Review of 'Radiation sensitivity tests of the HARMONIE 37h1 NWP model' by
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Overall:

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.

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. 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? 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..

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.

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.

3. Experiments and initial atmospheric conditions

4. Results and discussion

4.2.1

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.

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.

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.

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.
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.

Technical points:

Abstract:
4th line: 'blue' differences?

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