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Comment on acp-2021-5

Anonymous Referee #1

Referee comment on "Understanding the model representation of clouds based on visible and infrared satellite observations" by Stefan Geiss et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-5-RC1, 2021

Geiss et al. "Understanding the model representation of clouds based on visible and infrared satellite observations"

The paper discusses biases in the representation of clouds in convection-permitting simulations with the ICON-D2 model. The authors use a combination of visible satellite reflectances and infrared brightness temperatures to derive model shortcomings. Using satellite forward operators, observation equivalents are computed from model data which allow for a direct comparison with observations. The authors contrast uncertainties in the visible forward operator to sensitivities in model parameter setting. Based on their analysis result, the authors emphasize that the assumptions on subgrid-scale water clouds are the primary source for model biases in the visible spectrum and that the representation of these clouds need to be carefully revised to make further improvement possible.

I think the present study will become a valuable resource and I recommend the publication of the manuscript in ACP after major revision.

General Remarks
The paper is in general well written and structured. The objectives are clearly stated and all arguments are well supported. I don't see that language usage is of any concern. The manuscript discusses a relevant topic in atmospheric research, advanced analysis techniques are applied and the resulting scientific outcome is of interest for a wider audience.

General Comments
* Relationship to ISCCP-style analysis?: Combining VIS and IR as joint histograms or PDFs is not a new technique. There are a lot of different examples in literature which use joint histograms of cloud-optical thickness and cloud-top height to assess quality of climate

model, global and regional weather forecasts. Two respective examples are: Zhang, M. H., et al. (2005), Comparing clouds and their seasonal variations in 10 atmospheric general circulation models with satellite measurements, J. Geophys. Res., 110, D15S02, doi:10.1029/2004JD005021. & Otkin, J. A., & Greenwald, T. J. (2008). Comparison of WRF model-simulated and MODIS-derived cloud data. Monthly Weather Review, 136(6), 1957-1970. and much more references therein and also based on these papers. It feels like you had completely overlooked this branch of studies and their relationship to your research. Please add a comprehensive discussion on this topic in your introduction and in your results section (where it is appropriate).

* Connection to solar power prediction: To my opinion, the analysis that tries to establish a connection between satellite data and global irradiance measured at surface is the weakest part of your manuscript. I guess you try to make the argument that solar power prediction would improve if the representation of clouds (measure from space) is becoming more realistic. However, your analysis and the presented arguments do not support such a conclusion by now. I recommend you to revise the analysis in Sect. 3.3. It would be beneficial for the reader to show how biases in GHI are correlated with the biases in VIS and IR108. One would expect that lower GHI biases coincide with lower VIS biases which would support the conclusion that the use of visible satellite data is beneficial for ground-based irradiance predictions.

* Figure Quality: Please make sure that font sizes in your figures (e.g. axis labels, legends) are sufficiently large. Text in figures should not be significantly smaller than the text in the figure caption. Please update your figures accordingly!

Detailed Comments

L. 11: "modified ...settings": Please rephrase to make more clear that both, variations in model settings and forward operator uncertainties, have been considered.

L. 16: "VIS solar reflectance and global horizontal irradiance": Please make clear that the former in measured at TOA and the latter at surface.

L. 17: "will coincide" -> "can enable"?

L. 35: "are usually ... smaller" - Please support this statement with references!

L. 45: "minimization" -> reduction

L. 46/47: "Unfortunately, ..." Statement is very general and for sure not true for all current NWP systems. Please make it more specific and supported by references!

L. 51: "meteorological sensitive areas": Unclear what this means.

Fig. 1: Does not appear to be referenced in the right order. Labels are too small.

L. 58: "solar irradiance fluctuation" + "at surface" (or at ground). Also this statement needs to be support by a reference.

L. 65 full paragraph: This needs like a conclusion paragraph and is not in the right place here. Please rephrase and complete paragraph. This is the place where you can state your research question and outline how approach your research goal.

L. 78: "cloud climatology": Here, and everywhere else: Please avoid the term cloud climatology which is mis-leading because it refers to long-term (!) cloud statistics which is not the case in your study. Please use "time-mean statistics" (or "time-average") instead.

L. 86: "ICON-D2". Please name the model version here.

L. 96: "We performed six" + "additional"

L. 98: "The objective was to ..." Please rephrase sentence.

eq. (1): needs more explanation! I guess this is only a partial contribution to total cloud cover (might be indicated by subscript cc_turb). Is this cc contribution just added to the other contributions? What is q_sat? And where does the scheme come from (reference) and how should it be interpreted? Parameter B needs to be explained as well.

L. 125: "like the operational one." -> "like the operational one-moment scheme."

L. 129: "cloud-concentration number" -> cloud droplet number concentration"

L: 135: 2*10^4 to 4*10^4 hPa: This is definitely too large! Wrong units?

L. 146: "visible 0.6 um channel" Please specify if the visible reflectance is corrected by solar zenith angle. If yes, comparison in Fig. 8 would be inappropriate because GHI is scaled by a constant.

L. 151: "TCW" / TCI": I would prefer "LWP" and "IWP", liquid-water path & ice-water path is more commonly used.

Section 2.3 misses to tell how aerosol is treated.

Eq. (2):
* Consistency of symbols: You use low-case q in eq. (1) for content. And you use capital R as reflectance later. I suggest to use consistent symbols.
* Is this equation consistently applied to visible and infrared? Please comment on this aspect.
* How does this method compares to the generalized effective diameter in Senf and Deneke (2017), AR, eq. (B.3)?

L. 192/193: This is much too short! SGS clouds play an important role in your analysis. Please be much more explicit about your treatment of SGS clouds. What are the assumptions about microphysics (effective radius, adiabaticity) of SGS clouds? How does this impact cloud-optical thickness?

L. 197: "calibration offset" To my opinion, you are removing a systematic bias from the simulation which is fine in general. However, I would phrase it in that way.

L. 201: "spatial resolution" -> Please move to Sect. 2.2.

Sect. 2.4: What is the accuracy of GHI measurements?

L. 215: "... without coarsening and thinning" -> unclear

L. 220: I don't understand why you don't take the observation as a reference: eps = P(SIM) - P(OBS)?

Violin plots: I would recommend to skip the distribution outside a certain range (<10th and >90th percentiles) to increase readability of the plots in Fig. 12. Otherwise these plots are dominated by the extremes.

L. 224: CFAD -> reference

L. 224ff "Standard atmosphere .... ": Don't understand why you choose this distinction. Much more natural would be <273 K, (273 K... 243 K), <243K which would separate liquid, mixed-phase and ice clouds.

Fig. 4 + 5: Please use same projection as in Fig. 1 or the other way around. Please avoid histograms and use PDFs instead as you introduced PDFs as verifcation metrics.

L. 248: Fig. 4a & 4b -> wrong reference, 4b shows BTs.

Fig. 6 caption: White lines: What do they mean? "normalized by the sum" -> confusing. If you show PDFs then normalization is not a matter of choice: \int P(BT, R) dBT dR = 1!

Fig. 7: Observed BTs are higher than 300 K. Is the range > 300 K considered in the normalization of the PDFs?

Sect 3.2. Again, avoid the term "climatology".

L. 282 / L.284: There is a duplicate statement: "findings from previous studies"; "found in other studies" Please rephrase the two sentences.

Sect 3.3:
* Please see my general comment. What is the general idea of this analysis? I guess you like to show that GHI forecasts can improve when VIS / BT forecast are more realistic, right? Why don't you show the bias in GHI vs. the bias in VIS? Otherwise, the reader get the feeling that plotting hourly average GHI values against instantaneous VIS observations is rather inappropriate (see L. 312 - 14).
* Caption Fig. 8 "number of matches"-> unclear.
* Meaning and usefulness of lines in Fig. 8 is also unclear.
* Is scaling of GHI consistent with scaling of VIS radiances? See above.

L. 335: "imperfect parameterization" Again, a clearer description of the microphysics of SGS clouds would help.

L. 337: flat plateau for grid-scale clouds: Would this mean that this VIS bias can be resolved by proceeding to even higher resolutions, e.g. hecto-scale simulations? Could you comment on this? Are there any indications in the literature?

L. 347: "it seems that the subgrid water cloud parameterisation needs to be improved" -> or its coupling to the VISOP?

L. 353: "missing RT effects" -> unclear

L. 382ff: In this paragraph, it is not clear how you treat aerosols in the reference run.

L. 387: "Aerosol can scatter photons ..." Sentence reads weird. Please rephrase.

Interpretation of Fig. 11: It seems that carefully chosen aerosol can bring simulated VIS006 closest to the observation. Is this conclusion correct?

L. 398: "ice habit is thus not likely to cause large uncertainties..." This is only true because your scenes have a very high low-level cloud cover and semi-transparent cirrus overlays lower clouds, right?

L. 403: "simulation" -> "simulations"

L. 411 & l. 415: "pixels" I find "pixel" inappropriate for model data.

L. 420: "experiment VI" -> do you rather mean VII?

L. 424 / 425: "cloud top height is an important additional parameter" I guess you mean in addition to cloud-optical thickness? Please make this clear!

Fig. 12:
* It is hard to see the differences here. The plot are dominated by the extremes. Please trim the range of the PDFs e.g. within (-2, 2).
* Panel a & b should have the same size.
* y labels should be eps_n,d consistent with Sect. 2.5

L. 450: "to eliminate the errors of the reference run" + "in the IR108 channel"

L: 485: "solar satellite observations are novel for model evaluation". This might be true for RTTOV, but not for other forward operator methods. Please be more specific and discuss, if applicable, already existing advancements by others (e.g. in CRTM).

L. 490: "well-suited to improve model cloud parameterisations for better PW power production forecasts" This statement could be better supported by your analysis. To my feeling, you can show that better VIS / IR108 forecasts ultimately lead to improvements in GHI predictions.