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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Simulation of low clouds in the Southeast Pacific by the NCEP GFS: sensitivity to vertical mixing" *by* R. Sun et al.

P. Caldwell (Referee)

caldwell19@llnl.gov

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This is a very nice paper describing a new effort for improving the prediction of low clouds in the SE Pacific in the NCEP GFS model. The paper shows that by limiting the extent of shallow convection to within the PBL unless cloud-top entrainment instability (CTEI) is present and by turning off background diffusion above the PBL, a reasonable distribution of SE Pacific low cloud can be obtained. I found the paper to be (for the most part) very interesting and well written. I think the description of the CTEI implementation needs to be rewritten before the paper is accepted (see comment below), but otherwise my comments are quite minor. I look forward to reading the final version.

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Major comment:

I found the discussion of CTEI confusing. In particular, something must be wrong with the sentence on line 11 of p. 18473: Eq 1 is nondimensional, so what is less than 0.0001 K/Pa? I think you mean that the PBL is defined as where the vertical gradient of dtheta/dp is weak, eq 1 is satisfied, and eq 2 satisfies... well, I don't understand what you're getting at with eq 2. In particular, on line 11 of p. 18474 you write that the definition of kappa "can be rewritten as" an expression involving theta_l. Do you mean that Eq 1 can be rewritten as Eq 2 (this is at least approximately true in the PBL where theta_e is approx theta+L/cp*qv and theta_l is approx theta-L/cp*ql), or are you using kappa to describe another stability condition? It seems like the latter is true since you say that eq 2 gives the stability of the inversion against shallow convective penetration while I think of CTEI (eq 1) as being a local, cloud top instability rather than related to convective plumes. I recommend that if eq 1 and 2 are distinct you explain where eq 2 came from (give reference?) and call it something other than kappa to indicate it represents a different condition. I'm also confused why your inversion criterion involves eq 2 changing sign across layers (p 18473 line 12). Finally, you conclude p. 18474 by saying that you don't include one of the newer, more sophisticated CTEI criteria but you don't say why. You should.

I also wonder whether defining the inversion diagnostically as the height where dtheta/dp becomes large (which is, I think, your criterion when CTEI is not occurring) would lock the PBL into a certain layer since stratocumulus builds up against the inversion, reinforcing the inversion layer. Perhaps I don't understand your parameterization.

Minor Comments:

1. p. 18472, l. 11-12: Suggest rewriting as "In this scheme, the shallow-convective cloud top is defined as the highest layer below 0.7*Ps for which a test parcel from the second model layer is positively buoyant."

2. p. 18473, l. 10: why is the inversion constrained to be <0.65 Ps when convection

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can't even reach that high? Why not simplify by using the same upper bound for shallow convection and inversion height?

3. p. 18473, l. 28: I think you're missing the word "in" before Eq (1) and Eq(2).

4. p. 18474, l. 12: "for" is in math mode and lacks spaces.

5. p. 18475, 1st paragraph: since background diffusivity essentially acts as a lower limit for turbulent mixing, you might want to explain how the PBL scheme works here. If not here, sect 2 or where you talk about the turbulent mixing plot would also be ok places. Referencing articles on the PBL scheme is sufficient, but a couple of sentences would make the paper more self contained and interesting for those not familiar with the GFS implementation.

6. p. 18475, sect 4: What are you doing about spin up time? Do these simulations use data assimilation?

7. p. 18475, l . 23: "the criterion for instability expressed by Eq. (1) [add "and Eq (2)"] "?

8. p. 18476, l. 17: Wouldn't computing the sum of CTEI and ZEROBD cloud fraction increases be a better test of the linearity of their contributions than taking the max cldfrac between the 2 cases? The sentence as currently written seems redundant (e.g. with line 8).

9. p. 18477, 1st paragraph: the paper would be hugely enhanced by a comparison of vertical structure against obs. In particular, I think your inversion heights are too high. Isn't there VOCALS data you could compare against? Even if you can't compare against observations, a plot of inversion height for each of the experiments would be interesting. I'm interested in this because I can't figure out what determines the inversion height in your model and I wonder if it is roughly the same for all experiments... Perhaps you could also comment on the fact that radiative cooling increases turbulent mixing and hence cloudiness in your model, but doesn't drive entrainment which would

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decrease cloudiness through drying and by making the PBL deeper. This makes me worry that your model won't get the right low cloud feedback (I know this is a common situation in GCMs).

10. p. 18478, l. 11: overpredicted OLR is consistent with overpredicted low cloud and underpredicted higher clouds as suggested by Figs 1 and 3. Maybe you should mention this?

11. p. 1848, l. 25 (and elsewhere): I'm surprised RAS is acting here. Can you quantify how frequently it is triggered and what its effect is?

12. p. 18481 l. 13-14: I'd remove "greatly" and "much": the moisture profile is definitely improved, but its still not that great.

13. p. 18483, l. 28-29: "Tests have [add SHOWN]".

14. throughout: In Sect 4 you mention "Modified Arakawa-Schubert" then later talk about MAS. I assume they're the same thing. In sect 2 you define SAS="simplified Arakawa-Schubert", however. . . is this also the same thing?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 18467, 2010.