- **CC1**: 'Comment on egusphere-2024-1800', Petter Nyman, 16 Jul 2024

  Nice approach! Would be worth having a look at (and maybe citing) this paper which takes a very similar appraoch. See attached.
  https://www.publish.csiro.au/mo/es14011

*→ Thank you very much for sharing this bibliographical reference. Indeed, the article you have presented focuses on regional downscaling. While it treats the downscaling of climate data, it concerns mainly precipitation and longwave and shortwave radiation, in order to obtain an aridity index. The approach is very interesting and differs from this study mainly in the temporal resolution of the downscaling (monthly as opposed to hourly) and in the energy orientation, in order to calculate the energy maintained at ground level, whereas our article studies radiation in the main context of its availability for photosynthesis. In both cases, the slope, orientation and shading variables are taken into account for downscaling the radiation.*

- **RC1**: 'Comment on egusphere-2024-1800', Anonymous Referee #1, 27 Aug 2024

  Review of
  **Enhancing environmental models with a new downscaling method for global radiation in complex terrain**
  by Druel et al.

**General comments:**

This paper uses in situ observations of the incoming solar radiation to benchmark methods to calculate global, direct and diffuse radiation in complex terrain conditions in southeastern France. A method is selected and applied to adapt low spatial resolution estimates of these quantities from a meteorological forecast model. This atmospheric forcing is then used to drive two forest ecosystem models in order to assess the impact of the complex-terrain correction. It appears that the impact is tangible for spatial resolutions finer than 250 m. The paper is well written and results are clearly presented although the quality of the Figures and the Discussion could be improved. The R code routines are made available through gitlab. Maybe, it could be useful citing python routines that would be equivalent to the employed R routines.
Recommendation: minor revisions.

*→ Thank you very much for the review, which has helped us to significantly improve the quality of the manuscript. In line with what was pointed out, we have improved the quality of the figures, by reworking Figs. 1, 3, 4, 5 and 6 to make them more readable, and added a Table 2 which reproduces the values (equation parameters, r2 and RMSE) initially presented in Figs. 3.a-b. Similarly, the discussion has been completely rewritten, taking into account your comments. Finally, we have added in section 2.1.2 a reference for the python correspondence of the R routines used in this article: "Note that the same package is now available for python (on https://pypi.org/project/insolation/ and*

*https://www.meteoexploration.com/insol/).” Below are the specific responses to each of your comments.*

**Particular comments:**

- L. 89 (“luminosity”): do you mean "radiation"?

→*Thank you for pointing out this inaccuracy. We have replaced ‘luminosity’ with ‘radiation’ in the sentence.*

- L. 176: replace "J.m-2.s-1" by "W m-2"

→ *As a suggested simplification, we have replaced “J.m$^{-2}$.s$^{-1}$” by “W.m$^{-2}$”. Here, and throughout the manuscript.*

- L. 180 (solar constant): this estimate of the solar constant value given by Spitters et al. is a bit outdated. A more recent estimate is 1361 W m-2.

→ *Thank you for the opportunity to correct this point. Our source dates from 1978 (I.E.A). We have therefore updated it using the value of 1361 W m$^{-2}$, with the following reference: “Coddington, O., J. L. Lean, P. Pilewskie, M. Snow, and D. Lindholm, 2016: A Solar Irradiance Climate Data Record. Bull. Amer. Meteor. Soc., 97, 1265–1282, https://doi.org/10.1175/BAMS-D-14-00265.1.” The results presented in Figures 3 and 4 have been updated with this new value. The 0.66% difference in the solar constant does not imply any discernible difference in the results, which therefore remain consistent with the manuscript.*

- L. 192: it should be mentioned that the threshold and coefficient values given in Eq. 2 are valid for hourly radiation values.

→ *Indeed. To provide the reader with greater precision and simplify any reuse, information has been included in the text: “as recommended by de Jon (1980) for hourly radiation (described in Spitters et al., 1986, including values for daily radiation)”*

- L. 197: "circumsolar radiation" was not defined before. Could you define this quantity and explain why this further step is needed?

→ *Thank you for your comment. ‘Circumsolar radiation’ has now been defined (in a way that is understandable to everyone) and the need for this further step explained: “[...]: Under clear skies, diffuse irradiance is anisotropic, due to the presence of aerosols in the atmosphere, and the intensity is therefore higher in the direction of the sun. It is thus necessary to attribute the excess diffuse irradiance observed near the direction of global radiation to direct radiation.”*

- L. 282 (radiation measurements): Could you clarify what was measured? Global, direct, diffuse?

→ *Of course. We have now specified: “Global radiation measurements”*

- L. 349-351: What about longwave radiation? Do Castanea and Sureau use longwave radiation as an input? Longwave radiation is one of the drivers of the surface energy balance. Same question for atmospheric pressure.

→ *Currently, SurEau and Castanea models only use mostly shortwave radiation, and longwave radiation which is used to compute a linearised energy balance of the leaves is a*

*simple function of forced air-temperature. In future developments of the two models, it is planned to close the energy balance. In which case, it will be important to introduce longwave radiation. However this would require an accurate representation of the spatial distribution of the vegetation which is beyond the scope of our study and will be the subject of further studies. We have specified this in a new paragraph included at the end of the discussion.*

- L. 351: It seems that only solar radiation is downscaled. To what extent the downscaled radiation is consistent with the other atmospheric variables in their original low resolution? What is the impact of not accounting for longwave radiation in the calculations?

→ *Thank you for this very interesting insight. Indeed, other atmospheric variables such as temperature and precipitation would very much deserve to be downscaled along with radiation. Unfortunately, albeit these variables are impacted by topography, there is no simple process based approach to link these variables to topography. The adiabatic gradient that allows to link temperature and elevation neglect the inversion atmospheric decoupling which can be responsible for gradient inversion in complex terrain and deep valleys. As far as precipitation is concerned, Kriging techniques seem currently to offer the most interesting results. Specifically, longwawes as mentioned before are not used by the SurEau or CASTANEA models presented here. We have now developed further these topics in the discussion (last paragraph).*

- L. 367 (Whm2): Do you mean W m-2 ?

→ *Yes. That was a mistake. The flux radiation results presented are all in Watt per square meter. So W.m$^{-2}$. We have corrected this.*

- L. 391: Figure 3 contains a lot of information and the quality of this Figure needs to be improved. Symbols in Figs 3a-b are too small. I suggest splitting this Figure into two Figures, one for a-b, one for c. The scores and regression coefficients given in Figs 3a-b are difficult to read. Please create a new Table to properly present these values. What are the units for RMSE, in all subfigures?

→ As proposed, Figures 3.a-b and 3 have been separated, and a new Table 2 has been created and the units clarified. Finally, Figures 3.a.b. have been re-edited to make them more readable.

**Editorial comments:**

- L. 126: do you mean “further ensures”?
- Equations 2,3,6 are corrupted (?? and square symbols).
- L. 280: replace “dottle” by “dotted”.
- L. 391: Figure 3 contains a lot of information and the quality of this Figure needs to be improved. Symbols in Figs 3a-b are too small. I suggest splitting this Figure into two Figures, one for a-b, one for c. The scores and regression coefficients given in Figs 3a-b are difficult to read. Please create a new Table to properly present these values. What are the units for RMSE, in all subfigures?
- L. 423: Y axis should be the same in b and d. Figure labelling is not consistent: change a-c-b-d to a-b-c-d.
- L. 448: Figure 5: why using a new unit (MJ m-2) for radiation? This is confusing.
- L. 511: I would replace “greater” bu “coarser”.

→ *All editorial comments have been taken into account as proposed. Thank you again for your feedback. The equations were indeed strangely corrupted, the Figure 3 and 4 are now improved. We have only retained the MJ.m-2 values for Figure 5, as it is no longer a question of flux, but of a daily accumulation of radiation. We have made this clear in the legend.*

- **RC2**: 'Comment on egusphere-2024-1800', Anonymous Referee #2, 18 Sep 2024

The study applies downscaling and representation of various 3D effects in the mountains to Era5 radiation data for use in vegetation studies.
This is a valuable novel tool that combined existing methods. The conclusions are supported by the data.
However, the representation in text and figures could be clarified, and some imprecisions improved (see detailed comments below).
Furthermore, these 3D effects are also relevant for other contexts, such as weather and climate models (including the IFS model used to generate the Era5 data), and also for longwave radiation. It would be good to discuss these connections, how the current work differs from similar work in those contexts, and plans to deal with longwave radiation.

→ *Thank you for this review and for the precise and constructive comments that helped improve this manuscript. We have taken the various comments into account. In particular, we have re-edited figures 1, 3, 4, 5 and 6 to make them more readable, added a table, and rewritten the entire discussion. In particular, we have added to the discussion elements on downscaling other atmospheric variables, and proposals for downscaling longwave radiation. Finally, you will find detailed responses to each of your comments below.*

l.52: replace ¨biassed¨ by ¨biased

→ *Thank you for your comment, which has been taken into account.*

l.58: replace ¨though¨ by ¨through¨

→ *Thank you for your comment, which has been taken into account.*

l.68 and throughout: replace ¨leaf-temperature¨ by ¨leaf temperature¨

→ *Thank you for your comment, which has been taken into account.*

l.69 and throughout: replace water-status¨ by "water status"

→ *Thank you for your comment, which has been taken into account.*

l.77 and throughout: what are niche models? DO they model plant distribution?

→ *In order to concisely explain what niche models are, we have added the following: "[*niche models] (that predict the distribution of plants as a function of environmental variables)*"*

l.82-90: these effects have also been discussed and parametrised for numerical weather and climate models, with somewhat different terminology. See e.g. Buzzi, M., (2008). Challenges in operational numerical weather prediction at high resolution in complex terrain, PhD thesis,

ETH Zürich; A. V. Senkova, L. Rontu & H. Savijärvi (2007) Parametrization of orographic effects on surface radiation in HIRLAM, Tellus A: Dynamic Meteorology and Oceanography, 59:3, 279-291, DOI: 10.1111/j.1600-0870.2007.00235.x and citing literature. A comparison of the terminology and methods would be useful.

→ *With regard to terminology, we have reviewed the terms used. For the main ones, we have preferred to retain the term 'topography', although 'topography' and 'orography' are both used in articles on downscaling; we have retained the term 'aspect', which is more widely used than 'orientation'. On the other hand, the term 'skyview' is often used for what we have called the 'bowl effect', which we have therefore replaced. We have also enriched the model comparisons in the introduction, in particular with the approach of Müller & Scherer (2005) and the articles proposed, which are very relevant: "Shading and the skyview were taken into account at a later stage, in particular in the radiation parameterization scheme (Müller & Scherer, 2005) and in several of its applications (e.g. Senkova et al., 2007; Buzzi, 2008)". We have also compared the results and areas of application in the discussion.*

l.89: what does anisotropically (at 360\deg ) mean? Do you mean all directions equally? That would be isotropic.

→ *There was indeed an error, thank you for identifying it. We have corrected it as suggested.*

l.89: what does luminosity mean here? Is is equivalent to radiation or radiative flux?

→ *The term used was not clear. The term "radiation" was therefore used instead.*

l.90: The ¨bowl effect"- or skyview factor in the literature also occurs for longwave / thermal radiation, which also has an impact on temperature, humidity and plants. Does your method work in the longwave as well? Please discuss how you handle (or plan to handle) longwave radiation.

→ *As the article only focuses on shortwave radiation, we haven't directly tested their operation on longwave radiation. This is because the models (SurEau and CASTANEA) we have used don't need longwave to operate. Nevertheless, a last paragraph is now added on the subject in the discussion, focused on the perspective of downscaling the various atmospheric variables and the need for consistency between them. In addition, to be consistent with the literature, we now use the term 'skyview' instead of 'bowl effect'.*

l.124 and throughout: replace ¨relies on¨ by ¨can be applied to" when discussing the radiation input data

→ *Thank you for your comment, which has been taken into account.*

l.127: replace "on the" by "on"

→ *Thank you for your comment, which has been taken into account.*

l.131 what is the English name of Fagus sylvatica?

→ *We have added the common noun ("European beech") to the text, for greater clarity.*

Fig 1, lower left corner: instead of ´radiations´ radiation fluxes (in each direction)?

→ *Thanks. We have reedited Fig. 1 following your comment.*

l.176 and throughout: replace $J.m^{-2}.s^{-1}$ by $W\ m^{-2}$

→ *As a suggested simplification, we have replaced "*$J.m^{-2}.s^{-1}$*" with "*$W.m^{-2}$*".*

l.179: What is the source for these equations? And for the Fletcher method?

→ *Two references have been added to describe these three equations: "Spitters et al., 1986; Widén & Munkhammar, 2019". In addition, we have removed the designation "Fletcher method" because it seems that the attribution is not systematic and not referenced (and absent in the references indicated).*

l.180: 1370 $W\ m^{-2}$ is outdated for the solar ¨constant" (partially because it is not constant over time). The current value is 1361 $W\ m^{-2}$ (e.g. Coddington, O., J. L. Lean, P. Pilewskie, M. Snow, and D. Lindholm, 2016: A Solar Irradiance Climate Data Record. Bull. Amer. Meteor. Soc., 97, 1265–1282, https://doi.org/10.1175/BAMS-D-14-00265.1.) What is your source?

→ *Thanks. Our source is I.E.A. (1978): I.E.A. (International Energy Agency): An Introduction to Meteorological Measurements and Data. Handling for Solar Energy Applications, Handbook Int. Energy Agency, Dept. Energy U.S.A., Washington D.C, 1978. Probably outdated, as you have noted. So we have applied the new proposed value, changed the reference and redone the calculations for Figures 3 and 4. Note that this 0.66% difference does not lead to any discernible change in the results presented.*

l.185: accumulation or average over the time step? Both need the sine integration, but the variables / units are different: flux in $W\ m^{-2}$ vs energy in $J\ m^{-2}$.

→*Thank you for pointing out this important issue. In our case, as we're computing the value of* $R_0$*, which is a flux (*$W\ m^{-2}$*), it's an average. We have corrected that. Although, sin(beta) has no unit.*

l.187 / Eq.2: some strange symbols appear in this equation

→ *Thank you for identifying this technical problem. For equations 2 (and 3) these symbols appeared when generating the pdf. The problem has been corrected.*

l.198 / Eq.4: Is this related to reversing the Delta-Eddington scaling done in general circulation models to include diffuse radiation with angles close to the direct radiation in the direct calculation? What exactly is your definition of the circumsolar component?

→ *No, not exactly. But your comment implies that we weren't precise enough in describing this stage. So we have added: "Under clear skies, diffuse irradiance is anisotropic, due to the presence of aerosols in the atmosphere, and the intensity is therefore higher in the direction of the sun. It is thus necessary to attribute the excess diffuse irradiance observed near the direction of global radiation to direct radiation."*

l.200: What are intermediate sky conditions?

→ *The* $R_{circum}$ *is presented in equation 4 for clear sky and is null for fully cloud-covered sky. Between the two, we use the interpolation presented in equation 5. For greater clarity in the manuscript, we have specified "clear to cloudy skies".*

l.203 replace ¨both¨ by "the sum of" (else, R_g could also contain other terms)

→ *Thank you for your comment, which has been taken into account.*

l.212-218: Considerung that not that many people use R nowadays, do you have any idea if there are equivalent python functions?

→ *Indeed, J.G. Corripio -developer of the package in R- recently developed a python equivalent of the insolation package. At the end of the description, the text now has two links enabling you to find them directly: "Note that the same package is now available for python (on https://pypi.org/project/insolation/ and https://www.meteoexploration.com/insol/)."*

l.226 / Eq.6: again, strange symbols

→ *Thank you for identifying this technical problem. The missing part has been identified and the problem has been corrected.*

l.233: replace "the 360\deg horizontal surrounding topography" by "the surrounding topography in all 360\deg horizontal directions" or similar

→ *Thank you for your comment, which has been taken into account.*

l.238: Why the minimum?

→ *Based on the assumption that mountain topography essentially has slopes between 0° and +90° (no or few overhangs), and in order to save calculation time, when, starting from the horizon and with a step of 2°, there is no longer any shadow due to the topography, we consider that there is no shadow effect coming from a higher point.*

l.275-277: What are the English names of these plants?

→ *Thanks for the comment and sorry for the omission. The common names of the different species have been added: "downy oak (Quercus pubescens), evergreen oak (Quercus ilex), Aleppo pine (Pinus halepensis) [...] and Atlas cedar (Cedrus atlantica)"*

Fig.2: WHy did you choose the Era5-Tile with stations 3 and 2 instead of the one to the left with stations 4,5,6,7?

→ *That's a good question. The aim was to be consistent with the points studied and to have spatial heterogeneity in the topography. The tile located at the 'center of gravity' of the points studied seemed the best choice. Note that there is no direct link between the results from the points and those from the tile.*

l.284: not all stations are on the north face: station2 is on the ridge, station 1 on the south face (and going by the aspect(=azimuth?) angles in Table 1, stations 1,2,3 and 7 are all locally facing south

→ *Thank you for your comment. It is indeed a mistake and contradicts with the manuscript, such as table 1 as well as the text linked to figure 3 (e.g. line 397). We have corrected this.*

l.291: where the forest edges extending from the station to 30 m away, or where they at a distance of 30 m from the station?

→ *Thank you for pointing out the ambiguity of the sentence. 'Beyond' has been replaced by: " [forest edges extending] at a distance minimum of [30 m]".*

Table 1: is apect = azimuth?

→ *Yes. Both terms can be used. For greater precision, we have therefore indicated the correspondence in the text (line 85) and in the legend of Table 1.*

l.315 and throughout: please decide between CASTANEA and Castanea

→ *Thanks for that. The official nomenclature was applied: CASTANEA*

l.332-343: this description consist mostly of very technical expressions and is very hard to understand. Could you try to simplify some formulations? E.g. what does xylem embolism mean?

→ *Thank you for your thorough review. We've rewritten the entire paragraph to make it more accessible: "SurEau is a plant-hydraulic model that is dedicated to simulate the risk of drought-induced hydraulic failure due to xylem embolism, a leading mechanism of plant mortality under drought (Cochard et al 2021; Ruffault et al 2022). The model simulates water fluxes and water potential along the soil-plant hydraulic continuum at a half hourly time step and considers leaf stomata and its regulation, and cuticular transpiration plant organ. The model is parameterized with various measurable plant traits previously collected for the target species (Ruffault et al., 2022). In this study, drought-induced risk of hydraulic failure (or the percentage loss of hydraulic conductance) in the vascular system was used as a proxy for hydraulic risk during a given summer."*

l.379: replace ¨relationship" by ¨correlation¨

→ *Thank you for your comment, which has been taken into account.*

l.284: What does is stable in spring but high" mean? The correlation is high but does not improve with downscaling?

→ *Thank you, the wording was not very clear, but you got the idea. The text has therefore been modified as follows: "the correlation was stable and high (0.85) in spring but did not improve with downscaling"*

l.390: What does ¨show similar limited bias" mean?

→ *Thanks for your comment. We have been more specific with "[show] low and not significant [bias]"*

l.385: what do you mean by cotrary to Fig.3a)? In contrast? Please specify what that means: the uncorrected and corrected data show different behaviour

→ *Thank you for your comment. We have reworded the sentence as suggested: "Further analysis also revealed that the uncorrected (Fig. 3 a) and corrected (Fig. 3 b) seasonal data showed different behavior and so the equations of the seasonal curves for corrected ERA5-Land radiation closely aligned with the 1/1 line, in accordance with an important decrease in RMSE."*

Fig 3 a/b: These figures are very busy, with symbols and legends very small and hard to read. Please make them clearer, e.g. by removimg the various fit and correlation coefficients to a table and making lines and symbole thicker / larger

→ *Figures 3.a and 3.b have been simplified and separated from figure 3.c to improve readability. Table 2 has been created to contain the various fit and correlation coefficients of the figures, and the symbols, legends and axes have been enlarged.*

l.402: Where 13, 14 January cloudy or clear? Could you add a cloud fraction timeline (from observations, or Era5 if no observations are available)?

→ *The cloud fraction is not available in our observation and not in ERA5-land either. However, the radiation data from ERA5-Land is mainly affected by the presence of clouds*

*and aerosols (https://cds.climate.copernicus.eu/datasets/reanalysis-era5-land?tab=overview). The deviations from the sinusoidal standard curve (daytime only) of ERA5-Land are mainly explained by the presence of clouds on an hourly scale. ERA5-Land therefore only appears to represent the presence of clouds on 20 August 2016 (the date shown in Figure 4). On the other hand, ERA5-Land can easily fail to account for the heterogeneity of the cloud layer within a tile. This is indeed (see next comment) what explains the dip visible on 19 August 2016 at around 10am. Especially as the mark is present on both sites (but with a different intensity). To take this further, we have added a supplementary Figure S1, the equivalent of Figure 4, but with the addition of data from the Copernicus Atmosphere Monitoring Service (CAMS) solar radiation time-series (available online: https://ads.atmosphere.copernicus.eu/datasets/cams-solar-radiation-timeseries), with clear-sky and 'all sky' (with clouds) global radiation. We can clearly see that on 19 August there is a drop-out between the clear-sky and all sky products, supporting the hypothesis of the presence of clouds. On the other hand, we do not observe such a point drop-out in winter. This subject is discussed in more detail in the discussions, and the precise indication has been added to the text: "*Two three-day periods were selected for analysis: one in summer (19-21 August 2016) to observe the impact during peak sun exposure (on the 21st), a cloudy day (the 20), and an intermediate day (the 19); and another in winter (12 to 14 January 2017, cloudless days) [...]. The presence of clouds was assessed with data combining high-resolution cloud information is directly inferred from satellite observations, such as the Copernicus Atmosphere Monitoring Service (CAMS) solar radiation time-series data (available on https://ads.atmosphere.copernicus.eu/stac-browser/collections/cams-solar-radiation-timeseries, last access the 22/10/2024), and are represented on Fig. S1. The difference between sky-view and all sky radiation indicates the presence of clouds.*"*

l.415: Considering that the dip at 10 am is visible at both sites 1 and 5 (and generally, the timeline is qualitatively quite similar), I would assume that this might be the effect of clouds. Here, a cloud fraction timeline for comparison would help.

→ *Refer to previous answer. This point is discussed in section 4.1 with the new Figure S1 and an indication is added in the result section: "[possibly indicating a shadow] or the presence of localized clouds or fog".*

l.421: replace "on the contrary" by "by contrast"

→ *Thank you for your comment, which has been taken into account.*

l.421: replace "cloud-free day" by "cloud-free days". What does by two mean? By a factor of two?

→ *Thank you for your comment. Yes, it meant by a factor of two. We have corrected it.*

Fig 4b: Do you have an idea why the observations on January 14 are higher than the Era5 data? Is this uncertainty in clouds and therefore direct/diffuse partition?

→ *Indeed, it is plausible that there is a bias in the cloud cover or aerosol concentration in ERA5-Land impacting global radiation, and that this could lead to uncertainties in the direct/diffuse partition. Unfortunately, we have no evidence to support*

*these hypotheses. Note that in any case, The values remain within the same order of magnitude as those derived from CAMS data (see above and Fig. S1).*

l.439: remove "However¨, replace "mirrors the magnitude"by "is similar in magnitude"

→ *Thank you for your comment, which has been taken into account.*

l.443: Here, you present radiative energy (in J $m^{-2}$), i.e. you have integrated fluxes over a time span. What is the time span for this integration?

→ *Thank you for pointing out the inaccuracy. The time span is one day, which we have now specified in the text.*

l.454: replace "functioning" by "function"

→ *Thank you for your comment, which has been taken into account.*

l.461-462: What does the standard deviation introduced between the values mean? Which two outputs?

→ *The range between two values includes the values of all three different downscaling operations. This clarification was made as follows: " With the three different downscaling (from 8 km to 500 m, 250 m and 30 m), [...]"*

l.472: does mortality occur in all cases at 50% risk of hydraulic failure? Or in 50% of cases?

→ *When the threshold is reached, mortality occurs in all cases. To avoid misinterpretation, the sentence has been changed: "Setting at 50% the risk of hydraulic failure threshold at which trees die"*

l.475: What is a useful reserve?

→ *Thanks. This irrelevant term is now replaced by: "total soil available water accessible for the trees"*

l.491: and considering cloud uncertainty. Replace "example" by "days"

→ *Thank you for your suggestion to improve the syntax. However, as the discussion has been completely reworded, this sentence no longer appears.*

l.492: but actually, the radiation timeline looks similar, so the cloud fraction timeline might be similar, too

→ *We respectfully disagree with this comment. According to your comment on l.415 and the 'dip' on 19 August at 10am observed in summer (Fig. 4 c), there is a significant difference between the radiation timeline measured at the sites and that from ERA5-Land. This may be due, among other things, to a localized presence of clouds or fog, which would be present in an heterogeneous way at the two sites presented. We explore this possibility in more detail later in the discussion, justifying this hypothesis with CAMS data (Fig. S1: "For instance, in Figure 5.b, the dip around 10 a.m. the 19 August may suggest the presence of microclimatic conditions, such as clouds or fog, an effect not considered in our downscaling method. Actually, the original ERA5-Land data cannot depicts the presence of isolated clouds as it happens on the day presented for summer in Figure 5 as they provide averaged values of incoming radiation over the whole mesh area. Such occurrences could be tracked by using higher resolution solar radiation products such as those obtained from satellite*

*imagery and in particular geostationary satellites with a spatial resolution in the order of 2 to 3 km and a time resolution between 5 and 15 minutes (ex. Roerink et al. 2012, Bojanowski et al. 2014). Indeed, this dip may be associated with the presence of small clouds or fog capping Mont Ventoux during morning, signalled by the drop-out between the CAMS clear-sky and all sky (Fig. S1). Similarly, the small dip observed shortly after in Figure 5.a is actually related to the presence of small clouds of fog capping Mont Ventoux during morning, moving from one site to another.“”*

l.493: Remove "However"

→ *Thank you for your comment, which has been taken into account.*

l.496: is compensated: only on the large scale

→ *Of course. We have included this clarification in the text.*

l.502: there is also an impact on mountaintops: here, the 3D effects treated here INCREASE radiation, which will also be more pronounced with downsaling

→ *Your comment indicates that we haven't been explicit enough on this subject. The following text has therefore been added, supporting this assertion: “Due to energy equilibrium and conservation at large scale, this implies that an increase in radiation is observed on south-facing slopes or on mountaintops. This effect is particularly pronounced when the angle between the incoming direct radiation and the aspect of the relief (slope and azimuth) approaches perpendicularity relative to a flat surface.”*

l.506/507: cloud cover variability is not only explained by climatic heterogeneity (what exactly does that mean?), but also tha fact that clouds on a given day are both temporally and spatially highly variable

→ *In this study, we work with radiation data, including cloud cover, and hourly data. But indeed, climatic variability -spatial or temporal- is particularly important both for clouds (and therefore variation in radiation) and because of topographical heterogeneity. We have adapted the text accordingly: “This approach cannot capture the spatial and temporal heterogeneity of cloud cover, which can be especially significant in mountainous regions (Buzzi, 2008). This explains why, while daily patterns were effectively estimated, sub-daily variations were more difficult to capture.”*

l.508/509: as discussed before, this occurs at both sites, so might be due to clouds that fly over both sites

→ *Yes. The text now is explicit on this point. For more details, see comment about l.492.*

l.511/Fig.3: there is a clear improvement compared to the original, it just does not continuously improve between 500m and 250 m.

→ *We preferred to remain cautious about our conclusions. In line with your comments, we have been more direct: “Our analysis showed a clear but non-uniform improvement in radiation estimates as the resolution of our downscaling method increased. While no continuous improvement was observed at resolutions coarser than 250 m, a gradual improvement emerged for finer resolutions, down to 30 m.”*

l.515: What does introduces some variance mean here?

→ *This means that the signal has been degraded due to the inherent uncertainty of*

*the method and additional processing of the variable (in this case radiation). This clarification has been incorporated into the text.*

l.518: replace "with" by "together with"

→ *Thank you for your comment. However, as the discussion has been completely reworded, this sentence no longer appears.*

l.524: Due to slope angle / your hillshading, not original sun intensity, correct?

→ *Exactly. We have added this explanation: "This effect was particularly pronounced when the angle between the incoming direct radiation and the aspect of the relief (slope and azimuth) approaches perpendicularity relative to a flat surface."*

l.527: replace "in my mind" by "in mind"

→ *Thank you for your comment. However, as the discussion has been completely reworded, this sentence no longer appears.*

l.530: replace "threshold" by "thresholds"

→ *Thank you for your comment, which has been taken into account.*

l.545: replace "downscaled radiation is improved" by "Agreement with observations is improved for downscaled radiation"...

→ *Thank you for your comment, which has been taken into account.*

l.547: Again, there is improvement compared to the original already at coarser resolutions (e.g. 500m), it just does not increase monotonically

→ *As before, we modified the text: "Agreement with observations was improved for downscaled radiation compared to original ERA5-Land data, especially during winter months, due to the higher zenithal angle. This improvement was particularly significant and increased gradually after a certain spatial resolution (~ 150 m)."*

l.550: replace "on those" by "on those variables"

→ *Thank you for your comment, which has been taken into account.*

l.556: replace ¨relies on¨ by ¨can be applied to"

→ *Thank you for your comment, which has been taken into account.*

l.558: How do you propose to apply this method to other types of climate data?

→ *This is not the direct object of this study. Nevertheless, we have added a paragraph (the last one) to the discussion to address the issue of other climate variables and downscaling.*

Throughout: the placing of commata is worth checking

→ *Thanks. We have proofread the entire manuscript to check the placing of commas.*