

Anonymous Referee #2

Thank you very much for the positive feedback and all your relevant remarks and suggestions. We will try to answer all your questions in this document. The line numbers in our answers correspond to the numbers in the new corrected version.

Abstract: While SWD is defined on the first line, it might be helpful for clarity to mention somewhere in the abstract body how the sign of the bias is defined (e.g. where a positive bias is first mentioned, one might add “i.e. too much shortwave radiation reaching the ground” or similar).

Yes, we modified the sentence: “The 2020 bias is positive, with a value of 18 W m^2 , meaning that AROME overestimates the SWD. The root-mean-square-error [...]”.

L90: Single sites are not necessarily unrepresentative, particularly if long time series are considered that cover a variety of cloud regimes. I find the wording a bit strong here.

Yes, we agree. We modified the sentence: “Hence, it should not be limited to a single supersite, because such a site could be unrepresentative of the whole domain. The method should also allow to go...”

L137: I find the reference to Table 1 here a bit confusing. The text talks about 15 cloud types, yet the table lists only 6. The merging of the cloud types is discussed later in the text. Maybe instead of referencing Table1 in line 137, the text should refer the reader to section 2.2.3 where the table is explained (and first reference the table in that section).

We modified the text as follows L134: “The cloud type is a classification among 15 different classes: 4 corresponding to cloud free scenes and 11 corresponding to clouds scenes (see Section 2.2.3).”

L145: What is the sensitivity to size of neighbourhood, or choosing the nearest-neighbour instead of using a neighbourhood approach?

The Anonymous Referee 1 also asked this question. We invite you to read our response to this reviewer.

L188: I don't really see a more pronounced seasonal cycle. Does that statement apply to the low cloud category only, or to all cloud? If it applies to only low clouds, it is not easy to make out the seasonal cycle of CR0+CR1+CR4 in Fig. 3 since the categories are not grouped together. Please clarify.

We agree that the comparison of the seasonal cycle of both figures is not easy. Hence we modified the text to focus only on the seasonal cycles of cloud types, where fractional clouds (FC) and low clouds (LC) exhibit quite clear seasonal cycles. In contrast low clouds in the model do not show such a seasonal cycle (see figure below where we put side by side CR0, CR1, CR4).

L188: “the relative frequency of observed fractional clouds is higher in summer than in winter, while low clouds are overall less frequent in spring and summer, highlighting seasonal cycles in the observations that do not have obvious equivalents in the model.”.

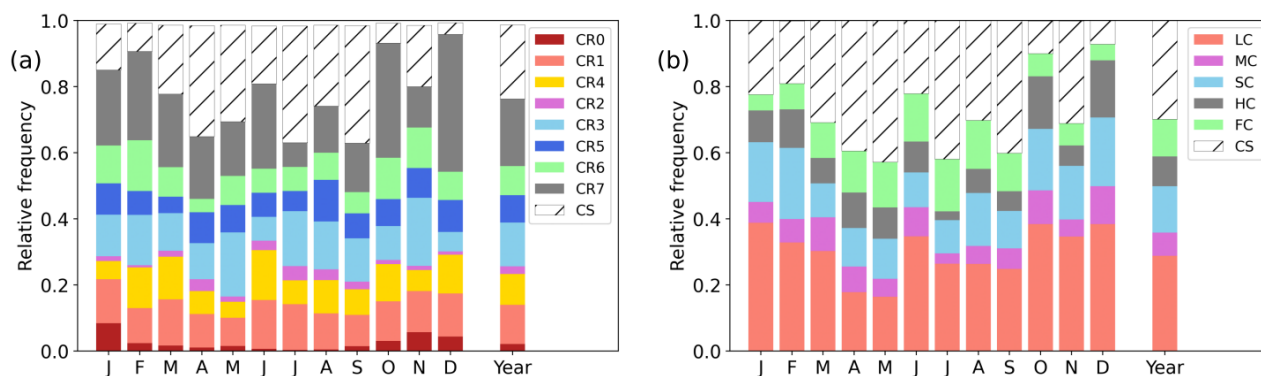


Figure 1 : Monthly relative frequency for each (a) cloud regime in AROME and clear skies (CS), (b) cloud type in the satellite images and clear skies (CS), over 2020 for the pixels including the piranometers.

L208: I'm not that familiar with various configurations of AROME. How does HARMONIE-AROME differ from the AROME version presented in the manuscript? Maybe a half-sentence would be useful here, e.g. stating

that HARMONIE-AROME uses the same microphysics and radiation schemes (if that is the case), to indicate that the results from the cited study apply.

We changed the sentence and added indications: “Interestingly the CSI distributions are similar to those reported by Nielsen et al. (2018) with HARMONIE-AROME (Bengtsson et al., 2017). Although these models share the same code, the operational configurations rely on different sets of parameterizations.”

Fig 12 does not add much to the discussion. I think it (and the few sentences discussing it) could probably be left out.

We agree that this figure is partly redundant with the previous ones. However, we think that it shows an important aspect we would like to insist on, which is the contributions of both positive and negative errors for all cloud regimes. This cannot be seen on the other figures and is now more clearly highlighted in the text :

L384: **“To further investigate the SDE associated to each cloud regime, Fig.12 shows the distribution of SWD errors for all AROME cloud regimes. For each CR the SDE is high compared to the mean flux, in particular for CR2, CR4, CR5 and CR6. Interestingly, it shows that for all cloud regimes the mean biases result from both positive and negative contributions, indicating that multiple sources of errors are involved.** The same is obtained for the distribution of SWD errors for observed cloud type in the satellite classification (not shown). **This suggests that improving the mean bias of individual cloud regimes would not necessarily imply much better forecasts. It also implies that more detailed observations are needed to better understand these errors and their sources.”**

Section 4.1.2: I found this section most difficult to follow, and also somewhat repetitive. It seems to mainly confirm conclusions that were already drawn previously from analyzing SWD and SD in cloud classes/types. That does not really surprise me, since I would expect the informational content found in SWD, SD and clear sky index (extensively discussed in sections 3.3) to be the same as found in the transmittance and its standard deviation.

E.g. L185: **“some optically thin clouds are not detected by NWC SAF product, which is a known caveat of passive sensors”** – L418: **“clouds may be present but not detected”**

Similarly, we have already seen that the contribution to the SWD bias from false alarm and missed cases is relatively small (corresponding to the conclusions on Line 430 and 434).

I would like this section to be more concise (what new information does the transmittance perspective contribute, that hasn't been seen the section 3.3 previously?), or maybe some of the additional information could be wrapped in with the discussions throughout section 3.3, eliminating 4.1.2.

We agree. For the readability of the article, we decided to remove Fig.13 and the text referring to it. As you point out, it seems clear that some clouds are undetected by satellite product. We kept section 4.1.2 (although much shortened) and the paragraph related to the impact of this non-detection of clouds on our results and to a possible solution of this issue. In order to keep the information about the fact that missed clouds and undetected simulated clouds have on average less impact on the SWD, we changed the text as follow:

We added at L297 : **“Note that the annual mean SWD in the model for the false alarm cases (436 W m^2) is much higher than the annual mean SWD in the model for the hit cases (296 W m^2), suggesting that undetected simulated clouds have on average less impact on the SWD than actually observed clouds.”**

We added at L321: **“Note that the annual mean SWD in the observations for miss cases (409 W m^2) is much higher than for hit cases (272 W m^2), suggesting that the clouds missed by the model have, on average, a small impact on the observed SWD.”**

We added L422: **“In addition, the temporal variability of SWD under clear sky is much larger in the observations than in the model (not shown).”**

L498: I am not sure how relevant lacking supercooled liquid is likely to be over France. The greatest impact on radiation is found in cold regions where models erroneously produce ice-only clouds. I'd expect most LC in France to contain liquid water under most conditions, except maybe for some winter conditions. Nevertheless AROME is used elsewhere, and the Scandinavian countries could certainly benefit from an improved representation of supercooled liquid.

Underestimation of supercooled liquid occurs also over France and causes problems as wind turbine blade and aircraft icing. Studies are carried out at Météo-France on this topic. This underestimation is mainly related to mid-altitude clouds in France, and not much to low clouds as in cold regions. We can't say to what extent this underestimation has a strong radiative impact, but it certainly exists.

Technical comments:

L25: should this link be in the text, or in a reference? Not sure what the journal's style guide suggests, but a long link in the text disrupts the flow a bit.

We agree. We used footnotes for the two links in the article for a better readability.

L38/39: wording: "optimal reserves optimizing storage."

Corrected in "optimizing storage".

L49: typo: short-terme, should be term, also: satellite should not be capitalised

Corrected.

L90: typo: word "not" is used twice

Corrected.

L218: Wording suggestion: Use "In contrast, "instead of "on the contrary" (applies to several places throughout the text)

Corrected.

L249: wording: "local and punctual" – suggestion: just use "localised". "punctual" means "being on time". Or if you are referring to time, then maybe "temporary" or "short-lived" might be better.

Corrected.

L294: The year is missing in reference Antoine et al. – looks like it is not published yet. In this case, maybe there should be a further comment behind the name, e.g. "in preprint", "under review" or similar (not sure what the journal prefers).

Indeed, this article was not yet published, but now it is. Corrected.

Fig 11: Labels of the cloud type (CRx) in each panel would make it easier to follow the discussion in the text

Yes, we changed the figure.

L393: remove parentheses around reference to Lucas-Picher et al.

Corrected.

L428: put Ackermann reference in parentheses

Corrected.

L520 and following: sentences are repeated

Corrected.

L542: Should the units be Gb, not Go?

Yes, corrected.