

Interactive comment on “Assessing the Stability of Surface Lights for use in Retrievals of Nocturnal Atmospheric Parameters” by Jeremy E. Solbrig et al.

Anonymous Referee #2

Received and published: 26 June 2019

Dear Authors, First let me thank you for a well-written manuscript on this interesting study. The topic of quantitative characterization of nighttime environments is an exciting area, where scientists are rediscovering fundamental consequences of radiative transfer in novel contexts.

I think this study is sound and publishable, even though it is somewhat preliminary and reads largely as an exploratory data analysis. It would benefit from a bulletized summary of the conclusions, and a further summary of priority areas for further research.

I have arranged my comments roughly from most to least scientific importance, with typographic errors at the bottom. Good luck in preparing revisions, and thank you for your

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hard work on this study. I appreciate your acknowledgement of the errors introduced by the nearest-neighbour interpolation used to generate your time series. However, you should specifically consider the impact of this resampling on the scenes which contain point sources. These point sources will not reliably be contained in the same pixel using your resampling approach, and the high variability and poor correlations over Iraq are likely to be primarily a function of this spatial error. Examination of isolated point sources such as gas wells is definitely a place where a more sophisticated approach to spatial data is required.

In your discussion of the maximum and minimum (and other stats) constructed scenes, you should clarify your expectations from these scenes. For pixels containing surface light sources, the brightness maximum should be the clearest night in the time series. For darker pixels adjacent to those sources, the brightness maximum should be the maximum atmospheric scattering (longest atmospheric path). This is consistent with what is seen in the patterns of correlation you found between satellite zenith and DNB, and should be discussed at the outset.

Page 11 line 15 end of Section 4. It's puzzling to say that DNB-IR correlation is beyond the scope of your study when the next section of the paper is about DNB-IR correlation. Since you are discussing the limitations of your study with regard to gas flares, this is a good place to mention the role of spatial resampling error in studying these areas.

I think it is worth noting that satellite aerosol retrieval algorithms going all the way back to Kaufman 1997 have used the variance in the visible brightness as a means of detecting and screening residual and subpixel cloud. This emphasizes both the opportunity and the challenge of using brightness variations as diagnostic of atmospheric conditions.

Page 17 line 29: I think the finding that the DNB radiance time series show the signal of forward scattering of anthropogenic light sources by land surfaces is a significant one and should be revisited here. Abstract-first sentence- this sentence is clumsy

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and perhaps ungrammatical. ‘Sensitivity’ is ‘information typically provided by visible spectrum observations’?

Page 12 Line 1 Start of Section 5. I think you can make this shorter by simply stating “The results below are shown for all sites including Iraq, but statistical strength for the Iraq domain is weaker due to smaller sample size for the reasons discussed above.”

Page 13 Line 24 ‘While this is discussed further in Sect. 5.3, suffice to say...’ Maybe just say “This effect is discussed in Section 5.3”

Page 15 line 19 “correlation...noisy” I think a better word is ‘weak’ when describing a correlation.

Page 15 line 21 “the same bias” do you mean the same trend?

Page 15 line 33 “DNB radiance and four brightness temperature from four” -> “DNB radiance and brightness temperature from four”

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