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Interactive comment on "Toward unification of the multiscale modeling of the atmosphere" *by* A. Arakawa et al.

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This paper is a very didactic introduction to two strategies which the authors elect in order to try and unify coarse and fine resolution aspects for what they indicate as a general aim, i.e. 'multiscale modelling'. Their comparison, showing the gap between the moist-static-energy budgets at the GCM scale vs. at the CRM scale, undoubtedly emphasises the difficulty of the task and helps understanding how both proposals 'Route I' and 'Route II' do address the challenge.

One may however regret that the authors do not envisage (or even mention) any other possible path (towards multiscale modelling) than the one of combining, in one way or the other, the results of numerical-physical computations performed at the said two extreme scales. If those are the only ones where, in the authors' opinion, numerical–physical modelling of convection (fully parameterised or fully resolved, so to say) is

meaningful, arguments should be brought forward to substantiate this claim. If not, it should be acknowledged somewhere that other 'routes' than II and (especially) I may exist, even if the authors do not favour them.

In line with being the transcription of a prestigious invited lecture, the paper makes best use of simple concepts and of the mention of published solid prior results. As such, the Abstract, the Introduction, the initial aspects of the derivation/justification of Route I, the description of Route II (within what is now a quite well established trade) and the Conclusion are nearly flawless. I just notice here the slightly irritating word-for-word repetition in the Conclusion of the last paragraph of Section 2.

I have more problems with Sub-sections 2.4 and 2.5 where the 'closure' and 'integration' aspects of the (rather new) proposal leading to Route I are explained.

a) First of all, I'm troubled when seeing that Equation (16), i.e. the 'practical side' of the method in the author's own words, is just the consequence of the radical (see below my second specific comment) choice of the asymptotic behaviour for the subgrid transport term when 'sigma' goes to one. Hence, if one trusts the results of Figure 8, it relates to a physics at quite high resolution, surely close to that of the CRM to be used for determining [(_-_)(_-_)]*. I wonder whether there could exist here a risk of self-fulfilment for the justification of this particular mathematical shape. b) Still in the same line of thoughts, the (1-sigma)^2 proportionality factor of Equation (16) is supposed to be universal for all scales from the GCM one (giving (_-_)_adj) to the one of the CRM (see below again my second specific comment). I have much difficulty imagining that all the richness of the observed organisation of convection between the scales of 50km to the scales of 1km (numbers given here as orders of magnitude) can be reduced to the existence of this hyper-simplified 'law'. c) Last but not least, the authors insist on the necessary quality of the 'cloud microphysics, turbulence and radiation' calculations and on the need to apply them to a wide range of resolutions. But, in my opinion, the strong non-linearities necessarily present in such computations when they interact with the parameterisation of 'sigma' can lead to strong errors if 'sigma' is indeed computed more so that (1-sigma)^2 represents the ratio of the sub-grid transport to its extreme value than for a physical description of the actual cloud macro-structure. Complex paths may exist to overcome this difficulty but I guess that the main credo of the method (the need to look at the physics only at the extreme scales) would then suffer exceptions, which would make the whole reasoning somehow less convincing.

In summary, I personally do not see how Route I, under the chosen constraint to only rely on computations done basically at the same scales as those of Route II can represent a complete alternative to the latter. Therefore, unless this problematic is better touched in the redaction, the paper could give the impression that only this 'family' of schemes is able to address the (very well explained) challenge of convective multi-scale modelling. And this is something which is not necessarily an absolute truth, in my opinion. I also consider this one-sided aspect of the paper as a pity, since the original ideas in the derivation of the method of Route I could probably play a quite interesting guideline role in the search for other solutions around the 'extension of parameterisation path'.

I also have two 'specific comments': - On line 22 of page 3188, it is unknown whether or not there is a condition of existence of water condensate (on top of the one of ascending vertical velocity) for defining the so-called "cloud points". This should better be clarified, even if it does not affect the basic reasoning of Section 2. - The establishment of Equation (9) represents quite a strong analytical jump. Not only are the above-mentioned possibilities of 'higher'-order dependency on '1-sigma' discarded, but it is assumed, for the sake of simplicity, that the asymptotic formulation applies whatever value 'sigma' may take. It is true that subsequent arguments, developed in Sub-section 2.3 will show that these choices lead to reasonable results. But since the latter are nevertheless far from being perfect, some discussion about more complex alternatives could be welcome, without necessarily exploring them.
