Author's response

This document has the following parts:

- Page 2-7: Reply to referee #1.
- Page 8-12: Reply to referee #2.
- Page 13: List summarising the primary changes made to the revised manuscript.

Reply to anonymous Referee #1

We would like to thank the referee for taking time to make this detailed and very helpful review of our paper. We appreciate this. We find that it has helped us to improve our work and strengthens our conclusions.

Please see our responses to referee #2 as well.

Anonymous Referee #1
Received and published: 30 January 2014

1 General comments
The paper presents a study on the sensitivity of shortwave radiation fluxes w.r.t. water vapor and ozone concentrations, aerosols, water clouds and ice clouds.

1. Actually, the primary purpose of the paper is to test the shortwave (SW) flux calculations in the HARMONIE 37h1 NWP model. The sensitivities of the SW fluxes with respect to various atmospheric variables are secondary results. We have modified the abstract so this is clearer.

Calculations of a single column version of the HARMONIE 37h1 NWP model (run in different configurations) are compared to DISORT calculations, which are taken as benchmark results. The atmospheric state, i.e. trace gas concentrations, liquid water content, ice water content, and cloud particle sizes, is the input to all models. The models then use different parameterizations to convert from these microphysical to optical properties which are required to solve the radiative transfer equation. As the authors mention in their introduction, their study includes these two steps: (1) conversion from microphysical to optical properties and (2) solving the radiative transfer problem. The second step is solved more accurately with DISORT, which is a radiative transfer solver based on the discrete ordinate method whereas the NWP uses a a simple radiation schemes based on the delta-Eddington approximation. But the first step which is also very important may be even less accurate for the DISORT calculations. A major weakness of the paper is that the authors do not describe which parameterizations are used for the DISORT calculations to convert from microphysical to optical properties.

2. These descriptions have now been added.

LibRadtran offers a variety of different parameterizations, more and less accurate ones, and it is also possible to directly feed optical properties to DISORT. The standard settings of libRadtran are not the most accurate ones. For example, libRadtran includes the parameterization by Fu 1996 for ice clouds which is also optionally used in the NWP model. Therefore it is not surprising that the NWP model agrees best to DISORT when the Fu 1996 parameterization is used.

In my opinion the two steps need to be investigated separately. In order to test the radiation scheme itself, the models must use exactly the same optical properties as input. It should be possible to extract the optical properties from the NWP model and feed them to DISORT.

3. The main reason for not comparing the pure radiative transfer schemes by using prescribed cloud optical properties (optical depth, single scattering albedo and asymmetry factor) was to allow inclusion of hlradia in the comparison. This scheme applies an integrated way of deriving the radiation effects on temperature directly from the cloud microphysical properties. We now make this clear in the revised manuscript. We have in fact investigated the first of these two steps separately for the IFS scheme. In order to make the new (Nielsen) cloud liquid optical property parametrization, we ran detailed Mie theory calculations, which we compared to all the IFS cloud liquid optical property parametrizations.

This is mentioned in the manuscript, but the detailed calculations were not included. We have now added these in: “Supplement 1: Mie calculations”. We have also added a supplement with an explicit test of the IFS delta-Eddington scheme (the second step) against DISORT with the same optical properties. These results are given in: “Supplement 2: Tests of the IFS delta-Eddington radiative transfer scheme.”

The accuracy of the parameterizations to convert from microphysical to optical properties may also be investigated using the libRadtran package with the most accurate settings, for this part the libRadtran settings need to be described in detail.

4. As mentioned above we have added a supplement with our Mie calculations. As already described in the paper we used the Mie algorithm developed by Warren Wiscombe (1980) for this purpose. We did not use libRadtran for this.

Currently the reader does not know on which parameterization the so called "benchmark results" are based. In several places it is obvious that the authors of the study have not used the most accurate settings. For these reasons I can not recommend to publish the study in its current status. The major revision of the study should include a comparison of DISORT and the NWP radiation schemes given the same optical properties and also a comparison of the various parameterizations (gas absorption, aerosol and clouds) where the most accurate settings of libRadtran should be compared to the various configurations of the NWP model.

5. Since the HIRLAM radiation scheme hlradia does not include an intermediate calculation of optical properties, a comparison of using these cannot be made for this. This is also the reason, why we did not do this originally. The IFS scheme does include this calculation, where the optical properties are given as input to a standard delta-Eddington (two stream) algorithm. We have now included a comparison of this calculation for a range of optical properties input to both the IFS scheme and DISORT with 30 streams. These results are given in: “Supplement 2: Tests of the IFS delta-Eddington radiative transfer scheme.”

2 Specific comments

Abstract
Please include some details about the NWP models, e.g. where in the HARMONIE NWP model used.

6. We guess that the referee here means “where is the HARMONIE NWP model used”. This is used in several European countries that are members or the HIRLAM-ALADIN consortium. We have added a sentence about this in the abstract.

Explain/expand the abbreviations IFS and hlradia.

7. IFS = Integrated Forecasting System has been added. hlradia is short for HIRLAM radiation scheme, as already mentioned. HIRLAM = High Resolution Limited Area Model - has been added.

Also the benchmark results should be described more detailed, i.e. it should be clear that very accurate, state-of-the-art parameterizations are used.

8. This is now specified in the abstract.

Methods

p 6778, l. 25: Which absorption parameterization is used for DISORT calculations? 1 nm spectral resolution does not make much sense, because in order to obtain the integrated solar flux the most accurate parameterization in libRadtran is the correlatedk-distribution by Kato 1999.

9. In the first version of the manuscript we used the SBDART/LOWTRAN 7 absorption parametrization. We have now redone the calculations with the more accurate Kato/HITRAN 2000 absorption parametrization. A comparison between using these two parametrizations is made in "Supplement 3: Comparison of SBDART LOWTRAN and Kato HITRAN calculations". Overall the relative differences in the clear sky computations are 2% or less. The relative differences in the cloudy sky computations are even less. Using the Kato (1999)/HITRAN 2000 absorption parametrization is certainly more precise; however, it does not have a major impact on our results or our conclusions.

Eq. 3-5, Table 2: How are the coefficients obtained? Are they fitted against detailed Mie calculations? This needs to be explained in detail.

10. As mentioned above (reply 3) we have added a supplement with our Mie calculations. Yes, the coefficients are obtained by fitting against detailed Mie calculations.

p. 6780, l. 19: Explain "hybrid coordinates"

11. Basically, the pressure-based hybrid vertical coordinate of HARMONIE follows the surface elevation at the lowest model level and smoothly converts towards isobaric levels higher in the atmosphere. "Hybrid coordinates" is a basic concept in meteorological modeling. A description of this can be found in Ch. 13: "Numerical modeling and prediction" of the textbook: "An Introduction to Dynamic Meteorology" by James R. Holton. We have added a reference to this.

Results

p. 6782, l. 23: The authors say that the difference at TOA comes from differences in the downward component of the fluxes. The only source of discrepancy is here the extraterrestrial spectrum. Which one is used in IFS?

12. 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. This makes the TOA downward solar fluxes agree between the schemes.

p. 6782, l 17ff.: "Detailed UVB/UVA estimations are not needed in general NWP computations and should be done separately by combining the modeled SW fluxes with the most recent ozone measurements." How should the modeled fluxes be combined with measurements? This is not at all clear.

13. This sentence has been shortened to: “Detailed UVB/UVA estimations are not needed in general NWP computations, since only the net fluxes at the model levels influence the simulated temperature.” The remark on how these could be done is irrelevant to the paper otherwise.

p. 6784: The difference between the models for large solar zenith angles is explained by the fact that the IFS radiation scheme includes a correction for the sphericity of the atmosphere whereas DISORT is a fully plane-parallel model. libRadtran includes also a pseudo-spherical version of DISORT. Why is this not used? It would be even better to use the fully spherical 1D Monte Carlo solver MYSTIC as benchmark, which is also freely available in the libRadtran package.

14. After re-running the experiments without aerosols, as suggested by referee #2, the large differences at the larger solar zenith angles disappear for the IFS radiation scheme and become a lot smaller for hlradia. Thus, the different aerosol parametrizations used in libRadtran, IFS and hlradia were the primary cause of these differences. We have changed the text accordingly. We have also rerun the solar zenith angle experiment with the pseudospherical DISORT solver of Arne Dahlback et al. available in libRadtran. For the solar zenith angle of 80 degrees an increase of +1.7% is seen relative to the plane parallel DISORT run. For the other experiments, the effect of using the pseudospherical solver is insignificant, as they are run at a solar zenith angle of 56 degrees.
    a. A 1D Monte Carlo algorithm was compared to DISORT by Hestenes, Nielsen, Zhao, Stamnes & Stamnes (Appl. Opt. 2007). The test showed that DISORT was both faster and more precise.

p. 6785: The aerosol experiment does not make much sense when different aerosol models, all of them not very accurate, are used. Here it is not clear, why DISORT with Shettle aerosol should produce more accurate results than the other models.

15. We agree. We have redone all calculations without aerosols to properly compare DISORT with IFS and hlradia. Also, we have removed the aerosol experiment from the paper.

p. 6786ff: Which liquid cloud parameterization is used for DISORT calculations? Also in this section it is not clear whether discrepancies are due to different radiation schemes or different parameterizations to compute optical properties.

16. The Hu and Stamnes (1993) cloud liquid optical property parametrization is used. For integrated SW irradiances Hu and Stamnes show this to be accurate within much less than 1% as compared with Mie theory. We have changed the text accordingly. Additionally, in the supplement on tests against Mie theory (“Supplement 1: Mie calculations”) in the revised paper we directly test the cloud liquid optical property parametrizations of the IFS radiation scheme against the Mie algorithm of Wiscombe (1980). Clear differences are shown here that can explain the bulk of the discrepancies using the various cloud inherent optical properties in HARMONIE.

p. 6786, l.10: Explain "cloud SW inhomogeneity factor"

17. The cloud SW inhomogeneity factor is a factor that is multiplied by the cloud water load. I.e. if the factor is 0.7, 30% of the cloud water is removed before performing the SW radiative transfer calculations. The argument behind doing so is to account for the inhomogeneous clouds within NWP grid boxes. It was introduced when NWP models had horizontal resolutions of ~10 – 100 km. It seems to be a remnant that it is still present in HARMONIE when this is run with 2 km horizontal resolution. We have changed “modified” to “multiplied” in the text to make this clearer.

p. 6786, l.15: DISORT calculations were done for horizontally homogeneous clouds, therefore the cloud SW inhomogeneity factor in the NWP models was set to 1. In libRadtran it is also possible to use different cloud overlap assumptions, this could be compared to calculations with other cloud inhomogeneity factors.

18. Cloud inhomogeneity and cloud overlap are two completely different things. In NWP models these are calculated independently of each other. A cloud can be inhomogeneous without overlapping any other clouds. We do not deal with the topic of cloud overlap in our investigation.

p. 6791: Which ice cloud parameterization is used for DISORT calculations?

19. We use the Fu (1996) cloud ice optical property parametrization. We have added this to the text.

p. 6792, l25ff: "In both DISORT, IFS and hlradia cloud ice is considered to consist of hexagonal crystals. In reality, cloud ice particles come in multiple shapes (Baker and Lawson, 2006;Lawson et al., 2006). As shown by Kahnert et al. (2008), these shapes significantly affect the SW forcing of the cloud. ...". In libRadtran it is possible to select various shapes as well as shape mixtures. Why is this option not used to obtain a more realistic benchmark result?

20. Innumerable realistic mixtures of the various cloud ice crystal shapes exist, why one of these should be better than another to use for the benchmark results, is hard to see. In our opinion more knowledge about how the cloud ice particle shapes vary is needed before it makes sense to include these in NWP radiative transfer modeling. For now we use only hexagonal crystals, in the future it is very likely that improvements can be made.

Conclusions

p. 6793: "A new optical property parameterization for liquid clouds has been developed. We have shown that this is better than the parameterizations currently available in HARMONIE." This is not shown because the DISORT setup for the cloud simulations is not described and it is not clear whether the DISORT results are more accurate that the currently available parameterizations in HARMONIE.

21. Here we disagree with the referee. Inherent optical properties calculated with our new parametrization *clearly* fit those calculated with Mie theory better than those calculated with the other parametrizations available in HARMONIE. We show this in detail in Supplement 1. It also gives *clearly* better results for both the global radiation comparison and for the net fluxes. We are very surprised that the referee here states that DISORT cannot be considered accurate when it is run with 30 streams. The accuracy of both Mie theory and the discrete ordinate method, when used with sufficient streams, we find to be well within the limit acceptable for a study like this, where the integrated SW irradiances of a NWP model are tested. DISORT is run with the Hu and Stamnes parametrization, which is widely used. For integrated SW transmitted irradiances this is accurate to much less than 1% as compared with exact Mie theory (Hu and Stamnes 1993). We choose here to assume that Hu and Stamnes are right in this claim. Such accuracy is sufficient for us.

p. 6794: "The SW cloud inhomogeneity factor should be changed from 0.7 (0.8) to 1.0 in all schemes applied in HARMONIE." It would be better to include a more accurate and fast radiation scheme, e.g. maximum random overlap.

22. This comment is based on the misunderstanding between cloud inhomogeneity and cloud overlap that we mention in the replies 17. and 18.

"The hlradia gaseous transmission coefficients should be tuned to the DISORT clear sky results presented here." It is not shown in this study that the DISORT clear sky results are more accurate than hlradia.

23. See comment 21. about the accuracy of the DISORT algorithm.

Reference
- *Kato, S., Ackerman, T. P., Mather, J. H., and Clothiaux, E.: The k–distribution method and correlated–k approximation for a shortwave radiative transfer model, J. Quant. Spectrosc. Radiat. Transfer, 62, 109–121, 1999.*

24. We have added this reference, and have recalculated all our results based on this.

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

15. We have added a full stop before "An increasingly negative …"

List of relevant changes to the manuscript GMDD-2013-150

- The Mie calculations made to test the cloud liquid optical property schemes in the IFS radiation scheme of the HARMONIE model have been included in a supplement to the paper.
- Tests have been made of the IFS delta-Eddington radiative transfer scheme against DISORT calculations. Some of these have been included in the manuscript and more of these have been included in a supplement to the paper.
- The DISORT calculations in the libRadtran framework have been redone with the Kato (1999)/HITRAN 2000 absorption parametrization as input.
- All experiments have been rerun with no aerosols, since the various models use different aerosol schemes.
- We have expanded the spectral range of the libRadtran/DISORT run to the full shortwave spectral range, as the previously used spectral range (280 nm – 3000 nm) lacked 2.6% of the solar flux at the top of the atmosphere.
- The solar zenith experiment has been rerun with the pseudospherical DISORT solver of Dahlback et al. (1991).
- The abstract and the introduction sections have been rewritten, so the purpose of the paper is clearer.
- We have added descriptions of the cloud parametrizations used in libRadtran for making the input to DISORT, in the cases where these were not clearly specified.