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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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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. 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.

Overall this paper has serious conceptual problems and should not be published in this form.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.

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.

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.

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

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.

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.

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.

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. 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.

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? 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. 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 ˆˆˆ an increasingly negative bias is seen below increasingly thicker clouds (Fig. 10)."

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