Atmos. Chem. Phys. Discuss., 7, S2799–S2806, 2007
www.atmos-chem-phys-discuss.net/7/S2799/2007/

Atmospheric
Chemistry
and Physics
Discussions

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

H. Tost et al.

Received and published: 29 June 2007

Reply to Referee #1:

We are grateful for the detailed comments, however, Referee#1 is very critical, and in many respects addresses model aspects that have been published previously. We find that some of the demands by Ref#1 cannot be met, and partly they are unreasonable. In our view it is not the focus of this study to adjust and "tune" model components, e.g. convection and lightning parameterisations, to the model system ECHAM5/MESSy, but to show that the lightning schemes as they are presented in the literature cannot not be straight-forwardly implemented in current GCMs. According to Ref#1 this is a "well-known fact" (but we perceive this differently). Moreover, in the descriptions of the parameterisations it is neither mentioned how to adjust the schemes to different models beyond individual scaling factors to observed lightning frequencies, nor that this is required. In fact, the scientific publication of parameterisations should serve the reproducibility of results without unexplained adjustment factors, so that the requirement by Ref#1 is in conflict with good scientific practice.

Nevertheless, on page 6784 of the manuscript it is mentioned that “....a new set of parameters might be required to give a better representation of the different convective conditions....”. In a revised manuscript this statement will be extended not only for reasons of resolution, but also the model physics in general and will be mentioned in the conclusions. Indeed, Allen and Pickering (2002) examined the relationship between observed flash rates and model-calculated mass flux fields from their convection scheme. Furthermore, Allen and Pickering(2002) refer to “lightning flash parameterisations for use in a global chemical transport model”, i.e. it should be applicable on the global scale.

We do not agree that the application of a mass flux parameterisation to different convection schemes is not valid. All schemes produce convective mass fluxes. Allen and Pickering did not mention that their parameterisation is only valid for their model. This is also not mentioned in the description of the Grewe et al.(2001) scheme, also based on the mass fluxes computed by the convection parameterisation. Furthermore, in our study we analyse the robustness of lightning parameterisations, e.g. for calculations under perturbed conditions (e.g. climate change scenarios), so that the tuning to present-day conditions would not be valid.

As we stated in the manuscript a failure/chaotic behaviour of such a parameterisation in combination with different convection schemes, does of course not mean that the parameterisation is not valid, but that there are weaknesses in the convection parameterisation itself or its behaviour in a atmospheric general circulation model (e.g. Page 6786,line 3-5) or in combinations of both parameterisations.

We agree that the spotty flash occurrences of the B1/G_updr combination should not happen. However, we analysed the reasons for this behaviour. Since the B1 convection parameterisation performs well with respect to the hydrological cycle and the outgoing long-wave radiation without any retuning (Tost et al., 2006), it is not obvious how this weakness can be overcome. Therefore, we found this lightning parameterisation study useful for testing the realistic behaviour of the convection parameterisations in the model, being one of the conclusions. In a revised version this will be emphasized more clearly.

Ref#1 mentions "it looks like not even the underlying convection parameterisations are adjusted to E5M1”. For this we refer to Tost et al. (2006). Ref#1 mentions that this is not the reason why the current study is rejected, but the next sentence states that he "is not willing to review a revised version of the manuscript unless these severe failings are addressed/corrected". It is quite apparent that Ref#1 is biased and the statement is not appropriate. Citations in the review are taken out of the context of the original manuscript by Tost et al.(2006), i.e.:” None of the simulations show a trend in the global average energy fluxes over the simulation period. Therefore, a stable model climate has been achieved in all cases.....”. Since no simulations with a longer simulation period than in Tost et al.(2006) have been published it “cannot be stated that this is also the case for longer integration periods”. Furthermore, the OLR values cited by Ref#1 are the most extreme ones and are not valid for all setups used in this study. The fact that the evaporation is not fully balanced by precipitation is a weakness of the convection parameterisation or its implementation in the GCM, which must be overcome for studies in which this is of importance, e.g. climate simulations, but is of minor relevance in the current study. Ref#1 states that "presenting a study focussing on convection, using convection parameterisations that are not tuned, is hardly acceptable". Why? Usually, convection parameterisations are tuned to TOA radiative fluxes, not to mass fluxes or precipitation rates as relevant for the present study. Furthermore, the precipitation distribution is even closer to the observations (as shown in Tost et al., 2006) with some of the alternative convection schemes.

Since we propose to reject most of the criticisms by Ref#1 in the general comments, we do not intend to completely revise the manuscript. Nevertheless, below we will respond to the additional comments since most contribute to the improvement of our manuscript:

P6769 L9: Since we applied the nudging technique to simulate the large-scale meteorological conditions of this particular year, a comparison with a climatology would provide little information. It is much more consistent to use observations coincident with the simulated meteorological conditions. Since for the daily climatology this data only exists for the year 1999, and the available time series does not have a time resolution sufficient enough to derive the diurnal cycle we indeed have to use a climatology for this parameter.

P6773 L11: This is actually done in the model. Both CG and IC flashes contribute to the emissions. The vertical distribution is performed according to Pickering et al (1998), as mentioned in the “Simulation setup” section. This will be clarified in greater detail in the revised manuscript version.

P6775 L15: This factor can be determined and will be added in a revised version.

P6776 L12: It is correct that the square root of the cloud thickness is included, but the “w” of Eq. 2 is a weighted average of the vertical motion. Consequently a very strong shallow convection (e.g. by a factor of 10 stronger than in the upper troposphere over 300 meters has the same influence as a 10 times weaker updraft over 3 km vertical extension of the cloud. A minimum extension of 3 km of a convective cloud is currently used as a requirement for possible lightning production. The total upward mass flux results from an ensemble of individual convective cloud events, both of shallow and deep convection type, leading to a vertical cloud extent sufficient to produce lightning, and also the strong mass fluxes in the lower troposphere characteristic for shallow convection. Therefore, shallow convection can contribute substantially to the total vertical velocity as used in Eq.2. Furthermore, since the spotty behaviour of B1/G_updr does not occur in the same simulation using the A_updr scheme taking the massfluxes at

0.44 sigma, this indicates the relevance of shallow convection. A second peak in the profile cannot be expected since the NOx is distributed in a C-shape between cloud top and cloud bottom of the cloud ensemble.

P6779 L16: As mentioned in the work of Price and Rind (1997) the cloud top height must be used as height above ground. Ref#1 correctly states that from the TRMM satellite only the cloud top height can be observed. Nevertheless, for a correct interpretation and implementation in the P_cth scheme this cloud top height must be seen in reference to the surface elevation. Consequently, the maximum vertical extension of the cloud is “cloud top” - “surface height”. This parameter has been used in Sect. 4.2 in combination with the P_cth scheme.

P6780 L5: This sentence will be reformulated. The strong precipitation events are not accessible from this dataset. We agree that this is probably the reason. This will be checked for the revised manuscript using the TRMM 3B42 product (3 hourly precipitation fields, which will contain the individual strong convective events), analysing a potential correlation between the observed flashes and this precipitation data (work currently in progress).

P6782 L19: A detailed analysis of the diurnal cycle of lightning has been performed by Nickolaenko et al.(2006); therefore we refer to it. Of course, since they use the same observations the observational patterns are similar. However, in our study we can show that the simulations using P_cth are in agreement with the observations.

P6783 L28: Due to the different meteorology produced by the convection schemes, the temperature profiles in the convective clouds are different. This results in different freezing altitudes. However, whether this is the dominant effect for the emission maximum and its location or whether there are other effects cannot be concluded.

P6784 L5: The number of flashes is rescaled to the observed flash rates. The same rescaling factors have been used for the emissions, but this results nevertheless in differences in the emission profiles (total amount of NOx, shape of profiles). The NO production per flash is a “constant” thus assuming that all flashes produce the same amount. (A differentiation between CG and IC flashes in the NOx per flash is taken into account, even though this is heavily under discussion; the values will be given in the revised model description). The differences must therefore result mainly from the different cloud top heights, freezing levels and the spectrum of convective and lightning events (large number of weak lightning activity versus low number of strong lightning activity events).

P6785 L15: The convective cloud top height is currently only indirectly compared by the derived product of the flash densities using the P_cth approach, but the statement which parameterisation agrees best with respect to the cloud top height can be added in the revised version (even though this hardly gives any new information compared to the P_cth derived flashes).

P6786 L10: The frequency spectra requested by Ref#1 would not help since the underlying problem results from the convection schemes. The individual ensemble members of mass fluxes are not accessible in the parameterisations, but only the effects of convection on the large-scale motion (this is the aim of a convection parameterisation). Consequently, from the schemes it is not possible to distinguish between a large number of small/weak convective subgrid cell clouds or a small number of large/strong convective clouds representing the “grid box mean value”. In fact, this is the main difference between oceanic and continental convection with respect to convective dynamics whereas it is only of minor importance for the large-scale circulation. We doubt that this is a problem restricted to E5/M1, but rather to parameterised convection in general, and therefore a solution with the current type of convection schemes does not exist. However, the use of convection super-parameterisations might partly overcome some of these problems, but these are still too computationally demanding to be used together with complex atmospheric chemistry.

P6786 L16: Convection parameterisations (including tuning in a model system) should represent realistic distributions of precipitation, radiation and temperature profiles.

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These requirements are met by most of the schemes (see Tost et al., 2006), whereas the convective dynamics are only of minor relevance in the development/application of a convection scheme; for instance the position of the ITCZ is probably o.k., but the flash density in the ITCZ crossing the equator is not captured accurately. We will reformulate some of the statements accordingly.

Figure 5: We archived hourly model output for the lightning data (otherwise the diurnal cycle could not be captured accurately). We could provide monthly mean values for the Figure but prefer to show that the variability in the model from day to day is very high (we applied a running mean of 15 days of the pre-calculated daily mean values). We do not question the results of Allen and Pickering (2002), but with our model we are not able to reproduce their findings. Consequently, it is not an easy task to produce correct temporal and special distribution of lightning in global models. If a comparison of the lightning data of several "tuned" models (comparable to an IPCC comparison) would be performed, we would expect a similar range of variation in the model which we define as an "uncertainty" with respect to this process.

To the use of the relaxation technique: We agree that the relaxation or "nudging" perturbs the model physics, and some effects do not occur in a simulation without this approach. Only temperature, divergence, vorticity and the surface pressure are very weakly relaxed against analysis data. This influences the convection scheme only indirectly via the temperature profile, the saturation water vapour, advected moisture, etc.. However, the use of the relaxation has been chosen since it provides the opportunity for point-to-point-comparisons with observations without statistical approaches and simulation periods of several decades, which are computationally expensive if atmospheric chemistry is simulated in detail. Since this is one of the main foci of future studies, we applied our analysis to model data obtained including the nudging technique. We selected the year 1999 for a detailed comparison with observations since both LIS/OTD and TRMM data are available for this specific year and this year is not affected by an El Niño event. We currently cannot provide a statement about the interannual variability of all the possible combinations, however, as shown in Jöckel et al.(2006), the variability of global NOx emissions using the T1/P_cth combination is only minor, whereas the El Niño event of 1998 has some effects with respect to the position and strength of the lightning activity.

P6774 L1: “The direct effect of the nudging is relatively small” means that the changes in temperature, pressure, etc. are of the order of only a few percent. However, the relaxation effects are directly applied to the large-scale quantities, while due to non-linearities a small perturbation can have some impact, e.g. changes in the strength of the mass fluxes. Since convection is a parameterised subgrid-scale process, the nudging effects on convection are difficult to analyse. Using a simulation with and without nudging, which has been done for this study as sensitivity tests results in different meteorological patterns and differences in convection, but it is not possible to infer whether the different large-scale meteorological patterns cause the changes in convection or the nudged temperature profiles.

P6777 L2: In the boundary layer no nudging has been applied, the relaxation coefficients follow a similar profile as in Jöckel et al. (2006) and are of similar strength.

The argumentation in a revised manuscript will be formulated less vague if the statements can be made from the analysis of the simulation without the relaxation. However, as mentioned above this is not straight-forward.

*) all citations can be found in the original manuscript

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