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RESEARCH NOTE

A Demonstration That the Claim That Brighter Lighting Reduces Crime Is Unfounded

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The major systematic review on street lighting and crime, Home Office Research Study 251, suggests that claims for the effectiveness of lighting against crime are justified. The review at first sight appears to be an appropriate statistical synthesis of all studies on street lighting and crime across the world. However on close examination, the statistical claims and methods are unfounded. In three cases examined there is a clear conflict between the evidence and the reviewers' interpretation of this. One of the principal problems is easily seen. The time-series of the original data from the Bristol study shows no good evidence for the crime reduction benefit of lighting. However the review gives the result for the same data as being extremely statistically significant. It is suggested that such a difference between the newly lit and the control areas occurring purely by chance is less than one in a billion, but this is manifestly wrong. Two other component studies, Birmingham and Dudley, are examined.

Introduction

A major flaw with the review Home Office Research Study 251 (HORS 251) Farrington and Welsh (2002) is to use methods that ignore the large variation (known as 'overdispersion') in the data and implicitly assume that crimes are independent events, which is implausible in the extreme.

The review compared the ratio of number of crimes before and after in an area that had brighter lighting introduced with the ratio of a similar ratio in a 'control' area which had no change in lighting. The ratio of ratios is called by the authors an 'odds ratio' (OR). If the OR is convincingly greater than one then it might be concluded that crime has been reduced in the newly lit area compared with the control. The results of the review are shown in essence in Figure 1 (Figure 3.1 in HORS 251). The rectangular point for each study (labelled at the side) shows the OR, generated from its data, and is surrounded by a 95 per cent confidence interval bar, within which the underlying OR might be expected to reside. (The confidence interval has been calculated using a standard formula for statistically independent occurrences but this is incorrect as is shown later.) We see that there is a tendency for the 13 studies to be displaced to the right of the vertical dashed line. This line represents an OR=1, suggesting that the study ORs are generally greater than 1, indicating that there has been a fall in crime in the newly lit area compared with the control. This conclusion seems to be formally strengthened by the point labelled Mean as this represents the weighted average of all 13 studies and shows an OR greater than 1 with a narrow confidence interval around it.

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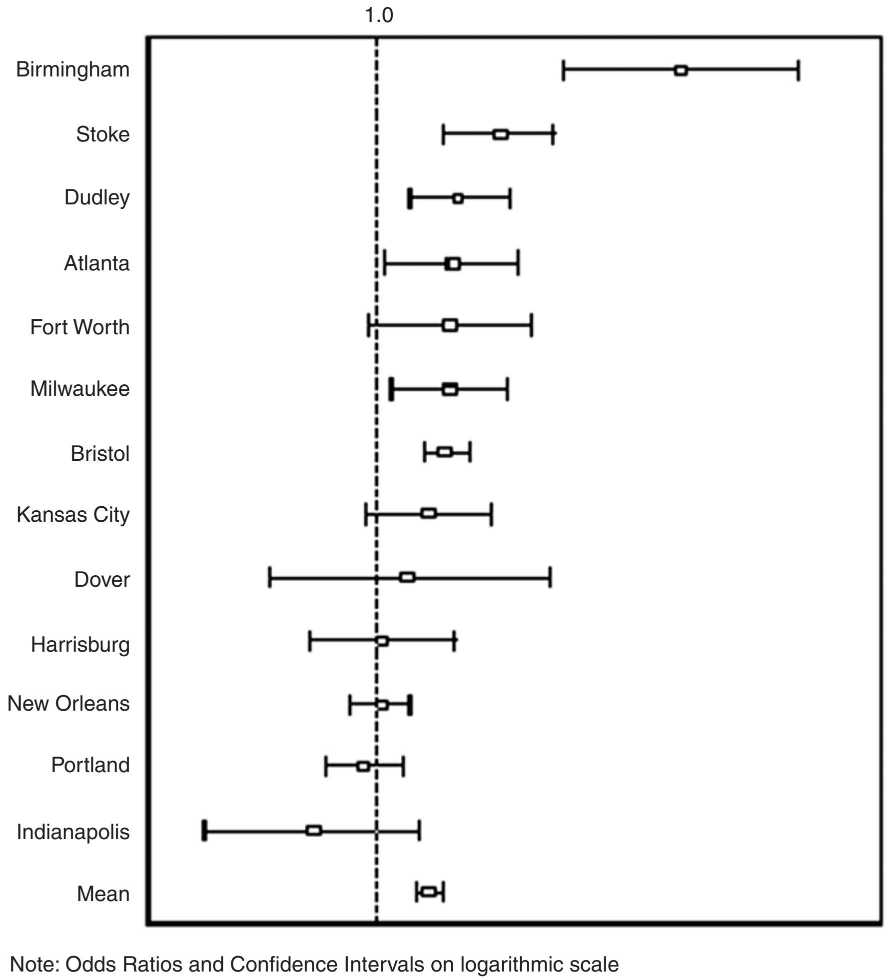

FIG. 1 Street lighting evaluations

However it will be shown that the true confidence interval should be very much wider and so it is not possible to say whether lighting reduces or indeed increases crime.

The Bristol Study

As but one example of the problem of overdispersion, one can examine the contribution from the Bristol study that used data on crime from the beginning of 1986 to mid 1990. The reviewers' interpretation of the result of this study is given by the point, with its narrow confidence interval halfway down the 'forest plot' shown (Figure 1, HORS 251 Figure 3.1). It is, according to Farrington and Welsh, key evidence showing the benefit of lighting as the confidence interval associated with this point is clearly on the benefit side of the no-effect dashed vertical line. But in fact their claim is incorrect. The treatment area in the Bristol study had brighter lighting introduced between July 1987 and

March 1989 and the control area had its lighting left unchanged. The reviewers in HORS 251compared the ratio of crimes committed in the first year and the final year in both areas. It is claimed by the reviewers that the benefit of lighting shown by this contributing study is clear. (The z-statistic of 6.6, calculated using the formula relying on statistical independence is consistent with its confidence interval in the forest plot, being very well displaced from the null line. The value of 6.6 corresponds to a chance occurrence of such an extreme value of about one in a billion. It would be extremely strong evidence that the new street lighting had reduced crime).

However, the original paper from which the data was taken (Shaftoe 1994) makes no such claim for the crime reduction benefit of lighting. Indeed it is easy to check the situation with the data from Shaftoe, plotted below, in Figure 2.

Just inspection of the plot of the two time series of number of crimes reported in the comparison areas and noting when the new lighting went in, shows nothing convincing to support a claim of the benefit of lighting. (Remember that the reviewers' claim is that this data shows a very highly statistically significant result for the benefit of lighting with probability of occurring by chance of less than 1 in a billion. Their claim is literally incredible).

The difficulty for HORS 251 is that a requirement of the statistical method used by its authors is that a typical range for a fluctuation is equivalent to approximately the square root of the mean. In this case this is say around 20 something for the control area and 30 something for the treatment area. As the fluctuations are seen in Figure 2

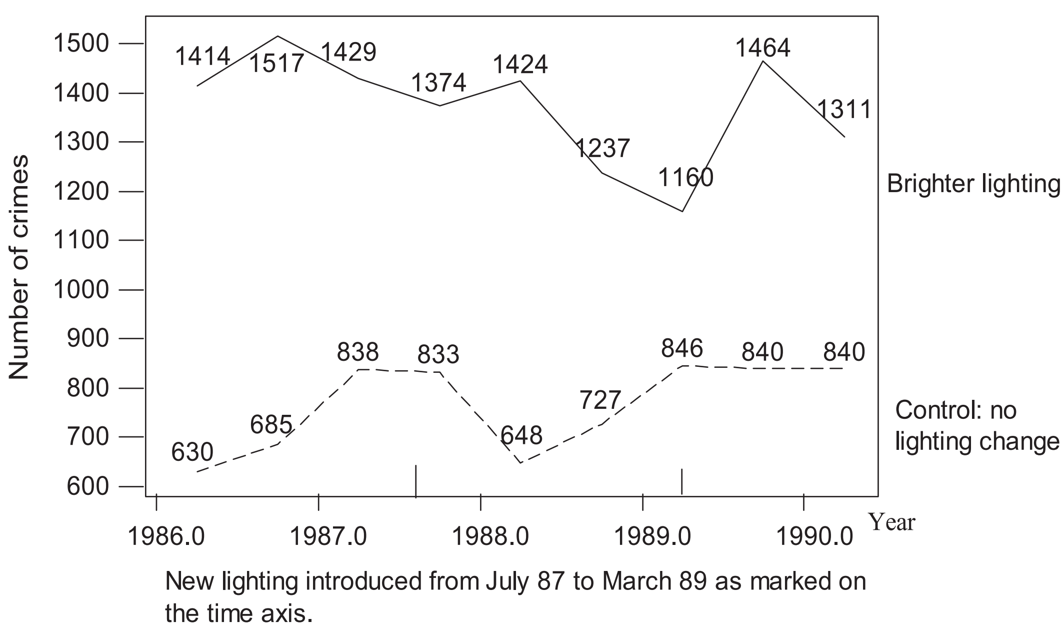

FIG. 2 Bristol: Number of crimes reported in half-year periods

to be instead on average around 100 or so, this shows that the wrong method has been used. Thus the results are invalid, because the reviewers' method is incorrect as it underestimates the true variability. This is consistent with the conclusions of Shaftoe, the original investigator in the Bristol study who could find no evidence that the new lighting reduced crime, in stark contradiction of the reviewers' claim.

Other Studies

It is not just the Bristol study, as the fundamental problem is with the method used in the review. The Birmingham Market Study, Poyner and Webb (1997), is another of those included in the review. The effect claimed in HORS 251 is shown at the top of the forest plot. However the data from the original Poyner and Webb (1997) paper clearly shows that there is excess variation, via the differences in the values recorded at the two time points, in each of the four settings, (treatment and control and before and after the intervention of brighter lighting). See Figure 3.

There is a much larger drop than the square root of the mean in the treatment area and also an excessively large rise in the control area, both occurring *before* the new lighting went in. It is therefore impossible to claim that the change in crime was anything to do with the new lighting installed over a short period in late 1983.

The same problem, of more variability than allowed for, occurs too in the Dudley study, Painter and Farrington (1997), also included in the review. This study was based around a household crime survey. It is clear in that paper that the spread of the number of crimes experienced by the households was much greater than the methods

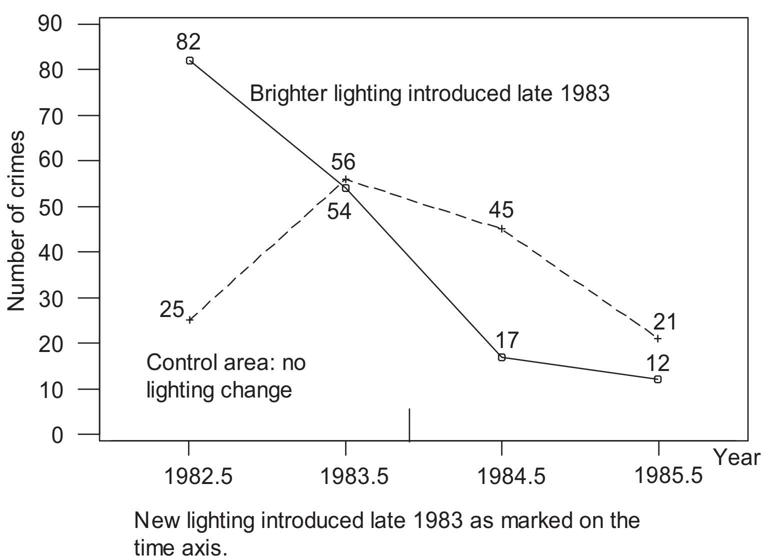

FIG. 3 Birmingham markets: Number of crimes reported

used in the review (and the original paper) warrant. The original paper also contains shortcomings such as inappropriate use of one-tailed testing, so that if analysed appropriately the study did not in fact detect the conventionally statistically significant effect that it was designed to find.

Overdispersion: Its Impact and Cause

Overdispersion is also indicated by the fact that the effect of lighting is more variable between studies than the variability (confidence intervals) that the authors give for the individual studies would suggest. This is consistent with the very large heterogeneity statistic, $Q = 56.9$, for the 13 studies.

One might ask why the variability in the individual studies is so much larger than that required by the method used by the reviewers. An answer is that crime events are not 'statistically independent' as the method used by the reviewers assumes, but are instead correlated. Crime is perpetrated by people. One criminal may be responsible for many crimes and so this one person changing behaviour can cause a large change in the number of crimes committed and recorded.

Different statistical methods are needed to deal with such variability. Where it has been possible to re-analyse the data the appropriate methods have not provided satisfactory evidence for brighter lighting reducing crime. The other studies included in the review will suffer from the same problem of extra-variability that the reviewers fail to account for. However we cannot estimate the effect for each study as the data, such as repeated measurements, do not exist to allow us to do so. However we can be sure that the confidence intervals of individual studies and the combined result will be much wider than that given in HORS 251.

Other Problems with HORS 251

There are also other problems with the review. One is of not comparing like with like, for the individual studies, in general. This is because brighter street lighting is applied to more crime-ridden areas and the comparison areas are less crime-ridden and this will lead to an effect known as 'regression to the mean'. (See Bland and Altman (1994) for a discussion of the regression to the mean effect. Many examples of errors, resulting from the lack of recognition of the effect, are given by Andersen (1990).) The regression to the mean effect is exemplified by the situation of performing a 'controlled' trial of treatment for the common cold on a group of people suffering badly whereas the people in the control group are not very poorly at all. After following up the patients some time later and finding them all virtually cold-free, a great success is claimed for the new treatment, as if this had been responsible for bringing down the cold symptoms in the badly ill group more than the standard treatment did in the not-so-ill group. In fact the groups have both returned to a more average state, naturally. Clearly this example shows the difficulty of not having the 'controls' in the same average state as the 'intervention' group at the start.

Indeed the Bristol data, over nine periods, show the regression to the mean effect. For a crime count in the series which is below average (765 in the control area, 1,370 in re-lit area) the next value tends to be greater i.e. above the line of equality corresponding to the same value after as before (see Figures 4 and 5).The reverse tends to be the case

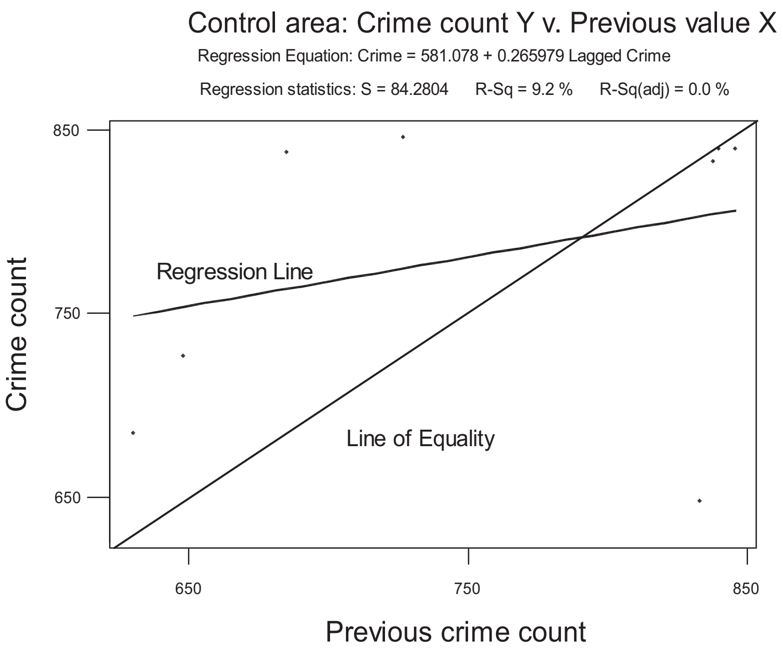

FIG. 4 Control area

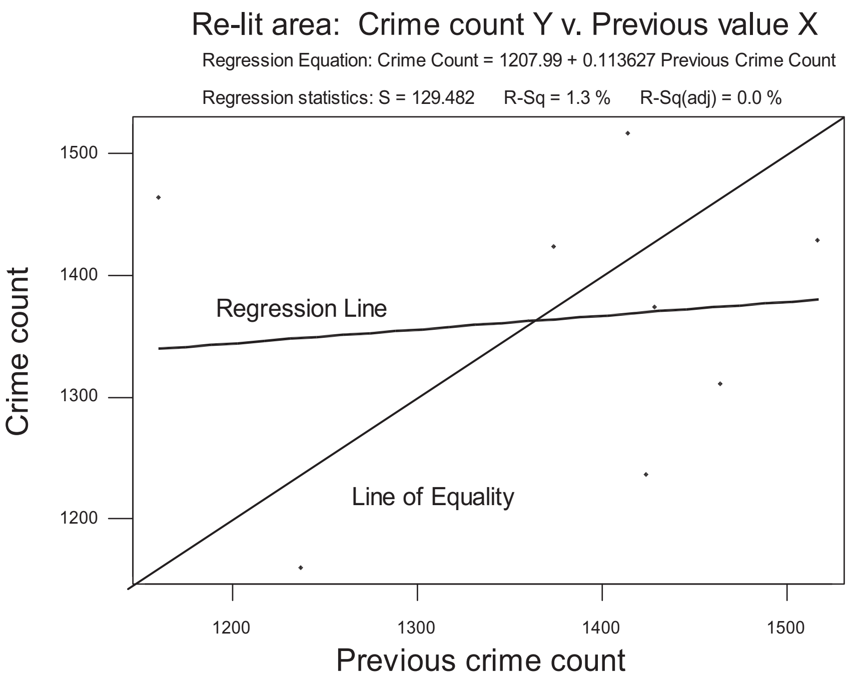

FIG. 5 Re-lit area

where a crime count is above average. This is just as expected in the regression to the mean effect. It is particularly strong when, as here, the correlation of crime counts to their immediate previous values is small. This fact is easily seen in a scatter-plot of crime count (Y) against its previous value (X) as the 'best-fit' (i.e. regression) line, for the mean crime count given the count of crime previously, is flat. This indicates that the correlation is indeed weak and regression to the mean is a serious problem.

There are further problems in HORS 251. For example, studies of small size are excluded for no good reason. There are a number of other shortcomings with the review, such as not giving the source of funding of the component studies as this information would be useful to see if bias towards the interests of funders might be a problem. However the Bristol study by itself is sufficient to indicate that the reviewers' results are untenable and that the claim that brighter lighting reduces crime is unfounded. Crime reduction is frequently presented as a potent argument for increased lighting—here it has been shown that there is no scientific basis for this claim.

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