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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Transport and chemical transformations influenced by shallow cumulus over land" *by* J. Vilà-Guerau de Arellano et al.

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Review of acpd-2005-0274 'Transport and chemical transformations influenced by shallow cumulus over land'

Reviewed by Bruce Denby

GENERAL COMMENTS

This article describes a set of LES studies intended to give insight into the effect of shallow cumulus convection on the chemical and transport processes in the atmospheric boundary layer. It carries out a set of studies using 2 LES models that look at 3 major points: 1) Dilution of the ABL concentrations by enhanced ABL growth, 2) The

extent to which turbulent mixing controls species reactivity and 3) perturbation of the photo-dissociation rate as a result of cloud formation.

The article is very well written and generally well structured. It is interesting to see LES models being used to study these effects and particularly that 2 separate models give quite similar results. The authors have a tendency to overemphasize the extremes in the study results, as do we all, and some of the conclusions in the study may not have required LES modelling for the process description but, nonetheless, the article does provide some insight into the different processes involved.

The manuscript should be accepted, with minor changes.

The following list is taken directly from the suggestions given to reviewers

1) Does the paper address relevant scientific questions within the scope of ACP? Yes 2) Does the paper present novel concepts, ideas, tools, or data? Yes 3) Are substantial conclusions reached? Yes 4) Are the scientific methods and assumptions valid and clearly outlined? Generally, yes 5) Are the results sufficient to support the interpretations and conclusions? Yes 6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes 7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes 8) Does the title clearly reflect the contents of the paper? Yes 9) Does the abstract provide a concise and complete summary? Yes 10) Is the overall presentation well structured and clear? Yes 11) Is the language fluent and precise? Yes 12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes 13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes, see comments below 14) Are the number and quality of references appropriate? Yes 15) Is the amount and quality of supplementary material appropriate? N/A

SPECIFIC COMMENTS

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p.1 par. 1: The authors state that shallow cumulus forms under the same conditions as pollutants tend to accumulate. Can the authors be more specific? Pollutants accumulate with low wind speeds, low ABL heights, poor mixing and stable conditions. Are these the conditions for shallow cumulus convection?

p.2 par. 2: “increase/decrease” can you clarify this? Do you mean both or just a non-defined change.

p.2 par. 3, Last sentence: This is the first time such a study has been carried out with two models. Am I to infer that this sort of study has been carried out with one model? If so references to these here would be useful.

p. 4, equ. 1,2: The reactive species being discussed clearly refers to the NOx, Ox reactions. Is there a particular reason why the authors refer to them as A, B and C rather than NO2, NO and O3? It does not make it any clearer to this reviewer. It may be useful for the reader to know the photostationary equilibrium of these species, given the standard clear sky disassociation rates and concentrations modelled. This would give the reader, especially in Section 4, a clearer idea of how different the LES results are from a simple well mixed boundary layer description.

p. 4: The surface fluxes used seem to indicate a rather high percentage of emitted NO2 for the total NOx emissions. Is there a reason for this? Is it already close to equilibrum?

Equation 6: Is there a reference to this equation or has it been derived?

p. 8. par. 1: This paragraph, in a way, gives the conclusion of the first study. That being that it is very important to accurately describe the effect of shallow cumulus on boundary layer dynamics in large scale CTMs. This, in this reviewers opinion, over emphasises that importance. Though one can point to a 50% difference in average concentration in the ABL, due to it’s increased extent, concentrations within the majority of the ABL do not actually vary so much at all, with a maximum decrease of 12%. The authors then go on to state that it is essential to accurately know various boundary

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conditions, e.g soil moisture, in such large scale models. This is an unrealistic request considering the sensitivity of the formation of Cu to the surface conditions. This reviewer would actually consider the most important effect of Cu, in regard to large scale models, to be the enhancement of the exchange of pollutants between the ABL with the free atmosphere. This is inferred to, when the night time residual layer is mentioned, but not discussed. (The final note in the conclusion actually refers to this as a future study, which this reviewer would strongly agree to).

p. 9 par 2: 'Ĕ greater cloud cover (<=0.5; the simulation Ĕ' This text is unclear to me. Does the simulation with greater cloud cover have a cloud cover less than 0.5?

Sec. 5: This section seems to mix two concepts. That being the effect of clouds on the photolysis rates, and hence on the concentration of the reactant species, and the effect of the Damkohler number. Never the less the authors try to use this to demonstrate that given particular sets of chemical reactions with Da>1 the effect of clouds on photolysis can be significant. It does not seem, however, suitable to dwell very long on the instantaneous results, rather than temporal and spatially averaged ones. The authors do not look at the instantaneous values of any other parameter, e.g. concentrations or vertical velocities, and the resulting discussion implies a significance that is truly misleading.

Sec. 6 Conclusions: There is a tendency in the conclusion to overstate the results of the study. The reviewer recommends these be reformulated to reflect the true situation, as a result of the study, rather than to try to exaggerate them. Firstly: the authors state 'Ĕ that the presence of clouds could lead to a decrease of 50% of the reactant mixing ratio ...'. They neglect to point out that this is a boundary layer depth average (including the Cu convection region), not a below cloud average, which shows a significantly lesser decrease. Secondly: they state that mass-flux parameterisations underestimate the flux at cloud base by a factor of 2. In the text this is given as a factor of 1.5. Thirdly: They state that as a result of the perturbation of the photolysis rates, due to the presence of Cu, that the instantaneous effect is of the order of 40%. They then

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state that these are smaller when averaged over time and space. In fact, they are significantly smaller. (See comment on Section 5)

TECHNICAL CORRECTIONS

The article was very well written and just a very few comments are given concerning grammar and other technical aspects

Throughout the paper: Understanding of the processes does not require continuous reference to UTC, in fact it is a little distracting. Local time is the important parameter to be considered and should be used throughout the paper and in the figures. One reference to the time difference between UTC and LT is sufficient..

Sec. 2.1 par. 1, grammar: “which are the driving forces for the formation of clouds”

Sec 2.3 par. 1, grammar: ‘Ĕ of the LES to reproduce the dynamics of a ..’

Sec. 2.4 par. 2: ‘(Equation 3)’

p. 8 par. 3: ‘Ĕ the average value of a generic scalarĔ’

p. 11 par. 2: ‘Ĕ by temporal and spatial averaging.’

p. 12 par. 2: ‘Ĕ that uses a photodissociation rate perturbed by Ĕ’

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 8811, 2005.