AND WELSH META-ANALYSIS	32
5.1 REASONABLE BENEFIT LIMITS	32
5.2 MORE ON THE DUDLEY AND STOKE-ON-TRENT STUDIES	36
5.3 THE BIRMINGHAM MARKET STUDY	37
5.4 THE DOVER CAR PARK STUDY	39
5.5 IMPROVING THE META-ANALYSIS	40
5.5.1 Conflicts of interest issues	42
5.5.2 Measure of effect	43
6 FEAR OF CRIME	43
7 LIGHTS, CAMERAS, SECURITY?	46
7.1 SECURITY LIGHTING	46
7.2 LIGHTING FOR CCTV CAMERAS	47
7.3 CCTV AND CRIME	48
7.3.1 Relevance to lighting and crime	48
7.3.2 Review and meta-analysis of CCTV and crime studies	49
8 DISCUSSION	50

9	CONCLUSIONS	51
9.1	EXISTING KNOWLEDGE OF LIGHTING EFFECTS ON CRIME	51
9.2	DIRECT AND INDIRECT EFFECTS OF LIGHTING	51
9.3	BIAS IN LIGHTING AND CRIME EXPERIMENTS	51
9.4	ASSESSMENT OF REVIEW AND META-ANALYSIS	52
9.5	DEBUNKING THE MYTH OF LIGHTING FOR CRIME PREVENTION	52
9.6	CCTV AS AN ALTERNATIVE FOR CRIME PREVENTION	53
9.7	SECURITY LIGHTING AND STREET LIGHTING	53
10	ACKNOWLEDGEMENTS	53
11	CONFLICTS OF INTEREST	53
12	REFERENCES	54

1 INTRODUCTION

Improvements to outdoor lighting are frequently mentioned in election promises, the news media and government planning documents as an option or action for crime reduction. Unfortunately, the common interpretation of ‘improvements’ as ‘more and brighter’ in this context is likely to lead to ineffective or even counterproductive outcomes, as will be seen.

At the outset, it is important to appreciate that there is a widespread belief in the effectiveness of artificial outdoor light at night as a means of reducing actual crime as well as allaying the fear of crime. Seemingly regardless of the balance of scientific evidence, lighting for crime prevention has long been incorporated as standard practice in fields such as architecture, urban design, policing, security, and the lighting industry itself.

2 ESTABLISHING THE FACTS

2.1 ASPECTS OF SCIENTIFIC METHOD

Early explanations of natural phenomena and human behaviour were derived by the *method of intuition* (eg ancient concept of Earth as the centre of the universe) or the *method of authority* (eg bishop announced date of creation as 4004 BC). These methods (eg Martinez-Papponi 2000 [66]) of trying to increase knowledge have been superseded by *scientific method* in the last four centuries, which is not to say that intuition and standing have no place in scientific progress.

Intuition and authoritative guidance assist expedience in the decision making of everyday life. However, they tend to intrude from there into public debate on contentious issues that require scientific input as a necessary part of resolution. Environmental issues provide many examples. The ends may encourage misuse of authority but seldom if ever justify it.

The effectiveness of rigorously applied scientific method is firmly established. But if some applicable rules and procedures are not followed properly in a particular study, the conclusions may be flawed. Described below are some of the more common traps that await the unwary and sometimes even the cognoscenti. This also indicates some of the reasons

why, even in favourable circumstances, it is not at all simple to make a reliable determination of the extent to which outdoor lighting does or does not affect crime.

Human behaviour is often studied in laboratory settings with extraneous factors eliminated or held constant by design and conduct of the experiment so that the cause-effect hypothesis being investigated is given the best prospect of a fair test and a reliable result. This ‘reductionist’ approach can be and often is applied outside the laboratory but as the complexity of the circumstances increases it also becomes more difficult to design experiments with full counterbalancing for all extraneous factors that could conceivably influence or confound the results. Results from such quasi-experiments tend to be less reliable, possibly to the point of being useless or even misleading in some cases.

Real-world aspects of the lighting and crime issue often appear to be too complex or are otherwise unsuitable for investigation with laboratory experiments. Systems outside laboratory settings tend to be on such a large scale that deliberate manipulation of variables for experimental purposes may be impracticable, and opportunistic use of regional changes (eg relighting for economic reasons) may be accompanied by undesirable constraints on the use or extent of experimental controls.¹ For behavioural studies in general, ethical aspects require no tangible risk of harm to individuals. Fully informed prior consent of individuals to be experimental subjects is required for laboratory experiments but may be impracticable for quasi-experiments.

Thus it would seem that deliberate on-off manipulation of outdoor lighting in populated areas might be unacceptable as a means of seeing what happens to the crime rate, regardless of any benefit that a decisive result might bring. It would certainly be wrong to reduce outdoor lighting so far as to reproduce the blackout conditions² of World War 2, for example, as there were high risks of traffic accidents, falls, and drownings in ponds and watercourses when people had to make their way about in natural darkness at night (HSHF 2002 [47]). But vision science indicates that present outdoor lighting could often be reduced by one or more powers of ten (‘log units’ in vision science jargon) without introducing undue mobility hazards.

Other ways of studying large complex systems have been developed within the constraints of scientific method. Top-down analysis of human activity systems (eg Checkland 1981 [20]) can sometimes give unique insights, but no specific example of its use has been found in connection with the lighting and crime issue. Another method is simply the purposeful painstaking observation and analysis of a system that is generally unavailable to deliberate manipulation, as in astronomical observation of the universe. Associations between observable characteristics may be sought on the basis of results of laboratory experi-

¹‘Control’ has a special meaning in the conduct of experiments, viz a test case not subject to the treatment but otherwise, as near as practicable, identical to the experimental case that will be treated. This meaning has been misappropriated by the advertising industry in describing television advertisements involving dangerous stunts as ‘recorded under controlled conditions’. This is not helpful to non-scientists when they are faced with a need to follow scientific arguments.

²Amateur and professional astronomers in countries affected by World War 2 mostly found themselves busy with the national war effort. Those who did get to do some astronomy at the time of lighting blackouts afterwards remarked on the substantial reduction in artificial skyglow and some consequent great advances this allowed in our understanding of the universe. The known presence of worrisome skyglow before the WW2 blackouts is also helpful in reconstructing the time course of the amount of ambient light outdoors at night in cities and towns.

ments. Useful statistical and logical inferences may be possible, allowing an ongoing cycle of extending and refining knowledge.

Scientific method allows development of hypotheses and their observational or experimental testing and analysis in a systematic cycle. This is the most effective way in which new knowledge can be gained. Where observations of a whole population are impracticable, results from samples may be generalised to the whole population. Usually, however, all else being equal, the smaller the sample then the greater is the risk of error in generalising.

2.2 RESEARCHERS AND BIAS

Scientific method requires minimisation or preferably the effective elimination of effects of both deliberate and unwitting bias by researchers. Ideally, researchers should be disinterested in which way their results will turn out. Human nature being what it is, this condition might seldom be achieved absolutely. For example, inconclusive or null findings are known to have less value than useful findings in academic career advancement, and lack of self-interest in such things appears to be rare. However, “Academic self interest is a legitimate part of the motivation to conduct research” (Levinsky 2002 [57]).

Regardless of the intellectual probity of individual researchers, compliance with the zero bias condition is equivocal when the results may have appreciable outcomes, beneficial or adverse, on an organisation that controls, funds or otherwise supports the research or pays for publication of the results. Unfortunately, such situations appear to influence authors far more than might be expected. There are known cases in which scientific papers and reports have been markedly compromised by this kind of bias. Numerous examples have been detected in research on efficacy of pharmaceutical drugs (Angell 2000 [2], Bodenheimer 2000 [12], Drazen and Curfman 2002 [30], van Kolfshoeten 2002 [113]), on tobacco smoking and ‘health’ (ie disease), on asbestos and cancer, and on the traffic hazards of vehicle tinted glazing, sponsored in each case by one or more industry stakeholders (Clark 1995 [21]). This seems bad enough, but compared with the problems of financial conflicts of interest, “non-financial conflicts of interests are more subtle yet more pervasive and cannot be eliminated” (Levinsky 2002 [57]). Conflict of interest is of concern in science generally, not just in the medical and pharmaceutical areas (Laurin 2002 [56]).

Of related concern is the use of industry-paid technical experts to prepare, present and examine adversarial evidence about the effects of light at night in municipal planning applications and appeals. It would appear to be more in the public interest if those judging the issue were able to receive independent non-adversarial advice, even if this were from experts nominated by interested parties. In the present context, planning and environment organisations and authorities, law enforcement agencies and the lighting and power industries all need to avoid bias because of its undesirable capacity to move a judgement away from the result merited by the facts. Ultimately the whole community is likely to lose out from unjustifiably influenced decisions, regardless of which party gains short-term benefits from the judgement.

3 OVERVIEW OF LIGHT AND CRIME STUDIES

There is too much literature on the subject for a complete survey here, and reliance has been placed on some existing reviews. Present views on the subject are polarised. Particular studies have been selected for mention to indicate why, and to facilitate resolution. Nearly all of the available material is from the USA and the UK. Useful formal studies have presumably been done in other countries but few indications of this were found in Internet searches or scans of reference lists in papers cited in this work. A more thorough search of the literature including eight large bibliographic databases by Farrington and Welsh (2002a,b) [34, 35] produced a more extensive collection, but again mostly of UK and USA origin.

A standard convention is followed in this document. If an increase in light is accompanied by an increase in crime, a positive correlation, it is called a positive association or effect, naturally. This possibility is not often mentioned in the crime prevention literature but if it is, it tends to be called a negative effect. To follow that usage here would be to perpetuate confusion. Increased crime accompanying increased light is therefore a positive association. A decrease in light accompanied by a decrease in crime is a positive association also. Increased light and decreased crime is a negative or inverse association, and so is decreased light and increased crime. Useful or beneficial effects are unambiguous – they mean a reduction of crime in any circumstances.

3.1 RESULTS FROM THE USA

The National Institute of Law Enforcement and Criminal Justice of the US Department of Justice presented a thorough study of sixty street lighting projects to the US Congress in February 1977 (Tien, O'Donnell, Barnet, Mirchandani and Pitu 1977 [107], IDA IS63 1998 [51]). The abstract states, in part:

“In particular, while there is no statistically significant evidence that street lighting impacts the level of crime, especially if crime displacement is taken into account, there is a strong indication that increased lighting – perhaps lighting uniformity – decreases the fear of crime.”

Twenty years later, the National Institute of Justice of the US Department of Justice presented an even more comprehensive report (Sherman, Gottfredson, MacKenzie, Eck, Reuter and Bushway 1997 [102]) on crime prevention to the US Congress in February 1997. The following quotes are from ‘*Conclusions for Open Urban Places*’ in Chapter 7 by Eck (1997) [31]:

“Not much has changed since Tien and his colleagues (1979) [?] gave their critical assessment of the impact of lighting on crime.”

“We may speculate that lighting is effective in some places, ineffective in others, and counter productive in still other circumstances.”

“Consider lighting at outside ATM machines, for example. An ATM user might feel safer when the ATM and its immediate surrounding area are well lit. However, this same lighting makes the patron more visible to passing offenders. Who the lighting serves is unclear.”

“Lighting has received considerable attention. Yet, evaluation designs are weak and the results are mixed. We can have very little confidence that improved lighting prevents crime, particularly since we do not know if offenders use lighting to their advantage. In the absence of better theories about when and where lighting can be effective, and rigorous evaluations of plausible lighting interventions, we cannot make any scientific assertions regarding the effectiveness of lighting. In short, the effectiveness of lighting is unknown.”

Eck (2002) [32] has since revised his views:

“The recent lighting studies from Great Britain appear to remove the lingering doubts about lighting’s efficacy. Lighting appears to work in public areas, especially residential communities. Generalizing beyond these types of settings is highly speculative, given the rudimentary nature of current lighting theory (Painter and Farrington, 1997 [82]). Lighting may be effective in some places, ineffective in others and counter-productive in still other circumstances. The problematic relationship between lighting and crime increases when one considers that offenders need light to detect potential targets in low-risk situations (Fleming and Burrows, 1986 [38]). As Pease (1999) [90] correctly points out, we should address the specific conditions where lighting is effective, rather than assume it is always effective.”

That lighting is sometimes effective against crime may be a truism. What needs to be resolved is the extent of any net benefit in practice.

Understandably, most people want the incidence of crime to be reduced. It appears to be widely believed by the public that more and brighter outdoor lighting would help. Of course, extending the belief to its ultimate stage means there should be little or no crime in the bright outdoor lighting conditions of daytime, but that is far from the facts. For example, 54 % of violent crime in the USA occurred between 6 am and 6 pm, and only 20 % of rapes involve unknown assailants at night (BJS 1999 [10]). Only 35 % of all burglaries in the USA are reported to have occurred at night, or 48 % of all burglaries for which the time of occurrence is known (UCR 1996 [111]). Note that these figures are for all reported crime in the whole of the USA, which gives them much face validity.³

In more recent details of crime by region in New Jersey (DLPS 2000 [29]), only the burglary data are partitioned into night, day and unknown time. The day rate for burglary is almost as much as the combined rate for night plus unknown time.

Graphs of percentage of violent crime as a function of time of day for years 1991 through 1996 are given in a US Juvenile Justice Bulletin (NCJRS 1999 [74]). Compared with adults, juveniles (under 18) tend to commit a greater proportion of violent crimes in the hours immediately after school gets out on school days. The difference is less pronounced for robberies. The juvenile violent crime rate on non-school days tends to peak in the evening,

³For comparison outside the USA, crime survey results indicated that nearly two-thirds of the total of assaults, burglaries, robberies, thefts and vandalism occurred after dark in England and Wales (Ramsay and Newton 1991 [96]). The percentage of burglaries at night was 62 %, and ten years later, another survey indicated it was about the same (ONS 2000 [76]). In the writer’s home state of Victoria, Australia, it is understood that most burglaries occur in daylight hours. Robberies and assaults in central Sydney, NSW, Australia showed a peak after midnight, especially on Saturday and Sunday nights (Jochelson 1997 [54]).

an hour or so earlier than the peak at about 10 pm to 11 pm for adult violent crime. On all days, the level of juvenile violence is already low during the time of day that juvenile curfew laws are in effect. All of the curves show a minimum at about 5 am to 6 am. Apart from the school-out peak, the crime rate rises more or less steadily during the day and early evening and falls steadily but more steeply after midnight. There is no obvious relationship to the large changes in light level over the 24 hours, other than the location of the peak in hours of natural darkness. Social factors such as the school attendance hours and the preponderance of daytime work and evening leisure time would appear to have a larger influence than light levels.

Rape and domestic violence are more likely to occur after sundown (Cohn 1993 [23]). Cohn noted that although domestic violence tends to be impulsive, rapes are often planned well in advance. Furthermore, social factors and biological photoperiodicities provide alternatives to explanations based on the direct visual effects of light-dark variation. Data from Maguire and Pastore (2002, Table 3.181) [64] indicate that about two thirds of all reported sexual assault and rape cases occur indoors where outdoor conditions could hardly have had much direct influence. Domestic violence also tends to occur indoors where illumination from outside in daytime and from artificial light at night is generally much brighter than it is outdoors at night.

Quinet and Nunn (1998) [95] analysed the number of calls for police service before and after additional streetlights were placed in Indianapolis neighbourhoods. Their results on the deterrent effect of increased lighting were inconclusive at best, but they didn't quite say so. They claimed that disentangling the effects of social disorganisation, police initiatives and behaviour patterns was beyond the scope of work on crime and the physical environment.

Schumacher and Leitner (1999) [98] described spatial crime displacement⁴ resulting from the most recent wave of urban renewal in Baltimore. (Perhaps, instead, it was an established phenomenon that happened to be observed during the urban renewal.) They expected that increased presence of security personnel, increased street lighting and increased pedestrian traffic would discourage criminal activity in redeveloped areas. "However, the crime rates throughout the city – and in the downtown, overall – remained at high levels despite the redevelopment. This suggested that the renewal programs did not eliminate, but merely displaced, the criminal activity..." possibly thereby indirectly harming the neighbourhoods affected by the displacement. They acknowledged the undesirability of such displacements but pointed out that the city's downtown renewal programs have generated a great deal of revenue and improved the city's image.

Loukaitou-Sideris, Liggett, Iseki and Thurlow (2001) [63] studied the effect of the built environment on crime at 60 bus stops in downtown Los Angeles. Although there were substantial differences between stops in crime incidence, no relationship was found between crime and the paucity of pedestrian lighting at the stop. This seemed to surprise the authors, who wrote "...we can by no means conclude that lighting is not important. For one, we did not account for lighting from near-by establishments. Also, the presence of a pedestrian light did not always mean that this light was lit at night." Sherman et al. (1997) [102] is not even listed in the references. The same applies to Sherman and Weisburd (1995) [101], who stated "Bus stops, pay telephones, and intensive lighting were common features of hot spots". Numerous other publications about this LA bus stop work were found in an Internet

⁴Challinger (1991) [19] is a useful and readily available account of five main types of crime displacement, although the original identifier of these (Repetto 1976 [97]) is not cited.

search. Several of these mention lighting as a crime prevention method or, apparently without justification, claim less crime at stops equipped with shelters and adequate lighting (eg Benson 2000 [9]).

Summing up, relatively short-term studies in the USA appear to indicate that *there is no clear overall effect of the amount of outdoor light or lighting either in increasing or decreasing actual crime rates*. This confirms earlier assessments (eg IDA IS51 1992 [50], IDA IS63 1998 [51]). Farrington and Welsh (2002a,b) [34, 35] concluded that there was a relatively small beneficial effect. Their work is discussed in Chapter 5 below.

3.2 RESULTS FROM THE UK

At the 1989 annual conference of the (UK) Institution of Lighting Engineers (ILE 1989 [52]), increased lighting in a multi-racial inner city area was reported as producing a 16% reduction in robberies, theft from cars, burglaries and vandalism in the following 12 months and a further 10% in the next 12 months. Shaftoe and Osborn (1996) [100] examined this work and concluded:

There “were some reductions in crimes committed at night but this could not be associated with the lighting improvements... It may have been that the lighting improvements did reduce crime in these streets, with some displacement to neighbouring streets in the same beat area. However, if all the schemes achieved was this kind of local displacement, it would be difficult to claim they had been a success.”

The ILE collected details of six street relighting schemes in the UK in 1991. These involved the replacement of 35 W low-pressure sodium (LPS) luminaires⁵ or mercury-vapour luminaires with 70 W high-pressure sodium (HPS) luminaires⁶ (Fisher 1997 [37]). Measured illuminances increased by anything from 1.9 to 40 times. The replacements changed the colour of the light from the quasi-monochromatic yellow of LPS or bluish-white of mercury vapour to the orange-white of HPS. HPS lamps are physically much more compact than LPS, allowing luminaire designers more scope for beam shaping and shading. The resulting glare also tends to be worse because HPS lamps have a much greater intrinsic luminance. In field experiments, it would be difficult to eliminate, counterbalance or otherwise disentangle any effects caused by these factors, not all of which are improvements of the sort that might be supposed to reduce crime or the fear of crime.

The ILE (1989) [52] claim appears to have been based at least partly on the early work of Painter. Ramsay and Newton (1991) [96] examined data from four reports by Painter about three small-scale increased lighting projects in parts of London and found important shortcomings in methodology and analysis, going so far as to append a “statistical health warning” to a table of data from the Edmonton project (Ramsay and Newton (1991), p 28 [96]). They queried the findings that the lighting changes had resulted in reductions in total (all-hours) crime, but accepted that the changes had been accompanied by substantial observed increases in utilisation of the relit area in one case. They noted that the projects had been funded by the local lighting industry.

⁵A luminaire is the complete lamp fitting including any housing, mounting, shielding, reflector, lens, lampholder, electrical components and wiring, and the lamp.

⁶LPS lamps are often called SOX in the UK literature, and likewise SON is used to indicate HPS lamps.

Ramsay and Newton (1991) [96] reviewed the literature and concluded that better street lighting had little if any demonstrated effect on actual crime. Nevertheless, fear of crime did diminish with brighter lighting and there was considerable public faith in lighting as a crime prevention measure. In interviews of over 300 experienced burglars, lighting was virtually not mentioned as a deterrent. In interviews with 45 street robbers, conditions had been dark in only about one eighth of all the offences, time of day was regarded as unimportant and only two robbers actually mentioned darkness as a contributing factor. There was a similar lack of concern about lighting in the choice of location for robbery. In interviews of nearly a hundred car thieves, only one mentioned unlit parking places as assisting the theft but nearly a quarter of the total mentioned seclusion. In both sets of interviews, being seen committing the crime was not of much concern to the offenders, as bystanders were considered generally to take no notice or to take no action.

Atkins, Husain and Storey (1991) [3] conducted a large and apparently thorough relighting study in Wandsworth, a London Borough. They found that the brighter lighting did not significantly change the relative proportion of day and night recorded crime, but interviews indicated that people in the relit areas did feel safer at night.

In a Glasgow neighbourhood, Nair, Ditton and Phillips (1993) [71] studied the effect of relighting the area surrounding the homes of respondents together with other environmental and security changes. They found "little improvement in victimisation or fear of victimisation could be documented" and "It is more likely that improved lighting is no panacea for all ills, and may only be effective under certain conditions".

Tilley and Webb (1994) [109] mentioned expenditure on increased lighting as an anti-crime measure in the UK but they found no evidence to justify this in the towns they studied.

Barker and Bridgeman (1994) [5] described an attempt by British Telecom in 1985 to reduce public telephone vandalism by fitting 24-hour lighting to the booths. The immediate result was the loss of 2000 light globes a year. Barker and Bridgeman provided a bibliography for guidance in the use of security lighting and other measures to prevent vandalism, but there is nothing in their report to justify use of lighting for this purpose.

Fisher (1997) [37] described features of a paper by Painter (1994a) [79], given at a conference of the Institution of Lighting Engineers. Relighting of streets increased pedestrian usage by males and by females. In three schemes, usage of relit roads and paths had increased between 34 and 101 %. Beneficial effects of relighting on crime were also reported for a housing estate in Dudley, UK.

Eck (1997) [31] summarised Painter (1994b) [80]:

"She examined lighting improvements on two separate street segments and a footpath, all located in 'crime prone' areas within London. Pedestrians were interviewed before and after the lighting improvement. All interviews were conducted after dark and were completed within 6 weeks of the relighting. No interviews were conducted in control areas. Substantial reductions in robberies, auto crimes, and threats were reported in two sites (86 percent, 79 percent). These crimes were eliminated in the third site, but the number of crimes before relighting was small so this could have been the result of other factors."

The size of the reductions will be of interest in connection with Section 5.2 below.

A score on the Scientific Methods Scale was applied to papers reviewed in the Sherman et al. (1997) [102] report. The score depends solely on the experimental design, and no

account is taken of adverse factors such as experimenter bias or unrecognised confounding of variables such as lighting intensity, spatial distribution, colour and glare. The design in Painter (1994b) [80] Painter (1994b) scored 2 (Eck 1997 [31]) on an ascending ordinal scale of 1 to 5, effectively meaning it was weak.

Shaftoe and Osborn (1996) [100] described a study of lighting improvements to individual streets and small areas with high crime rates in a multi-racial inner city part of Bristol. The purpose of the lighting changes was to reduce the fear of crime and actual crime in the high-crime localities. The result was “a patchwork of original lighting, new low pressure sodium lamps and, in particularly vulnerable areas, high pressure sodium lamps”. No discernable reductions in recorded crime could be attributed to the lighting changes. Farrington and Welsh (2002a,b) [34, 35] found the study difficult to interpret because the lighting changes were introduced over 28 months. Nevertheless, they managed to extract quantitative information indicating that the changes were effective in reducing crimes other than robbery.

Painter and Farrington (1997) [82] described the Dudley study in which crime victimisation survey interviews were done before and after part of a residential estate was relit. The results indicated a reduction of crime in the relit area in the daytime as well as at night. Painter and Farrington (1999a) [83] described a somewhat similar experiment in Stoke-on-Trent. The results indicated more of a victimisation decrease for nighttime than daytime in the experimental area after relighting. These and related papers are reviewed in detail in Chapter 4 below

An extensive statistical study of crime in Bexley, UK by Pascoe and Harrington-Lynn (1998) [88] indicated that internal and external lighting had little or no influence on crime rates.

Several other apparently relevant reports on this section topic are listed on the web pages of the Scottish Office Central Research Unit (SOCRU 2002 [104]).⁷

3.3 DIRECT AND INDIRECT EFFECTS

If a nearby light assists a burglar to defeat a door lock or force a window at night, the light has provided direct physical assistance in the commission of crime. If the light deters the burglar from starting, or makes the illegal activity visible to a neighbour who alerts the police, the light has had a direct anti-crime effect. Immediacy appears to be an essential characteristic of a direct effect in this context. If bright outdoor lighting somewhere attracts potential criminals who, individually or in company, are motivated or enabled to commit crimes subsequently at this place or elsewhere, this is described here as an indirect effect of lighting in aiding the commission of crime.⁸ A time delay appears to be an inherent characteristic of indirect effects.

A daytime effect on crime, increase or decrease, by what some commentators have called ‘switched-off outdoor lighting’ can hardly be regarded as anything but far-fetched as a direct effect. Nevertheless, there are known environmental and economic effects on crime incidence, eg weak seasonal effects (Jochelson 1997 [54], DCPC 2001 [25]) or, say, the effects of currency exchange rate changes on tourist numbers and hence numbers of tourists

⁷The full texts were not available online and hard copies proved impracticable for the writer to obtain.

⁸Care is required to avoid confusing the usage of ‘direct’ in this context with the usage, avoided in this document, of describing a correlation as ‘direct’ if it is positive. There is no problem in the descriptions of indirect effects and inverse or negative correlations.

as crime victims. These are indirect effects on crime. Light and lighting may possibly play a part in the first example.

The concept of indirect effects can be considered as complementary to the known beneficial and adverse direct effects of light on crime at night. Social or economic effects of changed night lighting on daytime crime seem perfectly reasonable to discuss, as Painter, Pease (1998, 1999) [81, 89, 90] and several others have done. But so far, the only sort of indirect effect mentioned appears to have been a beneficial daytime effect. There is no reason to expect that indirect effects cannot also act at night. It would depend on the time course of development and decay of the effect. Nor is there any reason to suppose that they can only be beneficial.

In the absence of firm knowledge or good reason about the direction of effects being investigated, it would seem important to keep an open mind about the directions for day and night in the course of analysis. It might be possible, for instance, to have an indirect effect in which the night and day segments effectively had opposite signs or different magnitudes, or both.

Switching a light on at night may start to aid or deter a burglar within milliseconds. Failure of lighting in an area may affect transport and result in crowds or deserted areas that change the pattern of crime over hours. Lighting in a public place may have a cumulative effect over years in determining pedestrian usage and opportunities for crime. The time for light to have an appreciable effect could well be a continuum, in which case the distinction between direct and indirect effects would be arbitrary. The distinction does seem to have practical value, however, without going so far as to define time constants for growth and decay of any particular effect. For the present purpose, it seems adequate to define direct effects of light on crime as those having a substantial influence on a criminal act at any time during its decision, commission or escape phases.

3.4 NEW JERSEY QUASI-EXPERIMENTS

This section is about a set of quasi-experiments devised specifically to illustrate difficulties that can arise with the before-after (or pre-post) experimental design incorporating experimental and control areas, as has commonly been used in lighting and crime studies.

These ready-made quasi-experiments have been constructed using real-world data for consecutive years, viz UCR total crime data for 1999 and 2000 in each of the 21 counties in New Jersey (DLPS 2000 [29]), along with a map and county population data from the US Census Bureau (2002) [112].

The total number of crimes for each county was converted to a crime rate per 100 000 of the county population,⁹ and the 21 counties were sorted in order of ascending crime rate for 1999. Pairs of counties were identified in which the crime rates matched within 10% of the larger crime rate of the pair. The minimum number of pairs considered adequate for this demonstration was chosen as ten in advance. Pairs with a shared length of boundary were identified from the New Jersey map. Here these are called contiguous pairs. As there were only five of them, pairs with a physical separation of one county were also selected.

⁹Unfortunately, the available population data were for 2001 rather than 1999. For the present purpose, a uniform scale error in the calculated 1999 crime rates is immaterial. Residual differential errors between the calculated and actual county crime rates for 1999 are expected to be too small to make any practical difference to the outcome of this exercise.

There were five of these pairs also, making up the required minimum. They are shown in Table 1, listed in ascending order of total crime rate for the first county of each pair, with the contiguous pairs identified by italics.

The idea is to consider each pair as a quasi-experiment in which a treatment is applied to one of the pair at the end of 1999. Here the first member in each case was selected as the experimental or treated county and the other is the control. The outcome of this demonstration is not dependent on this selection. Control counties need to be a good match with experimental counties, which is why they were chosen to be adjacent or nearby and to have comparable crime rates before the treatment. The crime figures after the treatment are the actual values for 2000. Of course, there was no deliberate treatment with lighting or anything else, ie null treatment.

For each of the ten pairs, the relative change in crime from 1999 to 2000 was calculated as the first county's ratio of change divided by the second county's ratio of change.¹⁰ These values are given in Table 1. The probability of each result arising by chance was determined with a χ^2 test. The contiguous pairs returned small changes that could be expected as chance results. However, four of the remaining five pairs exhibited unexpectedly substantial changes, some positive and some negative. No deliberate interventions (treatment) or other reasons for this are known, and the differences have arisen through interference from real-world conditions that are unknown here, apart from the one-county separation.

TABLE 1. Crime change in pairs of New Jersey Counties							
County Pairs	Population	Crime Rate /100 000	Crime 1999	Crime 2000	Relative Change in Crime, %	χ^2	Probability (1 df)
Sussex Hunterdon	146671 125135	522.9 576.2	767 721	947 597	49.1	29.55	p<0.0001
<i>Warren Morris</i>	105765 472859	731.8 773	774 3655	740 3737	-6.5	1.415	ns
<i>Monmouth Ocean</i>	622977 527207	1020.3 1055.2	6356 5563	6288 5605	-1.8	0.495	ns
<i>Monmouth Burlington</i>	622977 432121	1020.3 1124.2	6356 4858	6288 4826	-0.4	0.023	ns
<i>Ocean Burlington</i>	527207 432121	1055.2 1124.2	5563 4858	5605 4826	1.4	0.903	ns
Burlington Middlesex	432121 757191	1124.2 1127	4858 9291	4826 9638	-4.2	3.000	p<0.1
Passaic Union	491077 523396	1727 1750.3	8481 9161	7585 9484	-13.6	46.12	p<0.0001
Cape May Camden	102352 509350	1948.2 2062.4	1994 10505	2041 9601	12.0	10.78	p<0.005
<i>Cape May Cumberland</i>	102352 146289	1948.2 2153.3	1994 3150	2041 3163	1.9	0.226	ns
Camden Cumberland	509350 146289	2062.4 2153.3	10505 3150	9601 3163	-9.0	10.63	p<0.005

¹⁰For example, the calculation for the first pair is $(947/767)/(597/721) = 1.491$ or a 49% increase in crime in the first county relative to that in the second county.

3.5 HOTSPOTS AND CRIME DISPLACEMENT

Schumacher and Leitner (1999) [98] described spatial crime displacement observed during urban renewal in Baltimore. They expected that increased presence of security personnel, increased street lighting and increased pedestrian traffic in redeveloped areas would discourage criminal activity, but this did not happen:

“However, the crime rates throughout the city – and in the downtown, overall – remained at high levels despite the redevelopment. This suggested that the renewal programs did not eliminate, but merely displaced, the criminal activity...”

A key result in Schumacher and Leitner (1999) [98] is the spatial and temporal volatility and dynamism of the burglary hotspots they tracked with a geographic information system. Substantial changes in number, size and position of the hotspots took place in each of the two-year intervals shown from 1988 to 1996, and a substantial part of central Baltimore was affected by these changes in that time. This behaviour was ascribed to crime displacement for socio-economic reasons and to areas with reduced risk of apprehension. If the before-after experimental-control type of experiment and analysis had been applied with null treatment to these burglary hotspots with nearby areas used as controls, the experimental area would tend to show a relative decrease in burglary, a false benefit, because of displacement. This is a regression to the mean phenomenon. The control area would have shown an increase, no effect or a reduction in crime. Without spatial analysis, any appreciable change could be interpreted as either displacement of crime or diffusion of benefit from the experimental area, as is often claimed in such lighting and crime experiments.

Given the widespread belief that increased lighting will prevent crime, municipal officers who select areas for relighting might reasonably be expected to favour areas with increased crime instead of deciding purely on the basis of electrical or visibility criteria. Police advice may be sought or proffered.¹¹ Local officials may be reluctant to state the strategy publicly as tantamount to labelling identifiable areas as crime-prone, but politicians seem to be less inhibited, especially during election campaigns.

Any such bias in the relighting process tends to result in a confirmatory bias in the results of any subsequent investigation into the efficacy of relighting for crime prevention. The process therefore has a net positive feedback, which encourages its continuation.

All before-after lighting and crime experiments without time-series spatial observations and analysis of ambient light as well as crime should now be regarded as suspect if not invalid. The methods used to date demonstrate how *not* to study the relationships between street lighting and crime, regardless of the number of crime measures and the quality of statistical analysis applied.

4 SPECIFIC PROBLEMS WITH THE EVIDENCE

In general, the lighting and crime studies reviewed are notable for their shortcomings in design, conduct and analysis, and the disparities in results. Several of these studies were also reviewed by Lab (1997) [55], who was likewise critical:

¹¹The police may even go so far as to veto the type of lamp! (SOLS 2002 [105]).

“There are a variety of methodological problems throughout the lighting studies. One of the most problematic of the issues relates to the measurement of lighting. Various studies tend to differentiate between “relit” and “unrelit” areas of town without producing evidence of the increased level of illumination or the uniformity of the lighting (Tien et al., 1977 [107]). Simply altering the light fixtures does not guarantee an actual change in the amount of illumination. A related problem is the lack of information on the control areas and their lighting, besides the fact that these areas did not receive the new lights (Nair et al., 1993 [71]; Tien et al., 1977 [107]). Targeting high-crime areas and comparing them to lower crime areas may account for the failure of the project. Reduced crime in a high-crime area could be a regression artifact. This means that the unusually high offense levels return to a lower, more natural level over a period of time. A related problem is that of using short-term follow-up times, which could mask true results (Nair et al., 1993 [71])... Perhaps the most strident support for lighting is offered by Painter (1993) [78], based on a series of analyses conducted in England. Unfortunately, Painter fails to address a number of methodological concerns and inconsistencies (Nair et al., 1993 [71]), which leads to serious doubts about the efficacy of the results.”

Problematic studies are examined in detail in this chapter. This includes the Dudley and Stoke-on-Trent projects in particular, because:

- the effects found are larger than in most other studies,
- much publicity has been given to the results, leading to increased public expenditure on outdoor lighting in the UK and elsewhere, and
- the studies appear to have serious shortcomings.

4.1 PAINTER AND FARRINGTON (1997)

Marchant (2001) [65] criticised the publication vehicle for Painter and Farrington (1997) [82], a book restricted by title to claimed successful applications of Situational Crime Prevention, and therefore unacceptably biased in scientific terms.¹² This is no fault of the contributing authors, of course. The book editor’s preface to the paper claimed that conventional wisdom about lighting being ineffectual for crime prevention is changing largely because of Painter’s work and in the face of much skepticism. This assertion by the book editor is rather uncritical.

Marchant (2001) [65] noted that a lighting company had funded the research described by Painter and Farrington (1997) [82]. He pointed out that some of the statistical tests used

¹²The titles of Poyner and Webb (1987) [93], Grabosky and James (1995) [42] and several other works have a comparable restriction, indicating that this is a surprisingly common fault in the literature of Situational Crime Prevention. It goes beyond a mere stylistic blemish: for instance, Grabosky and James also called for more examples, subject to the condition: “Evidence of success as demonstrated in the results of scientific evaluation is a prerequisite for inclusion.” Given that false indications, beneficial and ‘maleficial’, are equally likely to result from statistical analysis of properly designed and conducted experiments in which there may be no true relationship, publishing collections confined to apparently beneficial results is not only bad science but misleads architects, urban designers, lighting and security professionals, insurance risk assessors, police and members of the public who are interested enough to read the material. In due course, this leads to unwitting perpetuation of the bias when readers prepare summary articles about supposedly well-established facts for further dissemination.

were one-tailed, justifiable only if there was prior evidence of no possibility for results to involve the other tail (an adverse effect of lighting). In some cases, the one-tailed test gave a statistically significant result when the appropriate two-tailed test does not. This leads to doubt about the conclusions drawn. Marchant also criticised the interview procedures, first quoting Painter and Farrington (1997):

“ ‘Unfortunately it was not possible to link up the before addresses with the after addresses, in order to carry out longitudinal analyses with each address acting as its own control.’ No explanation is given of the reasons but it results in key information being lost. This inability to link the address, is strange since further up the page it says ‘Of those re-interviewed 90 % were the same respondent as in the before survey, 7 % were the same household but a different respondent and 3 % were a different household at the same address’. So the interviewers certainly seem to have gone back to the same addresses and knew who they spoke to, so why could not the responses from each address before and after the introduction of brighter lighting be linked? Such a flaw seriously undermines the research and raises the question ‘if this aspect was messed up, were there other errors?’

It would also be useful to be told, which organisation actually carried out the interviewing.”

Following on from the last sentence, the writer thinks it would also be helpful to know the qualifications or training of the interviewers, details of how the quality of the interviews was monitored, whether any interviews were of unacceptable quality and what action, remedial or otherwise, was taken in those cases.

Crelin and Granata (2002) [24] drew attention to funding of the Painter and Farrington (1997) work by a lighting company. They also found some other problems:

“This study concludes that a higher level of illumination introduced into residential areas shows an effect on crime, determined by short-term results and established solely through before-and-after interviews with the area’s residents. It is true that great care has been taken to keep several aspects of this study from extraneous influence, however, much is still left open to interpretation... [The validity of the study appears] to rest upon what was alleged through resident interviews – not documented criminal acts.”

Police records of reported crime are known to underestimate total crime substantially by comparison with crime survey results. For this and other reasons, criminologists generally prefer to work with survey results. However, for Uniform Crime Reporting (UCR) total crime in the USA, the extent of underestimation did not change much since 1975 according to Bastian (1993) [6]. Marvell and Moody (1996) [67] used UCR data in their extensive study after acknowledging its shortcomings. Despite the problems, police records are independent of research studies done using these records. On the other hand, ad hoc surveys done in connection with particular studies are potentially open to bias.

Painter and Farrington (1997) pointed out that police records for Dudley were unsuitable for checking the survey results or as sources of experimental and control data “because of

changes in recording procedures and inadequacies of available data.” More details would have been helpful.

In the case of pedestrian and resident interviews done specifically to determine whether ‘improved’ lighting has affected crime, bias could arise from the wording or order of questions, despite the use of ‘double-blind’ procedures. In particular, the effect of brighter lighting in reducing fear of crime (see Chapter 6 below) and the common belief that lighting reduces actual crime could possibly bias recollections of crime. Places that were less brightly lit at night might seem more prone to crime. Any such bias could possibly apply to the places in daytime as well as night. Reliance on interviews could therefore have contributed to the difference between the Painter and Farrington results and the generally smaller or inconclusive effects found by other researchers using recorded crime data. In the absence of resolution of this issue, it could be argued that inclusion of complete figures for recorded crime should have been a condition for publication rather than an option.

There are other problems with Painter and Farrington (1997), including bias in the literature review. From the time when the experiment was done (ca 1992) to the time of publication, it was not justifiable to claim that a consensus on a beneficial effect of lighting on crime had been established in the journal literature. A beneficial effect may have been the result reported in Painter’s unpublished PhD thesis of 1995, but as is stated in the acknowledgement section of the paper, the thesis included the same (Dudley) experimental results. The literature review and the rest of the paper therefore should have been neutral in its approach to the topic. Instead, it is strongly biased towards the view that lighting does prevent crime, as is shown by the following quotes from the paper:

“The main aim of this project is to investigate the effects of improved street lighting as a crime prevention technique.”

It should have been a test for any effects of lighting changes on crime measures. As it stands, it pre-empts the results of the experiment.

“Modern interest in the relationship between street lighting and crime began in North America amidst the dramatic rise in crime which took place in the 1960s.”

This was a time when street lighting was also increasing dramatically, but that is not mentioned.

“In summary, the relationship between visibility, social surveillance and criminal opportunities is a consistently strong theme to emerge from the literature.”

This is based on Situational Crime Prevention theory, not experimental results. Taking it to its logical conclusion, there should be little crime in daytime, far from what actually happens.

“This design controls for the major threats to internal validity outlined above.”

No, an increase in lighting as a treatment should be counterbalanced in some way, such as by a decrease in lighting of similar magnitude as a simultaneous treatment in another experimental area, or, less effectively, a subsequent return of the lighting levels in the experimental area to their original values.

“The design and layout of the estates, and the type of dwellings, facilitated natural surveillance, which was particularly important for street lighting to be effective as a crime prevention strategy.”

These features doubtless also facilitated criminal choice of target and commission of crime, but this is not mentioned.

“The new lighting replaced the older type mercury lamps.”

The implication is that new is better than old, when there was no good reason to believe that the characteristics of the replacement lamps were any better or worse than those of the existing type of lamps in terms of any beneficial effect on crime, and they were certainly worse from the viewpoint of experimental design. (See below in this section for more details.)

“The British Standard (BS5489 Part 3) lists three categories of lighting levels corresponding to low, medium and high crime risk areas and levels of traffic and pedestrian usage.”

This statement pre-empts the experiment, as greater risk of actual crime is stated in the standard as requiring brighter lighting. This is a fault of the standard rather than the paper, but the statement is not queried in the paper. It also raises the issue of why the standard expressed this view when the balance of available evidence was inconclusive if not against it at the time it was written.¹³

“Also, it permits the displacement of crime from the experimental area to the control area.”

This pre-empts the experiment by failing to mention the opposite effect as an equal possibility.

“In addition to leading to a positive change in resident opinions and physically creating a brighter and safer environment, street lighting...”

The inclusion of ‘safer’ pre-empts the experiment.

The following statement is made in the conclusions section of Painter and Farrington (1997):

“In short, improved street lighting has no negative effects and has demonstrated benefits for law abiding citizens.”

At the time, there was an extensive and readily available literature to the contrary about the adverse health, safety, ecological, greenhouse and other environmental effects of artificial light at night (eg IDA 2002 [49], LiteLynx 2002 [61]). Nobody should recommend increasing what is already an environmental problem without seeking expert advice and discussing or at least drawing attention to the broader ramifications.

¹³Australian Standards on road and public lighting issued in 1997 and 1999 also imply that lighting prevents crime but give inadequate justification for this.

Additional problems of the study relate to possible confounding effects of unreported changes in colour rendering, light distribution and glare. In the case of the mercury-vapour lamps that were replaced, it is well known that they have a long operational lifetime, during which the light output drops steadily and substantially. Local council staff decided that the existing lighting was in a bad state of repair and that the area would be relit. If this state arose because the lamps were nearing, or at, the end of their useful life, then a far better experimental treatment would have been to replace the used lamps by unused ones of the same type as is usually done. This would have avoided the confounding by colour, beam pattern and column spacing that actually happened with the substitution of high-pressure sodium. The mean increase in illumination would have been comparable in the two cases, or could have been made so.

In the 'before' survey, the proportion of respondents who reported seeing police in the last month in the experimental area was 17.4 %, and for the control area, 27.5 %. In the 'after' survey, the values were 38.2 % and 30.7 % respectively. This means that for the sampled months, police presence in the experimental area relative to the control area increased by $(0.382/0.307)/(0.174/0.275)$, ie a 1.97 times increase after the relighting. The extent to which a relative doubling of police presence is expected to have affected the commission of crime is important.

According to Weatherburn (2002) [117], the best-conducted US study on effect of police numbers on crime is Marvell and Moody (1996) [67]. Using their result on p 632, a 10% increase in number of US city police will bring about a 2.9% decrease in total crime, corrected for under-reporting. Levitt (1997) [58] pointed out that (US) cities tend to hire more police as election time approaches, and that such increases reduce violent crime more than they reduce property crime. It is not known if elections or any other event affected the number of police in Dudley during the experiment, but the total number of sightings in the after survey was 53 % greater than in the before survey.

Not all examinations of the problem find a beneficial effect of police on crime. After a major terrorist attack in Buenos Aires, police were reallocated to guard ethnic properties around the clock. The police had to stay close to the assigned properties. Di Tella and Schargrodsy (2001) [26] made use of police records of car theft before and after the attack. Car thefts decreased in the blocks including the guarded properties, but increased concurrently in surrounding blocks. Overall, car thefts were not decreased, merely displaced. In this case, substantially increased police presence did not reduce car theft. While this has lessons relating to the spatial extent of deterrence around a single stationary police officer, it seems unlikely to apply to the change in police presence during the Dudley study.

Goodman (2002) [41] modelled total recorded crime as a function of socioeconomic and demographic variables for 92 midsize US cities. Up to thirteen variables such as population, percent of vacant houses, number of school dropouts and unemployment rate were used. A decomposition process allowed isolation of crime as a function only of police-related variables such as the growth of police numbers resulting from prior city budgeting decisions. For the mean number of police per city, 356.6, the crime rate per 100 000 population is 8926.5. From the model results, a 10% increase in police number over the mean would reduce crime by 98.8, ie a 1.1% decrease. This takes account of the increase in recorded crime as an artifact of increased police numbers (eg Walker 2002 [115]).

Goodman was further able to decompose the police-related variables, isolating the actual crime-reducing effect of increasing the number of police in a city with all other variables held

constant. For a 10% increase in the mean number of police, the effect of this in isolation is a decrease of 10.3 % in crime. Obviously crime would not disappear altogether or even become negative if police numbers merely doubled, so linear extrapolation is not appropriate for increases of this order.

A doubling of police numbers represents a 10% increase repeated about 7 times. Using Marvell and Moody's estimate, a doubling of police should reduce crime to $(0.971)^7$, ie 0.814, or a reduction of 18.6 %. Using Goodman's first estimate, the reduction should be to $(0.989)^7$, ie 0.925 or a reduction of 7.5 %. Goodman's second estimate is $(0.897)^7$, ie 0.467 or a reduction of 53 %. Of Goodman's two estimates, the second is more relevant to the Dudley situation.

The relative reduction in prevalence of all crime actually reported by Painter and Farrington was 21.1 %. This is certainly of a magnitude that could result from a relative doubling in police presence that took place after the lighting intervention, insofar as the 'previous month' response given by respondents is representative of the situation for the 'previous 12 months'.

Painter and Farrington performed a logistic regression analysis to check whether differences in police presence and proportions of people over 60 on the estates influenced the results. According to this test the differences did not influence the results, so they dismissed the importance of the variation in police presence and concluded that the relighting caused the observed crime reduction. This appears to fly in the face of the facts.

It seems more likely that the reduction in crime in the relit area was a consequence of a relatively greater police presence than a lighting change. If the lighting change did anything at all, it would seem possible that it affected police presence instead. Presumably, police are affected by the dark like anyone else, including criminals, and it would hardly be a surprise if they spent relatively more time in the brighter area after relighting, to the extent that they had any discretion to exercise. This could even have been a conscious choice on the basis that the relit area must be a higher crime area requiring their presence because the council had relit it. Given the known effect of police presence on crime and the unknown effect of lighting, the former is the more parsimonious explanation of the crime results.

Pedestrian use of two streets in the Dudley experimental and control areas was monitored for 3.0 hours on each of two nights in March 1992 and March 1993. The paper stated that the weather in each of these two periods was similar, cold and dry. No reason is given for the absence of quantitative measures of weather characteristics. It would have seemed important to report dry-bulb temperature readings at least. Effective temperature measures incorporating humidity and wind chill factors would have been better again. However, any effect of weather on pedestrian numbers would have applied equally to control and experimental areas, so the absence of physical measures of weather does not justify discounting of the pedestrian results in this case.

There is no doubt that substantial changes were observed in behaviour of the residents. Standard statistical tests indicated small probabilities of chance occurrence of many of these changes, provided that unknown confounding effects were absent. But as demonstrated by the New Jersey quasi-experiments of Section 3.4, real world quasi-experiments are notoriously subject to non-trivial unknown influences. This adds to the reasons why the Dudley results are unconvincing.

The Dudley experiment involved many different before-after measures. Farrington and Welsh (2002a,b) [34, 35] derived an odds ratio (see Chapter 5 below), a single measure

representing a relative change in total crime for the control area divided by the change for the experimental area.¹⁴ The value they derived is 1.44 (representing a 44% relative increase in crime in the control area), with $p < 0.05$, meaning that a result of this size could be expected to arise by chance less than once in every twenty of a large number of trials. A larger change than this took place with the first pair of counties in Table 1, with a probability of getting this result by chance less than once in every ten thousand comparisons of county pairs.

The point of this comparison is that such variations can arise more readily than expected in the real world, because of unknown influences rather than lighting or other deliberate interventions. The argument is strengthened by the larger numbers of crimes in the county pairs compared with those in the Dudley experiment: for example, the New Jersey county crime counts were far less vulnerable than the crime counts for the Dudley areas to artifacts from one or a few habitual criminals changing their preferred locality of operations. The comparison casts considerable doubt on the attribution of the observed changes in crime in Dudley to the lighting intervention. The many other faults described in the Dudley experiment and its reporting add to this doubt, suggesting that the claimed beneficial effect of lighting on crime is not reliable.

This conclusion appears to be generally applicable to other existing real-world experiments on lighting and crime. Future experiments on this topic will need to involve more safeguards against confounding by uncontrolled variables.

4.2 PAINTER AND FARRINGTON (2001A)

Painter and Farrington (2001a) [85] is a report of crime surveys of young people in the Dudley study. The research was funded by a lighting company. The paper begins with a discussion of the Scientific Methods Scale used by Sherman et al. (1997) [102]. Eck's (1997) [31] brief description of this ordinal scale is:

“As in earlier chapters, evaluations were graded using the scientific methods score (1 = correlations between tactics and crime and studies without pre-intervention measures; 2 = pre-post designs without control places; 3 = pre-post designs with controls or time-series designs with at least five time periods prior to the intervention; 4 = studies of interventions in a large sample of places compared to similar places without interventions; and 5 = randomized controlled experiments.”

Painter and Farrington used the score of various experimental designs to indicate ‘methodological quality’, which appears to make too much of it. Methodological quality might be regarded as a somewhat broader term that could include, for example, the rigour with which scientific method is followed and the full range of precautions taken to minimise threats to validity.

Regardless of this semantic issue, Painter and Farrington claimed that their Dudley experiment reached Level 4 on the Scientific Methods Scale. Although they quoted a different definition that allows for the control of additional relevant factors, it seems problematical whether this is sufficient to overcome the limitations of just one experimental area and one

¹⁴The odds ratio is the reciprocal of the expression for numerical relative change in crime in the quasi-experiments in Section 3.4.

control area in the Dudley experiment. If their claim is accepted, then the New Jersey experiment of Section 4.2 might also have refinements contrived for it to reach Level 4. Possibly Level 4 and certainly Level 3 and all lower levels of real-world quasi-experiments on lighting interventions and crime, as performed to date, appear to be unacceptably likely to generate unreliable results.

As Painter and Farrington explained, Level 5 is impracticable to implement for studies of crime effects of lighting interventions. The Level 5 requirement for substantial numbers of randomly selected ‘units’ could usually be met for other purposes with households or individuals as the units, but hardly with areas such as blocks of houses as the units required in street lighting studies. Therefore, lower-level designs have to be used, but they need to be bolstered by some further safeguards. Eck mentioned time-series designs in Level 3, but Painter and Farrington’s list does not include this detail. It seems that time-series measures at least would need to be incorporated into Level 3 designs and perhaps even into Level 4 designs in order to get reliable answers in real-world studies of lighting interventions and crime.

Painter and Farrington (2001a) did not mention whether there were any shops, pubs or other commercial areas present in or adjacent to the Dudley experimental and control areas as potential or actual crime hotspots. They cited Painter and Farrington (1999b) [84] about the claimed cost savings of the Dudley and Stoke-on-Trent crime reductions attributed to relighting but this has no bearing on the credibility of either study.

The adult ‘before’ survey results indicated “If anything, the experimental area was slightly worse on crime”. In the self-reported delinquency survey also, the experimental area was again worse on crime before the intervention. The effect of this would be a small artifactual increase in the prospects of an apparent reduction in crime in the experimental area. This does not negate the results, but makes them less reliable.

In the young persons self-reported delinquency before survey, the questions asked about previous offences were for ‘ever’ rather than for the previous 12 months, as had to be the case in the ‘after’ survey. This could be expected to have inflated the before results for the experimental and control areas. The experimental area before score for self-reported delinquency was 1.55 and for the control area, 1.50. The outcome would again be a small artifactual increase in the probability that a relative reduction in offending would be found in the experimental area after the treatment. The authors did recognise that the ‘ever’ condition clouded the issue, but the explanation for why this was done (“advantage of obtaining more complete information on offending before”) seems quite inappropriate.

A survey of perceived effects of improved street lighting was also done. Although most residents in both estates were aware of the lighting change, no explanation is given for the absence of the control after part of this survey.

In discussing the results for outdoor victimisations, Painter and Farrington (2001a, p 275) stated “Disappointingly, there was no significant tendency for victimization to decline more in the experimental area than in the control area.” The first word indicates that the authors were not disinterested in the outcome. Furthermore, in the discussion of the overall results, the distinction between supposed lighting effects on fear of crime and actual crime seems blurred at times.

The results were ‘mixed’ to some extent, but the authors did not see this as a warning that their basic premises were flawed. Instead, they tried to explain some discrepancies with a supposition that stretches credibility (p 279):

“The most surprising result is that victimization of young people did not decrease more in the experimental area than in the control area. The qualitative data suggested that, whereas crimes by young people decreased, pestering of young people by older people did not decrease. Possibly, the improved street lighting inhibited offending by younger offenders against older victims but not offending by older offenders against younger victims.”

Painter and Farrington (2001a, p278) gave more information than in their 1997 paper about the conduct of the experiment: there was regular contact between the principal experimenter and the fieldwork supervisor, local estate housing officers and the police. The principal experimenter also attended Tenants’ Association meetings. Numerous opportunities could therefore have arisen for unwitting bias in comments to influence the surveys, which is not to say that this ever happened. Regardless, contact with tenants who may have been survey respondents before the final interviews does not seem to have been good practice. The double-blind interview procedure would not have provided an effective barrier against any bias thereby introduced.

Painter and Farrington (2001a) also provided information that was not in the 1997 paper about the absence of police records from the before-after comparisons. In the Conclusions, it is stated that police-recorded crime had been planned for inclusion in the study. This raises the issue of why it did not occur, given the potential value of the study for crime prevention. It seems reasonable to expect that the police should have ensured the provision of appropriately compatible crime records for the duration of the experiment, along with records of patrol durations in the experimental and control areas.

Given the potential importance of the work in influencing the expenditure of large sums on lighting in the UK and elsewhere, more pro-active cooperation might also have been expected from the council. In the planning stages, this would have allowed discussion of prospects of counterbalanced lighting treatment (decrease as well as increase), temporarily extended use of mercury-vapour lamps to reduce confounding and photometric surveys of the experimental and control areas. Neither of the Dudley papers mention these matters, but to be fair, the issues are doubtless more obvious in hindsight.

If the confounding of the Dudley experiment by the change in relative police visibility or any factor other than increased light is accepted as an explanation of the results, then the Painter and Farrington (2001a) [85] paper has rather limited value.

4.3 PAINTER AND FARRINGTON (1999A)

The Stoke-on-Trent study (Painter and Farrington (1999a) [83]) broadly follows the arrangements used in the Dudley study. The literature survey runs to eight pages. It is clearly based on the assumption that increased lighting reduces crime and virtually no mention is made of the possibility of no effect or an increase in crime. At the time the experiment was performed, 1992 and 1993, there was no justification for such a one-sided view.

On the basis of perceived need,¹⁵ local council staff decided that the street lighting in a certain area of Stoke-on-Trent would be converted to high-pressure sodium luminaires. This became the experimental area. Nearby areas separated from the experimental area by physical features were chosen to be the control. Untreated areas contiguous with the

¹⁵Again there was no explicit reason or reasons given for the relighting decision.

experimental area were called 'adjacent', providing a second control. The paper does not describe the existing street lighting in the adjacent and control areas; presumably it was "older, domestic-type incandescent lamps" like that originally present in the experimental area. The column spacing was reduced from 50 m to 38 m. The maintenance and energy costs doubled in the relit area and the "amount of useful light increased fivefold". As in the Dudley study, the photometric details are inadequately described, again ignoring the Tien et al. (1977) [107] warning of this as a contributory factor in poor experimental results.

Crime surveys were conducted in the experimental, adjacent and control areas before and after the lighting intervention. In the before survey, the prevalence of all crime in the experimental area was 69.2 % greater than in the control area, and the adjacent area crime was 63.0 % greater than in the control area. In the after survey, these values had changed to 11.7 % greater and 14.4 % greater respectively. Prevalence of crime in the control area increased from 34.1 % before to 38.3 % after. These figures suggest that crime in the experimental and adjacent areas was relatively elevated by comparison with the control area to begin with and fell in the course of the experiment. Regression to the mean would be a possible explanation for much of the observed changes.

In the before survey, the proportion of respondents who reported seeing police in the last month in the experimental area was 21.1 %. For the adjacent area, the figure was 25.9 %, and for the control area, 58.0 %. In the after survey, the values were 9.7, 12.4 and 11.1 % respectively. The surveyed police presence fell in all three areas but the largest change was in the control area, from 58.0 % before to 11.1 % after. This means that for the sampled months, police presence in the experimental area relative to the control area increased by $(0.097/0.211)/(0.111/0.580)$, ie a 2.40 times increase after the relighting. For the adjacent area the relative increase was 2.50 times. This suggests strongly that the observed reductions in prevalence of crime in experimental and adjacent areas relative to the control area were a result of the relative more-than-doubling in police presence that took place after the lighting intervention, insofar as the 'previous month' response given by respondents does actually represent the situation for the 'previous 12 months' of the crime surveys. Police force areas were restructured in March 1992, ie during the period covered by the before crime survey. No other information is given in the paper about police deployment. Crimes recorded by the police for the police area covering the experimental, adjacent and control areas showed little overall change over the period of the study.

Painter and Farrington were aware of this confounding by the changes in police presence but as in the Dudley study, dismissed it on the basis of results of a logistic regression analysis. This again flies in the face of reason, given the known substantial effect of police presence in deterring crime. The similarity of the relative reductions in crime for the experimental and adjacent areas strongly suggests that the cause was the relative increase in police presence, a factor common to both areas, rather than the relighting, which was confined to the experimental area.

Given that the relative police presence in the experimental area apparently doubled or more after the relighting in both the Dudley and Stoke-on-Trent studies, it seems odd that Painter and Farrington did not discuss this in appropriate detail.

In interviews of seven police officers who had patrolled the Stoke-on-Trent areas, all expressed a preference for the relit area as being easier for them to work in. No information was given on whether the police had the discretion, or had been directed, to spend relatively more time in the experimental area after it had been relit.

Pedestrian street use of relit, adjacent and control areas was monitored for 2.5 hours on each of two nights in December 1992 and December 1993. The paper stated that the weather in each of these two periods was similar, cool and dry. As in the Dudley study, no reason was given for the absence of quantitative measures of weather characteristics.

The pedestrian counts showed an increase of 71 % in all pedestrian traffic after relighting in the experimental area. If it is accepted that this is reliably greater than the increase of 34 % in the adjacent area and 32 % in the control area, this raises a substantial issue that is not dealt with by the authors. These counts indicate that the adjacent area was like the control area. In practical terms, the brighter lighting of the experimental area would have affected the illumination in each part of the adjacent area only to a distance of about one pole-spacing, 50 m or less. This suggests that the adjacent area should have been treated as a control, in that it was largely unaffected by the relighting. But Painter and Farrington ignored this in concluding:

“Interestingly, decreases in crime in the adjacent area were almost as great as in the experimental area. This suggests that there was no displacement of crime, but rather a diffusion of the benefits of improved street lighting. Conceivably, the improved lighting in the experimental area deterred potential offenders not only in this area but in the adjacent area as well, since the areas were not clearly delimited. The qualitative data showing how information about the areas was communicated, and how relighting led to increased community pride in the adjacent area, supported this hypothesis.”

If the adjacent areas were indeed controls unaffected by the treatment and closely matched to the experimental area by proximity and similarity of housing, the most parsimonious explanation of the results is that relighting had no reliable effect on crime, and that crime in the original control area increased relatively because of the relative reduction of police presence or because of other asymmetric factors.

For the several reasons given, the writer is unable to accept that the claimed beneficial effect on crime in the Stoke-on-Trent study was caused by increased illumination when it could also have been caused by any one or more of choice of control areas, change in police presence, change in glare, change in beam pattern, change in inter-pole distance, change in lighting colour, or change in flicker modulation depth. On top of all this is the potential for unwitting bias from conflict of interest. Funding for the work was provided by a representative of a lighting company and by the Midlands Electricity Board. Both funding sources stood to benefit from the finding that a beneficial effect on crime resulted from increased lighting.

4.4 PAINTER AND FARRINGTON (1999B, 2001B)

Painter and Farrington (1999b, 2001b) [84, 86] are successive recalculations of the monetary value of the crime reduction claimed for the Dudley and Stoke-on-Trent relighting projects. Painter and Farrington (1999b) [84] stated:

“Thus, in the case of lighting improvements, if crime is being shuffled from relit to darker areas, or from night to day, the total amount of crime would not be reduced.”

It is not at all obvious how or why brighter lighting at night as a claimed crime prevention measure might displace crime into the much brighter conditions of daylight while there was also the expectation that crime would be displaced into darker conditions.

High-pressure sodium lights are yet again described as white, but the “older domestic type tungsten lamps” they replaced in Stoke-on-Trent are actually whiter.

Information not previously given about the Stoke-on-Trent project is that other crime prevention strategies continued to run during the course of the experiment. These strategies were monitored but no details are given about how this was done, whether or not they were a threat to the validity of the experimental results, and what actions were taken if they were a threat, and when.

Following the Painter and Farrington (2001b) [86] paper are printed comments by a representative of the lighting company that funded the work. He said he was “glad that [the work] had a positive outcome” and that it was a powerful tool he hoped would be used to seek funds for public lighting in the face of competing interventions.¹⁶ Another commentator wrote of the “good news that it [the paper] brings to those who believe in the value of lighting.” Revealing though they may be, comments like these appear out of place in a scientific journal and lower its standing. The authors’ reply then puts the need for dose-response studies along with interviews of offenders and victims. They might usefully consider or reconsider the offender interviews described by Ramsay and Newton (1991) [96] (see Section 3.2), in which lighting was generally discounted as a factor.

An additional serious problem affecting the Dudley and Stoke-on-Trent studies is described in Section 5.2 below. Meanwhile, any one or more of the several shortcomings already described may be sufficiently serious to invalidate the conclusions, regardless of Scientific Methods Scores, the number of variables studied and the sophistication of the statistical analysis. Most of the findings of the two studies appear to be unreliable, including the supposed cost savings brought about by relighting expenditure. The effects of brighter lighting in increasing pedestrian numbers and reducing fear of crime might be thought reasonable and innocuous, but there is more on this in Section 5.2, and much more to come in Part 2 of this work.

4.5 PEASE (1998, 1999)

The two works [89, 90] discussed here are reviews rather than experiments. They are included because they give further insights into lighting and crime experiments, especially the Dudley and Stoke-on-Trent studies, and the influence of Situational Crime Prevention theory.

Pease (1999) [90] reviewed the lighting and crime literature and claimed that Painter’s recent research provided firm evidence that where street lighting improvements were successful, they reduced crime by day as well as at night, probably because of changes in street use, enhanced community pride and sense of area ownership. Pease’s re-analysis of the data also suggested that beneficial lighting effects are greater in chronically victimised areas.

Pease (1999) drew attention to the “rash” of existing reviews on lighting and crime while adding another. Apart from typographical errors and other signs of hasty preparation, the many more serious faults in his review virtually guarantee that more reviews will follow. An

¹⁶Presumably this refers to CCTV.

example is inconsistency between the review's own summary and similar material in another document issued earlier as a summary:

"4. In the most recent and sophisticated studies, street lighting improvements are associated with crime reductions in daytime as well as during the hours of darkness. This invites *speculation* that the effects of lighting work through community pride and sense of ownership as well as more directly through surveillance of offenders. Re-analysis of data from these studies suggests that lighting effects are greater in chronically victimised areas;" (Pease 1999, the full review).

"4. Street lighting improvements, where successful, are associated with crime reductions in daytime as well as during the hours of darkness. This result is of fundamental importance. It means that the effects of lighting work through something more general than improvement in the surveillability of potential offenders at night. The most plausible reasons for this pattern concerns [sic] changes in street use, enhanced community pride and sense of area ownership. Re-analysis of data from these studies suggests that lighting effects are greater in chronically victimised areas, which is of particular importance for integration of street lighting in other schemes devised under the provisions of the Crime and Disorder Act 1998;" (Pease 1998, the separate summary).

At least some of the factors that Pease saw as important are unlikely to have been recognized as potentially important when the original studies were designed. It is therefore unlikely that these factors would have been properly controlled for, if at all, nor might sufficient data have been collected, if any, to allow reliable conclusions to be drawn about them. Even if Pease's speculation about the reasons for the claimed effects of lighting is accepted as worth testing scientifically, new studies rather than reanalysis of existing data would be preferable to test the hypotheses.

In the summary document, Pease (1998) [89] stated:

"Crime prevention practitioners have always included lighting as part of their toolbox, and have advocated its use accordingly."

The statement may be true but is no proof at all of the effectiveness of lighting, merely a belief about it that has been maintained in recent years despite a shortage of reliable supporting evidence. Failure to qualify the statement accordingly could be interpreted as bias by omission.

Pease mentioned a view attributed to the UK Home Office that street lighting does not have effects in reducing crime, and he expressed concern that this view could exclude or limit the role of street lighting in local crime and disorder prevention strategies required under the UK Crime and Disorder Act 1998. At the time, neither the view nor the attribution was at odds with the consensus of research findings on the subject. The Home Office had funded some of the studies involved. A computer search through the text of the Act found no mention of lighting. Guidance documents on the Home Office website indicate that lighting can be included in programs set up under the Act to reduce fear of crime. Any lack of effectiveness in preventing crime does not appear to block the use of lighting. There is nothing to prevent trials of, say, *reduced intensity* or *reduced glare* street lighting as possible

crime and fear of crime reduction measures. But this is clearly not what Pease had in mind when he stated it was “timely to consider the effect of street lighting on crime afresh”.

Pease showed that the Atkins, Husain and Storey (1991) [3] data could be re-analysed to indicate that relighting in the Wandsworth part of London, over the years 1984 to 1989, reduced crime in the area, contrary to the original findings. In the absence of sufficient data for a proper time-series analysis, Pease used an abridged method that was invalid for several reasons. Although admitting that the procedure was only suggestive, he still quoted a numerical probability to support his views. But Pease’s work does draw attention to serious shortcomings in the Atkins, Husain and Storey study: for example, the weak experimental design, the presence of insufficiently identified data in an appendix, and an unjustified belief that lighting changes would not bring about social changes that could have some effect on crime in daylight.

Pease stated that his concern provided the motivation for his review of research evidence. The (UK) Lighting Industry Federation funded this review (Pease 1998). Publication of the review by the Institution of Lighting Engineers was prearranged to take place, regardless of the findings, as an indication of the reviewer’s independence. This information about motivation, funding and publication is given only in the summary document. Under the constraints of scientific method, the prearrangement might have been a necessary condition to indicate an effectively unbiased approach, but not a sufficient condition. The funding source and publication guarantee should not have been omitted from the full text of the review, which carries a later date than the summary document.

The Campbell Collaboration is a body committed to publishing high quality reviews of the effects of sociological and educational interventions. Its policy on conflict of interest includes the following statements (Campbell Collaboration 2002 [17]):

“Reviewers should report any conflict of interest capable of influencing their judgments, including personal, political, academic, and other possible conflicts, as well as financial conflicts. It is impossible to abolish conflict of interest, since the only person who does not have some vested interest in a subject is somebody who knows nothing about it... Disclosing a conflict of interest does not necessarily reduce the worth of a review and it does not imply dishonesty. However, conflicts of interest can influence judgments in subtle ways.”

“It is a matter of Campbell Collaboration policy that direct funding from a single source with a vested interest in the results of the review is not acceptable.”

Pease’s review does not comply with the long established requirement for a scientific review to have face validity in terms of freedom from conflict of interest.

In Pease’s review, the strength of the scientific evidence against lighting effects on crime is given unduly scant coverage. The massive and rigorous study of Sherman et al. (1997) [102] is referenced but its conclusions are ignored. The sole mention it gets from Pease is dismissal of part of just one sentence as opinion.

Pease’s review includes a ‘selected annotated bibliography’ of 13 papers and book chapters limited to those claiming some sort of beneficial effect of lighting in reducing crime or fear of crime, a further demonstration of unacceptable bias. Other specific examples are Pease’s descriptions of those sharing his views as “children of light”, and of those “...yet to be convinced of lighting effects on crime” as “disciples of darkness” having “dogmatic”

and “reactive” views, not to mention his assessment of Painter’s Dudley and Stoke work as a “technical tour de force” and “the last word”.

Pease (1999) [90] made much of Painter’s conclusion that increased lighting is an effective crime prevention measure when targeted to small ‘crime hotspots’. But this approach actually favours the return of false beneficial results, because:

- relative fluctuations in crime measures tend to be larger as the area selected becomes smaller,
- selecting a hotspot for treatment is to select an area that is experiencing crime at a level above the local and regional mean trends,
- crime in adjacent areas selected as controls is likely, or even constrained, to be lower than the local and regional mean trends,
- over time, regression to the mean tends to reduce crime both intrinsically and relatively in the hotspot area, independently of any effects of treatment applied to the hotspot area, and
- known hotspots tend to attract police attention (Sherman et al. 1997, Chapter 8) [102], which reduces the number of crimes committed (Marvell and Moody 1996 [67], Goodman 2002 [41]).

Painter and Farrington mentioned the regression to the mean effect in their papers as a potential threat to internal validity but Pease did not discuss it.

Overall, Pease’s review looks to be one-sided and unconvincing. Although it appears justifiable to continue testing for effects of lighting *changes* on day and night crime rates and displacement in new ad hoc studies, researchers do need to be reasonably disinterested in the outcome and a necessary but not sufficient condition for this is financial independence from lighting and related industries. In no way does this statement imply that the works referred to were subject to any sort of conscious bias because of the support arrangements, far from it. The issue is purely about the known need for a high degree of compliance with this aspect of scientific method.

4.6 QUINET AND NUNN (1998)

The Quinet and Nunn (1998) [95] paper reads as though based on a belief that more lighting does prevent crime and all that is required to demonstrate this is a sufficiently well designed and executed study. Nowhere do they mention the possibility that lighting may have no effect or even aid the commission of crime, as discussed by Eck (1997) [31], a work not reviewed although it had been available for well over a year before their publication date of December 1998. Their review does include uncritical brief summaries of Painter (1990, 1994b) [77, 80] and Painter and Farrington (1997) [82], but their own field study was based on data “... for particular crimes, and only for crimes occurring at night (the only meaningful time period to use when assessing the impact of lighting)”. This flatly contradicts one of the key claims of Painter and Farrington (1997). The work was supported by funding from the Indiana Electric Association.

Quinet and Nunn analysed the number of calls for police service before and after additional streetlights were placed in Indianapolis neighbourhoods to ‘enhance’ the lighting. Residents groups had the extra lights installed in areas where they thought they were needed, ie areas with apparently high crime incidence. Non-contiguous areas without lighting changes were included in the analysis as controls. Apart from the numbers of lights, no

details are given of the existing and additional lighting: whether the lamps were identical in type, output, beam spread, glare, mounting height etc., whether the numbers of poles and their spacings were changed and so on.

The analysis did not include displacement effects: “Either you have a control area free of influences from the experimental area, or you do not.” Some of the results suggest that criminals either did not know of this constraint, or ignored it. As Quinet and Nunn put it, some of the results of their “vigorous [sic] scientific assessment” were “mixed”, “very mixed”, “extremely mixed”, and “counter to expectations”, and “Although none of these differences were statistically significant changes, they are nonetheless suggestive of the expected deterrent influence of enhanced street lighting.” Despite their attempts to explain away the unexpected results, the overall finding is at best inconclusive. Of course, ‘mixed’ results are precisely what a statistically based study is likely to produce when the variables are unrelated, and also when a genuine relationship between the variables is either too weak for reliable detection using the available data and tests or is substantially affected by uncontrolled factors.

Quinet and Nunn concluded “The analysis of the target areas suggests that enhanced street lighting in particular neighborhoods is sometimes associated with concurrent reductions in reported crime.” The results really justified them saying ‘increases’ instead. Farrington and Welsh (2002a,b) [34, 35] reassessed their results and found an odds ratio of 0.75, meaning that crime was 25 % *more* likely in the experimental areas than in the control areas after the lighting treatment.

5 THE FARRINGTON AND WELSH META-ANALYSIS

5.1 REASONABLE BENEFIT LIMITS

A meta-analysis involves pooling separate experimental determinations of some quantity to give a weighted average likely to be more accurate than any of the individual contributing values. The accuracy of the result is likely to be improved if experiments of poor quality are given low weighting or discarded altogether. Important parts of the process are to collect all available relevant studies and to assess them against a rigid set of quality criteria. Provided that some key facts have not been overlooked or misinterpreted, the combined review and meta-analysis process can be expected to give results that are more reliable and accurate than results from typical single studies, and far better than generally indicated by experience, anecdotes or common beliefs.

Farrington and Welsh (2002a,b) [34, 35] is a review and meta-analysis of results of UK and US experiments on increased lighting for crime prevention. The search for accounts of street lighting and crime experiments was commendably thorough. The text shows evidence of favouring the view that lighting has a beneficial effect. For example, it has a heading “How may improved street lighting reduce crime?” Of the seven paragraphs in this section, six state or imply that lighting is beneficial and only one discusses some possible exceptions. There is also an uncritical reproduction of statements from a book version of Pease (1999) [90].

Eight papers from the USA and five from the UK were selected for the meta-analysis on the basis that they had sufficiently good experimental designs. This was necessarily a compromise between having plenty of results, some of which are of poor quality, or fewer results of better quality.

To provide a common basis for comparison, an odds ratio was calculated for each experimental result, given by

$$OR = Cca.Ceb/(Ccb.Cea)$$

where the number of crimes in the control area before the intervention is Ccb, and after, Cca. Likewise, Ceb and Cea are the number of crimes in the experimental area before and after the intervention. The odds ratio represents the proportional change in crime in the control area compared with the experimental area. It allows for extraneous influences that affect the crime levels in the experimental and control areas equally during either the 'before' or 'after' periods or both. An odds ratio of 1 means that the intervention had no net effect on crime. A greater value represents a beneficial result of more lighting, and less than 1 indicates a counterproductive effect.

The odds ratios for the 13 studies ranged from 3.82 to 0.75. The overall weighted odds ratio was 1.25, with a 95% confidence interval of 1.18 to 1.32. This means that a well designed and conducted 'standard' lighting intervention could most likely result in a 20% reduction of total crime ($1 - 1/1.25 = 0.2$) in the relit area as a beneficial outcome. Needless to say, the authors would be justified in feeling pleased at being first to achieve such an important and conclusive result after decades of claim and counterclaim about the overall effect of lighting on the incidence of crime. But there is still some checking to do.

Despite the Tien et al. (1977) [107] warning about inadequate photometry, the situation has not improved markedly. Farrington and Welsh (2002a,b) searched their 13 selected papers for measures of the lighting changes and were able to state the after/before intensity¹⁷ ratio in only seven cases. For the US studies, this ratio was given as 7 in Milwaukee, 4 in Atlanta, 3 in Fort Worth and 2 in Portland. They missed one in Quinet and Nunn (1998) [95], where the treatment was to double the number of street lights, presumably doubling the intensity and the mean illuminance. In the UK, it was 5 in Stoke-on-Trent, approximately 2 in Bristol and 2 in Dudley. The arithmetic mean ratio for eight cases is 3.375. Thus, the Farrington and Welsh meta-analysis tells us a lighting increase of about 3.375 times will tend to produce an odds ratio of 1.25, ie a reduction of 20% in crime in the experimental area relative to the control area.

What is conspicuously missing is the range of illuminance values over which this relationship was derived. For predictive purposes, this range would also be the minimum for which the odds ratio could be expected to apply. The papers available to the writer have quite limited information about the actual illuminance in each experimental area before or after the 13 lighting interventions. The illuminance range therefore has to be estimated. It appears reasonable to search the literature for other clues about before or after values, about values that have been measured in other lighting and crime experiments, and about the values used in lighting practice.

Fisher (1997, Table 5) [37] gave minimum before values of 0.1 to 1.3 lux and minimum after values of 2.5 to 4 lux for six UK lighting and crime studies, and values in his tables of minimum, average and maximum recommended values from the British and Australian Standards for road and public lighting range from <0.07 lux in service to 350 lux (Fisher 1997, Tables 2, 6). Nair et al. (1997) [72] mentioned measured extremes of 1 and 32 lux in their experiment, although the most representative before value was more than 1 lux. Vermeulen (1992) [114] gave a desirable operating range for ccd video cameras as 8.2 to 32

¹⁷The peak or mean illuminance ratio may be more appropriate. The difference from an intensity ratio can be substantial, depending, inter alia, on differences in how the light is distributed before and after the intervention.

lux. Data points on graphs in Boyce and Rea (1990) [13] range from about 0.1 to 80 lux, and in Boyce et al. (2000) [14], about 0.1 to 180 lux. Philips (2002) [91] recommended 300 to 500 lux for shop interior lighting. Pollard (1994) [92] gave a maximum of 900 lux for building floodlighting. The writer has measured peak values of over 1400 lux at a footpath in Melbourne at night, which is well into the range of natural daylight. As mentioned in Section 7.2 below, values of over 450 lux resulted from relighting of railway stations as one of several supposed crime-prevention measures (Carr and Spring 1993 [18]). For the present discussion only, an upper bound to values that have been used in relighting experiments, including relit indoor retail areas such as that in Poyner and Webb (1997) [94], is chosen to be 1000 lux as conservative and convenient.

The lower limit might readily be taken as 0.1 lux. However, this happens to be the lowest indication available on typical good-quality hand-held light meters, a reason to expect that lower before values have been, or could have been, present in actual experiments or real-world areas selected for lighting or additional lighting. There is another reason also. Illuminance from the first quarter moon at night is less than 0.1 lux, but common experience is that even this is enough to be seen as markedly brighter and perceived as safer than natural moonless night outdoor illuminances, which can be several factors of ten dimmer than 0.1 lux. Therefore the lower limit for lighting and crime experiments in the real world can be taken to be at least as low as 0.01 lux.

Thus in round figures, every point of the range 0.01 lux to 1000 lux has been, could have been or is still likely to be within the range encompassed by lighting and crime experiments. The experiments included in the meta-analysis appear to have covered most of this range, and the result of the meta-analysis should therefore be representative of lighting changes over this range, at least. There may well be some non-linearity in dose-response over this range, but the unstated assumption of Farrington and Welsh is that the odds ratio for a given lighting change anywhere in the range is constant as a first approximation. From any starting point within this range, a lighting increase of 3.375 times should produce a crime-reduction odds ratio of 1.25 according to the meta-analysis. Even if the 'real' value varies with absolute value of illuminance, with the value given being typical or some sort of average, it would not overturn the present argument.

If a lighting intervention takes place from the low end of the range, there is no reason to suppose that the new lighting could not then be usefully increased a second time from 0.03375 lux to 0.114 lux as an intervention for a further crime reduction. After all, the starting points for the 13 studies appear to have been spread over much of the range in question, and at least in the papers to hand there is no mention of how many prior 'improvements' have contributed to the present lighting levels and what the effects on crime might have been on each occasion. This process could continue until 1000 lux would be exceeded, at least. In this case, nine serial interventions would bring the after illuminance to 568 lux, and the net odds ratio would be the ninth power of 1.25. This works out at 7.45. From the definition of odds ratio, this means crime in the control area would increase to be 7.45 times that in the experimental area. So by adding lots of light to a really dim area to bring it up to light levels typical of retail sales areas, crime in the treated area relative to the control area should reduce to about 13 % of its original incidence. In practical terms, this would be a dramatic reduction.

Whether such changes are obtained in a single large step or as a succession of two or more smaller steps should not matter – either the derived odds ratio applies uniformly across

the lighting range or the total effect of successive applications of a non-constant odds ratio should be the same. Otherwise crime level under a particular lighting level at a given place would have to depend on the history of lighting changes, seemingly a rather implausible proposition. If large lighting increases did indeed produce such large reductions in crime it would have been obvious long ago, but nothing of the sort appears to have been reported, even anecdotally. The conclusion is that the magnitude of the odds ratio derived by Farrington and Welsh is improbably large. The true value, assuming a constant effect over the likely lighting range, must be smaller than 1.25.

Not only should the most likely value for the odds ratio have a credible magnitude but its 95% confidence interval limits should also meet this condition. The present lower value, 1.18, raised to the ninth power, is 4.435. This represents a crime reduction to just 22.5 %, again beyond any likelihood of practical realisation. When raised to the ninth power, the present upper limit of 1.32 leads to a value of 12.17. Crime in the treated area would thus drop to 8.22 %, even further from any reasonable expectation. *The meta-analysis result must be erroneous*, not mathematically or statistically, but as a guide to the real effect of lighting changes on crime.

If it is accepted that the upper limit for the 95% confidence interval has to be within reason, the overall crime reduction would need to be no more than 50 %, say, and even this might be thought rather optimistic. The total odds ratio for nine typical treatments would thus be 2.0. The ninth root of this is 1.080. To a first approximation, the whole 95% confidence interval for the overall result shown on the Farrington and Welsh forest diagram would need to be shifted leftwards on the logarithmic scale so that its rightmost limit was at the position for an odds ratio of 1.08. When this is done, the lower limit for the 95% confidence interval is actually to the left of the odds ratio = 1.0 axis. This revised result has the overall odds ratio for the 13 studies at about 1.05, which is not significantly different from the 'no effect' odds ratio value of 1.0.

Assuming that the meta-analysis processes and algorithms are correct, some or all of the experimental odds ratios must be too large. If they are all inflated by the same proportion as found here, because of some biasing effect such as funding bias or targeting high crime areas, the individual results would all be about 20 % too large. If a correction of this size is applied to all of the 13 studies, in 11 cases the corrected value still lies within the 95% confidence interval. This is not a reason to apply such a correction, merely a demonstration that a systematic bias smaller than the 95% confidence interval can change the overall result from lighting preventing crime to lighting having no effect on crime. It would even be possible for the true result to be a small counterproductive effect of lighting on crime, falling within the 95% confidence interval of the 'corrected' odds ratio. It is difficult to accept the meta-analysis result as showing anything definite at all.

Looking again at the data from the studies selected for the meta-analysis, some are confined to measures of crime at night, while others are for crime by day and night. Thus the meta-analysis indiscriminately mixes direct and indirect effects of lighting at night with indirect effects by day. It is possible that some of the indirect effects could have time constants of several years, which is longer than the sampling period generally employed in lighting and crime experiments to date. No account is taken of sampling periods in the meta-analysis, which adds to the uncertainty of what the result is supposed to mean. But there does not appear to be any reasonable way in which the erroneously large result could have arisen

simply because of the mixture of direct and indirect effects in the individual odds ratios and in the overall result.

There is no obvious way in which any supposed non-linear dose-response relationship could retrieve the situation for increased lighting as a crime-reducing intervention. This casts suspicion on the review process as not rejecting unacceptably faulty studies. This topic is taken up in Section 5.5 below.

The title of Farrington and Welsh (2002a,b) and the text in places restricts the scope of the experiments reviewed to street lighting but lighting interventions inside a market hall and inside a car park building are included in the analysis. The reasons for their inclusion are not objectionable; instead, the problem lies with the restriction to street lighting. ‘Public lighting’ might have been a better choice. The market and car park studies are of particular interest for other reasons and are discussed in Sections 5.3 and 5.4.

5.2 MORE ON THE DUDLEY AND STOKE-ON-TRENT STUDIES

After the relighting treatment in Dudley, the illuminance was a minimum of 2.5 lux and a maximum of 6 lux. The treatment was a lighting increase of more than a factor of 2, but rounded to 2 in Farrington and Welsh (2002a,b). Taking it as 2, the initial luminance comes out as between 1.25 lux and 3 lux. When relighting is eventually extended to all of the surrounding areas, it is quite possible that someone will come along in due course and see a need for relighting what was the experimental area. Given the popularity of lighting for the pride of place and crime prevention mindset, this would appear quite likely. There appears to be no reason why this process should stop there, so it will continue until the streets are lit to near daylight levels or at least to levels currently found in downtown areas of big cities and the entrances to large suburban shopping malls. From a representative starting value of 2 lux, say, eight treatments in succession would bring the representative illuminance to 512 lux.

The Dudley study odds ratio assigned by Farrington and Welsh, 1.44, means that each of the eight treatments would reduce crime in the experimental area, relative to the control, to $1/1.44$ of its initial value, ie 0.694. Eight such treatments in succession would therefore reduce crime to the eighth power of 0.694, ie 0.054. If crime really could be reduced by 95% by intense floodlighting of residential streets it would have become standard practice years ago, at least in suburbs populated by the well-off. The experimental result for the Dudley study is therefore improbably large, presumably from confounding by the asymmetric change in police presence or from some unknown factor. Crime reductions like that in the Dudley study or larger were claimed for relighting increments in presentations to conferences of the (UK) Institution of Lighting Engineers on at least two occasions (1989 and 1994), apparently without sufficient comment to alert the researcher to the problem before publication of at least five more papers with similar errors.

In the conclusions to the Painter and Farrington (1997, p 225) paper [82], the authors state:

“Another key issue is the ‘dose-response’ curve relating street lighting and crime; it may be that improved street lighting decreased crime in Dudley because the improvement was so dramatic.”

The total response range of the human eye, from absolute threshold to the brightest tolerable light, is over 11 log units or 10¹¹ (100 billion) in luminance. The lighting increase in Dudley was a factor of a little over 2, equivalent to about 0.3 log unit, quite a small part of the total range. This highlights the contrast between the extensive behavioural data gathering and statistical analysis in the study and the paucity of attention to photometric aspects of the treatment and effects of this on visual performance.

It is not only the result for crime effect that is too large in the Dudley study. The social effects claimed would compound to massive changes with eight successive treatments.¹⁸ This is further reason to suspect that something is fundamentally wrong with the study.

Similar problems arise with the Stoke-on-Trent results. The odds ratio for crime is 1.72 and the lighting increment was a factor of five (0.7 log unit). From a supposed 1-lux starting point, four successive treatments would reach 625 lux. Crime in the experimental area would be expected to fall to 0.114, ie 11.4 %, after four treatments. The crime result for the initial treatment is again overlarge, and so must be the beneficial social effects reported.

The foregoing discussion would be valid for a uniform lighting increment affecting the whole of the experimental area. Increased lighting was only applied to the main streets, however. If crimes were only committed within the area directly illuminated by these lights, then the argument presented above would apply. Given the physical form and placement of the houses on the estate, however, there would be many areas that were partly or fully shaded from the street lights. At night, these areas would receive light from natural sources, from artificial skyglow, from escaping room light, and from porch and security lights. All of these could be expected to affect experimental and control areas equally. In both areas, this light would be incremented by scattered and reflected street light. The mean lighting treatment increment in the experimental area would therefore be less than the treatment increment in the relit streets. This would not matter if crime had only taken place in the streets and not in the dimmer areas, but no information about the location of crime within the experimental areas is given in any of the papers by Painter and Farrington. Burglaries at least could be expected to take place sometimes in dimly lit areas. The after/before illuminance ratio quoted for the experimental estates is therefore overstated. Even more of the mean lighting treatments could therefore be fitted into the range from 0.01 lux to 1000 lux. This bolsters the conclusion that the claimed crime reduction results for a single treatment are improbably large.

5.3 THE BIRMINGHAM MARKET STUDY

The largest odds ratio reported for the 13 studies included in the meta-analysis was for the Birmingham Market study. This study is reviewed here to see if the large odds ratio is justifiable.

Poyner and Webb (1997) [94] is an edited version of an earlier paper by Poyner and Webb (1987) [93]. It is about attempts to deter the theft of purses from shopping bags usually carried by women in one of the largest English retail market places, centred on the Birmingham Bull Ring.

Table 2 reproduces key data and results of the study. The number of stalls in each area of the market in April 1985 is shown. These numbers had been approximately steady in the

¹⁸Even four successive treatments might be enough to have people dancing in the streets!

first three parts of the market but the number of stalls in the fourth part grew substantially over the seven years covered. At the end of this time, traders complained that the market overall was less busy than formerly but no data are given about this. A trading decline could have contributed to the falling total crime numbers as a consequence of there being less of the crowding that was used to advantage by the thieves.

YEAR PLACE	1978	1982	1983	1984	1985
Rag Market (large shed)	52	82	54	17	12 (552 stalls)
Open Market	54	21	45	33	12 (158 stalls)
Market Hall	20	4	11	12	9 (197 stalls)
Flea Market (outdoors)	- (few stalls)	-	2	2	- (231 stalls)
TOTALS	126	107	112	64	33
EVENTS	Original study	Police unit operating until end of 1982. Open Market partly roofed	Wider aisles installed in Open Market, early 1983	New ceiling and lighting installed in Rag Market, late 1983	

The thefts tended to be restricted to midday to 2 pm on Tuesdays and 1 pm to 4 pm on Friday and Saturday, the three busiest days of the week when all four of the markets listed were open. Daylight is bright at the times mentioned, possibly indicating that light was facilitating the thefts rather than deterring them. This possibility is reinforced by the fact that the crimes were mostly committed in the summer months. The authors assumed that police were more active in the Open Market and Market Hall in 1982, thereby displacing crime to the Rag Market. This allowed the 1982 Rag Market crime peak to be ignored and the new lighting in the Rag Market to be identified as the reason for the drop in crime from 1983 through 1985, although this time without apparent displacement.

Poyner and Webb stated that the reduction in crime in the Rag Market in 1983 and 1984 was

“the first clear evidence found by the authors to show that improved illumination levels reduce crime. It is perhaps paradoxical that the crime concerned only occurs during daylight hours.”

It is even more paradoxical that it was mostly during early afternoons in summer. Presumably ingress of daylight through windows, skylights etc. was supplementing the existing indoor artificial lighting system and facilitating crime but putting in a ceiling (a confounding change by itself) and replacement artificial lighting of greater electrical efficiency is supposed to do the opposite. Another explanation is that the Rag Market or the four markets were the location for a crime hotspot that reached its peak in 1982 and 1983 and was in decline or moving laterally thereafter. A third explanation came from the security staff at

the market, who believed that congestion was a primary facilitator of the thefts. They had moved stall holders within the Rag Market to reduce congestion and the bumping that was part of the purse-stealing action. The relocation added further confounding to this unplanned and poorly controlled 'experiment'.

Farrington and Welsh (2002a,b) assigned an odds ratio of 3.82 to the lighting intervention in the Rag Market. This appears to have been reached by summing the Rag Market figures for 1982 and 1983 as the experimental before value, with the after value likewise as the sum of the 1984 and 1985 figures. Open Market and Market Hall data were pooled and summed similarly for the control values. But data for 1982 are suspect because of the police presence and the confounding change in aisle width introduced in the experimental area in early 1983. Eck (1997) [31] mentioned only the aisle widening as a factor in the reduction of the purse thefts, not lighting, and stated that simultaneous changes in nearby markets made them unsuitable as control places so there was "no evidence about background trends".

Poyner and Webb (1997) [94] gave no photometric details beyond mention of "improved illumination levels". At a shopping mall described as 'dingy' and in need of more lighting, the illuminance was about 40 lux (Horner 2002) [46]. For this discussion, assume that this was the before condition in the Rag Market, and that the treatment doubled the illuminance. Four treatments like this in succession would have brought the illuminance to 640 lux. With an odds ratio of 3.82, four treatments in succession would give an overall odds ratio of 213, reducing crime to less than 0.5 % in the treated area. Even a single additional lighting treatment would produce an overall odds ratio of 14.6, which would mean a crime reduction to 6.9 %, still well beyond any reasonable expectations. Leaving the 1982 data out gives an odds ratio of 2.19. Although this is a more credible value than 3.82, it is still greater than the next two largest values of the 13 studies (which were Stoke-on-Trent and Dudley). It leads to an improbably large crime reduction, to 21 %, for just two successive treatments.

Poyner and Webb (1997) [94] concluded that "the two original hypotheses ... have been proved to a considerable extent." Hypotheses about a non-closed system cannot be proved at all, however.

The case for attributing any, let alone all, of the reduction in crime to the lighting change appears to be so problematical that the study should have not have been included in the meta-analysis. The same conclusion could be reached by considering the odds ratio to be so large as to be an outlier justifying exclusion.

5.4 THE DOVER CAR PARK STUDY

The meta-analysis includes the Poyner and Webb (1987) [93] study of theft of and from cars in a multi-storey, long-stay council car park in Dover (UK). By 1983, the authorities realised that their security program, which combined private security officers patrolling the car park at night and random visits from council inspectors during the day, was not working. Vandalism was a problem – graffiti; broken windows; damage to lifts, doors, sand buckets and fire extinguishers; and defecation on stairs.

Geason and Wilson (1990) [40] described the interventions as including:

"... gaps between the low walls around the ground floor of the car park were filled with wire mesh; the pedestrian entrance by the staircase was fitted with a self-closing steel door so that it could be used only as an exit; lighting at

the main entrance and the pedestrian exit door was improved; and to provide surveillance, an office was built beside the main entrance and leased to a taxi firm operating 24 hours a day.”

These modifications limited access by pedestrians. Although supervision of the entrances and exits was not particularly thorough, the presence of security guards or manned barriers appeared to reduce the incidence of car theft greatly. Adjacent open-air car parks were used as the control. However, theft of items *from* cars actually increased by nearly as much as car thefts reduced. Poyner and Webb concluded that car thefts were committed by outsiders with no business in the car park, but thefts *from* cars were done either by legitimate users tempted by opportunity, or people who drove in specifically to steal. Environmental prevention measures worked against the car thieves, but were [worse than?] useless in dealing with determined petty thieves.

Although Farrington and Welsh recognised that added fencing and entrance supervision had confounded the effect of increased lighting, they ascribed the overall reduction in crime to the effect of the lighting changes. There seems to be no compelling reason why any of the change in crime at all can be claimed to be a result of lighting changes, so it seems that this study should have not have been included in the meta-analysis either. An alternative would be to apportion contributions from all of the interventions and reduce the odds ratio (1.14) to that proportion attributed to lighting. If lighting were estimated to have contributed one-third, say, the odds ratio would be the cube root of 1.14, ie 1.045.

Apportioning all of the crime reduction to lighting has the effect of overestimating the relevant odds ratio, which results in the weighted average being an overestimate by a smaller amount.

5.5 IMPROVING THE META-ANALYSIS

The largest odds ratio in the meta-analysis of the five British studies of Farrington and Welsh (2002a,b) is 3.82. This is from the case discussed above in Section 5.2. Not only is this odds ratio suspect because of its size but the inclusion of the study itself is questionable because of the doubt that lighting was responsible for any part of the observed change in crime, let alone all of it. Again this makes the overall odds ratio larger than it should be.

The next largest odds ratios of the five are 1.72 for the Stoke-on-Trent study and 1.44 for the Dudley study. Serious doubts have been raised above about the validity of both of these studies and their inclusion in the meta-analysis is therefore questionable. From the preceding section, the Dover study also looks as though it should not have been included. This leaves just the Bristol study, which is suspect because of the long gap between before and after measures, the hotchpotch of drawn-out lighting changes and the choice of high-crime areas for special lighting treatment. It is therefore hard to give any credence at all to the meta-analysis of the UK studies, which returned an overall odds ratio of 1.42.

The associated lighting increase is not known for three of the five UK studies and its mean for the other two was about 3.5 times. The mean for the five studies is not likely to be much different. The overall odds ratio for the UK is improbably large for any likely value of the mean lighting increase.

The eight US studies produced weighted combined odds ratios of 1.02 for property crimes, 1.07 for violence crimes and 1.08 overall.¹⁹ Of these studies, only the Indianapolis one of Quinet and Nunn (1998) [95] is reviewed above, and it certainly has unfortunate features. Farrington and Welsh drew attention to a number of shortcomings in the remaining studies. Lab (1997) [55] did also in two cases, giving less favourable assessments as can be seen:

a. Fort Worth

“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 & Sullivan, 1979, p. 75 [59]). Since crime in the whole city stayed constant (a decrease of 1.1 %), it may 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.” (Farrington and Welsh 2002a,b)

“Lewis and Sullivan found that a threefold increase in lighting did not appear to reduce crime in areas of Fort Worth, Texas.” (Lab (1997) [55])

b. Atlanta

“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, pp. 11-12). There was an increase in assaults in the experimental area, but the number was 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.” (Farrington and Welsh 2002a,b)

“The City of Atlanta found that the relit areas of a high-crime census tract experienced a greater increase in robbery and assault compared to unrelit areas (Atlanta, 1975).” (Lab 1997)

Farrington and Welsh (2002a,b) assigned odds ratios of 1.38 ($p < 0.1$) to Fort Worth and 1.39 ($p < 0.05$) to Atlanta.

In relation to the British studies, Farrington and Welsh stated:

“In most cases, the experimental area was chosen for relighting because it was a high crime area. This high crime rate raises the problem of ‘regression to the mean’; an area that has a high crime rate at one time is likely to have a lower

¹⁹The overall total crime value does not fall between the overall values for violence and property crimes because the total for Milwaukee is much larger than the values given for violence and property crimes and because only the totals of these two categories was available or acceptable for Fort Worth and Indianapolis. Some of the contributing measures were also for different durations.

crime rate at another time. To investigate this possibility, long time series of crimes before and after the intervention in experimental and control areas are needed.”

Only the Bristol study of the British set includes time-series observations. Crime values were given for nine successive six-month periods but the treatment was staged over 28 months. This study does not meet the usual criteria for a time-series analysis of treatment effects, eg at least five before time periods.

Farrington and Welsh did not mention the regression to the mean problem in relation to the US studies. But the likelihood again is that relighting of particular areas was done to try to control an existing high crime rate, not because the opportunity was available for well-matched experimental and control areas for scientific purposes. Unless there is evidence to show that this confounding effect did not apply or was insignificant, the US results remain suspect.

If the eight US studies are accepted in the absence of further information, Farrington and Welsh’s finding of an overall odds ratio of 1.08 for them becomes the result of the meta-analysis. But the problems do not end there.

Farrington and Welsh cited a reference that the reader has to consult to try to reproduce the weighting factors used to combine the various estimates of the odds ratio. One should not have to ‘second guess’ the authors in this way. Explicit details of the weighting factors or other essential features of the process should have been given so that readers could readily check the calculations for themselves, and examine the effect of removing one or more of the studies from the pool used to find the best estimate of the odds ratio.²⁰

5.5.1 Conflicts of interest issues

The Cochrane Handbook (Clarke and Oxman 2002 [22]) sets out necessary requirements for acceptable scientific quality of healthcare review articles. It was used as a model for the Campbell Collaboration (2002) [17] guidelines. In the case of conflict of interest issues, the two are virtually identical: reviewers should report any conflict of interest capable of influencing their judgements, including personal, political, academic and other possible conflicts, as well as financial conflicts. It is hardly surprising that the rules for good science are consistent across disciplines. Within the healthcare discipline, information about sources of funding is considered a desirable inclusion in trials reports also (Moher, Schultz and Altman 2001 [69]).

The Dudley and Stoke-on-Trent papers both acknowledge the managing director of a lighting company for funding the research. Insofar as the funding provided the means to do the research and thereby benefitted both authors, and that one of the authors was also an author of the review, it would seem that these two potential sources of bias should have been mentioned explicitly in the review, but they were not. Farrington and Petrosino (2001) [33] suggested a solution to the problem of a review author also being an author of one or more of the included papers; this is for the review to have an additional author, one who had not previously worked on the topic. This is understood to be the case for the review and meta-analysis in question, but halving potential bias does not eliminate it. Under the Collaboration

²⁰A pair of possible weightings for the UK odds ratio and the US odds ratio can be found from the combined odds ratio as the roots of a quadratic equation.

guidelines it is up to authors to decide whether to mention potential sources of bias. In this case, its non-mention does not seem justified, particularly in view of the unpalatably large effects of financial and nonfinancial conflicts of interest that have been described recently in leading scientific journals (see Section 2.1 above.)

5.5.2 Measure of effect

The odds ratio used by Farrington and Welsh (2002a,b) seems to be inverse of that required. It most directly represents a change in crime in the control area relative to the experimental area, which is not the most convenient arrangement when making practical use of the result. In an ideal quasi-experiment, extraneous influences would be negligible and so would be the actual after/before change in the control area. In further lighting and crime experiments and analysis, it is suggested that the measure of effect should be the reciprocal of the odds ratio used by Farrington and Welsh. Then it would directly describe the effective after/before change in crime in the treated area. A decrease in crime would give an odds ratio of less than 1, and an increase in crime, an odds ratio of greater than 1. To avoid possible confusion, the quantity might usefully be given a different name, say 'crime ratio', 'crime response', or 'crime effect ratio'. It would also seem useful to try to keep direct and indirect effects separate, or at least night and day effects.

6 FEAR OF CRIME

The literature on fear of crime is extensive (Semmens 1999 [99]), although much of it is not about lighting. Other bibliographies (eg Nottingham 2001 [75]) provide additional material. Sherman et al. (1997) [102] has a critical review, including the use of lighting to allay fear. The widely accepted position is that people tend to fear crime more when they are in dark or dimly lit places, especially if there are no others or a small number of strangers about. In given circumstances, fear of crime is almost universally reported as being greater at night than it is by day, regardless of what the risks of actual crime are. Females are generally reported as being more fearful of crime than males. Survey data (eg Maguire and Pastore 2002, Table 2.4.1 [64]) tend to support these statements. It therefore reasonable to use artificial lighting to reduce fear of crime provided that this does not materially increase the risk of actual crime or cause some other adverse effect such as affecting driver vision or threatening biodiversity.

Caminada and van Bommel (1980) [16] devised experiments based on the observation that people tend to feel safer when there is enough ambient light to recognise faces of others nearby. They found that semi-cylindrical illuminance is a better indicator than other measures, such as vertical plane illuminance, for defining the face recognition distance. The cylinder axis remains vertical. The azimuth of the axial plane can be in either of two orientations, as appropriate: one parallel to the street length and the other normal to it. For face recognition at 4 m and 10 m, the semi-cylindrical illuminance needs to be 0.8 and 2.7 lux respectively, provided there are no sources of excessive glare present. At 4 m, an alert person was thought able to take evasive or defensive action if a threat were perceived. The 10 m distance was considered to provide a greater margin for comfort.

Caminada and van Bommel's work provides an empirical quantitative basis for lighting as a means to reduce fear. The concept of face recognition distance is mentioned in the

1987 to 1990 setting of a British code of practice for lighting of roads and pedestrian areas (Ramsay and Newton 1991 [96]), but three levels were set according to the perceived risk of crime. Caminada and van Bommel's values, rounded up to 1 and 3 lux, reappear as the minimum and average illuminances for the *lowest* perceived risk level. Their preference for semicylindrical illuminances was not adopted at the time, but the lighting profession still discusses the issue.

It is instructive to compare the values just given, 0.8 and 2.7 lux, with the minimum values in UK relighting studies (Fisher 1997 [37]): 'before', 0.1 to 1.3 lux, and 'after', 2.5 to 4 lux. It is not surprising that interviewed subjects responded favourably about the effect of brighter lighting in reducing fear. But in getting this reduction, average illuminances were increased by as much as 11 times and minimum illuminances by up to 40 times in particular experiments. The practical significance of such increases extends to possible short- and longer-term effects on the actual crime rate.

Nair, McNair and Ditton (1997) [72] described "unexpected benefits" to young pedestrians from increased lighting in a Glasgow street. The change in this case involved a 40% reduction in energy use with new high-pressure sodium lamps and a better light distribution instead of older lamps described as of the same SON-T type. Because of commitments of the authors on other relighting projects at the time, the only data available for analysis was interview material collected on two days before relighting and more collected on two days a year later. Analysis of before and after interviews indicated that pedestrian fears were reliably lessened after the relighting. Recorded actual crime results were not available for analysis. In comments printed with the paper, Simons criticised the lack of detail about the quality and quantity of the lighting, and stated that the method was not sufficiently robust to allow firm conclusions to be drawn. In further comments, Painter and Farrington criticised the use of inappropriate statistical techniques to analyse the data and pointed out various numerical errors. They concluded by saying "in short, this paper demonstrates how *not* to analyse the relationship between street lighting and crime." The absence of reference, either in the paper or in the comments, to any of the prior papers finding no lighting-crime relationship is not good science.

Tulloch, Lupton, Blood, Tulloch, Jennett and Enders (1998) [110] has over 600 pages on fear of crime, not all of them getting to the point quickly. Its references to lighting are often references to Painter's work, which seems to be accepted uncritically as "an impressive record of achievement." Even assertions about remarkably good social changes brought about by imprecisely quantified lighting changes are not queried. The boundary between apparently genuine changes in fear of crime and dubious claims about actual crime as affected by lighting is often indistinct in this report. Although Painter's 'euphoric' or worse approach (pp 172, 173, 175) does lead to a warning, pages are devoted uncritically to her "'enhanced street lighting' discourse... a winner at the conference!" Despite its publication date, the report does not mention Sherman et al. (1997) [102] or its chapter by Eck (1997) [31], or the book by Lab (1997) [55], both of which deal with lighting and some of Painter's work.

Although the recommendations of the Tulloch et al. report are aligned with common experience that lighting does tend to allay *fear* of crime, they would surely have been different if the authors had been more aware of the weakness of the case for lighting as a crime deterrent. The report needs to be reissued with substantial corrections or withdrawn altogether so that it will not continue encouraging authorities to spend money on lighting as a supposed

crime prevention strategy. Greater caution is also required in advising authorities on the use of lighting to allay fear.²¹

Boyce, Eklund, Hamilton and Bruno (2000) [14] carried out four field studies on fear of crime in streets of New York City and Albany, NY, and in urban and suburban parking lots. They cited papers by Painter, early and late, as evidence that lighting reduces crime as well as fear of crime, but did not mention papers by others with inconclusive or contrary findings on actual crime and lighting. They did pay heed to the need to reduce lighting energy waste and adverse environmental effects of excessive light.

In the field studies, groups of subjects visited sites and answered questionnaires about perceptions of lighting quality and safety in the night visits and about safety in the day visits. Sufficient good quality lighting allowed the perception of safety at night to approach but not exceed that in daytime: “lighting cannot make an urban parking lot be perceived to be as safe as a suburban parking lot”. At night, horizontal illuminance of about 20 lux on the ground typically brought the perception of safety close to the daytime value. Some of the areas studied had existing illuminances approaching 200 lux. Good lighting judgements consistently involved attributes of ‘bright’, ‘even’, ‘comfortable’, ‘not glaring’, ‘extensive’ and ‘well matched to site’. Bad lighting had the opposite attributes. The writer interprets this as supporting a notion that the perception of good (fear-reducing) lighting can be retained while reducing illuminance provided that glare is sufficiently reduced also.

Boyce et al. also studied the effect of luminaire spectral radiance differences on perceived safety and lighting characteristics in a parking lot at night. The perception of safety was not affected by spectral differences. This is tantamount to saying that colour, at least within the range of chromaticities produced by present artificial light sources, has no effect by itself. The extent of special consideration needed for colour vision deficient with the protan defect was not raised.

A lighting company appears to have conducted its own research into the longstanding question of whether the colour of light has any influence on alleviation of fear. The lamp manufacturer has published the company’s account of it (Bennett 2000? [8]). The literature review is brief and limited to Pease (1999) [90]. Results of a relighting project were examined by a survey. The method of selection and the number of individuals participating is not stated, just the percentages of responses. The previous lights were high-pressure sodium (HPS) with orange-white light (ie the type described by Painter (eg 1994a [79]) as ‘white’ and claimed to reduce crime and fear of crime). It was claimed that the new (really-) white lights were preferred by 100 % of respondents. The conclusion was that “the perceived risk of crime can be substantially reduced through the introduction of white light.” This contradicts Painter’s results for the cases where a reduction in perceived risk was claimed when bluish-white mercury-vapour (in Dudley) or warm-white incandescent lamps (in Stoke-on-Trent) were replaced by HPS. There is no mention of controls, reliability of the results or confounding by differences in light distribution or light output.²² Brief reports of the finding, mostly with no reference to the source, have been circulating in trade magazines of the lighting industry, which presumably accepts them in good faith as fact. In the normal course of

²¹Part 2 goes even further than this.

²²All of the outdoor luminaires shown in the four illustrations of the paper appear to be emitting an appreciable amount of light at and above the horizontal, contrary to a company representative’s views about minimising light pollution and glare (Baldrey 2000 [4]).

events, the security industry could be expected to accept the relayed information as factual also.

The California Energy Commission is currently funding research on outdoor lighting levels, including fear of crime aspects. Preliminary reports mention some paradoxical results (Lighting.com 2002 [60], NBI 2002 [73]). People ask for moonlight-equivalent illumination and then reject, as too dim, levels hundreds of times greater. Glare and uniformity seems to be a factor in this. White light appears to be better than yellow light. More account needs to be taken of vehicle headlight glare as it affects pedestrian vision and of outdoor lighting glare affecting driver vision.

7 LIGHTS, CAMERAS, SECURITY?

7.1 SECURITY LIGHTING

Boyce and Rea (1990) [13] used low-pressure sodium lamps and high-pressure sodium lamps with street lighting and floodlighting distributions along with rural moonless darkness in an experiment where alerted 'guards' had to detect and recognise 'intruders' walking along a path or moving so as not to be seen in a large open area. Lighting was mostly better than darkness for detection and recognition, but there was little difference between floodlighting and darkness in the open area task. There was no disadvantage for low-pressure sodium lamps in detection or in face recognition, but a (reasonable) caution not based on the results at hand was given against its use when colour recognition could be important. Recognition was better with the more diffuse light distribution (street lighting). Vertical plane illuminances of between 4 and 10 lux were recommended for security lighting installations to give a high level of detection and recognition. The desirability of limiting light spill was not mentioned.

This experiment gave the greatest possible advantage to the guards and the greatest possible disadvantage to the intruders. For instance, the guards had the lights above and behind them, while the intruders had to move towards these intense glare sources. Reversed lighting direction relative to the intruder and guard positions must also occur in practice but it was not mentioned let alone subject to investigation. Nor is this the only practical case in which lighting will tend to aid intruders more than it hampers them. Most security-lit areas are not under continuous surveillance by guards, and prospective intruders may be able to time their incursions appropriately, in which case the lighting may then be a distinct advantage.

The recommended illuminances are doubtless accepted by the security lighting industry as impeccable scientific guidance. Regardless, far brighter installations are commonplace in many developed countries. Presumably, more is considered better, with the excess dependent on how much extra the client can be induced to pay for.

The situation is changing somewhat with the increasing use of sensor-operated lighting and CCTV systems. Nevertheless, security lighting remains in widespread use, signalling the presence of valuable items and ready to assist lawbreakers when there are no police or security personnel close by.

7.2 LIGHTING FOR CCTV CAMERAS

Closed circuit television (CCTV) has been included in this document partly because it is often in competition with street and other public lighting for government crime-prevention funding. For 1996 through 1998, more than three quarters of spending by the UK Home Office on crime prevention was for CCTV systems (Welsh and Farrington 2002, p 44 [116]). Another reason is that it also has its own lighting requirements.

The last decade of the twentieth century saw a rapid rise in the deployment of video surveillance, despite concerns about civil liberty and increasing imaging capability (Honest and Charman 1992 [45]). Existing cameras need artificial light to operate properly at night. Vermeulen (1992) [114] showed that a charge-coupled-device (ccd) video camera with a typical good quality objective lens required a scene illuminance of 26 lux for excellent picture quality.²³ The required values depend on the type of lamp in use.

Video cameras and high-pressure sodium lights were installed at metropolitan railway stations in Melbourne about a decade ago (Carr and Spring 1993 [18]). The luminaires used are fully shielded (ie, confining the directly emitted light to the horizontal direction and below), but generally both the direct glare from the lights and the lit surfaces of the station tend to be unpleasantly bright by comparison with illumination in most of the surrounding area. Adjacent car parks for rail travellers are also overlit with semi cutoff high-pressure sodium lamps, which are much worse as sources of glare.

A photometric survey²⁴ of two of the stations and one of the adjacent car parks indicated that the horizontal illuminance in the carpark and bus shelter ranged from 25 lux to 71 lux. On station ramps and platforms, the range was 88 lux to 452 lux, with a typical value of about 300 lux, over ten times brighter than Vermeulen found to be sufficient for use with CCTV, and also over ten times brighter than is required to reduce fear of crime to near daylight values (Boyce et al. 2000 [14]).

The glare and steep illumination gradients cause visibility losses in the vicinity, particularly for elderly persons and others with increased intraocular light scatter. It would appear difficult to separate overbright lighting and presence of the cameras in any attempts to assess effects on crime and fear of crime.

For video camera installations in general, attempts at cost cutting might increase the need for bright lighting because brighter scenes allow the video camera lenses to be operated with smaller aperture stops (numerically larger f-numbers). This allows the use of cheaper lenses as prime cost items, but imposes greater prime costs for light fittings and greater operating costs for electricity. The desire for sharp images over a greater range of distance could also lead to demands for brighter lighting because smaller aperture stops give increased depth of focus at the cost of dimmer images.

Note that for a given amount of illumination from a high-pressure sodium (HPS) lamp, about 25 % more light is required from a low-pressure sodium (LPS) lamp for the same picture quality (Vermeulen 1992 [114]). HPS is more effective because it is richer in red and near-infrared radiation for which the ccd image sensors are relatively much more sensitive than the eye is.

²³Note that vertical plane illuminances of over 1000 lux are used for live colour TV broadcasting of sporting events.

²⁴The survey was conducted by members of the Outdoor Lighting Improvement Section of the Astronomical Society of Victoria.

7.3 CCTV AND CRIME

7.3.1 Relevance to lighting and crime

The literature on CCTV surveillance is extensive. The technical literature on the optics, electronics and displays is fine for engineers. The literature on the use of CCTV for crime deterrence and detection sometimes gives the impression that even basic concepts of the technology are not well understood. Unfortunately this often appears to impact on the value of reports on the effectiveness of CCTV. This section is a review of papers that were readily available and appeared to have something of possible relevance to the main subject of this document.

Closed circuit television (CCTV) appeared to have an initial beneficial effect in reducing car theft in car parks but the evidence for sustained effects was equivocal (Tilley 1993 [108]). Brown (1995) [15] was more optimistic, although this judgement was qualified by evidence that crime was merely displaced to areas out of camera range. Sherman et al. (1997, Chapters 7 and 10) [102] concluded that CCTV was of limited value. In the Bexley (UK) crime study, it was not possible to demonstrate any statistically reliable deterrent effect of CCTV. The authors were only able to say that CCTV was thought to have some value for providing prosecution evidence (Pascoe and Harrington-Lynn 1998 [88]).

Painter and Tilley (1999) [87] is a collection of articles on issues including CCTV and lighting. It includes a version of the review by Pease (1999) [90].

In an attempt to counter an image of Glasgow as a dangerous high-crime area, 32 CCTV cameras were installed in city centre streets in 1994 (Ditton 1999 [27]). No significant reductions in crime resulted in the following year, nor was there any reliable change in the crime clear-up rate. Most incidents occurred between midnight and 4 am and the least between 6 and 10 am. Subsequent viewing of tapes did assist the police in clearing up some serious crimes. Only 41 % of persons questioned in the city were aware of the cameras 15 months after installation. The presence of the cameras had limited impact on public concern.

Twelve video cameras installed 2 years earlier in the nearby town of Airdrie produced somewhat different results. Overall, recorded crime fell and detections rose in Airdrie after camera installation, but in Glasgow recorded crime rose and detections fell. However, in both locations, some more specific types of recorded crimes fell and some others rose. Both schemes were pronounced successful, albeit in different ways (Ditton and Short 1999 [28]). Lighting, extra or not, is not mentioned, so any possible confounding by lighting differences cannot be ascertained. Flett (1999) [39] reported a more pessimistic view by Ditton, who said that the cameras were not cost effective, producing one arrest every 40 days. Nor had there been any sign of the investment, jobs or visitors the cameras were supposed to bring.

Munro (2000) [70] reported that the outdoor CCTV system installed in Melbourne's central business district was costly in terms of bringing offenders to justice in its initial years. Proponents of the system have since had additional funds allocated for its expansion in the belief that performance will thereby improve.

This is far from a full account of the CCTV and crime literature (eg there is more at SOCRU (2002) [104]). The virtually complete absence of lighting details is noticeable in the papers seen, precluding judgement on any possible confounding by additional lighting. Likewise, reference to the extensive high quality literature on the poor vigilance performance of humans monitoring TV screens for infrequent events seems far short of appropriate.

Video cameras with secure recording were installed in Melbourne taxis during 2002 as a reaction to passenger offences. Video frames are recorded when the brake pedal is depressed. The short distances between cameras and occupants give detailed images of the offences and offenders, better than for typical fixed installation CCTV.

Usually, near-infrared light sources are required inside cars where visible interior light for the video camera would unacceptably handicap driver vision at night. If visible-light-blocking filters are used in the cameras, the system can be relatively insensitive to the wide variation in externally incident light. Monochrome cameras are usually required, but they are less expensive than colour cameras and perform better in dim light.

Extending this technology to street CCTV systems could help to reduce any need for supplementary outdoor lighting. Cameras with greater capability at low light levels are already in use in some applications where their higher cost can be justified. Dim lighting, possibly supplemented by near-infrared sources, is already a practical surveillance option and no extra lighting at all will become so in due course.

7.3.2 Review and meta-analysis of CCTV and crime studies

Welsh and Farrington (2002) [116] is a review and meta-analysis of studies that deal with the effectiveness of CCTV systems in preventing crime (or more realistically, deterring, recording and detecting crime). It is both timely and systematic. Commendable effort appears to have gone into collecting as many relevant studies as possible. Of 46 reports assessed, 22 were considered rigorous enough for inclusion in the meta-analysis but four of these did not provide sufficient data, leaving 18.

Farrington and Welsh (2002a,b) [34, 35] dealt at length with the Scientific Methods Score of the lighting studies reviewed but there is no mention of the Scientific Methods Score at all in their review of CCTV studies, and no explanation for this difference between the two reviews.

The main result of the meta-analysis was that CCTV reliably reduced crime overall by only 4%. All nine of the 18 studies that showed evidence of a beneficial effect of CCTV on crime were from the UK. The other nine studies showed no evidence of a desirable effect on crime. This group included one from the UK and all of the five studies from North America. This almost parallels the lighting meta-analysis, where the overall odds ratio for the UK studies was much larger than that for the USA studies.

Welsh and Farrington stated that few of the studies attempted to control for the regression to the mean problem. From their Home Office quote that funds are made available in the UK for CCTV in town and city centres, car parks, crime hot-spots and residential areas, it seems likely that installation of CCTV is often done to try to control an existing high crime rate. Therefore many of the UK results, at least, could be affected by regression to the mean effects as a source of bias. If the bias amounts to several percent, this could readily account for much or all of the 4% found by Welsh and Farrington for the crime-reduction effect of CCTV. It is not easy to accept that even this small effect is reliable.

The shortcomings of most of the studies often appear to be quite basic, such as having multiple confounding interventions, poor matching or even the absence of controls, unreported data (eg lighting and any supplemental lighting, and even the number of video cameras), and poor or no statistical analysis. The better studies tended to give results that were less beneficial. Poorly designed, badly conducted and inadequately reported experiments are

largely a waste of research funds, which are scarce at the best of times. They tend to discredit science as a reliable and efficient way of solving practical problems. This issue appears to need much more attention and remedial action by funding agencies, academic/research centres and professional bodies in future.

Welsh and Farrington made it clear that future funding of CCTV systems should be based on high quality scientific evidence. Even if their meta-analysis result is accepted, a 4% reduction in crime seems a small payoff for the initial and operating costs of CCTV. Furthermore, there is some evidence to suggest that part of the 4% reduction in crime could merely have been spatial displacement beyond the range of individual cameras.

Perhaps the most rigorous examination so far reported for an individual CCTV study would be that of Farrington, Bennett and Welsh (2002) [36]. The result is the most counterproductive effect on crime of the 17 studies shown in the Welsh and Farrington (2002, Figure 3.1) [116] forest diagram. On this basis, there should be no more money spent on new installations of CCTV for crime prevention. Regardless, there is a good case for running the better existing studies in reverse by removing the cameras as the treatment. Justifying the expenditure to do this should not be difficult.

8 DISCUSSION

As one of a great many possible examples, consider a study of crime in Adelaide, South Australia, in relation to urban design (Bell 1991 [7]). High crime areas were identified as being “a poorly lit area along the River Torrens” and “magnets of human activity” in the inner-city area. The writer recalls being surprised at the time by seeing two of these inner-city places, Rundle Street and Rundle Mall, well lit to very brightly lit despite their high crime reputation. Regardless, the urban design guidelines adopted by Bell for dealing with the crime problem included “safe paths, footpaths, security and lighting”, and “lighting for safety”. Claimed justification for the approach included a review of the relevant literature. But it is quite clear that no scientific justification existed then for lighting as a crime prevention technique; it was merely a theoretical supposition of Situational Crime Prevention.

A more recent example is by Smith (1996) [103]: “The single most important CPTED security feature is lighting.” CPTED is the field of crime prevention through environmental design (eg Michael 2002 [68]), a subset of Situational Crime Prevention. Pease (1998) [89] also noted the importance of lighting to crime prevention practitioners. This reliance on what has been and still is of dubious value fits badly with the generally positive outcomes that appear to result from application of the many other techniques of Situational Crime Prevention (Eck 1997 [31]).

Government authorities in the UK in particular have been increasingly inclined to install more and brighter lighting as a supposed crime prevention measure following the publicity given to the Dudley and Stoke-on-Trent studies (eg ILE 1999 [53]; Pease 1998, 1999 [89, 90]; Painter 1999 [81]). This has presumably been spurred on by the provisions of the UK Crime and Disorder Act 1998. An indication that something is wrong is the 28% rise in street crime in the UK in the year ending April 2002 (Hoge 2002 [43]). If the Farrington and Welsh (2002a,b) [34, 35] meta-analysis result had been allowed to stand unchallenged, the result could well be even more expenditure on outdoor lighting. Insofar as such expenditure has

not gone or would not go into other apparently more successful crime prevention programs, lighting expenditure would therefore be counterproductive.

These issues are taken up in Part 2 of this work.

9 CONCLUSIONS

9.1 EXISTING KNOWLEDGE OF LIGHTING EFFECTS ON CRIME

Common experience, confirmed by experiments, is that artificial light at night tends to allay the fear of crime. Any deterrent effect on *actual* crime is difficult to investigate with field studies, partly because of pervasive extraneous influences. Crime-reducing, nil, uncertain, and crime-increasing effects of light at night have variously been reported for night or day or both, separately or combined or both. Thorough scientific reviews published in 1977 and 1997 in the USA concluded that the effect of lighting on actual crime was unknown. Nevertheless, crime prevention practitioners there and elsewhere, and even some academics, have asserted for decades that lighting is an important weapon, or even the most important weapon, in the fight against crime. UK work published since 1997 has increased academic acceptance that crime prevention effects of lighting do apply in some circumstances, although this UK work has been criticised by others for its procedural and analytical shortcomings.

9.2 DIRECT AND INDIRECT EFFECTS OF LIGHTING

New terminology is defined to improve understanding of existing outdoor lighting and crime studies and to assist the formulation of new studies:

Direct effects of light may aid or hinder criminal acts at night, at the time of actual or intended commission.

Indirect effects of light act through intervening social factors, generally with time delays, and may influence crime by day as well as at night.

9.3 BIAS IN LIGHTING AND CRIME EXPERIMENTS

Conflict of interest is a serious current problem in scientific work. The bias it leads to in results has long been known but its extent has only become apparent in recent years through the excess of results favouring stakeholder sponsors in medical and pharmaceutical trials. Lack of due disinterest of researchers in the findings tends to bias results. Bias from non-financial conflicts of interest is also recognised as a substantial problem. It diminishes the likelihood of reporting and publication of null results or results that are counter to expectations. Bias therefore has to be suspected in lighting and crime studies that were partly or fully supported by lighting-related interests or performed by researchers including one or more having an undue desire for a particular result.

Evidence of bias was found in some of the papers reviewed, manifested as one or more of inappropriate emphasis on why lighting should or even would reduce crime, or failure to search for, mention, discuss or otherwise give due weight to contrary views and facts. In general, conflicts of interest can be expected to bias crime prevention studies to favour beneficial effects of lighting.

9.4 ASSESSMENT OF REVIEW AND META-ANALYSIS

Where separate scientific experiments give differing results for a particular quantity, the mean of pooled results generally provides a more accurate estimate. A formal review and meta-analysis can usually do even better by rejecting poorly conducted experiments and weighting the results of those remaining. A recent review and meta-analysis of lighting and crime studies examined was found to include an indiscriminate mix of direct and indirect effects. The meta-analysis result derived for a typical relighting treatment is impractically beneficial. This follows from the several successive treatments that would be possible in practice, thereby allowing compounded reductions in crime well beyond anything likely on present indications. This error appears to have arisen because of inadequate photometric quantification of lighting treatment, contrary to warnings in the 1977 and 1997 reviews mentioned in Section 9.1 above.

Of the 13 studies included in the meta-analysis, serious procedural or analytical shortcomings or both were found in at least five of them. Photometric aspects are inadequately presented in all of these five studies also. A consequence is that researchers failed to recognise when systematic and other errors led to false or overlarge beneficial effects. Removal of suspect studies from the meta-analysis brings the weighted average result much closer to a null effect. Forcing the upper limit for the 95% confidence interval to have a realistic value leads to a null result if all studies are included. It appears that a counterproductive effect of lighting on crime would emerge from discarding the five problematical studies and applying corrections for the various forms of bias thought likely to have acted in some or all of the remaining studies.

The best that can be concluded at this stage is that there appears to be no compelling evidence for any appreciable net direct beneficial effect of increased outdoor lighting in reducing actual crime at night or for net indirect beneficial effects by night or day. National lighting standards should not state or imply that outdoor lighting has any value for crime prevention or deterrence.

9.5 DEBUNKING THE MYTH OF LIGHTING FOR CRIME PREVENTION

To the extent that outdoor lighting is intended to prevent crime, its capital and operating costs appear to be a waste of public and private funds. It may even be counterproductive. In the case of industrial and commercial infrastructure, the cost burden reduces industrial competitiveness and hinders economic growth.

News media have uncritically perpetuated the myth of increased lighting for crime prevention. Journalists and others concerned need to check the facts more carefully. An extensive pro-lighting campaign started in the 1990s appears to have swayed many UK authorities to install brighter outdoor lighting as a supposed crime reduction measure. The UK Crime and Disorder Act 1998 may have accelerated this process. In the year ending April 2002, street crime in the UK was reported as having increased by 28 %, quite inconsistent with the claimed crime-prevention effect of lighting.

9.6 CCTV AS AN ALTERNATIVE FOR CRIME PREVENTION

Closed-circuit television (CCTV) competes with lighting for available crime-prevention funds. A recent meta-analysis of CCTV intervention studies indicated that the effect was a mere 4% reduction in crime. However, even this meagre result may be unreliable or erroneous because of extraneous confounding factors in real-world experiments. Funding saved by stopping expenditure on more and brighter lighting for crime prevention should not be diverted to CCTV.

9.7 SECURITY LIGHTING AND STREET LIGHTING

The lighting and crime issue needs to be better understood. The issue is addressed in Part 2 of this work. Meanwhile, governments should ensure that resources are not wasted by the installation of any more security lighting at all, and that no street lighting or other public lighting extensions or upgrades are agreed where the justification includes or implies crime prevention. Funding earmarked for crime-prevention lighting should be redirected to rectification of existing overbright and glary outdoor lighting.

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Finally, this report exists only because the very patient support of my dear wife made it possible.

11 CONFLICTS OF INTEREST

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The writer has been a member of the Astronomical Society of Victoria since 1955 and a member of the British Astronomical Association since 1957, is a consultant in optics, visual optics and lighting, and is an inactive shareholder in an industrial photometry company.

12 REFERENCES

Notes following references state explicitly if the writer has seen only part or none of the cited work.

The Internet links may need to be truncated at a slash or dot to get to an accessible page first. Gaps need to be filled by underline characters if the links are keyed in separately.

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