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Interactive Comment

Interactive comment on "Uncertainties in future climate predictions due to convection parameterisations" *by* H. Rybka and H. Tost

Anonymous Referee #2

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The paper "Uncertainties in future climate predictions due to convective parameterisations" tests the sensitivity of climate simulations to the use of different convective parameterizations. In contrast to a previous paper, the focus is on the impact of a climate change scenario. They also give a little attention to sensitivity to horizontal resolution. I enjoyed reading the paper and would like to see it published with minor revisions. I have some suggestions that I think would improve the paper. The authors may or may not implement these proposed changes.

In this reviewer's opinion, to enhance the paper's scientific value, it could include more explanation of what is happening physically with the different convective parameterizations. First, I think a brief explanation – a paragraph or two - of the main physical differences in the parameterizations would be very helpful for the reader. The authors

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point the reader to a previous paper by Tost et al (2006), but this is an important enough aspect for this study that some information – especially also as it relates to the results - should be given. Additionally, one aspect that might be interesting would be to look at the averaged tendencies that actually are coming out of the convection schemes. Also, are there any impacts that are directly caused by the parameterizations ? To give examples, stronger downdrafts may lead to a direct cooling of the lowest levels, strong mid-level detrainment may lead to a signal in tracer transport as well as heating and drying rates (I'm thinking of the results for "EC" here). Or is the sensitivity caused almost exclusively from indirect effects such as radiation interactions or changed large-scale circulations? Finally, is there anything that a convective parameterization developer can learn from your study?

Did you see any major differences in the averaged precipitation distribution? Or a change in split between resolved and non-resolved precipitation?

Some specific comments:

Abstract: "e.g. the . . .. . . microphysics" What are you looking at in terms of microphysics? I think this part of the sentence does not belong here, or needs to be rephrased. It is included in the first part of this sentence, but is not a good "exempli gratia" in this reviewer's opinion. I think if you use e.g. here, put in more examples other than only simplified cloud microphysics.

Pg. 2: "The change of the earth's . . ..and outgoing radiation" I find this sentence hard to read.

pg.3, line 4: "(i.e., Arakawa and Schubert, 1974 . . ..Nober and Graf, 2005)" This list is far from complete. Are the authors only talking about convective schemes for global modeling or climate modeling? If not, many new interesting approaches (including stochastic parameterizations and "super-parameterizations") exist.

Bottom of Pg.5, section 2.2: Info about the individual parameterizations and their im-

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plementation is given in Tost et al. 2006. I think it would be useful to have a subsection here with the main differences, such as: what determines cloud top, what are the differences in the microphysics that are used, are there any differences in how the environment is modulated, is there anything that could lead to very different mass flux profiles?

Pg. 8, line 14: In general, I like your choice of graphics displays. But here it may be nice to see a magnification of the difference (difference field of the difference) . How sure are you about the different representation of microphysics causing these differences? Couldn't it also relate to mass flux assumptions? Could differences in entrainment/detrainment, downdrafts, or location of the cloud top cause these differences? See also my general comment. To me this looks like it could also be related to a detrainment level for a particular parameterization (also looking at dq and the radon differences, maybe due to mid-level clouds? Maybe over done in EC? Or under done in the other approaches?). An average of the tendencies from the parameterizations – if possibleâĂŤcould be useful for interpretation.

Pg. 9, line2-3: Can you find an argument for why dT is larger for the higher resolution run in the upper levels, and whether that trend might continue with even better resolution?

Pg. 9/10: Why are there such large dT differences over Africa and not so much over South America with the lower resolution? Is the behaviour of the different convection schemes similar for this comparison? Or are there significant regional differences among the simulations?

Pg. 14, section 4.4: Here you correctly bring in the influence of entrainment/detrainment rates, which I believe may indeed be the more important parameters impacting cloud types. Does the chosen microphysics (within the convective parameterizations) also have an impact? Do any of the parameterizations consider aerosol effects?

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Pg. 18 With “cloud scheme” you mean the online calculation with the ISCCP simulator?

Conclusions: Do you have any advice for convective parameterization developers? Any way to find out what needs improvement most? Or which of the schemes tested may be better for climate simulations?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 26893, 2013.

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