Author's response "HARMONIE 37h1 radiation sensitivity tests"

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We wish to thank the topical editor Dr. Volker Grewe for his professional handling of the editing of this paper.

In this document our responses to the referees can be found. First our repsonse to anonymous referee #1 is given from page 2 to page 3, then our response to anonymous referee #2 is given from page 4 to page 9.

Reply to referee # 1 “HARMONIE 37h1 radiation sensitivity tests”

This reply is written in response to the review by anonymous referee #1 submitted April 27^{th} 2014 of our revised manuscript: “HARMONIE 37h1 radiation sensitivity tests” that was submitted on April 4^{th} 2014. In the following we quote the review and reply to this when apt. Our replies are written in italic. But first of all, we again wish to thank the referee for taking time to make these detailed and very helpful reviews of our paper. It is greatly appreciated.

> “The paper presents tests of SW flux calculations in a NWP model. Compared to the initial version the paper has been improved considerably. One of my major concerns was, that the setup for the reference model (DISORT in the framework of libradtran) was not described, the necessary descriptions have now been added. The other major concern was about the methodology, I suggested to compare the conversion from microphysical to optical properties and the radiative transfer solvers separately. The authors have now made clear that this is not possible for one of the schemes (hlradia) they compare, and for the other they made separate comparisons and provide results as a supplement. As presented now, the reader understands the methodology and also the results, i.e. the performance of SW radiation schemes in different conditions. Therefore I recommend to publish the paper technical corrections (see below).
>
> Abstract:
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> “blue differences” -> What does this term mean in this context, when you look at integrated irradiances, how can the difference have a color?”

1. The word blue has now been removed from the abstract. This was a remnant from the proof reading of the manuscript.

> “Introduction:
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> p.1, right column: “... could by parameterized ...” by -> be”

2. This has now been corrected following the suggestion of the referee.

> “p.1, right column, bottom: The 14 references in the bracket should be moved into the sentence before to clarify, what they are related to.”

3. These references have now been moved into the paragraph and placed where appropriate.

> "p.3, Sec 2.1: "... was run for the full SW spectrum of the Kato et al. correlated-k algorithm with absorption ... " -> "was run for the full SW sprectrum using the Kato et al. Correlated-k parameterization which includes absorption ..." "

4. This has now been corrected following the suggestion of the referee.

> "p5: "The primary reason for this decrease (in global radiation) is pressure broadening of the water vapor absorption lines." -> Is this really the reason? Does the line width matter when you integrate over the full spectrum? I don't see why the line broadening should affect the integrated results."

5. According to Chou and Kouvaris (1986) the absorption is saturated quickly near the line centres; pressure broadened absorption lines cover the vast spectral regions between lines better. This can be compared difference between a free kick wall of 5 football players standing next to each other and 5 footballers standing in a row perpendicular to the way they would normally stand. The footballers standing next to each other will be more likely to stop a free kick than those standing in a row, even though the integrated amount of footballers is the same.

> "p.14: "In the DISORT version that is currently available in libRadtran the assumption of Lambertian surface reflection is also made" -> This is not true, in libRadtran several BRDFs are available for DISORT"

6. We have now removed the sentences in section 4.4, where it was stated that DISORT in libRadtran can only handle Lambertian BRDFs.

Reply to referee # 2 "HARMONIE 37h1 radiation sensitivity tests"

This reply is written in response to the review by anonymous referee #2 submitted April 23^{th} 2014 of our revised manuscript: "HARMONIE 37h1 radiation sensitivity tests" that was submitted on April 4^{th} 2014. In the following we quote the review and reply to this when apt. Our replies are written in italic. But first, we again wish to thank the referee for taking time to make these detailed and very helpful reviews of our paper. It is greatly appreciated.

> "See my report but in summary:
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> I suggest the justification for choosing the specific benchmarks be improved. The label DISORT to label a mixture of cloud scheme and radiative transfer be eliminated.

1. We have now corrected this. See our reply #3 for details.

> Factors which are outside the range of the benchmarks (e.g. inhomogeniety, aerosols, crystal shapes) be eliminated from the benchmarkling discussion and moved to a section on other factors which will need to be investigated further if you really feel they need to be mentioned."

2. We think that it is important to include the discussion of the inhomogeniety factor. Please see our reply #9 also for details on this. In our revised versions of the paper all experiments were done without aerosols and the mentioning of these was removed from the discussion section. In our opinion the crystal shape issue should be mentioned in the discussion section.

> "Overall:
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> The paper has improved but still needs work. Some of my objections are probably nit-picking but the use of DISORT as the label for a combination of DISORT and a cloud scheme really annoys me (which might say more about me than the paper?) Its use in this way covers some assumptions which are not necessarily valid, e.g. DISORT is a valid standard for radiative transfer comparisons because it uses less angular assumptions than the fast aschemes in the model but the cloud optical schemes and the spectral information do not have the same 'respectability' and lumping them

together obscures this. The essential point for me is that the paper's aim is to provide a technique/method to track down errors in NWP SW fluxes by lumping together the radiative transfer and cloud optical property schemes and that approach is flawed. The justification is that one of the schemes tested is a combination of radiative transfer and cloud optics that can't be split but anyone tracking such errors will have to treat each part of the process separately and there are reasonably well established techniques for doing that. The authors seem to understand this at some level because they have split the problem up for the supplementary material but they have left the emphasis on a combined process as the basis for the paper. I agree that the SW fluxes are a result of multiple process in the model but the only way to really sort out its problems is to pull those processes apart and assess the performance of each part against relevant data. The authors agree with this and then lump two independent processes together as one component. A symptom of this is the use of 'DISORT model' as the label for the cloudy sky benchmarks when the benchmark is in fact a combination of DISORT radiative transfer and some optical property parameterizations (HU & Stamnes, Fu). This is also evident at the end of section 1."

3. This is a valid point of contention. We have now changed the text and the figures, so that the label 'DISORT' is not used for both the initial parametrizations and the radiative transfer calculations. We now use libRadtran/DISORT when speaking of the overall simulations. When DISORT is used together with the cloud liquid and cloud ice parametrizations of Hu and Stamnes, or Fu, we now refer to HS-DISORT or Fu-DISORT, respectively. The same point can be made for the clear sky computations, which we now refer to as Kato-DISORT. This is a compromise between referring to all the references used in the computations and using a bulk referral, as before, which can certainly be confusing. We have added an explanation of this nomenclature in section 2.1.

"There are some nice aspects of the work presented in the paper. The new optical property scheme which provides a better fit to Mie scattering deserves more prominence rather than being relegated to a supplement."

4. We have now added the comparison made for spectral band 4 (690 nm–1190 nm) in the main paper.

"The idea of interfacing a 'fast' SW scheme with implicit cloud properties to an NWP model is a valid research topic and I can see that comparing its results with a 'state-of-the-art' combination of radiative transfer and cloud optics scheme makes some sense if only to check the sanity of the choice and quantify the scale of the errors which are being traded off for computational efficiency. There should be more justification for the choice of the benchmark; DISORT is a valid standard for the radiative transfer but why choose the associated optical property scheme?"

5. See our reply #12 where we discuss the accuracy of the Hu & Stamnes cloud optical property parametrization.

"Ultimately the arbiter must be the performance of the NWP SW fluxes against observations to see if the trade-off is viable but that could be listed as future work.."

6. We have now clarified in our concluding remarks that we will perform 3D HARMONIE experiments that are tested against observations of global radiation. This was not explicitly mentioned before.

"Specif section comments:

1. Introduction:

The listing of sources of error in the first para is too obscure; turbulent fluxes, evaporation, precipitation etc do influence the result but basically through their influence on the cloud field. Trying to improve SW fluxes by fiddling with PBL fluxes would be pointless you would be better off tuning your cloud scheme. Concentrate on primary causes and lump the secondary source of error into something like 'input cloud properties. You could even suggest that a number of feedback loops are also involved."

7. We have now replaced the text:

"There can be multiple causes of biases in SW fluxes: The release of precipitation could be parametrized incorrectly; the liquid or ice water load in the clouds or atmospheric aerosols could be wrong; the surface radiative properties could be described incorrectly; the amount of soil water available for evaporation could be wrong; the fluxes of turbulent energy could be parametrized incorrectly; the radiative transfer calculations could be wrong; etc. Some of these causes are not directly related to the SW fluxes but affect the fluxes indirectly through, for example, the formation and lifetime of clouds and fog."

with the text:

"Biases in the predicted downwelling, reflected and net shortwave (SW) fluxes could be due to inaccurate atmospheric radiative transfer calculations. However, errors could also be related to incorrect input to the radiation calculations: incorrect cloudiness (liquid or ice water loads and their distributions) or poorly known properties of the

clouds (size and shape of the cloud particles), which influence the SW transmittance and absorptance. Under clear sky conditions, atmospheric aerosol loads and aerosol optical properties may not be described completely. Surface radiative properties like albedo may be represented inaccurately, for example because of poor knowledge of the existence of snow and ice cover on land and water surfaces. The amount of soil moisture or the treatment of evaporation could affect the formation and lifetime of clouds and fog, which indirectly affect the radiation fluxes. Interaction between multiple factors could lead to feedbacks, which again would affect the SW fluxes."

> "2. Methods
>
> The use of DISORT to label the combination of DISORT and the 2 chosen cloud optical property schemes is confusing and misleading. It would be better to replace DISORT when it is being used in the sense of the combination of schemes as REFERENCE or maybe REF/W and REF/I (or DISORT/HS etc) for the separate cases throughout the rest of the paper as well. You use IFS Fouquart etc and you should do the same for the DISORT based cases."

8. See our reply #3.

> "3. Experiments and initial atmospheric conditions
>
> 4. Results and discussion
>
> 4.2.1
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> The cloud inhomogeniety factor is used to try to correct the effective cloud optical depth for sub-grid variations in cloud thickness. It is an ad-hoc correction to account for sub-grid scale structure. Unless you are using the same correction in the benchmark calculation why would you expect to get agreement? The way to test the inhomogeniety factor is in the NWP model so its use here is irrelevant."

9. In this study we tested a released version of the HARMONIE NWP model. The SW scheme contained a SW inhomogeneity factor by default. Therefore, it is relevant to include a presentation of the large impact this factor has on the SW radiation output from the model. We did not expect to get agreement. Instead, we simply wanted to understand and test the components important for the SW radiation calcuations in HARMONIE. We have shown that the cloud inhomogeniety factor is clearly very relevant in this context as it should not be in the SW scheme and is a remnant from lower-resolution model versions.

"4th para:

It is not the 'accurate' DISORT you are comparing with; DISORT is a radiative transfer code. You feed it optical properties and it gives you radiation. You have compared results with input cloud optical properties in the supplementary material which is the correct way to do the benchmark. See my comments for section 2.

10. See our reply #3.

4.3.2

A statement like 'Thus, the benchmarking quality of the DISORT run, in this case, is no better than the correctness of the basic assumption made on the cloud ice particle shape' could have been used in all the previous sections. Note that DISORT does not make assumptions about crystal shape the Fu cloud scheme does that.

11. See our reply #3.

"4.4

The conclusion that the larger errors are due to the spectral bands rather than the angular assumptions in the radiative transfer could be confirmed by doing one of the comparisons for each band separately.

Conclusions:

How do you know the new optical property scheme is better? You only know it agrees better with whatever you used to generate optical properties for DISORT."

12. That is not correct, as we have also shown that it compares better to Mie theory calculations than the alternative parametrizations available in HARMONIE. Regarding the cloud liquid optical properties that we use as input for DISORT, these are calculated with the parametrization made by Hu & Stamnes from their 1993 paper: "An Accurate Parameterization of the Radiative Properties of Water Clouds Suitable for Use in Climate Models." The referee seems to suggest that this parametrization is not accurate. Here we would like to point out the this parametrization is widely used in many models and that the paper has been widely cited. We choose to trust Hu & Stamnes in their claim that their parametrization is accurate (to within $< 1\%$ for integrated SW fluxes), as many other modellers have done before us. We may be wrong in this assumption. If the referee can prove that this parametrization is not as accurate as claimed, we suggest that the referee publish these results.

I'm glad you removed the aerosols from the paper as your results basically just showed that you get different results if you use different schemes and I can't really see why you have included them as a supplementary material. Either way I would remove the 'further' from the last sentence of the paper."

13. We have now removed 'further' from the last sentence of the paper.

"Technical points:

Abstract:

4th line: 'blue' differences?"

14. The word blue has now been removed from the abstract. This was a remnant from the proof reading of the manuscript.

"Conclusions:

'- The effect of changing SW cloud inhomogeniety factor ...' is missing 'the'."

15. The "the" has now been added.