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 intensity[17] 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 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

[17] The 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.

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]).

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

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 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).

> "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.

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

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 ex-

> "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 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.

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, p 278) 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.

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

[19] The 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.

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

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

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 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

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 Aries, police were reallocated to guard ethnic properties around the clock. The police had to stay close to the assigned properties. Di Tella and Schargrodsky (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]).

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

[21] Part 2 goes even further than this.

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."

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 (Honess 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

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

> 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 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

[12] The 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.

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.

mined 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	146671	522.9	767	947	49.1	29.55	p<0.0001
Hunterdon	125135	576.2	721	597			
Warren	105765	731.8	774	740	–6.5	1.415	ns
Morris	472859	773	3655	3737			
Monmouth	622977	1020.3	6356	6288	–1.8	0.495	ns
Ocean	527207	1055.2	5563	5605			
Monmouth	622977	1020.3	6356	6288	–0.4	0.023	ns
Burlington	432121	1124.2	4858	4826			
Ocean	527207	1055.2	5563	5605	1.4	0.903	ns
Burlington	432121	1124.2	4858	4826			
Burlington	432121	1124.2	4858	4826	–4.2	3.000	p<0.1
Middlesex	757191	1127	9291	9638			
Passaic	491077	1727	8481	7585	–13.6	46.12	p<0.0001
Union	523396	1750.3	9161	9484			
Cape May	102352	1948.2	1994	2041	12.0	10.78	p<0.005
Camden	509350	2062.4	10505	9601			
Cape May	102352	1948.2	1994	2041	1.9	0.226	ns
Cumberland	146289	2153.3	3150	3163			
Camden	509350	2062.4	10505	9601	–9.0	10.63	p<0.005
Cumberland	146289	2153.3	3150	3163			

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

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,

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 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

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

[8] Care 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.

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

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.

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 bibliog-

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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[4] Baldrey, P. (2000) *Lighting and crime in the UK*. IDA Newsletter 44, December. Tucson, AZ: International Dark-Sky Association. `http://www.darksky.org/` 22

[5] Barker, M. and Bridgeman, C. (1994) Preventing vandalism. What works? *Crime Detection and Prevention Series Paper 56*. London: Home Office, Police Research Group. `http://www.homeoffice.gov.uk/rds/prgpdfs/fcdps56.pdf` 6

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[8] Bennett, R. (2000?) White light reduces fear. *The Philips Lamps and Gear Magazine*, 3(2), 14-17. `http://www.urbislighting.com/img/WhiteLight.pdf` 21

[9] Benson, J. (2000) *Haynes Foundation researcher completes study on LA's most crime-ridden bus stops*. Press release, 2000-03-14. Los Angeles, CA: Haynes Foundation. `http://www.haynesfoundation.org/news/pr_031400.htm` 4

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, 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 displacement[4] 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

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

[22] Clarke, M. and Oxman, A. D. (eds). (2002) Cochrane Reviewers Handbook 4.1.5 (April). In *The Cochrane Library*, Issue 2, 2002. Oxford: Update Software. http://www.cochrane.org/cochrane/hbook.htm (Updated quarterly) 5.5.1

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[25] DCPC (2001) *Reporting crime in the Melbourne Central Business District.* Third report of the Drugs and Crime Prevention Committee, Parliament of Victoria. Melbourne: State of Victoria. http://www.parliament.vic.gov.au/dcpc/Reports%20in%20PDF/3rd_Crime_Report.pdf 8

[26] Di Tella, R. and Schargrodsky, E. (2001) *Using a terrorist attack to estimate the effect of police on crime*. Draft, 2001-04-30. http://www.people.hbs.edu/rditella/papers/using_terrorist_%20attack.pdf 13

[27] Ditton, J. (1999) The effect of closed circuit television cameras on recorded crime rates and public concern about crime in Glasgow. *The Scottish Office Central Research Unit Main Findings*, No. 30, 4 pp. http://www.scotcrim.u-net.com/researchc2.htm 7.3.1

[28] Ditton, J. and Short, E. (1999) Yes, it works – no, it doesn't: comparing the effects of open-street CCTV in two adjacent town centres. *Crime Prevention Studies*, 10, 201-223, or at The Scottish Centre for Criminology, http://www.scotcrim.u-net.com/researchc.htm . 7.3.1

[29] DLPS (2000) *New Jersey Uniform Crime Reporting (January – June 1999/2000)*. Department of Law and Public Safety. New Jersey: State of New Jersey. http://www.state.nj.us/lps/njsp/info/pdf/crime_indx_00.pdf 3, 3.4

[30] Drazen, J. M. and Curfman, G. D. (2002) Financial associations of authors. (Editorial) *New England Journal of Medicine*, 346, 1901-1902. Comment at http://www.medscape.com/viewarticle/436548_print . 2.2

[31] Eck, J. E. (1997) Preventing crime at places. Chapter 7 in Sherman, L. W., Gottfredson, D., MacKenzie, D., Eck, J., Reuter, P. and Bushway, S. (1997) http://www.ncjrs.org/works/index.htm 3.1, 6, 4.2, 4.6, 5.3, 6, 8, 80

[32] Eck, J. E. (2002) Preventing crime at places. In Sherman, L. W., Farrington, D. P., Welsh, B. C. and MacKenzie, D. L. (2002) *Evidence-Based Crime Prevention*, p 273. London: Routledge. 3.1

[33] Farrington, D. P. and Petrosino, A. (2001) The Campbell Collaboration Crime and Justice Group. *The Annals of the American Academy of Political and Social Science*, 578, 35-49. (November) 5.5.1

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."

[46] Horner, R. E. (2002) Lights, center, action. How improving a center's lighting program can increase its profits. *High-Benefit Lighting*. Silver Spring, Maryland: The National Lighting Bureau. `http://www.nlb.org/publications/art_lightscenteraction.html` 5.3

[47] HSHF (2002) *The Blackout*. UK: Home Sweet Home Front. `http://www.homesweethomefront.co.uk/web_pages/hshf_blackout_pg.htm` 2

[48] ICOLE (2002) *Crime quick reference guide. Information on lighting and crime in the United States*. Indiana: The Indiana Council on Outdoor Lighting Education (ICOLE). `http://home.att.net/~icole/crime_ref_guide.html` 108, 109

[49] IDA (2002) *Website*, International Dark-Sky Association (IDA). Tucson, AZ: International Dark-Sky Association. `http://www.darksky.org/` 13

[50] IDA IS51 (1992) Lighting and crime. *Information Sheet 51*, April. Tucson, AZ: International Dark-Sky Association. `http://www.darksky.org/` 4

[51] IDA IS63 (1998) U.S. Department of Justice study of street lighting and crime. *Information Sheet 63*, January. Tucson, AZ: International Dark-Sky Association. `http://www.darksky.org/` 3.1, 4, 107

[52] ILE (1989) *Annual Conference of the Institute of Lighting Engineers*. Rugby, UK: Institution of Lighting Engineers. (Cited by Shaftoe and Osborn (1996), details and comments in `http://crime-prevention.org/ICPAN/papers/sni/coun_e.htm` .) 3.2, 6

[53] ILE (1999) A practical guide to the development of a public lighting policy for local authorities. *Technical Report 24*. Rugby, UK: Institution of Lighting Engineers. 8, 11

[54] Jochelson, R. (1997) *Crime and place: an analysis of assaults and robberies in central Sydney*. New South Wales Bureau of Crime Statistics and Research. Sydney, NSW: Attorney General's Department. `http://www.lawlink.nsw.gov.au/bocsar1.nsf/files/r43.pdf/$file/r43.pdf` (NB The Internet version does not include the maps, which need to be obtained separately in B&W hard copy form. The original document is required to see the colour for time and day differentiation.) 3, 8

[55] Lab, S. P. (1997) *Crime prevention. Approaches, practice and evaluations*. 3rd ed. Cincinnati, OH: Anderson Publishing Co. (Extract from its Chapter 3 'The physical environment and crime' seen at `http://www.securitymanagement.com/library/000484.html` .) 4, 19, 19, 6, 78

[56] Laurin, T. C. (2002) Science journals require credibility. (Editorial) *Photonics Spectra*, (May), p 13. 2.2

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 conditions[2] 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

[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.

[2] Amateur 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.

[68] Michael, S. (2002) *The CPTED Page – Intro. Resources. Annotated bibliography*. http://www.thecptedpage.wsu.edu/Resources.html 8

[69] Moher, D., Schultz, K. F. and Altman, D. G. (2001) The CONSORT statement: revised recommendations for improving the quality of reports of parallel-group randomised trials. *The Lancet*, 357, 1191-1194. (2001-04-14) http://www.consort-statement.org/lancet.pdf 5.5.1

[70] Munro, I. (2000) City's $1m cameras catch 10 offenders. *The Age*, 2000-08-09, p 7. Melbourne: Fairfax. 7.3.1

[71] Nair, G., Ditton, J. and Phillips, S. (1993) Environmental improvements and the fear of crime: the sad case of the 'Pond' area in Glasgow. *British Journal of Criminology*, 33(4), 555-561. (Abstract at http://www.scotcrim.u-net.com/researchf10.htm.) 6, 4, 78

[72] Nair, G., McNair, D. and Ditton, J. (1997) Street lighting: unexpected benefits to young pedestrians from improvement. *Lighting Research & Technology*, 29(3), 143-148. 17, 6

[73] NBI (2002) *Integrated Energy Systems- Productivity and Building Science Program*. A program of the California Energy Commission Public Interest Energy Research (PIER) Program. California: New Buildings Institute, Inc. http://www.newbuildings.org/pier/ 22

[74] NCJRS (1999) Violence after school. *Juvenile Justice Bulletin, 1999 report series*. NCJ 178992, National Criminal Justice Reference Service. Washington, DC: Department of Justice. http://www.ncjrs.org/ 3

[75] Nottingham (2001) Online bibliographies *VDCLP10 (Lighting and light pollution)* and *UR19 (Crime, social safety and planning)*. Nottingham, UK: The University of Nottingham. http://www.nottingham.ac.uk/library/ 6

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[78] Painter, K. (1993) (No details available. Cited by Nair, Ditton and Phillips1993 [71] and Lab (1997) [55].) 4

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[94] Poyner, B. and Webb, B. (1997) Reducing theft from shopping bags in city center markets. In Clarke, R. V. (ed.), *Situational Crime Prevention: Successful case studies*, 2nd ed., pp 83-89. ISBN 0911577-38-4 (or -6) Monsey, NY: Criminal Justice Press or Albany, NY: Harrow and Heston or Guilderland, NY: Clarke, Harrow and Heston. 17, 5.3

[95] Quinet, K. D. and Nunn, S. (1998) Illuminating crime: the impact of street lighting on calls for police service. *Evaluation Review*, 22(6), 751-779. 3, 4.6, 17, 19, 93

[96] Ramsay, M. and Newton, R. (1991) The effect of better street lighting on crime and fear: a review. *Crime Prevention Unit Paper 29*. London: Home Office. `http://www.homeoffice.gov.uk/` 3, 6, 16, 6

[97] Reppetto, T. A. (1976) Crime prevention and the displacement phenomenon. *Crime and Delinquency*, 22, 166-177. (Not seen; cited by Farrington and Welsh (2002a,b).) 4

[98] Schumacher, B. J. and Leitner, M. (1999) *Spatial crime displacement resulting from large-scale urban renewal programs in the city of Baltimore, MD: a GIS modeling approach*. Presented at The IV International Conference on GeoComputation, Mary Washington College, Fredericksburg, VA, USA, 1999-07-25 to 28. `http://www.geovista.psu.edu/sites/geocomp99/Gc99/047/gc_047.htm` 3, 3.5

[99] Semmens, N. (1999) Working bibliography on fear of crime @ 29.07.99 (513 entries). Criminology Institute, Faculty of Law, Sheffield University, UK. `http://usitweb.shef.ac.uk/~lwp98ncs/fearof.htm` 6

[100] Shaftoe, H. and Osborn, S. (1996) Crime prevention and security in Great Britain. Part 2: Examples and conclusions. Ch. 7: Bristol City Council. Lighting improvements to a multi-racial inner city area. *Proceedings, Towards World Change conference for international crime prevention practitioners*, Vancouver, British Columbia, 1996-03-31 to 1996-04-04. `http://crime-prevention.org/ICPAN/documents/index.html` 3.2, 6

[101] Sherman, L. W. and Weisburd, D. (1995) General deterrent effects of police patrol in crime 'hot spots': a randomized, controlled trial. *Justice Quarterly*, 12, 625-649. (Not seen; quoted by Taylor (1998) [106].) 4

[102] Sherman, L. W., Gottfredson, D., MacKenzie, D., Eck, J., Reuter, P. and Bushway, S. (1997) *Preventing crime: What works, what doesn't, what's promising. A report to*

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

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