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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Impact of planetary boundary layer turbulence on model climate and tracer transport" *by* E. L. McGrath-Spangler et al.

Anonymous Referee #1

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OVERVIEW

This paper presents results of a sensitivity study with the GEOS-5 atmospheric GCM. What might be thought of as a small detail in the turbulence scheme is changed: the turbulence length scale. Three alternative formulations for the length scale are tested, each are estimates of PBL depth that have been studied in other contexts; two are eddy diffusion coefficient based and one is based on the bulk Richardson number. The results are presented by exploring the impact on mean climate, with a particular emphasis on aerosol and trace gas distributions. Since the determination of this length scale plays directly into the local turbulent mixing, it has important consequences for the structure of the boundary layer and lower troposphere, including mixing between (near) surface quantities and the free troposphere. The large-scale circulation is ul-

timately affected by the choice, and the text shows differences in the low-level winds, temperature, specific humidity, and surface pressure to drive that point home. Because of the impact on mixing, the aerosol optical thickness is altered (though the pattern remains qualitatively similar for the three schemes). Changes in the dust distribution are emphasized, since the emission of dust is related to winds which are also changed by the model changes. The CO and CO2 distributions are similarly explored. The main difference among the schemes seems to be between the K-based and Ri-based schemes, with the Ri-based one having a shallower nighttime boundary layer. This study follows a diagnostic study by some of the authors that was previously published in ACP, which is probably the reason this manuscript was submitted to ACP rather than a model-development journal or a journal with more focus on large-scale climate phenomena; this choice seems fine to me, especially given the emphasis on aerosol and trace gas distributions. The methodology and analysis is reasonable, and the results are interesting on their own as a study of the impact of the boundary layer on global climate. There are some weaknesses in the paper that could be addressed in a revised manuscript. Generally the paper comes off as a little too much "show-and-tell" and is a little light on providing an assessment of the processes that are leading to the differences among the schemes. One glaring omission is that there is no evidence for this diurnal effect of the Ri-based scheme except to reference the previous diagnostic study; much of the explanation for the results falls onto understanding the diurnal variation of the boundary layer and how it differs with different forms of the length scale, so I think there should be a section/subsection devoted to a more detailed discussion of it.

COMMENTS

1. Introduction - This introduction works fairly well, but I was struck by the strong emphasis on aerosol effects. Since the PBL depth (I'll call it h) is being used to control the strength of turbulence, there are more fundamental processes tied up with the changes being made to the model such as cloud cover, transitions between convective

regimes, moistening of the lower troposphere, etc. Also dynamics like low-level jets will be impacted by nocturnal changes in h (potentially), as are things like the placement, strength, and geometry of convective zones. Some of these are dealt with later in the paper, but I was surprised that the role of the boundary layer in moderating the global circulation, energy and water cycles wasn't stated more strongly in the first few paragraphs.

2. pg 31631, line 6: "the cubed sphere dynamical core" - I think this is not quite the right way to say this. It is a finite volume dynamical core that happens to be using a cubed sphere grid.

3. Section 2.1: I found it interesting that changing this length scale is only altering the local part of the turbulent mixing (if I understand correctly), and changes in the state will impact the non-local turbulence indirectly only. It does bring up the question of what the relative roles of the local and nonlocal mixing are. This study shows the local component's influence (mostly). Since the nonlocal is especially relevant for thermals and cloud-top driven turbulence, should we infer this is why the biggest effects here are found in the stable boundary layer regimes? Any comments on this general topic would be of interest for readers interested in boundary layer parameterization.

4. Section 2.5: The validation data section is short, but it could be even shorter. There's not a lot of information there other than names and references.

5. By the beginning of Section 3, I was surprised to see no reference to the Seibert et al. (DOI: 10.1016/S1352-2310(99)00349-0) study of method of determining mixing height.

6. pg 31637, lines 2-4: The statement that a shallower PBL entrains cooler, moister air seems an overgeneralization. This is probably true regionally, but wouldn't other regions be different. For example, under subsidence in subtropical oceans, wouldn't a lower PBL top entrain warmer, drier air? Or maybe there's an interpretation difference, cooler and moister than what?

7. pg 31639, around line 6, related to Figure 3: The changes in the meridional circulation seem to indicate changes in the ITCZ, but it isn't quite clear whether the change is a shift in position or a change in strength. Is there an associated change in zonal mean precipitation that could help to clarify?

8. pg 31639, related to Figure 4: Similar to the previous question, but here it seems the patterns might indicate that there are changes in the position/strength/variability of the midlatitude jets. The differences are mostly insignificant, but possibly because the runs are too short to have an adequate sample (though w/ 10 ensemble members, one might have hoped for decent signal to noise). Several follow up questions: - is the jet different? - are there differences in the baroclinic zones that manifest either as changes in the eddy transports or precipitation or anything? - do these differences becomes statistically significant if the runs are extended?

9. noted at page 31641, but true throughout: Picking out small regions makes the text rather cluttered. Can generalizations be made using, say, scatter plots that show the change in 2m specific humidity versus the change in soil moisture? Similarly in other parts of the text, some relationships are noted, but it is very hard to tell if there is an underlying principle at work, or if the feature is coincidence or a combination of many processes (and not understood). In this sense, the maps are fine as a first look, but it would be more informative to see if there are quantitative patterns within the map that are related to the physics of the model independent of the spatial distribution.

10. I wasn't sure why at Figure 9 we go back to looking at all three schemes when the two k-based schemes were already shown to be similar to each other.

11. pg 31642, related to the dust transport: It would be interesting to see how the dust gets out of the PBL in the different configurations. Presumably the differences in the low-level wind speed also have a diurnal component, so is there a chain of interactions as the wind and PBL depth change through the day, lifting the dust to different levels in the different configurations during the daytime before the PBL becomes stable at

nighttime? What does the diurnal cycle of dust emission look like over the Sahara? What does the dust transport tendency look like through the diurnal cycle- especially, does the dust get transported downstream much more efficiently at nighttime when the free tropospheric wind is strong than during the daytime when the PBL turbulence is likely dominating the transport at lower levels? This also hits on the point made in the overview that the paper invokes the diurnal cycle a lot, but doesn't show any results to support that reasoning (although there's no reason to doubt that the reasoning is correct).

12. pg 31644, line 8-9: Are the stability and PBL depth strongly correlated in these regions?

13. One could argue that the PBL depth definitions used here are just showing model sensitivity to any process that affects low-level mixing. Changing part of the Lock Scheme might similarly impact the climate, swapping the turbulence scheme(s) completely even while retaining the PBL depth calculation would change the climate, changing the shallow convection scheme would change the climate, or even changing the cloud physics would change the mixing by interacting with radiation and the turbulence. So is it fair in the end to say that the PBL is so important, or just that we must be cognizant of the interactions of the processes representing subgridscale mixing? This comment occurred to me as I read through the conclusions section, and it might be worth commenting on the interpretation of these results. Similarly, the mixing of the lower troposphere has recently been noted as being very important for climate change (Sherwood et al. 10.1038/nature12829), which might be worth mentioning specifically.

TECHNICAL COMMENTS

1. pg 31630, lines 17-19: The sentence starting with "Use of the PBL depth..." doesn't read very well, I think because the phrase "has been done" sounds too informal, and maybe somewhat vague.

2. pg 31639, line 27: increase should probably be changed to 'stronger'

3. pg 31645, line 29: "SD" wasn't defined, and it would just be easier to write standard deviation.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 31627, 2014.