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Interactive Comment

Interactive comment on "Two adaptive radiative transfer schemes for numerical weather prediction models" *by* V. Venema et al.

V. Venema et al.

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GENERAL COMMENTS

Referee #3 wrote: "I found this paper to be very interesting, highly relevant and useful for the development of radiative transfer schemes in NWP and Climate Models. I would recommend this paper for publication subject to only a few minor comments. The paper provides a proof-of-concept that adaptive radiative transfer schemes of two kinds are workable and accurate enough to be used in today's models. As a proof-of-concept, I think it is acceptable that a simple evaluation of surface fluxes is used, although there will clearly need to be a more complete validation, using full profiles of heating rates, before the schemes can be considered for implementation into full models."

We would like to thank referee #3 for his nice and constructive comments. We will

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include the full heating rate profiles in our upcoming paper, where the algorithms will be implemented in the COSMO model.

Referee #3 wrote: "One concern with regard to the spatial local-search scheme is how this may be combined with the increasingly more sophisticated use of surface properties in today's models (i.e. sub-grid "tiles" of different albedos/temperatures in Climate Models, or the interaction of direct short-wave radiation with surface slopes in NWP models). There may also be problems in coastal regions where not only the surface properties change, but other factors such as the number of cloud condensation nuclei will change. Will this scheme not cause problems for the better resolution of the surface?"

One has to keep in mind, that for another intrinsic parameterisation, you may need to develop another adaptive scheme that fits to the intrinsic calculation and the dynamical model. Furthermore, the answer to this question depends strongly on what kind of scheme one is comparing the adaptive schemes with. For fairness one should compare the adaptive schemes to parameterisations that need similar computational resources.

A number of state-of-the-art radiative transfer schemes are actually going towards a coarser spatial resolution of the radiative transfer calculation than for the dynamics to reduce the computational load. Compared to these schemes, the adaptive schemes are able to better exploit the high resolution surface properties.

If the radiative transfer scheme only knows the grid averaged albedo, i.e. if the tiles are limited to the ground module, the situation would be the same as the one in our paper. The tiles would only allow subscale variability in surface temperature due to the variability in albedo.

If the CCN concentration influences the cloud properties, which is not the case for the version of the COSMO model in this paper, it may be wise to use optical depth instead of LWP. Furthermore, if needed the CCN concentration could be included in the similarity index of the spatial scheme. The regression constants of the predictors of

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the temporal adaptive scheme would likely change and the aerosol concentration could be needed as an additional predictor. A realistic description of CCN would not have a discontinuous jump at the coast, but rather a gradual transition due to turbulent mixing in high-resolution models or because the coast line is not well defined for large-scale models. Consequently, adjacent grid boxes may still be quite similar, even in a CCN scenario.

For radiative transfer schemes that take the surface slope into account in the short wave regime and the sky view factor in the long wave region, these two parameters may have to be included in the similarity index of the spatial adaptive scheme. Alternatively, it may be possible to correct for the surface geometry, similar to the current corrections for the solar zenith angle.

If the radiative transfer calculation would utilise a tile or mosaic approach in a similar way as some ground modules do, one could include a variance of the albedo in the similarity index. Although this might not be necessary as even the albedo itself is already not very important; not only cloud properties correlate in space, but also the properties of the surface, including albedo and the variability of the albedo.

An adaptive scheme that would perform radiative transfer calculations for multiple tiles per column could be realised at relatively little additional computational costs compared to a grid scale schemes. In this way, the adaptive scheme could exploit the better resolution of the surface in a way that would likely be seen as computationally too costly for traditional schemes.

Referee #3 wrote: "The temporal scheme would have no such problems (which may perhaps be pointed out in its defence). With the use of a more sophisticated extrinsic parametrisation such as a neural net or a simple physical parametrisation that takes proper account of clouds, I can see good potential for this scheme."

The temporal scheme will likely handle these more sophisticated parameterisations gracefully. However, one cannot be fully sure that there will be "no such problems", as

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the inclusion of CCN and surface slopes could reduce the explained variance (relative to the intrinsic calculation) of the regression scheme. The temporal adaptive scheme could need additional additional/other predictors. We do not see this as a problem, but rather see adaptive parameterisations as a way to be able to apply such more elaborate parameterisations. We expect that typically a large part of the additional predictive power of a more intricate intrinsic parameterisation will be maintained by a suitable adaptive scheme that utilises this intrinsic parameterisation. In the mean time we have found two fitting articles that list a number of simple radiative transfer codes for the short wave and long wave region; we have added these references.

Referee #3 wrote: "The layout of the paper focuses heavily on the results and discussion and would benefit from a clearer and more detailed description of how the adaptive schemes work. In particular, the description of the spatial local-search scheme should explain what the generalisation algorithm does with the results from the selected intrinsic calculation (does it simply correct for solar zenith angle?)."

The treatment of the solar zenith angle was missing in the original text, we have added: "The infrared surface net flux of the most similar nearby column is copied; the solar net flux is corrected for the change in surface albedo and solar zenith angle, in other words the transmittance of the most similar known column is taken." Additionally some smaller clarifications have been made.

SPECIFIC COMMENTS

Referee #3 wrote: "1. For the temporal perturbation scheme, is there a maximum time between calls to the intrinsic scheme for a given grid-box? If not, it appears this would be a simple solution to the errors produced by this scheme and alluded to in the first paragraph of section 5.3."

There is no explicit maximum time between calls to the intrinsic scheme. However, the intrinsic scheme is called for those columns that have the largest change in solar net flux (i.e. we do not look at the change in the transmittance). As the SZA changes during

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the day, this does set an implicit maximum time between calls. Still it is an interesting idea to implement an explicit maximum time or to calculate a change index that is a weighted average of the change in solar net flux and the time since the last call to the intrinsic calculation.

Referee #3 wrote: "2. At the end of section 5.4 you mention that the ice water path was found to be insignificant as a selection parameter. I find this quite surprising, especially in the longwave where I have found variations in the ice condensate to have a greater impact than variations in the liquid water condensate. It may be that the models for which your scheme was tested did not contain a significant amount of ice, and this should be mentioned if it is the case."

That the ice water path is not a significant selection parameter for the spatial scheme, does not imply directly that ice clouds are not significant for radiative transfer nor that there are not enough ice clouds in the model. It could also be because ice clouds are smoother than water clouds at scales above 2.8 km, and thus adjacent columns main differ in liquid water path rather than ice water path. Furthermore, it could be that the ice water path correlates strongly with parameters that were considered. Additionally one should consider that the total cloud cover also includes the ice clouds.

Referee #3 wrote: "3. In the discussion, p. 7254, line 12, you state "An extension to the full vertical profile is trivial for the spatial local-search scheme". While it may be trivial to expand the technique, it is far from obvious that this will work particularly well. It will be a lot harder to find a similar profile that produces similar heating rates than it is to find similar integrated quantities. I think this point should be mentioned."

That the schemes work so well is because cloud fields are highly correlated, i.e. that adjacent cloud profiles are very similar. (The scheme would not work as well if (instead of a local search) we would search in a global database of all possible atmospheric states for a column with a similar cloud cover, LWP and albedo.) We have seen in this study that the inclusion of the cloud base height in the spatial scheme resulted in little

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improvement. This is likely because the columns that were similar with respect to the integrated quantities LWP and cloud cover also have a similar cloud base height.

Thus, we expect that the performance for the atmospheric heating rates will be similar to the surface net flux. However, the reviewer is right in that this first needs to be demonstrated. We changed the sentence to: "An extension to the full vertical profile is technically trivial for the spatial local-search scheme, but it will need to be demonstrated whether the gains are similar."

Referee #3 wrote: "4. Discussion p. 7257, line 24. I find the example of a McICA scheme slightly anomalous in that this need be no more computationally intensive than a conventional two-stream scheme. Although, I do see that it is possible to have a more rigorous McICA scheme (ie that uses more than one "k-term" for each sub-column) that would be more computationally expensive and may prove beneficial."

Yes, we were thinking of a McICA implementation that would calculate more sub-columns to reduce the variance of the result. We now explicitly write: "For example, a computationally expensive version of the Monte Carlo Independent Column Approximation (McICA) parameterisation (Barker et al., 2002; Pincus et al., 2003) with a larger than typical number of ICA calculations could be utilised to calibrate a conventional 948;-two-stream scheme, which would calibrate a generalisation."

TECHNICAL CORRECTIONS

The technical corrections have all been implemented.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 7235, 2007.