Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Comparison of regional meteorology-chemistry models with satellite cloud products over Europe" *by* Rocío Baró et al.

Anonymous Referee #2

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This paper is an evaluation of six simulations over Europe against satellite observations. It includes three different models with one that has three different combinations of microphysics, radiation and aerosol parameterizations. The evaluated variables are cloud fraction, cloud optical depth, cloud liquid water path and cloud ice water path. The results are interesting since these quantities are an important source of uncertainties in atmospheric models. However, major revisions are needed to clarify the text, tables and figures; and additional information is needed to complete this evaluation, both for the observation and for the model analysis.

General comments:

- Figures are overwhelming: too many panels with poor color-scale choices. It results in too much information in each figure, such that it discourages the reader to look

into the details of the results. To reduce the number of panels, two seasons could be chosen together with the annual mean. Please remove the empty space for the missing simulations and explain it in the text. Remove the subtitles that cannot be read anyway. Choose color-scales (both for the observations and for the simulations) that highlight the important messages you want to show for each variables.

- The text is too long, particularly the introduction. The focus of the paper is not the ARI nor the ACI but the cloud variables that are evaluated. A short paragraph on the context of these simulations (within the AQMEII project) would be sufficient. The rest of the introduction should bring the reader rapidly to the main focus of the paper.

- The abstract should be re-written with the focus of the paper in mind (and a good revision for missing words, wrong terminology, appropriate use of English language).

- There are many basic English errors. The text should be reviewed by a native English-speaker.

- Moreover, there is room for improvements in the writting since many ideas and concepts are confusing. Good revisions would certainly improve the quality of the paper.

- With respect to the different variables evaluated, I would suggest to present CWP, CIP before COD since the two water content variables have a direct impact on the later variable (COD). Moreover, a cloud total water content may be of interest since some models may have a diagnostic separation between ice and liquid water content (even for the satellite retrieval?).

- Please clarify if CWP and CIP are evaluated for in-cloud values or not. If it is for grid point values (not in-cloud), how can you interpret the results for CWP, CIP and COD if a model does not provide CFR?

- For the conclusion, the last two paragraphs seem out of the subject. A more general conclusion is needed (and much shorter). What does it tell us about the use of these models to assess ARI or ACI? One idea per paragraph would help fluidity.

- It seems to be an assessment of models that will be used for aerosols studies, maybe a description of how microphysics schemes are connected to aerosols would be interesting. For example, does the aerosol concentrations are used as CCN? If so, does it have an impact on the evaluated variables?

Specific comments:

-The title is misleading since it suggests a focus on the chemistry/aerosols.

-Be careful of the name of the variables and their abbreviations. Maybe a use of more "traditional" abbreviations would ease the reading of the text. For example: cloud fraction (CF), cloud liquid water path (LWP), cloud ice water path (IWP).

-The Mean absolute error (MAE) does not add any useful information. Standard deviation or RSME would be more useful.

-Table 1 is really not clear: short lines are not aligned together. Instead, repeat the information if necessary and use some highlighting if you want to show the differences between the 3 WRF simulations. Table 3 could be put into a 4 panels figure. This summary of results is important but information is lost with all the numbers.

-Table 4 could also be put into a figure.

Introduction:

-page 3, Lines 30-35, clarify the context of this study, without putting too much details on all the working groups, etc.

-page 4, lines 6-10: this study is not evaluating any of the aerosols effects nor the aerosols-clouds effects. Please rephrase.

Methodology:

-page 4, lines 24-26: This information on a specific model is not at the right place (it is not the general idea of this paragraph). Should be put elsewhere.

-It would be a good place to highlight some differences between the models, for example with respect to their microphysics schemes, to the connection with the aerosols, and to the connection with the radiative transfer scheme.

Results:

-page 7: why not choose the usual seasons (DJF, JJA …)? It could be easier to compare with other studies.

-Figure 1: more information is needed for the CFR between 0 and 1, please change the color-scale.

-Page 7, line11: CFR values are generally higher than 0. Please comment the other values between 0 and 1.

-page 7, line 21: which negative bias?

-Page 7, lines 23-24: More information is needed on the satellite limits of detection, uncertainties, etc. Is this an hypothesis or is it stated in an article?

-The number of days available should be indicated somewhere in the graphics to indicate the validity of the statistics (for example in the corner of each panel).

-Page 8, line 8: either the number concentration or the mixing ratio (these are very different things)

-page 8, line 21: "others". Please, be more specific.

-Page 8, COD: An important question is how COD is calculated in each model? Are all the ice categories used in this calculations? Are the effective radii explicitly calculated? Or is it a parameterized function, or even a fixed value? Even if the microphysics provides such information, it is often not used in the radiation transfer scheme. Is the COD presented here is a representative value of what the radiative transfer scheme "sees" or does it comes out of the microphysics scheme? What are the hypotheses about effective radii in the satellite retrievals?

-Page 8, last paragraph for CIP: "some areas...". Please, be more specific.

-Page8, CIP: what is the satellite uncertainties related to ice water path retrievals? This must be high and certainly close to the model biases. If so, what can the authors conclude about that variable for the different simulations? This aspect would be interesting to be developed in the text.

-Page 8, CIP: When talking about hydrometeor categories, be specific in the way it is called. "number of hydrometeors" is confusing and is not the general accepted term.

-Page 9, lines 1-2: A more important question about the number of ice categories in the different microphysics scheme, is what categories are actually passed to the radiation and are part of the CIP (and COD) calculations in the different models? For example, snow, as a distinct category, is often ignored for radiation but may be part of the COD calculation. What are the hypothesis used in the satellite observations?

-Page 9, CWP: Comparison with other studies of satellite retrievals are a good step, but it does not translate into total cloud CWP... (studies presented here are for low and middle clouds only?!).

-Page 9, line 20: How is the diagnostic CWP from the UK4 model is better or worst than the other models? Please comment. It seems that this model is in the middle of the model performance for CWP. What about CIP for this model, is it diagnosed as well? It should be mentioned. What is the diagnostic? Is it only a function of temperature?

-For NL2 and UK4, since no CFR are provided, how can CWP and CIP biases can be interpreted? In other words, does a CWP or CIP bias include a CFR bias? This is important to be clarified.

Technical corrections: (many corrections on the formulation are not listed since they are too many)

Abstract:

-line1: "On-line coupled" ...please rephrase

-line 9: "cloud liquid ice path" ... cloud ice water path

-line 10: "CWP bias is broadly overestimated"... either keep : a large positive bias or CWP is broadly overestimated

-line 12: "capacity" ... please rephrase

Introduction:

-line 25: "integrated meteorology-atmospheric chemistry" ... rephrase

-line 25: "demanded" ... not the right term

-line 27: problem with the citation and parenthesis

Methodology:

-page 4, line 16: "allow analyzing", please rephrase.

-page 4, line 19: use the same variable names and abbreviations as before (see the abstract)

-page 4, line 31: "According to...", please, do not refer to a table like this. This comment is valid for every table of the article.

-Page 5, line12: correct the citation parentheses.

-page 5, line 14: "we". Please rephrase.

-page 5, line 17: repetition of the line 14. Please rephrase.

-page 5 line 18: "monthly summary". Please rephrase or explain.

-page 5, line20: "BRDF" not defined

-page 5 line 31: last phrase to be rewritten.

-Page 6: equation 1: MBE not defined. Please include Pi, Oi in the equation.

-Page 6, lines 17-19: Please rephrase.

Results:

-page 7, line 26: "sea" please use always the same terms (ocean vs. sea)

-page 8, line 1, line 3: missing words

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-114/acp-2018-114-RC2-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-114, 2018.