Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-31-AC2, 2020

Biogeosciences Discussions
Open Access
EGU

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

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Received and published: 27 June 2020

Dear Reviewer 1, We sincerely thank Reviewer 1 (Anonymous Referee #2) for his comprehensive comments, which will be of great help in improving our manuscript. Please see below for a point-by-point response to your comments.

1. 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

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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 manuscript and explaining more clearly what was done and its implications. Author response: Yes, there are some obvious results that are not the core of our study and we should state it clearly. Although we analyze PAR, c and k, our main interest is on c and k. Our important (and less obvious) findings are that: a) The PDFs of c and k have the same shape at each site. b) We identified three types of PDF: unimodal with low dispersion (Unimodal I), unimodal with high dispersion (Unimodal II), and bimodal. c) PDFs are unimodal for all the dry life zones studied, bimodal for low rainfall (< $\sim$1000 mm/yr) humid life zones and unimodal with high dispersion for high rainfall (2000-4000 mm/yr) humid zones. d) There is one Holdridge life-zone class that is a triple point where the three PDF types occur. It is the "Moist forest" with ETp ratio between 0.5 and 1, annual precipitation between 1000 and 2000 mm and humidity province Humid. The variability of PAR is of course explained by latitude and solar geometry, but c and k variability seem to be strongly influenced by the local (maybe regional) climate/life zones. If the Editor agrees, we will write our manuscript more clearly.

2. 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. Author response: We model atmospheric attenuation as a function of wavelength for a clean and dry atmosphere along the day, and then integrate over the PAR spectral domain and daylength to obtain the daily PAR. We do not model attenuation by clouds and aerosols but expect this attenuation to be quantified by the cloudiness index (c).

3. Page 1 Line 22: light attenuation is not random, it is a function of the physics of the atmosphere. Author response: Yes. What is random is the amount of aerosols and clouds in the atmosphere. We will write it more clearly.

4. 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. Author response: 1) We will discuss in more detail the use daily c and k including the most relevant references (e.g., Utrillas et al. (2018), Ineichen (2016), Engerer & Mills (2014), Tran (2013), Hollands & Suehrcke (2013), Harrouni (2008), Ianetz & Kudish (2008), Polo et al. (2008), Tovar-Pescador (2008), Allen et al. (2006), Assunção et al. (2003), Cañada et al. (2003), Ibánez et al. (2002), Hansen (1999), Martinez-Lozano et al. (1999), Skartveit & Olseth (1992), Gordon & Hochman (1984), Olseth & Skartveit (1984), Bendt et al. (1981), and Liu & Jordan (1960)). 2) Although cloudiness could be predictable in some regions, our interest is not in a forecasting model. Our objective is to know the statistical behavior of c and k in relation to local climate and life zone to use it as a driver for ecohydrological and biogeochemical stochastic models in energy-limited ecosystems (see Muñoz et al. 2020). We will clarify this point in the manuscript. Allen, R.G., Trezza, R., & Tasumi, M. (2006). Analytical integrated functions for daily solar radiation on slopes. Agricultural and Forest Meteorology, 139, 55–73. https://doi.org/10.1016/j.agrformet.2006.05.012 Assunção, H. F., Escobedo, J. F., & Oliveira, A. P. (2003). Modelling frequency distributions of 5 minute-averaged solar radiation indexes using Beta probability functions. Theoretical and Applied Climatology, 75(3–4), 213–224. https://doi.org/10.1007/s00704-003-0733-9 Bendt, P.,

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to global broad band irradiation in Valencia, Spain. International Journal of Climatology, 19(8), 903–911. https://doi.org/10.1002/(SICI)1097-0088(19990630)19:8<903::AID-JOC400>3.0.CO;2-N Muñoz, E., Ochoa, A., Poveda, G., & Rodríguez-Iturbe, I. (2020). Probabilistic soil moisture dynamics of water- and energy-limited ecosystems. Earth-ArXiv. https://doi.org/10.31223/osf.io/au4tb Olseth, J.A., & Skartveit, A. (1984). A probability density function for daily insolation within the temperate storm belts. Solar Energy, 33(6), 533–542. https://doi.org/10.1016/0038-092X(84)90008-2 Polo, J., Zarzalejo, L. F., & Ramírez, L. (2008). Solar Radiation Derived from Satellite Images. In V. Badescu (Ed.), Modeling Solar Radiation at the Earth's Surface (pp. 449–461). Springer-Verlag Berlin Heidelberg. Skartveit, A., & Olseth, J. A. (1992). The probability density and autocorrelation of short-term global and beam irradiance. Solar Energy, 49(6), 477–487. https://doi.org/10.1016/0038-092X(92)90155-4 Tovar-Pescador, J. (2008). Modelling the statistical properties of solar radiation and proposal of a technique based on Boltzmann statistics. In V. Badescu (Ed.), Modeling Solar Radiation at the Earth's Surface: Recent Advances (pp. 55–91). https://doi.org/10.1007/978-3-540-77455-6_3 Tran, V. L. (2013). Stochastic models of solar radiation processes. Université d'Orléans. Utrillas, M.P., Marín, M.J., Esteve, A.R., Salazar, G., Suárez, H., Gandía, S., & Martínez-Lozano, J.A. (2018). Relationship between erythemal UV and broadband solar irradiation at high altitude in Northwestern Argentina. Energy, 162(August), 136–147. https://doi.org/10.1016/j.energy.2018.08.021

5. 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. Author response: We are interested in characterizing c and k for PAR to use them as inputs in stochastic ecohydrological and biogeochemical modeling. We will incorporate this context in the manuscript.

6. Adding the Holdridge/Koppen zones to Table 1 would be an improvement. Author response: It will be done.

7. Why only these 28 sites? There are a number of high-latitude sites with long-term

consistent PPFD, for example. Author response: We initially took information from 292 sites from FLUXNET. The initial criteria to select the sites to analyze were: records with a length greater than 5 years (76 sites) and with complete time series of precipitation data (59). Then, we visually selected the sites with suitable information and were not very close to each other. As the sites obtained were mainly located in the northern hemisphere, we were laxer selecting sites in the southern hemisphere. For those, we chose the sites with longer time series, without apparent errors and precipitation information.

8. 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. Author response: We agree with your comment. We will expand this idea in the manuscript.

9. 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 nonmolecular absorbers (and reflecters) are excluded. Author response: We will clarify it.

10. Why is forward / back scattering of 0.5 assumed? Please elaborate in the text. Author response: This parameter is very difficult to estimate. We used 0.5 arbitrarily and expect that the uncertainty and variability of this parameter be expressed in the clarity index (c).

11. In equation 5, how much do higher-order reflectances typically contribute? It might not be minor, I'm not sure. Author response: It may be important in the presence of a high surface albedo. Indeed, we got some c>1 values during winter in sites with seasonal snow. We report this effect in line 20 of page 6. When c>1 the contribution of high-order reflectances is clear, but it could go unnoticed when c<1. The c index

represents uncertainty from several sources, and that is why it has different names as “cloudiness index”, “clear-sky index” and “clarity index”. In our approach, c should also include the uncertainty of albedo and high order reflection.

12. 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. Author response: We use the seasonal variation of atmospheric ozone from Iqbal (1983, p.89) that gives the total amount of ozone in a vertical column of air for several latitudes. We interpolate for the latitude and doy of interest.

13. 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. Author response: This is a good point and deserves an interesting statistical approach, but it is out of our scope for now. We use rainfall because we have in mind the connection between our statistical characterization of c and k and the ecohydrological and biogeochemical models of Rodríguez-Iturbe and collaborators. That family of models study the stochastic balance equation (for water, carbon, nitrogen) of the soil forced by a Poisson process rainfall model with parameter lambda. In a future research, we plan to study the joint PDF of daily rainfall (occurrence and amount) and PAR. We will incorporate this context in the introduction and conclusions of the manuscript.

14. 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. Author response: AT-Neu was selected because it has long and high-quality records. You are right about the local orography at this site. That local trait, as well as Holdridge life zone or Köppen climate zone, must be reflected in the PDF of c and k.

15. Figures S1 to S28 is a bit too much information, even for a supplement. For Fig.

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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 synthesize this than to create 28 figures in a Supplement? Author response: We agree that panel (c) inf Figures S1 to S28 is not very informative. We think, however, that panels (a), (b) and (d) to (i) are a good support of the analysis. Removing panel (c) does not reduce too much the amount of information in the Supplement. We would appreciate more suggestions about this from you, the other reviewers and the editor.

16. 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? Author response: We agree that some statements are obvious for PAR and they should be removed/rewritten, but they are not so obvious for c and k. The analysis of ACF is not clear and we will rewrite it. We analyzed the effect of the local climate on the shape of the PDF of c and k by visual inspection. However, it would be very enlightening to quantify it by a variance decomposition, as you suggest. We can do it if the editor agrees.

17. 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? Author response: "Bimodal" and "unimodal" are terms used in tropical hydrology to describe the shape of the annual regime of hydrologic variables. Unimodal refers to one mode and bimodal to two modes (e.g. two rainfall seasons, two growing seasons (Knoben et al. (2019), Ur-

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rea et al. (2019), Herrmann & Mohr (2011)). We will incorporate this explanation in the manuscript. Herrmann, S.M., & Mohr, K.I. (2011). A Continental-Scale Classification of Rainfall Seasonality Regimes in Africa Based on Gridded Precipitation and Land Surface Temperature Products. Journal of Applied Meteorology and Climatology, 50(12), 2504–2513. https://doi.org/10.1175/JAMC-D-11-024.1 Knoben, W.J.M., Woods, R.A., & Freer, J.E. (2019). Global bimodal precipitation seasonality: A systematic overview. International Journal of Climatology, 39(1), 558–567. https://doi.org/10.1002/joc.5786 Urrea, V., Ochoa, A., & Mesa, O. (2019). Seasonality of Rainfall in Colombia. Water Resources Research, 55(5), 4149–4162. https://doi.org/10.1029/2018WR023316

18. 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. Author response: This paragraph is not clear and we will rewrite it. The main objective of this study is to characterize the PDF of c and k according to the local climate/life zone. Our afterward interest is to use this characterization as an input to stochastic ecohydrological and biogeochemical models (e.g. Ridolfi et al. (2003), Manzoni et al (2004), Botter et al. (2018), Runyan and D'Odorico (2019), Manzoni et al. (2020), Muñoz et al. (2020)), but it will be useful also in other fields such as agronomy, geotechnics and natural hazard analysis. Botter, G., Daly, E., Porporato, A., Rodríguez-Iturbe, I., & Rinaldo, A. (2008). Probabilistic dynamics of soil nitrate: Coupling of ecohydrological and biogeochemical processes. Water Resources Research, 44(3), n/a-n/a. https://doi.org/10.1029/2007WR006108 Manzoni, S., Porporato, A., D'Odorico, P., Laio, F., & Rodriguez-Iturbe, I. (2004). Soil nutrient cycles as a nonlinear dynamical system. Nonlinear Processes in Geophysics, 11(5/6), 589–598. https://doi.org/10.5194/npg-11-589-2004 Manzoni, S., Chakrawal, A., Fischer, T., Schimel, J. P., Porporato, A., & Vico, G. (2020). Rainfall intensification increases the contribution of rewetting pulses to soil respiration. Biogeosciences Discussions, 1–25. https://doi.org/10.5194/bg-2020-95 Muñoz, E., Ochoa, A., Poveda, G., & Rodríguez-Iturbe, I. (2020). Probabilistic soil moisture dynamics of water- and energy-limited ecosystems. EarthArXiv. https://doi.org/10.31223/osf.io/au4tb Ridolfi, L., D'Odorico, P., Porporato, A., & Rodriguez-Iturbe, I. (2003). The influence of stochas-

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19. 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? Author response: We will improve writing. All panels in Fig. 4 are for AT-Neu, top panels for c index and bottom panels for k index. The difference between top panels and bottom panels comes from the clean and dry atmosphere and is an obvious result as you pointed out before. We use Fig. 4 to show that the bimodal PDF of c and k is the superposition of two unimodal PDFs, ones for dry and one for wet days. Fig.3 suggests that sites within the same life zone have c and k PDFs with similar shapes. Moreover, the shape of the PDFs is similar between neighboring life zones. Our interpretation of this pattern is that the life zone is the result of adaptation to the local climate, although at large spatial and time scales the climate could be a result of the ecosystem. We will clarify this point in the manuscript.

20. 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? Author response: No, we didn't because we are not doing several tests at a time. We test all combinations of pairs of months. We can change the green/red in Fig. 5. We used the two tests because AD is more sensitive to the tails of the distribution while KS to the center of the distribution. We will discuss it in the manuscript, confronting the result with both tests. The p-values in the boxes are greater than 1 because they are multiplied by 100 to show more decimals using less space. It will be clarified in the legend of the figure.

21. 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? Author response: We are comparing the monthly PDFs of c for the dry-days and wet-days samples to inspect for the existence (and duration) of c and k seasons (i.e. groups of months where c and k have the same PDF). We will write it more clearly.

22. Figure 6: Please avoid rainbow color schemes (https://eos.org/features/the-end-of-therainbow-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. Author response: Thank you for pointing this out, we will change it to a two-color band. Yes, it is PAR0/PARcda and changes with latitude in an obvious way. However, this effect is asymmetric between the northern and southern hemispheres. This asymmetry is also obvious and is explained by the eccentricity in the Earth's orbit. We will remove this figure and this analysis.

23. 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. 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 Author response: We will separate Results and Discussion in two sections and will add the necessary subsections to have a more clear structure.

24. 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? Author response: Please see our response to comment 17

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on “unimodal” and “bimodal” terms. Since latitude is not enough to explain the shape of the PDF of c and k (e.g. USA-FPe, 48°N, is unimodal while CH-Oe1, 47.3°N, is bimodal), we extended our analysis to the Holdridge life zones looking for bioclimatic traits in c and k statistics. We use daily data and do not analyze any intra-diurnal variability. We will revise the manuscript to avoid this misinterpretation. On ecosystems and cloudiness: Yes, we expect that ecosystems in the same life zone have similar cloudiness regimes, not necessarily because they force atmospheric processes (these could happen at large spatial and temporal scales) but because they are the result of adaptation to the local climate.

25. Also, one would expect that a manuscript submitted to Biogeosciences would discuss the importance of the findings to biogeosciences. 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. Author response: Our study has important findings to biogeoscience and we will highlight them. Please see details in our response to your comment 18.

Estefanía Muñoz Andrés Ochoa .

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-31, 2020.

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