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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of topography on black carbon transport to the southern Tibetan Plateau during pre-monsoon season and its climatic implication" *by* Meixin Zhang et al.

Anonymous Referee #1

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General comments:

The significance as addressed by "Impact of topography on black carbon transport to the southern Tibetan Plateau during pre-monsoon season and its climatic implication" is backed here. Understanding the sources and transport of aerosols becomes a hot topic in regional environmental studies because of their serious influence on the environment, climate, and (more vitally, human health). This could be very interesting and important coming to the Tibetan Plateau, an elevated region with relative few human activities which seems to be isolated from the world, considering its role in global atmospheric circulations and water resources feeding billions of people. Simulating the

transport is one of the most powerful approaches, but becomes very challenging to this region due to the complex topography.

However, I concern about the quality in science as well as presentation, as explained below.

Of firstly questioned about the scientific quality is the application of nudging, which dumps the physics of model leading to energy unbalanced. As the authors intended to investigate the impact of topography, the experiments should then be precisely controlled as the difference comes from the representation of topography. Obviously, the nudging violates the control, bringing varying information from the forcing reanalysis data. This means that the difference between simulations may also be contributed by, in addition to resolutions, the reanalysis data via nudging.

Of second questioned is the conclusion from their results; it is unclear that if it is because of the more valleys resolved, though the 4-km simulation yields larger BC flux which is somehow associated with the valleys resolved by the 4-km resolution (NOTE: not the valleys resolved by 20-km). Fine resolution may result to more valleys, but these valleys meanwhile become small and irregular shaped. Moreover, complex terrain tends to yield weak near-surface wind speed due to the stronger orographic drag in both forms of gravity wave and turbulence.

Of third questioned is that some regional modeling studies (not CHEM-focused) over this region were ignored by the authors, but these studies are close related to the concerned topic. These studies generally found that fine-resolution simulations yield weaker surface wind speed compared to coarse-resolution, which is opposite to this study. This deserves a further check or discussion.

Of final questioned is the balance between their short-period simulations (focusing on a special case) and their climatic implication.

With regards to the presentation quality, there are too many stuffs (especially in sec-

tions of Introduction and Methodology) that are not directly related to the main topic presented but some vital information missing. The latter is fatal because it led to the lack of reasonability of their design of the model experiment. In particular, I would not to say that the authors presented Methodology correctly, which is expected to state how to deal with the question argued in the Introduction and why the approach(es) can be appropriate to resolve the question. To be more detailed, I found no text addressed why the authors chose WRF-CHEM, why did nudging, why selected those parametrization schemes, and how these approaches are related to their goal (to answer how the representation of topography impacts on simulation of BC transport). Moreover, descriptions of some analyses were also missing: 1) how the flux was calculated? based on model levels or interpolated pressure levels? 2) If it is the latter, how the influence of interpolation was considered? 3) Have the u and v been rotated? 4) How was the difference between different resolutions (grid spacing) calculated? regrided? and how? 5) and so on.

The language may also required to be polished by a native speaker. The problem is not much with the grammar but the lack of logic in the context, which could be due to inappropriate usage of some words.

Specific comments:

Section 2.1.1: Most of the model description are not related to and cannot assist to resolve the main issue. However, specific description of some diagnosis used in the analyses were not presented.

L199-200: Does the model use z vertical coordination as revealed by fig2?

L205-207: Why 'probability distribution' (actually not pdf but normalized histogram as presented by Fig S1) to reveal the difference in topography?

L208-209: Why the simulation period and analysis period?

L210-211: ECMWF has many products of reanalysis data; which?

L212: Why u, v, T but not PHI?

L213-214: Citation here refers to?

L216: Identical wave number for both domains? If so, why?

L221-227: Simulation period is 2016 but the quasi-global simulation that provide chemical initial and boundary conditions is done before 2013, considering the reference cited herein?

Section 2.1.2: A completed table of model configuration here could be better

Section 2.1.3: Emissions data described seem older that 2016?

L247: Biomass burning emission not of anthropogenic?

L262: 'nadir'?

L265: 'identical'?

L269: Why 'AOD at 600 nm', while MODIS AOD at 550 nm?

L273-277: BC measurement: when? how? uncertainty?

Section 3.1: The initial chemical condition and the emission at the two sites of the simulation should be presented so as to discuss simulated transport of BC; moreover, the difference of terrain height (similar to fig5c) could reveal something.

L302-304: Why? Because of convergence? Or just because of the direction towards the TP?

L317-318: Meaningless to compare column and surface BC (fig5 vs fig8)

L321-322: Something represents local circulation thanks to the difference to that of upper-air?

L333-336: Reasonably? No, the transport is not related to the concentration change, but the divergence is.

Section 3.2: A) I would rather expect two separated parts of flux, height-crossline plot of BC concentration and wind speed, so that we can diagnose the difference is due to either overall more column BC or wind speed, or both of them. B) I would also expect spatial pattern of column (or lower model levels) BC transport.

L346-347: A) Prevailing westerlies, but 'northward' or 'southward' accounted here? B) Can it be sensitive to the cross-line defined? How will the result be move the cross-line towards or backwards the TP? See fig11, lower daytime transport towards TP than nighttime at north to $\sim$29.5 deg N.

L353-357: Again, why diurnal cycle of local circulation while daily mean of large scale circulation?

L359-360: It seems to be truetrue to explain the diurnal cycle. But, 4-km simulation seems has shallower PBL compared to 20-km while larger BC transport than 20-km? Explanation?

L368-390: A) Two slices can serve as an example but cannot be used to draw a general conclusion; B) If BC transport can or not overcome ridges more depends on the height of the ridge and the vertical profile of BC concentration, as well as wind direction; as A), only two slices are insufficient to draw a general conclusion that BC transport can overcome ridges, and this conclusion is lack of a certain context (how high the ridges are).

L391-392: Can the result shown in fig13 be sensitive to the location of the cross-line? It needs a check.

L410-421: It is unclear how the authors applied the 20-km resolution topography to the 4-km simulation. Does it mean that 5 by 5 grids at 4-km resolution have identical terrain height as the corresponding grid of 20-km resolution? If it is of this case (I guess it is), does it really represent a 20-km resolution topography? Thinking about the slope of neighbouring grids (0, 0, 0, 0, a huge value, 0, 0 ...)? ... NO, this check (if it is

topographical impact) makes no sense.

L428: fig15: How about the region other than the TP, especially the south? (For fig5, 8, 16, 17, why the region other than the TP is masked? Without this part as well as boundary conditions, it is not able to check the mass balance, which is however fatal for understand transport)

Section 3.3: The snow difference between different resolutions further indicates that not only topography play a role in the model experiments. For example, the adaptation of physical schemes to different resolution may also play a role.

Technical comments:

L259: Abbreviation without the full name that it stands for. Here 'MODIS' as an example. Please recheck.

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