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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[105] SOLS (2002) *Minutes of meeting, 2002-07-10, Subcommittee on outdoor lighting standards, Public Works and Transportation Committee*. Maui, Hawaii: Council of the County of Maui. http://dynamics.org/~altenber/PROJECTS/STARRY_NIGHTS/REFS/MauiPoliceReOLS.html 11

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[111] UCR (1996) *FBI press release for the 1995 FBI Uniform Crime Report*. Washington, DC: Department of Justice, Federal Bureau of Investigation. http://www.fbi.gov/ucr/ucr95prs.htm 3.1

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.

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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.

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[64] Maguire, K. and Pastore, A. L. (eds) (2002) *Sourcebook of criminal justice statistics* (Online). http://www.albany.edu/sourcebook/ 3, 6

[65] Marchant, P. (2001) *Lighting and crime: what is the effect?* Submission to the Leeds (UK) Development and Sustainability Scrutiny Board, October. Personal communication, 2002-05-09. 4.1, 12

[66] Martinez-Papponi, B. L. (2000) *Scientific Method*. http://www.unm.edu/~blmartin/conduct.html 2.1

[67] Marvell, T. B. and Moody, C. E. (1996) Specification problems, police levels, and crime rates. *Criminology*, 34(4), 609-646. 12, 13, 4.5

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 experiments. 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 Kolfschooten 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, "nonfinancial 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

[34] Farrington, D. P. and Welsh, B. C. (2002a) Improved street lighting and crime prevention. *Justice Quarterly*, 19(2), 313-331. 3, 4, 6, 13, 4.6, 5.1, 7.3.2, 8

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[45] Honess, T. and Charman, E. (1992) Closed circuit television in public places: its acceptability and perceived effectiveness. *Crime Prevention Unit Series Paper 35*. London: Home Office, Police Department, Police Research Group. http://www.homeoffice.gov.uk/prgpubs/fcpu35.pdf 7.2

> "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]

[3]For 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]).

[10] BJS (1999) *Characteristics of crime*. Report, Bureau of Justice Statistics. Washington, DC: Department of Justice. http://www.ojp.usdoj.gov/bjs/cvict_c.htm#findings 3.1

[11] BJS (2002b) Table 11. *Per capita total justice expenditure by activity for all governments, fiscal years 1980-99*. Bureau of Justice Statistics. Washington, DC: Department of Justice. http://www.ojp.usdoj.gov/bjs/pub/pdf/charts.pdf

[12] Bodenheimer (2000) Uneasy alliance. Clinical investigators and the pharmaceutical industry. Health Policy Report. *New England Journal of Medicine*, 342(20), 1539-1544. (2000-05-18) 2.2

[13] Boyce, P. R. and Rea, M. S. (1990) Security lighting: effects of illuminance and light source on the capabilities of guards and intruders. *Lighting Research and Technology*, 22(2), 57-79. 17, 7.1

[14] Boyce, P. R., Eklund, N. H., Hamilton, B. J. and Bruno, L. D. (2000) Perceptions of safety at night in different lighting conditions. *Lighting Research and Technology*, 32(2), 79-91. 17, 21, 24

[15] Brown, B. (1995) CCTV in town centres: three case studies. *Crime Detection and Prevention Series*, Paper 68, Police Research Group. London: Home Office. http://www.homeoffice.gov.uk/, or available through ICOLE (2002) 7.3.1

[16] Caminada, J. F. and van Bommel, W. J. M. (1980) Residential area lighting. *Engineering Report 43*, Lighting Design and Engineering Centre. Eindhoven, The Netherlands: NV Philips Gloeilampenfabrieken. 6

[17] Campbell Collaboration (2002) *The Campbell Collaboration Guidelines*. http://www.campbellcollaboration.org/index.html 4.5, 5.5.1

[18] Carr, K. and Spring, G. (1993) Public transport safety: a community right and a communal responsibility. In Clarke, R. V. (ed.), *Crime Prevention Studies*, vol. 1, pp 147-155. Monsey, NY: Criminal Justice Press. 17, 23

[19] Challinger, D. (1991) Prevention or displacement? In McKillop, S. and Vernon, J. (eds), *National overview on crime prevention*, conference held 1991-06-04 to 06. Australian Institute of Criminology Conference Proceedings No.15, pp 29-35. ISBN 0642184526 ; ISSN 1034-5086. Canberra, ACT: Australian Institute of Criminology. http://www.aic.gov.au/publications/proceedings/ 4

[20] Checkland, P. (1981) *Systems thinking, systems practice*. Chichester, UK: Wiley. 2

[21] Clark, B. A. J. (1995) Mismatches between driver visual capabilities and road vehicle standards. *Road and Transport Research*, 5(2), 92-117. 2.2

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 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) luminaires[5] or mercury-vapour luminaires with 70 W high-pressure sodium (HPS) luminaires[6] (Fisher 1997 [37]). Measured illuminances increased by anything from 1.9 to 40 times. The replacements changed

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

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

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.

10 ACKNOWLEDGEMENTS

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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

The Astronomical Society of Victoria funded the acquisition of ILE (1999) [53], Painter (1999) [81], Pease (1999) [90] and a Hagner EC1 luxmeter for use in this and other outdoor lighting improvement projects. Otherwise, the project was self-funded by the writer.

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.

raphy 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

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 impracticably

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. 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 deter-

[9] Unfortunately, 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.

[10] For 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.

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

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:

> "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

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

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 survey[24] 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.

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.

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

> 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

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 deficients 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 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

[22] All 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]).

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.

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

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

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 to1990 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

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

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 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

[20] A 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.

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 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.

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.

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 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

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

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 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.

TABLE 2. Recorded thefts from shopping bags, March to August each year

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

planation 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.

[16] Presumably this refers to CCTV.

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:

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

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.l 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

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