Bad Science: Comments on the paper 'Quantifying the impact of road lighting on road safety — A New Zealand Study' by Jackett & Frith (2013).

Dr Paul MARCHANT, CStat

Key Words: Road Traffic Collisions, Traffic Accidents, Road Lighting, Poor Study Design, Poor Statistical Practice, Statistical Errors, 'Cosmetic' Analyses, Poor Publishing Practices, Openness, Transparency.

Abstract

The paper of Jackett & Frith (2013), which purports to show considerable gains for road safety with increasing road luminance, is seriously flawed. It asserts that increasing the luminance on roads *causes* improvements in road safety. Its cross-sectional design fails to rule out major potential confounders. Using a longitudinal design would be a far superior approach. The paper exhibits poor statistical practice. The selection process for the relatively small sample of urban roads is unclear and the post hoc processing of the data is questionable. The analysis is seriously deficient, as variables which indicate detrimental effects of increased road lighting are removed from the modelling without proper justification and other variables are not included in the first analysis yet appear in the subsequent cosmetic analyses. The latter give an illusion of false certainty. The data collected, which would allow checking, is not published. The practice of the journal in which the paper appeared is seriously deficient in not allowing the publication of critical responses. Although being used to promote increased road lighting, the paper's claim disagrees with results from better quality research.

1. Introduction

The paper examined here, Jackett and Frith (2013) 'Quantifying the impact of road lighting on road safety — A New Zealand Study' (JF), presents a study that took the number of night and day crashes, on

a sample of roads, and modelled the crash ratio on the measured road lighting characteristics. It claims from the analysis that brighter road lighting causes greater road safety. However, the study and paper are seriously flawed and because of this, the claim it makes is unfounded. A major problem with the JF paper is the implicit assumption that the *correlation* between (increased) road luminance and (reduced) night to day road crash ratio is indicating a *causal* relationship. An example of such a spurious inference would be to blame the sale of ice creams as causing drowning deaths, because when more ice creams are sold, more drowning deaths tend to occur. The confounder in this case is warm weather, suitable both for eating ice cream and for entering water. A cause is what makes something happen when the 'dose' of the cause is changed. One can think of the output listening to the cause and obeying, therefore making the response happen. The JF paper contains much other poor statistical practise including seriously deficient analysis. Yet despite these extremely serious flaws the paper is widely cited.

The issue of poor science is of great current concern and was the subject of the research integrity inquiry by the UK Parliament (Commons) Science and Technology Committee, (2018). The author of this current work published a paper on some problems with lighting research, Marchant, (2017).

This paper extends an article in the joint publication of the Royal Statistical Society and the American Statistical Association, 'Significance', Marchant, (2019).

2. Overall Points

Finding sure-fire ways of reducing the heavy toll in death and injury caused by road traffic collisions (RTCs) would be a great benefit, see World Health Organization (2018). Increasing road lighting is widely believed to be a way to markedly reduce RTCs at night. However, what sound evidence is there for this belief? The JF paper, examined here, makes a strong claim for large road safety benefit as road luminance is increased. However, there are very serious problems with this work, so its claim is unfounded.

The JF study has very poor design. This is because the study is what JF class as 'relational', which means correlational through its 'cross-sectional' nature. That is, the study takes only one set of measurements (lighting characteristics and night and day crash numbers) and compares the night to day crash ratios between roads. (The crash ratio of course will vary if either the numerator or the denominator or both vary). The flaw in the JF study design is that any other quantities that are correlated with the quantity of interest (here, road luminance) could be the actual causes of the variation of night to day crash ratios, rather than the lighting itself.

One such 'confounder', a quantity correlated with road luminance that directly influences the road safety measure, could simply be the ratios of the numbers exposed to crash-risk night and day. Other examples might be the characteristics of the different kinds of traffic (associated with both vehicles and their drivers) using the roads night and day. For example, the types of driver and their behaviours may vary between busy (tending to be more brightly lit) and quiet (tending to be more dimly lit) roads. For example, anyone "under the influence" or wishing to drive over the speed limit might be more likely to choose a seemingly quiet road to avoid attention, but they may not find it quite as empty as assumed and thus become involved in an RTC.

Studies involving more than one set of measurements across time (longitudinal studies) with lighting being changed within the time period are far superior. A longitudinal study allows the effect of changed lighting to be seen by comparing a road with itself (before and after the change), as the road's other characteristics are likely to remain closely constant. Therefore, the attribution of cause is much more secure. As stated above, as but one example, the result of the JF study will be affected by the ratio of the numbers exposed to crash-risk, night to day and for a valid result, this ratio must have no dependence on the type of road lighting in operation. The result of a longitudinal study is much less likely to be affected by the ratio of numbers exposed to risk as each road is being compared with itself and this ratio for a given road is unlikely change very much when relit. See Appendix for the advantage of longitudinal studies in this regard.

The Cochrane Collaboration systematic review on street lighting for preventing road traffic injuries, by Beyer and Ker (2010), although having other problems, as discussed in its Feedback section, did have inclusion criteria to assist in achieving some 'control'. The review states: '*randomised controlled trials, quasi-randomised controlled trials and controlled before-after studies (CBAs) were eligible for inclusion in this systematic review.*' Therefore, the poor design quality of the JF study would mean it would not have been eligible for inclusion. JF briefly mention '*the traditional Before and After study'* and state that using their design allowed a much larger sample size. However, a large sample size may simply give a false sense of certainty while yielding a wrong conclusion due to confounding.

The much larger scale LANTERNS project (Perkins et al, 2015), commissioned by the UK National Institute for Health Research (NIHR), is a longitudinal study that investigated the effect of lighting change in England and Wales. Despite the title of the LANTERNS project being 'What is the effect of reduced street lighting on crime and road traffic injuries at night? A mixed-methods study', the study made a variation from protocol. This variation was to also include 'change to white light', such as LEDs, from such as low-pressure sodium and is therefore about increasing road illumination on some road segments as well as reducing it on others; (the reductions were: dimming, part night lighting, and switch off). The study did not detect any statistically significant effect of any of the four types of change to road lighting on personal injury accidents. That is, all the 95% confidence intervals around the aggregate point estimates of the effect of all four types of lighting change, (change to white light on 15833 km of roads, as well as the three types of reductions on others) include zero and so found no sound scientific evidence of any change. As in the JF study, LANTERNS also used the night to day accident ratio as the outcome measure. (Incidentally the LANTERNS study also found null results when examining crime, that is all the 95% confidence intervals around the aggregate point estimates of the effect of all four types of lighting change included zero).

The JF study just looks at roads in urban areas with more than 10 crashes, in the period 2006–2010, yet the population of interest is surely all urban roads and the results are likely to be taken as also applying to roads which are less crash prone. It is unclear how the roads in the research were in fact selected. Nine of New Zealand's 67 territorial local authorities were used. Four criteria were applied:

• had at least 10 injury+non injury crashes, 2006–2010

• had no significant road lighting changes in the period 2006–2010

• had a similar level of lighting along their length

• had places to stop safely and measure the lighting.

It is not made clear if the 152 road sections selected are all the roads in the 9 authorities that met these criteria or if some other criteria were also in use.

Additionally, according to JF's footnote 2 '*Some sites were subsequently shortened, subdivided or deleted to improve homogeneity*'. It is not properly made clear what motivated this action, nor is the extent of this post-hoc procedure given. No sensitivity analysis is presented to indicate what effect this had on the results. The action to '*improve homogeneity'* will have the effect of reducing the estimate of statistical uncertainty and therefore will tend to increase the chances of finding statistical significance. Chasing statistical significance is a malign, unscientific practice mentioned in the inquiry into research integrity by the UK Parliament (Commons) Science and Technology Committee (2018).

Note, the LANTERNS project did not arbitrarily exclude road segments with a small number of crashes.

Importantly, the full data, with which to check the results, is not provided by JF. Even descriptive statistics of the variables used are not given. No Declaration of Interest statement is made. No reference is made to how the study was commissioned or how it was paid for. No plan, known as a protocol, stating how the study would proceed, made in advance of executing the study, and against which what actually transpired can be assessed, is available. In contrast, the

LANTERNS protocol is available on the project website, given below, in References, Perkins et al, (2015).

3. Detailed Points

The size of the JF study is relatively small with 7944 crashes on 270 km of road. In contrast, the LANTERNS project, had 859935 collisions in the 62 local authorities available in the data set and at the end of the study period, in 2013, around 40000 km of road had lighting changes to be assessed. The LANTERNS study provides confidence intervals of the estimate of effect whereas JF regrettably use a crude star-system to indicate p-value ranges to indicate statistical significance.

3.1 JF's First Analyses

The modelling in JF section 3.2 using Generalised Linear Models leading to their Table 1 might have some validity, but without access to the data one cannot be sure. Although a '*Poisson multiplicative model*' is stated, it would seem that a binomial model would be the proper model for the data as it is the ratio, of night to day crashes, that is of interest. The values of the coefficients in Table 1 obtained from the modelling are such that the night to day crash ratio decreases as the coefficient value becomes more negative and vice versa. Model 1 with more terms, and therefore better fitting, has two other statistically significant coefficient estimates, in addition to Average Luminance. (Note the Average Luminance coefficient value given for Model 1, -.038, appears to be a possible typographical error: - a misplaced decimal point).

Table 1 From JF page 141

Table 1
Summary results of three models using the Poisson Multiplicative Model to predict the number of night time crashes.

		Independent variables				
Model No.	Constant term (a)	L_{avg}, Average Luminance	TI, Threshold Increment	Uo, Overall Uniformity	Ul, Longitudinal Uniformity	Colour (White=1)
1	−0.84	−.038**	1.08*	0.07	−0.08	0.35*
2	−0.81	−0.38**	0.95*			
3	−0.62	−0.44**				

Notes: The number of * indicates the significance of the parameter. * = two standard errors (significant at p<=0.05), ** = three standard errors (highly significant)

The two other statistically significant coefficient estimates, in Model 1 are Threshold Increment and Colour (with White =1). These two coefficients have magnitudes that are similar to, or larger than that of the Average Luminance coefficient, assuming its value should be -0.38. However, both Threshold Increment and White light values are positive so in the direction of a larger, that is detrimental, night to day crash ratios; in other words, a greater number of crashes occurring at night relative to day.

One of these statistically significant coefficients is the effect of white road lighting and it suggests that the effect of having white light increases night to day road crashes by 42%, by calculating exp(0.35). However, this finding is lightly dismissed by the authors in the second to last paragraph on p141 by mere assertion. No call is made in the paper for an investigation; in case rolling out more white light causes more crashes. One may wonder, in the circumstances, had the effect for white light been of similar magnitude but in the opposite (that is beneficial) direction, whether the effect would have been similarly discounted.

Nothing is said about the Threshold Increment variable, even though if this increases by 0.1 units the point estimate would suggest an estimated increase in road crashes of over 10%, by calculating exp(0.1x1.08). No reason is given for its dismissal from Model 3.

No discussion of the model selection is given, and the absence of the deviance statistics is unwelcome. However, the deviance (fit statistic)

would seemingly worsen significantly in simplifying the models when going from Model 1 to 3. Reporting the Akaike Information Criterion would be useful in balancing model fit against model complexity. No model checking, e.g. through examination of residuals, is mentioned. We are not given evidence that any of the models are, in fact, appropriate for the data.

Some further issues are that: 1) No consideration is given to the presence of statistical interactions; to see for example, whether the effect of Threshold Increment is different for different values of Average Luminance and 2) No reason is given as to why the variables that are brought into the subsequent erroneous, seemingly cosmetic 'grouped data' analyses are not entered into the first analysis and the results reported.

3.2 The Grouped Data Analyses

The further analyses using grouped data (Section 3.3) are inappropriate. Grouping data destroys information and results clearly depend on how the grouping is done, through the choice of the number of groups and the group boundaries. Grouping and combining the data masks the inherent variation and uncertainty. It is stated in JF (Section 3.1 Methods) '*Data from streets with a similar average luminance (0.25 cd/m2 band width) were then combined. With a larger crash sample in each group the night to day crash ratio could be more reliably estimated …*'. Again, it seems that statistical significance is being chased by unsound means. JF give a reference to Scott (1980) to justify the process but the small, old study only grouped data into 3 bands to perform initial exploration of the effect of the seven lighting variables; there were only a small number of cases that were complete. The Scott final analysis used ungrouped data.

The JF plots seem to arise from using the SPSS 'Curvefit' procedure. This runs an ordinary least squares simple linear regression on the logarithm of the night to day crash ratio values and then exponentiates the fit back to the original crash ratio scale. Displaying the R^2-values is misleading and should not be used for the analysis of count data. Presenting these plots give an illusory impression of a high degree certainty, as in Figure 2 of the JF paper (see below) with the display of $R^2 = 0.99$, which is remarkably high.

From JF page 142

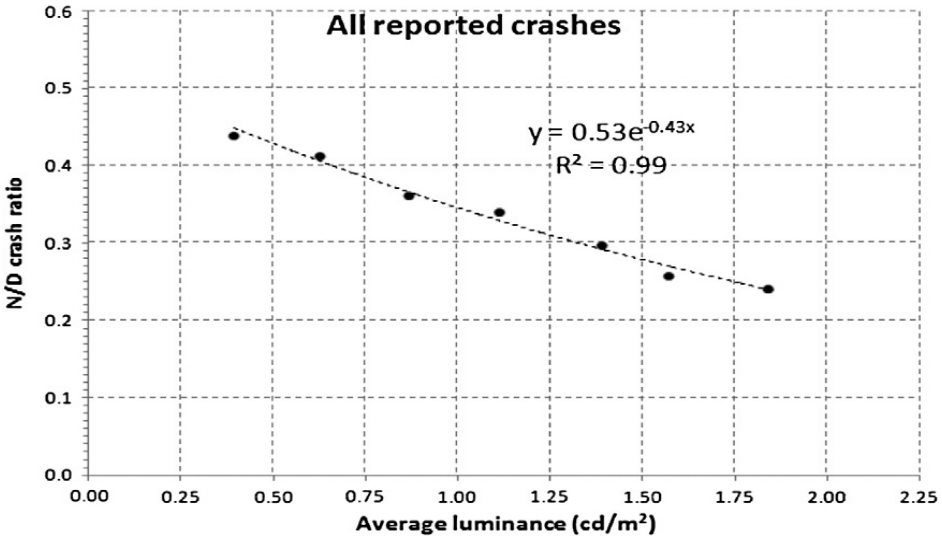


Fig. 2. The relationship between average luminance and the night to day crash ratio for all reported crashes.

It is puzzling why the variables used in JF's Figures 3, 4 & 5 (traffic volume, intersection and wet / dry road) were not included in the original generalised linear modelling, as might be expected in an appropriate statistical analysis.

From JF page 143

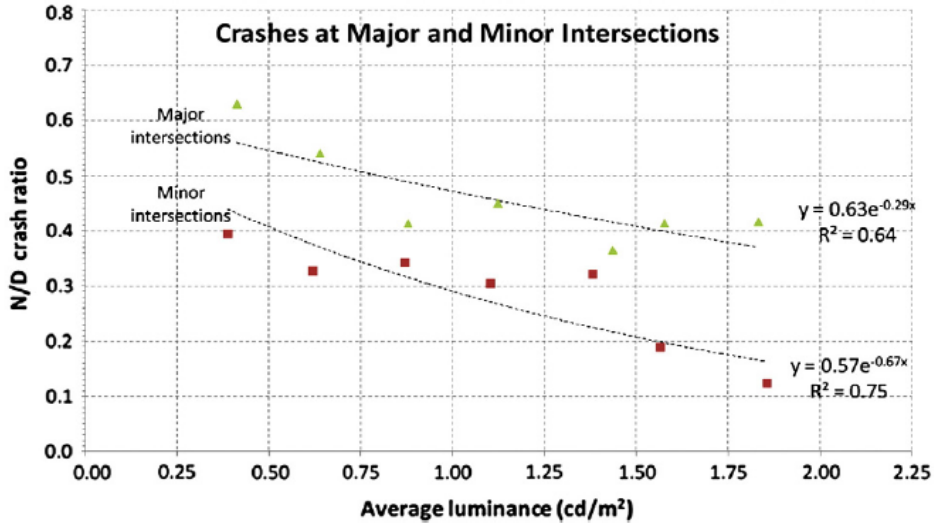


Fig. 4. The relationship between average luminance and the night to day ratio for intersection crashes for Major (traffic signals and roundabouts), Minor (other intersections).

What would be somewhat helpful to the reader of JF is a scatterplot of all 152 measurements of the N/D ratio, together with their

confidence intervals, against the luminance measurements, such that the scatterplot also indicates both the Threshold Increment and whether White Light was used. Ultimately, the properly scientific open practise would be to provide a table of the data from the 152 road sections giving both night and day crash numbers and the values of all the other variables collected. This would allow more informative plots to be produced and indeed an appropriate analysis to be performed. It is essential to have access to the initial data set before '*Some sites were subsequently shortened, subdivided or deleted to improve homogeneity*', along with the data set analysed, on which the paper is based. (Jackett and Frith have not only not published their data, but also have not responded to requests to gain access to it).

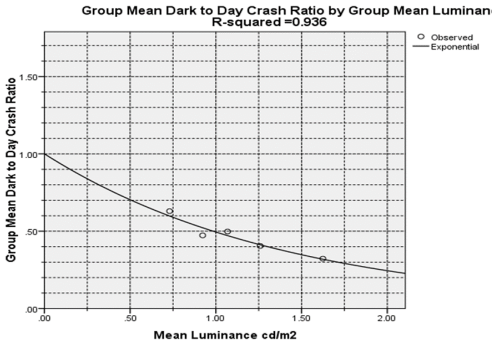
3.2.1 An illustration of the effect of grouping data

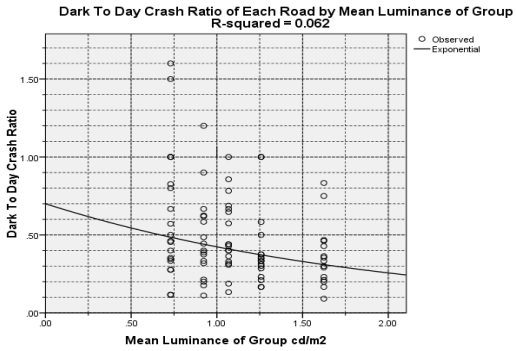
Given that JF's data was not made available, there now follows a short aside giving an illustrative example of the effect of using grouped data through using the data on individual roads of Hargroves & Scott (1979), a study of similarly flawed cross-sectional design. Hargroves and Scott (HS) did not perform grouped data (cosmetic) analyses but used the appropriate generalised linear model approach and commendably had their data printed as part of the paper. These data can be used to illustrate the flaw in the cosmetic nature of the graphs produced by JF. Here the HS 89 data are put into 5 roughly equal-sized luminance bands, each containing roughly 18 cases. The end-points, of the luminance bands are: 0.25 to 0.85, 0.87 to 0.97, 0.98 to 1.16, 1.18 to 1.35 and 1.40 to 2.03.

The left-hand figure below shows the result of taking the means of the night to day crash ratios within the five road luminance bands and running a regression on the logarithm of the crash ratio before transforming back. The analysis by this spurious means yields an impressive R^2 of 0.936. The right-hand figure shows the data points that comprise the luminance bands and the analysis that takes into account this obvious variation yields the small value of $R^2 = 0.062$.

The night to day crash ratio plotted against the mean group luminance values for mean and individual data.

The night to day crash ratio plotted against the mean group luminance values for mean and individual data.





Using the mean gives an impressive squared correlation coefficient, $R^2 = 0.936$

Using the individual 89 data points gives $R^2 = 0.062$ and a totally different, more realistic view

Using the mean gives an impressive squared correlation coefficient, $R^2 = 0.936$. Using the individual 89 data points gives $R^2 = 0.062$ and a totally different, more realistic view .

The above illustrates that working with aggregate values hides variation and hence uncertainty and it is therefore misleading to present research this way. (If data are weighted the R^2 values for grouped and individual situations are, as above, very different). It should be recognised that it is inappropriate to create the night to day crash ratios and run standard OLS linear regressions on the logarithm transformed data as underlies the above. (R^2 is not a suitable measure for count data). Note, if the number of night or day crashes is zero such cases cannot be included in such an analysis.

The important point is that the appropriate way to proceed is to use a model on the individual cases in which; 1) the count nature of the response is properly treated, 2) appropriate predictors are not excluded, 3) generalised linear modelling assumptions are respected, 4)

the fitted model is checked, 5) full details are reported and 6) data is made available to others.

4. Some Consequences of the JF study

There are of course real-world consequences of poor quality research. The JF work is being used to justify the introduction of extensive new lighting.

The Institute of Public Works Engineering Australasia report, titled 'SLSC Roadmap: Smart Lighting Smart Controls' (2016/17) is a pitch to 'accelerate to the deployment of LED street lights and smart controls in Australia and New Zealand'. The 100+ page report is available from http://www.slsc.org.au/slsc-publications/slsc-roadmap

SLSC Council members, see page ii of the document, seem to be predominantly a consortium of lighting and energy interests and the technical advisory group has a similar sort of membership.

Below is an image of the Roadmap document's disclaimer. The disclaimer does not seem to offer much in the way of guaranteeing that the Roadmap contains trustworthy statements.

Disclaimer

This work was performed with due care and in accordance with professional standards. However, the views expressed in the document are solely those of IPWEA and its advisers, Strategic Lighting Partners and Next Energy. Any representation, statement, opinion or advice, expressed or implied in this publication is made in good faith but on the basis that IPWEA, Strategic Lighting Partners and Next Energy are not liable (whether by reason of negligence, lack of care or otherwise) to any person for any damage or loss whatsoever which has occurred or may occur in relation to that person taking or not taking (as the case may be) action in respect of any representation, statement or advice referred to here.

The JF paper is mentioned in Section iv, of 3.2.2 Road Safety, concerning Street Lighting Levels on page 9 of the Roadmap. On page 10 the $R^2 = 0.99$ graph is reproduced. Clearly nobody from the many organisations and technical advisors engaged in SLSC spotted the

fact that the $R^2 = 0.99$, impressive as it is, is a gross misrepresentation of the empirical evidence. Neither it seems was the inference of causation from the correlation of luminance with the night to day crash ratio subject to scrutiny and scepticism.

In its section 3.2.3, the Roadmap references Steinbach et al (2015) which is a short version of the previously mentioned Perkins et al, 2015 NIHR 'LANTERNS' report 'What is the effect of reduced street lighting on crime and road traffic injuries at night? A mixed-methods study'. As noted earlier the study made a variation from protocol to include changing to white / LED light (from such as low-pressure sodium), despite the title of the project being about reductions. The Roadmap document makes an excuse for LANTERNS not finding any effect, on the RTC rate, through lighting change. (As stated earlier, all the 95% confidence intervals around the aggregate point estimates of the effects of all the types lighting change studied, change to white / LED as well as reductions, include zero and so detected no good evidence of any change according to conventional scientific criteria.) The Roadmap however says on page 13, *'The fact is that all interventions to reduce lighting were prudently designed by UK councils to take place where and when road and street traffic was at its lowest levels and traffic accident frequency was at its lowest ….'*. It fails to point out that LANTERNS also investigated change to white / LED light, which operate throughout the night, and failed to find any effect for this. (Note that part of the reason for changing roads to white /LED light is because of the belief that it improves road safety, e.g. see, Croydon and Lewisham Boroughs Street Lighting PFI: Final Business Case. London, UK, 2011). White / LED lights are installed on typical roads not just those with low traffic levels. In fact, for the 15833km of road length that changed to white / LED light, the 95% confidence interval for night to day risk ratio, after change to before change, was (0.93, 1.09). That is the confidence interval goes from a 7% reduction, through zero to a 9% increase in risk, so the verdict is no detectable change.

Regrettably, the JF work is also cited by the Royal Society of New Zealand (2018) in its report 'Impacts of Artificial Blue Light on Health and the Environment, Evidence Summary' The JF paper is reference 84 of the downloadable documents and is cited as providing evidence

for lighting being 'an effective road safety measure'. (Other work of these authors is also cited in references 83, 89 and 161.)

The Journal that published the JF paper does not allow responses to its previously published papers that the journal has already published. The journal is IATSS Research (IATSS stands for International Association of Traffic and Safety Sciences) In response to the simple query "I do not see a method by which an article can be published criticising a previously published paper for its severe methodological short-comings" the reply received was, "Thank you for your query. We regret to inform you that we do not publish commentary." Email communication from Celina David, Journal Manager. Clearly in contrast to the policy of IATSS Research (an Elsevier publication), journals should always allow serious comment on previously published papers; such comment is also known as post-publication review. Journals clearly need to employ properly qualified statistical reviewers as part of the reviewing process to trap grave errors before a paper is published.

5. Conclusion

From the evidence given and without having access to the full data, very little credence can be given to the conclusion drawn in the JF paper. The assumption that any correlation detected, between night to day crash ratio and lighting, is showing that variation of lighting is *causing* variation in the crash ratio is clearly highly suspect. This is because the unsuitable study-design fails to eliminate plausible alternative causal explanations. The JF study certainly *does not* show what would happen to the night to day crash ratio if a road were to be relit with brighter lighting. The poor statistical approach it exhibits is of great concerns since human lives and injuries are involved. Journals in general and IATSS Research in particular need to guarantee statistical rectitude and encourage post-publication review. A general concern is that much research, in many fields, suffers from poor research practice, thereby threatening research integrity as discussed in the research integrity inquiry of the UK Parliament (Commons) Science and Technology Committee (2018).

It would be of benefit if the incremental roll out of large-scale public projects were to be done as rigorous scientific experiments, with clear

pre-defined measures of success, so that any claimed benefits could be checked as the implementation proceeds. By this means any programme could be stopped or adjusted if it was found to be not delivering its objectives. More success with policy implementations might arise, if advocates who stand to gain financially by a proposal's acceptance, share some of the financial risk of implementation failure. Doing so might encourage such advocates to be careful in only adducing sound scientific evidence for any proposal put forward.

5. Declaration of Interest.

The author has concerns about light pollution affecting astronomical observations and biodiversity, so has been motivated to examine the claims of benefit for increased public lighting. The author has previously published work critical of claims of substantial public safety benefit of increasing lighting. No funding was sought or obtained for this work.

Note by Author

This article was previously published in the, now sadly defunct, *World Transport Policy and Practice, 26(2) pp 8 – 20, March 2020*.

References

Beyer F R & Ker K (2010) Street lighting for preventing road traffic injuries. The Cochrane Library https://www.cochrane.org/

Hargroves R A & Scott P P (1979) Measurements of Road Lighting and Accidents, Public Lighting pp213-221, December

The Institute of Public Works Engineering Australasia, SLSC Roadmap: Smart Lighting Smart Controls 2016/17, downloadable from http://www.slsc.org.au/slsc-publications/slsc-roadmap

Jackett M & Frith W (2013) Quantifying the impact of road lighting on road safety — A New Zealand Study IATSS Research 36 (International Association of Traffic and Safety Sciences) pp 139-145 https://www.sciencedirect.com/science/article/pii/S0386111212000325?via%3Dihub

Marchant P R, (2017) Why Lighting Claims Might Well Be Wrong, International Journal of Sustainable Lighting 19 69-74 https://doi.org/10.26607/ijsl.v19i1.71 http://www.lightingjournal.org/index.php/path/article/view/71/79

Marchant P, (2019) Do brighter whiter street lights improve road safety? Significance Vol 16, Issue 5 pp8-9, a joint publication of the Royal Statistical Society and the American Statistical Association https://doi.org/10.1111/j.1740-9713.2019.01313.x

Perkins, C, Steinbach, R, Tompson, L, Green, J, Johnson, S, Grundy, C, Edwards, P et al. (2015) What is the effect of reduced street lighting on crime and road traffic injuries at night? A mixed-methods study. Available from: https://www.journalslibrary.nihr.ac.uk/programmes/phr/11300402/#/.

The Royal Society of New Zealand, (November 2018) 'Impacts of Artificial Blue Light on Health and the Environment, Evidence Summary', downloadable from https://royalsociety.org.nz/major-issues-and-projects/blue-light-aotearoa/

Scott P P, (1980) The Relationship Between Road Lighting Quality and Accident Frequency,

in: TRRL laboratory report LR 929, https://trl.co.uk/reports/LR929

Steinbach R, Perkins C, Tompson L, Johnson S, Armstrong B, Green J, Grundy C, Wilkinson P, Edwards P. (2015) The effect of reduced street lighting on road casualties and crime in England and Wales: controlled interrupted time series analysis. Journal of Epidemiology and Community Health 2015 69(11):1118-1124. Available from: https://jech.bmj.com/content/69/11/1118

UK Parliament (Commons) Science and Technology Committee, (July 2018) Report into Research Integrity, HC350. https://publications.parliament.uk/pa/cm201719/cmselect/cmsctech/350/350.pdf The inquiry website is https://www.parliament.uk/business/committees/committees-a-z/commons-select/science-and-technology-committee/inquiries/parliament-2017/research-integrity-17-19/ The Committee page is

https://www.parliament.uk/business/committees/committees-a-z/commons-select/science-and-technology-committee/

World Health Organization (2018) Road Traffic Injuries Factsheet https://www.who.int/news-room/fact-sheets/detail/road-traffic-injuries

Corresponding Author; Paul Marchant
p.marchant@lightupevidence.com

Appendix

A simple example showing the superiority of using a longitudinal design instead of cross-sectional

To show effect of the number exposed to risk in night to day crash ratios: comparing cross-sectional and longitudinal approaches.

An issue of importance in road crash studies is the number, or rate (per unit time) of crashes 'C' adjusted for the number exposed to risk 'F', the flow of traffic. This would seem to be sensible. Then using subscripts 'N' for Night and 'D' for Day, the quantity of interest is no longer C_N/C_D but becomes C_N/C_D / $(F_N/F_D) = \frac{C_N F_D}{C_D F_N}$. However, without measurement of the number exposed to risk both night and day, we cannot know the size of this quantity. The ratio of the number exposed to risk Night to Day may vary considerably between different roads and may well be related to the nature of the road, which in turn may be key to the lighting chosen. Such considerations make any claim that it is the variation of lighting alone which is responsible for the variation of C_N/C_D crash ratio highly suspect.

Now for a 'Before and After' study (that is a longitudinal approach) where lighting is changed in-between, the quantity of interest for a road is the ratio, before and after, of the above quantity, $\frac{C_N F_D}{C_D F_N}$.

Using B to indicate 'Before' and A for 'After' we obtain the relevant quantity, the ratio of ratios.

$$\left[\frac{C_{NA} F_{DA}}{C_{DA} F_{NA}}\right] / \left[\frac{C_{NB} F_{DB}}{C_{DB} F_{NB}}\right]$$

A value of less than one would denote a reduction in the night to day crash ratio. A 'statistically significant' value of less than one would be taken as evidence that the change of lighting has been successful in reducing the night to day crash ratio.

Because one might expect the ratio of the night to day numbers exposed to risk to be approximately stable, before and after on a given road, these F_N/F_D values therefore cancel in above expression. Of course, it would be useful to have accurate measurements of traffic, flow F_N and F_D, to put into the above expression. However, in the

absence of such information it is reasonable to assume the values for traffic flow will closely cancel in the expression.

The ratio of after to before ratios of night to day crash numbers, $\frac{C_{NA}C_{DB}}{C_{NB}C_{DA}}$, in a longitudinal study is therefore likely to be a better indicator of whether new lights have improved or worsened matters as it is much less affected by ignorance of the numbers exposed to risk, as in the case of a cross-sectional study. It is these values, $\frac{C_{NA}C_{DB}}{C_{NB}C_{DA}}$, of the after to before ratio of ratios for every road in the study sample, together with their appropriate estimates of uncertainty, that need to be analysed in a longitudinal study.

The argument against a cross-sectional design, as opposed to using a longitudinal one, is the same for any other cause of the number of road crashes that is correlated with lighting (that is, one which is different from night to day numbers exposed to risk).

Therefore, as shown above, longitudinal studies give more trustworthy results when ascertaining what effect road lighting might have on road crashes as it tends to rule out extraneous impacts, as these are likely to remain more-or-less the same at the end as at the beginning