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Interactive comment on "Radiation sensitivity tests of the HARMONIE 37h1 NWP model" *by* K. P. Nielsen et al.

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Reply to anonymous Referee #2

First of all, we would like to thank the referee for taking time to make this detailed and very helpful review of our paper. It is greatly appreciated. We find that it has helped us to improve our work and strengthens our conclusions. In particular rerunning all experiments without aerosols is an important improvement.

Please see our responses to referee #1 as well.

Anonymous Referee #2

Received and published: 7 February 2014

General comments:

The aim of this paper "is to understand the differences between the available radiation parametrizations in terms of the solar (shortwave, SW) radiation fluxes compared to an accurate reference." It compares an NWP radiative transfer parameterization (coupled to a couple of different cloud optical property schemes) and a simple inexpensive broad-band scheme (with its own cloud scheme) against DISORT and an unspecified cloud optical scheme as a standard (a combination which I will refer to as libradtran). I don't agree that "The results of such a comparison will indicate where the NWP SW radiation parametrizations need improvement." unless the aim is to reproduce the libradtran results. How do you know that this is better than what you already have for what you really want (i.e. better performance of an NWP system?). How this relates to the idea of comparing NWP models to observations is not made clear.

1. We agree that the bottom line in NWP modeling of solar radiation (SW) fluxes is how well it compares to observations. In this paper we, however, do not deal with observations. In the national weather services where NWP models are applied operationally, such verifications against observations are done routinely. When biases are found in SW fluxes the challenge is to find out exactly what the cause of this is. This is not trivial as there can be multiple causes for such biases in a complex NWP model: The release of precipitation could be parametrized incorrectly; the amount of water load in the clouds could be wrong; the climatological amount of soil water available for evaporation could be wrong; the fluxes of turbulent energy could by parametrized incorrectly; the radiative transfer calculations in SW and LW could be wrong; etc. Very often multiple solutions exist to a correcting an observed SW bias, where several of the mentioned parametrizations or assumptions could be tuned to correct a given bias. This equifinality needs to be resolved. One way of doing this is by testing each of physical processes in the NWP model

separately. Here we present a focused study of only one component of the HARMONIE NWP modelling system, i.e. the SW radiative transfer calculations. Our particular focus on this component is because it has not previously been studied in the HARMONIE NWP model. We show that different parametrizations and assumptions within this have a large impact on the modelled SW fluxes. Thus, our study gives important new information for the HARMONIE modelling community. The method we use to study this may also be applied for other models and as such be useful for the broader NWP community. We have added this explanation in the introduction section.

If the underlying motivation of the work is to improve the radiative transfer in an NWP model I don't think this is the way to go! DISORT is an accurate radiative transport solver; not a proxy for the real world and this study demonstrates the obvious point that different parameterizations for cloud or aerosol optical properties give different results. Convolving the cloud optical properties variations with the radiative transport variations tells you less than studying either on their own. In all the cloud property variation experiments it would be more useful if there was some comparison of the actual cloud optical properties generated in the cloud layer for each scheme and it would make the explanations of the differences in the radiation easier.

2. We did in fact make a comparison of the actual cloud optical properties generated with each scheme. This was done using Mie theory as now described in "Supplement 1: Mie calculations." In the submitted manuscript we mentioned this, but did not find it necessary to include these in paper. Mie calculations of cloud droplets have previously been described in detail in the paper: "Light scattering in planetary atmospheres" by Hansen and Travis (1974).

Overall this paper has serious conceptual problems and should not be published in this form. The methodology used to evaluate cloudy sky results is severely flawed and there is insufficient information about the calculations being used as benchmarks.

3. After having followed the specific comments of both you (referee #2) and referee #1 we do see improvements in the results, however, we have found no indication that our methodology used to evaluate cloudy sky results should be "severely flawed". We have added detailed information about the calculations in the revised paper. See also our responses to referee #2.

Specific comments:

Clear sky experiments.

There are differences in the extraterrestrial downwards flux between all 3 radiation schemes. Is this due to the use of different solar data sets or the differences in spectral range? What is the source for the solar spectral information for the IFS? It doesn't seem to be given anywhere that I can find (even the original references). You could normalise the spectral integrals to make them equal and remove this difference.

4. In IFS the formulae of Paltridge and Platt (1976) are used for the TOA solar irradiances. A comment on this has now been added to the text. The 6 SW spectral bands have the following fractions at the top of the atmosphere: 0.1917%, 13.5708%, 32.2135%, 32.6158%, 18.0608% and 3.3473%. The TOA spectra were, however, not the reason for the differences seen. The fact that DISORT was only run for the spectral interval 280 nm – 3001 nm is the reason. We have now recalculated all results with the full short wave spectrum used in the Kato (1999) correlated-k algorithm. Normalizing the spectral integrals – as suggested by the referee – would be wrong.

The discussion on large solar zenith angle errors is not really relevant. The fact that DISORT does not have a correction for non-planar geometry or atmospheric refraction is not relevant to improving the NWP radiation schemes – they need to have such corrections to get more realistic results.

5. We agree and have removed this.

I can see no real reason for including the aerosol experiment. The aerosol properties are different. When you get differences how do you tell if it is the way the radiation scheme interacts with the aerosol optical properties or just the difference in the aerosols? I strongly suggest you remove this experiment unless you can redo it and keep the aerosol OPTICAL properties the same in all calculations.

6. We agree and have removed this.

I hope all the other experiments had the aerosol turned off otherwise it is a confounding factor.

7. Aerosols were turned on in most of the experiments. We have now redone all the experiments without the aerosols. The differences in the results caused by doing this are described in: "Supplement 4: Comparison of running with and without default aerosols". Overall running without aerosols is a better way to run these experiments. Doing so strengthens the main conclusions made in the original manuscript.

Cloudy sky experiments.

All of these suffer from lack of knowledge of what is actually used in the libradtran results. If you could specify the cloud optical properties to be the same in the DISORT

and two-stream code you could get an estimate of the errors in the two-stream approximation but if the cloud optical properties are different I'm not sure what you can usefully conclude given that it is unlikely that any of them are optimal in the context of real-world NWP.

8. When the Fu (1996) cloud ice optical properties are used as input both to DISORT and to the IFS radiation scheme in HARMONIE, this is an implicit test of giving the same optical properties as input although the IFS radiation scheme only has 6 spectral bands. A more explicit test of the second step where the delta-Eddington scheme is tested directly against DISORT with the same optical properties in a single spectral band has been added in a separate supplement to the revised paper: "Supplement 2: Tests of the IFS delta-Eddington radiative transfer scheme."

I can see why you wanted to mention cloud inhomogeneity as a difference that needs to be accounted for (your Para: 3.2.2 for example) but in the end it does not contribute much to your comparisons since all your schemes use different values and in the end you put it to 1.0 so that you can use the DISORT results anyway.

9. We have run all the cloud experiments with the default cloud inhomogeneity factors set to 0.7 (IFS) and 0.8 (hlradia). The differences of these relative to DISORT were of course large. Having established this, we only include these results for experiment 5 and rather focus on the differences that are not related to this factor.

The differences between the different schemes in Figs. 10-13 are dramatic, interesting and could possibly be investigated further by looking at the actual cloud optical properties (i.e. optical depth, asymmetry factor, single scattering albedo) as well to get *some useful insight. If so you still need to show results for a comparison with the same cloud optical properties in all radiative transport codes to separate their errors from the differences in the different schemes used to get optical properties from cloud physical properties.*

10. As mentioned in reply 2. we have in fact investigated the inherent cloud optical properties for each of the cloud liquid optical property schemes in the IFS radiation scheme. For hlradia this is not possible, as the intermediate computation of these properties is not done in this radiation scheme. We have now also added a test in which the delta-Eddington (two stream) radiative transfer module in the IFS radiation scheme is compared directly with DISORT (with 30 streams) given the same optical properties. The results of this test are given in the supplement to the revised paper mentioned in reply 8.

Conclusions.

The conclusions are all relative to libradtran results. By this I mean that you have assumed that the libradtran results are the ones to aspire to and you need to try to adjust your current schemes to reproduce them. The libradtran schemes could very well give better results if implemented in the NWP model but you have not established that here.

11. As we discuss in reply 1. NWP models have a lot of different parametrizations that contribute to the correctness of the SW flux output. Thus, the radiative transfer scheme is only one piece of the puzzle. If these other parametrizations have been adjusted to compensate for errors in the radiative transfer scheme, implementing a better radiative transfer scheme could very well worsen the results at first, until the other

schemes are readjusted. For instance, if the cloud schemes have been adjusted to give the clouds 30% extra cloud water load, this would compensate for the cloud inhomogeneity factor of 0.7. Our aim is not to implement the benchmark libRadtran schemes directly to NWP, where the parametrizations need to be optimised for speed and performance. We agree that at a later stage, systematic comparison of the predicted by our NWP model short- and long-wave radiation fluxes with observations will be necessary. Now we are still at the first step, comparing against the detailed benchmark radiation calculations.

I accept that DISORT used in conjunction with a good spectral scheme is a good standard to test radiative transfer parameterizations but if you want to apply it to cloud you should be specifying the cloud optical properties and not the cloud microphysics. The parameterization of cloud optical properties from cloud microphysical properties is a separate problem and needs to tackled using separate criteria.

12. As mentioned in replies 2. and 10. we have in fact analyzed the cloud optical properties separately.

1. Finding good agreement for clear sky is not surprising (apart from possible differences in the extraterrestrial incoming radiation) since the physics is relatively well understood. It really only depends on the radiative transport parameterization and some sort of spectral averaging scheme and these have been developed to be as accurate as possible for clear skies. 2. That the Fu scheme looks better than the Fu-Liou scheme probably means the libradtran is using a scheme closer to Fu than to Fu-Liou. 3. The Nielsen scheme might be giving better results because it represents variations in the variation of the asymmetry parameter better or because its basic properties are closer to those in the libradtran scheme. How could you tell?

13. We can tell because we have done detailed comparisons with Mie calculations (see “Supplement 1: Mie calculations.”)

4. Tuning the hiradia scheme might make it agree with the libradtran results for some cases but make it worse in others. Given the known spectral variations in gaseous and cloud optical properties which it cannot hope to describe it does a pretty good job as it is.

The conclusions for future work involving testing are quite reasonable but I don’t think the proposals to change various parameters are necessarily justified by the results here.

1. There is always scope for re-parameterizing the spectral bands in a model, however, the choice needs to be made with tradeoffs between accuracy and efficiency for the particular situation. The case for dropping the high energy band is good and you have to wonder why the designer of the original scheme decided to keep it! 2. The choice of inhomogeneity factor should not be determined by comparisons with DISORT calculations; it should be determined by looking at the cloudy sky results at the NWP model resolution amongst other things. It is supposed to allow for sub-grid scale variations in cloud properties and is certainly a candidate for further investigation. 3. Tuning the hlradia scheme to DISORT will only tune it to the MLS atmosphere and the cloud scheme used.

14. For experiment 1 we have now tested the other AFGL atmospheric profiles as well. In the first figure we have added the result of the MLW profile also. These tests show the importance of the atmospheric profile on the water vapour absorption. Since the other AFGL profiles all have less SW transmittance for the same integrated water vapour paths, and hlradia had a positive bias, it still makes sense to tune hlradia. Following up on this work, we will do this against the set of AFGL profiles rather

than just the MLS profile.

Otherwise the other proposals are good.

Technical notes:

The following sentence (page 6787, line 27) is probably missing an ’and’: “As for the global radiation, the net fluxes mostly have a positive bias both below and above the clouds when the Fouquart parametrization is used . . . an increasingly negative bias is seen below increasingly thicker clouds (Fig. 10).”

15. We have added a full stop before “An increasingly negative ...”

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/6/C2941/2014/gmdd-6-C2941-2014-supplement.zip

Interactive comment on Geosci. Model Dev. Discuss., 6, 6775, 2013.