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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling the aerosol chemical composition of the tropopause over the Tibetan Plateau during the Asian summer monsoon" *by* Jianzhong Ma et al.

Anonymous Referee #1

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General comments

Ma et al. study the composition of the Asian Tropopause Aerosol Layer (ATAL), a feature associated with the upper tropospheric Asian Summer Monsoon (ASM) anticyclone. The ASM, and particularly ATAL, has received quite some attention recently. Timely publication of this paper might e.g. inform upcoming studies from the StratoClim project (http://www.stratoclim.org/), which addresses similar research questions.

The paper is based on a high resolution global simulation with a detailed representation of gas phase chemistry, aerosols and particularly mineral dust. Previous studies disagree on the ATAL composition. Ma et al. highlight the importance of the latter, also

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Discussion paper

discussing the source areas. The methodology is sound, the paper nicely complements previous studies, it is very well written and well suited for ACP(D).

Before publication in ACP the following three aspects need to be addressed, which might or might not need major revisions:

(1) From the references provided it is not clear to me, if the used or a similar setup has ever been evaluated for the EMAC model. If there is such an evaluation, please specifically refer to it. If there is no such evaluation of the T106L90 resolution with chemistry yet, a separate evaluation paper (e.g. in GMD) might be considered - or at least a dedicated supplement. The solitary comparison to a measured profile of aerosol extinction coefficients is just a start. Furthermore, there are a lot of comparative statements about the simulation in the text. It is often not clear, whether those refer to observations, other simulations, or something else. This needs to be clarified, e.g. by an explicit evaluation against observations or other simulations.

(2) Convection and the associated emission of lightning NOx plays a central role in the ASM anticyclone, affecting also nitrate aerosols. Convection and lightning NOx emissions are known to depend on model resolution. How does the simulation perform with respect to observed convection? Are the lightning NOx emissions in a reasonable range, globally and within the anticyclone? Those are notorious uncertainties in modelling, but the discussion of the relative importance of nitrate aerosols depends on the availability of NOx.

(3) Please consider to add a statement about data availability (model code, setup).

Specific comments

In the following URLs or full citations are only given for references not provided in the draft. Otherwise please refer to the references' list in Ma et al.

P1L23: emissions, chemistry and *transport*

P2L3: Isn't the UTLS rather defined by the exchange processes between stratosphere and troposphere than by some exact distance to the (which?) tropopause? Please discuss or specifically provide a citation for that definition.

P2L15: The ASM anticyclone is not only driven by deep convection, but also by the interaction of orography with a sea breeze and heating of the Tibetan plateau. No need to discuss in detail. Just appreciate the complexity, particularly since your simulation represents topography better than coarser simulations (see P4L18).

P4L18: The Jöckel et al. papers often refer to T42L90, which seems to be some kind of standard resolution for EMAC and should also be mentioned here.

P5L1: If there is an evaluation for T106L90, please specifically refer to that.

P5L5: Consider adding Jöckel et al. (2016) to the listing of EMAC model evaluation studies.

P5L9: Consider to move the statement about spin-up from the end of the section to here.

P5L11: Please provide more details about the chemical mechanism or provide a citation to make it reproducible.

P6L7: Isn't the aerosol just facilitating gas phase reactions in this context, i.e. educts and products are both gaseous?

P6L23: Is there something like a "DLR-MACCity" inventory? Is this MACCity with species redistributed to the chemical mechanism used by Jöckel et al. (2016)?

P6L25: Please elaborate to avoid quotation marks. Is the worst case scenario the best match for real emissions in that period?

P6L28: Lightning emissions depend on model resolution and parameterizations contain tuning factors. How big is your annual global lightning NOx emissions or towards which value did you tune? Please consider adding a more thorough analysis and dis-

cussion of lightning NOx, because this is a crucial aspect of atmospheric composition in the ASM anticyclone (Barret et al., 2016; Gottschaldt et al., 2018) and affects nitrate aerosols.

P7L16: Is “crustal species” = minerals of the Earth’s crust? Why do the given species not sum up to 100 percent?

P7L21: Do you use the same inventory for volcanic SO2 as Brühl et al (2018)?

P7L34: Were there any major eruptions that should have been included?

P8L9: Is this one of the nudging profiles used by Jöckel et al. (2016)?

P8L24: “regionally averaged” . . . Which region? The one covered by fig. 2?

P8L27: There is eddy shedding to the east and to the west of the ASM anticyclone.

P8L31: “well represented” . . . Such a statement needs comparisons to observations.

P9L56: Those studies did not use EMAC, did they? Consider rephrasing.

P9L8: Why not showing the average of multiple years? Is 2011 somehow representative for the climatologigal mean state?

P9L18: Consider scanning the literature for results of the StratoClim campaign(s). For instance, Brunamonti et al. 2018 (https://www.atmos-chem-phys.net/18/15937/2018/) show aerosol measurements within the ASM anticyclone.

P9L33: Consider putting this into perspective by discussing differences between 2011 and 2015.

P10L5: "reproduced" ... To which observations do you refer here?

P10L9: Are there observations for 2011, showing a similar anti-ATAL effect?

P10L32: Why do you focus on 2011, given that it is an unusual year for ATAL?

P11L22: This is only true without dynamic instabilities (see e.g. Gottschaldt et al.,

2018). Please consider a more differentiated formulation.

P11L31: This statement seems to contradict the previous statement about blocking (P11L22).

P12L13: Is there a reason ror not using isentropic coordinates, which should better reflect lateral transport?

P12L15: Consider rephrasing the rationale for not showing those: Ok for comparisons to corresponding observations, but distinction might make sense for analysing model results only.

P12L26: Do you refer to the interior or the surroundings of the ASM anticyclone here?

P13L7: It's probably not only due to the dust scheme, but also to the high resolution.

P13L10: Did Fadnavis et al. not consider volcanic sulfate, e.g. via some inventory?

P13L18: Such a striking discrepancy deserves elaboration. A thorough discussion of the differences might be difficult, but are there any ideas to explain this?

P13L23: This is much less than simulated by EMAC (fig. 10, 11) -> Reasons for this discrepancy?

P14L4: Consider a more cautious formulation here, because there is no consensus with other studies.

P14L19: Tagging the emission regions would help here. However, the connection of the plume over the Tibetan plateau to the surface is convincing. Consider to substitute "spatial distribution" by a more explicit formulation.

P14L23: This is strange, because near surface concentrations in Fig. 12 are not increased north of the conduit. However, it's hard to judge from fig. 3. The apparent shift to the north could just be an artefact of the colour scale and the white line over the maximum in Fig. 3. Please clarify.

P15L15-17: Didn't Brühl et al. just consider T106L31? Please try to disentangle the effects of vertical and horizontal resolution more clearly.

P15L25-28: Do you recommend T106L90 for this type of study in general OR only if there was some improvements to the convection scheme?

P15L25-28: T63L90 fits best to observations, because the convection scheme was developed for this resolution. Is this correct?

P15L25-28: You claim that T106L90 is better for transport over complex terrain. That is intuitively clear, but strictly would need to be shown. However, what is dominating transport to the UTLS: convection or orographic forcing from the terrain?

P15L25-28: Another point to consider is whether or not the occurrence of convective events is represented more realistically by T63 or T106. Please discuss those aspects to better justify your recommendation for T106L90. Consider comparing convection or lightning activity to observations to get a better idea of how realistically your simulation is (see also General comments).

P15L32: "improve" . . . Compared to what?

P16L1: "enhance" . . . Compared to what?

P28L4: Consider using SI units

P30L3: Confusing description of the quantity: Do you mean just the tropospheric burden?

Technical corrections

P31, Fig. 4: Figure blurry -> consider higher resolution or a vector graphics format

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