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Interactive comment on "Bioclimatic traits in statistical properties of daily photosynthetically active radiation" *by* Estefanía Muñoz and Andrés Ochoa

Anonymous Referee #2

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Munoz and Ochoa explore patterns of PAR across different latitudes and climate zones. The analysis may be important to the extent that it helps organize and communicate variability in photosynthetically active radiation to the biogeosciences community. At the moment it does not, but I feel that it might. Namely, if the amount of variation in PAR, c, and k explained by solar geometry (obvious) and climate (less obvious) could be determined I could see how obvious aspects of the manuscript could be placed in the context of information that could be quite enlightening for our understanding of how light reaches the surface across the globe. If the authors can do this I feel that the manuscript could be acceptable for publication; at the moment the findings are largely either obvious or unclear, but the latter can be fixed by restructuring the

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manuscript and explaining more clearly what was done and its implications. Regarding "Attenuation of light throughout the atmosphere can be calculated by using an attenuation law (e.g. the Beer–Lambert law), but this requires to know the atmospheric optical depth, which is seldom the case" it is also important to note comprehensive atmospheric modeling that seeks to understand the dynamics of atmospheric transmissivity, reflectivity, and absorptivity as a function of wavelength and layer of the atmosphere. Such models are great but difficult to implement at large scales. Page 1 Line 22: light attenuation is not random, it is a function of the physics of the atmosphere. Page 1 Line 25: More evidence is needed that this is the case in the form of references. The Introduction as a whole was a bit too brief. Specifically the notion that c and k are stochastic needs to be addressed in more detail. In many regions, clouds are rather predictable like in areas where sea breezes create weather systems that are easy to anticipate. Fog is another atmospheric phenomena that is expected and predictable in certain times and certain reasons. I had coastal California ecosystems in mind when writing that but then noted that this paper was published just today. (https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2020GL088428) Please expand the introduction to discuss the variables that change c and k. Note also that PAR and the shortwave bands overlap, but incompletely. If you are studying PAR, simply explain why and what the important differences are. Adding the Holdridge / Koppen zones to Table 1 would be an improvement. Why only these 28 sites? There are a number of high-latitude sites with long-term consistent PPFD, for example. Page 4 Line 6: this is true but requires elaboration: 'troublesome when using the Beer–Lambert law'. It is certainly troublesome if the atmosphere is considered to be one layer because atmospheric attenuation will vary dramatically by layer over time, but a layer-by-layer implementation of the Beer-Lambert Law over short time scales may be quite accurate. . .but difficult to implement After equation 4: 'transmittance due to molecular absorbers of': please note that this is for the clean and dry atmosphere for this particular calculation ('cda') so that people realize why aerosols and other non-molecular absorbers (and reflecters) are excluded. Why is forward / back scattering of

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0.5 assumed? Please elaborate in the text. In equation 5, how much do higher-order reflectances typically contribute? It might not be minor, I'm not sure. In equation 6, how is ozone derived? Is it weighted for its distribution throughout the atmospheric column? (A simple mean wouldn't do). I note a reference to Iqbal (1983) but elaboration would help the reader. Section 3.3: PAR itself is an excellent proxy for cloudiness. Why is precipitation used? Of course it is almost always cloudy when rain is observed but of course more often than not there are clouds but no rain. Page 6 line 19: Was AT-Neu chosen because it is the first alphabetically? This site is in a north-facing mountain valley and there will be shielding of the sun by mountains to the east and west in the early morning and late afternoon.

Figures S1 to S28 is a bit too much information, even for a supplement. For Fig. 2C of course there is a 180 day negative autocorrelation because of solar geometry. It is interesting to see that c and k have somewhat more complicated long-term autocorrelation functions but is there a better way to syntheize this than to create 28 figures in a Supplement? P. 6 L. 22: Too many of these statements are obvious and follow directly from the solar zenith angle and the amount of atmosphere that a beam has to travel through when the sun is not directly overhead. Also, what does this statement mean 'In these sites, climatic seasonality is very weak since the low ACF after removing the astronomical seasonality.' That the statistics of PAR, c, and k are controlled by solar geometry rather than climate? Of course this isn't surprising but it would be interesting to see that proportion of the variables are explained by climate vs. solar geometry, like a variance decomposition. How much of the variability at each site is explained by these two factors and does Koeppen climate classification help explain some of the variability that is not explained by latitude alone? It is still not clear to me what 'bimodal' means. This is a scale-dependent term. More than one peak per day? More than one peak per season? The statement on page 7 line 9 isn't supported directly by a figure and I am still confused as to what the major objective of the manuscript is. P 8: reword: allowing to analyze schematically Does Fig. 4 directed at the notion that some sites have darker clouds than others because of the distributions of c and

k on wet and dry days? Figure 5: was a Bonferroni correction applied to significance values? Also, please do not simultaneously use red and green in the same figure. Also, why are both KS and AD tests used? What advantages do they each have and why not choose just one? Are the values in the boxes p-values and why are they frequently greater than 1? The paragraph after Figure 5 is confusing (p. 11 line 1). I'm not sure what it means: are the data being used to define when seasons begin and end? Figure 6: Please avoid rainbow color schemes (https://eos.org/features/the-end-of-the-rainbow-color-schemes-for-improved-data-graphics). Also, the relationship between k and c is merely PAR_0 / PAR_cda. This figure only shows how much atmosphere there is which of course is greater at high latitudes in winter when the sun is arriving at an angle (no idea what is happening with US-SRM). It is an inefficient way of showing the effects of the solar zenith angle on surface radiation. I cannot emphasize enough how important it is to have very clear subsections when writing a combined Results and Discussion section. The section jumps surprisingly to different topics throughout and is very difficult to follow. Please add subsections at a minimum to help the reader interpret the flow of the argument. Bottom of page 13: I am still not sure what bimodal means in this context and why the analysis is extended to Holdridge life zones. Do some of these ecosystems have expected diurnal or seasonal variability in cloudiness such that grouping the analysis by life zone makes sense? Also, one would expect that a manuscript submitted to Biogeosciences would discuss the importance of the findings to biogeoscience. In this case the role of PAR in controlling photosynthesis is a logical connection. The paper would be stronger if implications for biogeoscience were discussed in more detail. I want to very strongly recommend that the analysis have separate Results and Discussions sections to make it easier to follow and to make the importance of the analysis more clear.

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