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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lightning and convection parameterisations – uncertainties inglobal modelling" *by* H. Tost et al.

Anonymous Referee #1

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*) General comments

In Tost et al. (2006) the authors implemented four state of the art convection parameterisations in the AC-GCM ECHAM5/MESSy and mainly discussed the effects on the hydrological cycle in the model.

Their new manuscript can be regarded as a continuation of this work, focussing on convective dynamics (e.g. convective mass fluxes). Four different lightning flash rate parameterisations are introduced to relate modelled cloud properties to observable flash rates.

This is an important task, and I really appreciate the effort the authors have made. Unfortunately, I cannot recommend this study for publication in its current form.

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There is strong evidence that none of the lightning parameterisations has been adjusted to the authors model before presenting this study. It is not enough to "adjust" an "individual scaling factor" which does nothing more than forcing the simulated total number of flashes into the range of observations. By this, the authors do by the no means present "uncertainties" of "global lightning modelling" but simply the well-known fact that the use of unadjusted parameterisations can lead to inexplicable results.

Please let me explain what the authors seem to have missed, exemplary for the Allen and Pickering (2002) parameterisation "A_updr": Allen and Pickering examined the relationship between observed flash rates and model-calculated mass flux fields coming from their convection scheme, which was a relaxed Arakawa-Schubert scheme. A fourth order polynomial was fit with the mass flux as the independent variable, and the flash rates as the dependent variable. That is were the parameters a-e (see equation 3 in the authors manuscript) come from. So, how can the authors expect that these parameters can be valid for a complete different model (E5/M1) with four different convection schemes (all producing different mass flux strengths and distributions)? (As no numerical values for parameters a-e are given by the authors, I have to assume that the Allen and Pickering setup was used.) The same holds for the "G_updr" parameterisation, where the original parameters from Grewe et al. (2001) have been used. These parameters cannot be valid for E5/M1 with all four different convection parameterisations. I will give just two examples from the authors text:

P6776 L7: "The combination of the B1 convection with the G_updr lightning results in very spotty flash occurrences." That looks like your "correlation" is dominated by the extreme values of the updraft distribution. Of course, this must not happen.

P6781 L14: "(...) local extrema govern the flash densities in this simulation setup." Again, this is exactly what must not happen and has to be avoided by adjusting the parameters.

Consequently, much more work has to be done by the authors before this study can be re-submitted, namely to adjust the lightning flash rate parameterisations for the different convection schemes in E5/M1.

(Furthermore, it looks like not even the underlying convection parameterisations are adjusted for E5/M1. This is an assumption and therefore not the reason why I reject the current study. Nevertheless, if the authors actual manuscript is still based on the simulation setup described in Tost et al. (2006) (please tell me if this is true or not), I'm not willing to review a revised version of this manuscript unless these severe failings are addressed/corrected: Tost et al. (2006) state that "no tuning of the model system has been applied"! E.g., globally averaged precipitation is not balanced by the evaporation in nearly all of the simulation setups (except for the "original" Tiedtke scheme). It is stated that some "results must be analysed with care". Though using a GCM it is stated that a "stable model climate" cannot be guarantied "for longer integration periods". Top of the atmosphere outgoing longwave radiation differs by up to -7.9 W/m2 between the simulations. Local differences in upper tropospheric temperatures of up to 10K are presented. Important micro-physical parameters like "efficiency of precipitation formation" are not adjusted. Personally, I think that presenting a study focussing on convection, using convection parameterisations that are not tuned, is hardly acceptable.)

*) Further comments

P6769 L9: The authors state that an extensive lightning climatology has been established over the last decade. I would like to encourage the authors to use this climatology in all their figures, not only in figure 6.

P6773 L11: "(...) the fractionating into cloud-to-ground and in-cloud CAN be determined." IS it done in the model? If so, do you assume different NO production for both kinds of flashes? How is the vertical distribution done? Is the Pickering et al. (1998) profile used for total emissions? Or just for those from cloud-to-ground flashes? How do you treat emissions from in-cloud flashes?

P6775 L15: "oceanic flash densities are (...) too low by approximately a factor of 2

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to 10." It would be interesting to additionally know about the global mean value of this factor.

P6776 L12: Here you are talking about the combination of G_updr and B1? In this case I do not think that "strong shallow convection" can be used as a possible explanation. The effect of shallow convection seems to be damped in G_updr by the use of the square root of the cloud thickness, see your equation (2). Furthermore, if shallow convection really had an influence on the flash rates, and consequently on the NO emissions, wouldn't we expect to see this as a second peak at lower altitudes in your vertical profiles (figure 7b)?

P6779 L16: Here you discuss figure 4? I.e., you applied TRMM cloud top height observations to P_cth (equation 1)? Then, how can you see an effect of "high surface elevation which leads to a smaller vertical extension of the cloud"? You do not have information on cloud thickness from TRMM, and even if you had such information, it does not go into the parameterisation of P_cth.

P6780 L5: "(...) a significant correlation (...) for the precipitation based lightning scheme results mainly from the suitable fit of the strong precipitation events (stronger than 7 mm/day) (...)". I find this sentence somewhat confusing, maybe because of the word "mainly". The restriction to convective events stronger than 7 mm/day is a prerequisite for the applicability of A_prec. So, wouldn't we expect that the "significant correlation" results EXCLUSIVELY from these events? Please explain.

P6782 L19: What do you mean by "also in agreement with the results of Nickolaenko et al. (2006)"? They used (nearly) the same observational data set as you did (OTD from 1995 to 2000), so of course I would expect that both "results" (i.e. observations) are in agreement? Please explain.

P6783 L28: "The latter effect CAN result from the different freezing altitude (...)". Just to be sure that I understood it right: the freezing altitude IS different, but one cannot say if this is the reason for the smaller magnitude of the emission maximum?

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P6784 L5: "(...) the overall amount of emitted NOx differs substantially (factor of 2) (...)". I do not understand this. You mean that you scaled the number of flashes (which is nice for comparing the figures, but which is of no importance in the model), but you did not scale the NO emissions per flash? Why not? Though it is not a major point for this study, I strongly encourage the authors to re-scale the emission per flash in their new version of this study. This will be necessary, at the latest, in the authors "following publication" investigating the "impacts of these emissions on atmospheric chemistry".

P6785 L15: "The convective cloud top heights differ substantially (...)". Could you please say for which model setup you found the best agreement with your TRMM observations?

P6786 L10: "Oceanic convection is almost as intense as continental convection with respect to the updraft strength (...)". You mean that in E5/M1 convective events over the ocean produce the same strong updrafts as events over continents? This would definitely be a major failing of your model system. Could you please provide frequency distributions for the updraft strength over land and sea to clarify this? I think this point is essential. If both distributions were the same, then obviously the prerequisites for the applicability of A_updr and G_updr would not be given in E5/M1, and it really would not make sense to discuss them in this study. (By the way: I really do not see why the use of "grid box mean values" should be any explanation for that?)

P6786 L16: Isn't it astonishing that with E5/M1 none of the different convection parameterisations seems to give reasonable results in the tropics? Not even the Tiedtke scheme, which I think was designed for tropical deep convection? It is possible that the weakness lies in the parameterisations themselves, but is this likely? Did the authors find any hints in the literature that support this assumption? I would like to strongly encourage the authors to further investigate this severe problem (maybe before resubmitting their new study?).

Figure 5: Please redraw the figure using monthly mean values for the model data.

Though of course this does not improve the results, it enhances the readability of the figure. (Your model output is archived every 5 hours (according to Joeckel et al., 2006)? In that case you may also use a 110-day running average centred on the 15th of each month, in order to be consistent with the LIS/OTD data.) Furthermore, looking at figure 6 in Allen and Pickering (2002) clearly shows that the simulation of a correct annual cycle is possible with A_updr. Please consider this when talking about "uncertainties" in the A_updr parameterisation.

*) The use of a relaxation technique

Though this is not a major point of my review, I strongly encourage the authors to present their new study without using a relaxation technique. Newtonian relaxation has the potential to strongly disturb the model physics, especially when looking at convective events. Personally, I do not think that all of the unexplained model behaviour is caused by the relaxation, but how can we be sure? Even the authors are not sure, as will be pointed out below, so presenting a free running study would eliminate at least this uncertainty.

P6773 L23: Why do the authors present a single model year, though using a AC-GCM (which should be designed to be run over decades)? I would like to encourage the authors to present a longer simulation period in their new study (e.g. 6 years, as it was done by Tost et al., 2006).

P6773 L22: "Because of the feedback (...) the meteorology is different for each simulation". This is correct, but a one by one comparison does not seem to be essential in this study. Do the authors expect a significant year to year variability in the lightning distribution? If so: how do you justify the use of the year 1999? Was it a "typical" year?

P6774 L1: What do you mean by "(...) the influence of the nudging is RELATIVELY small (...)"? What is your measure for "small"? What time scales did you use for the relaxation? (I could not find values for a T42 model version with 31 layers in either of the three references on page 6773 line 26.)

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P6777 L2: "(...) this MIGHT be partly caused by the nudging". Why do you say "might"? As you say the influence of nudging is "relatively small" (see above) I suppose you compared two studies with and without relaxation? So you can clearly say if it IS an effect of nudging or not. Please do so.

P6777 L2 (continued): This sounds like you are performing relaxation in the boundary layer? Why? According to Joeckel et al. (2006) this should be avoided due to "possible inconsistencies between the boundary layer representations of the ECMWF and ECHAM5 models".

P6777 L5: Again, please do not say "this CAN cause a decrease of the convective activity". You can easily compare two studies with and without relaxation and present a precise statement. Please, do so! If relaxation really decreases convective activity, this would be a strong argument for not using relaxation in a study focussing on convection.

P6777 L6: "A simulation without nudging showed substantially stronger mass fluxes in the middle and upper troposphere (...)". But then this is a contradiction to your statement that "the influence of the nudging is relatively small".

P6786 L9: "However, this is MAYBE caused by the nudging." Please, if you decide to present a study using a relaxation technique, you really have to investigate its impact on your model results. Obviously you performed studies without nudging (see page 6777 line 6), so just saying "maybe" or "might be" or "can cause" is hardly acceptable.

*) References: All references are from the authors manuscript.

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