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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Explicit cloud-top entrainment parameterization in the global climate model ECHAM5-HAM" *by* C. Siegenthaler-Le Drian et al.

P. Caldwell (Referee)

caldwell19@llnl.gov

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I think the implemented changes (conserved-variable turbulent diffusion, explicit stratocumulus-top entrainment parameterization, and parameterized effect of subgrid-scale cloudtop radiative cooling) are generally good ideas though I have some reservations about the details as noted below. The impact of these changes is (to my mind) mostly negative, but I think this just illustrates the difficulty of improving pieces of the model without fixing other components which were developed/tuned to compensate for the older schemes' biases. I really appreciate the fact that simulation features were generally given explanations in terms of the model physics (though I disagree with

some of these interpretations).

I ended up writing a lot of comments, more because I found the paper interesting than because I think it is bad. Although there are a lot of improvements that could be made to the parameterizations and to the analysis (and in particular, I would not be comfortable using the current parameterizations in my GCM), I think the paper is interesting and deserves to be published. I vacillate between recommending major and minor revisions.

Major comments:

1. I am uncomfortable with your method for deciding where to apply your entrainment parameterization. If you look at a plot of low cloud amount, you will see that there is a great deal of low cloud at mid and high latitudes which should be entraining following the same physics as subtropical clouds, but don't get the same treatment in your model. It seems artificial to handle them differently. Further, the sudden switching on of the parameterization at LTS=20K could easily result in problems where the parameterization flickers on and off, resulting in bizarre behavior. Additionally, LTS is expected to increase in the future so one could imagine that low cloud feedback in your ECHAM version may be controlled by an increase in the frequency with which your entrainment parameterization is called. Since your parameterization should really be applied everywhere a cloud-topped PBL is encountered, this would be really artificial and embarrassing.

I really think you should work harder to derive a parameterization that can be applied universally, with its effect becoming negligible in places we don't expect entrainment. In this regard, I'm not sure that including entrainment in shallow Cu areas will cause problems because convective cloud fraction is typically quite low so most of the area will use the clear-sky rate anyways. You will need to handle the problem that entrainment gets strong as inversion strength decreases and make your cloud top entrainment consistent with the convective schemes though.

2. I'm nervous about your inclusion of cloudtop longwave cooling in the buoyancy flux and w* equations. First, I'm unclear what ΔF_{rad} means. Is it the flux across the cell whose top is cloud top and whose bottom is (since your clouds are 1 cell thick) the cloud base? Second, I don't think α is "the fraction of divergence occurring inside the discontinuous inversion". The mathematical meaning and justification for this is unclear - radiative destabilization anywhere in the cloud will induce turbulence. This is actually occurring in your STD case as the top cell in the cloud (which happens to be the only cell in the cloud) becomes cooler than that below it. As you note, this causes turbulence at cloud base (again, just because your cloud is 1 cell thick) rather than cloud top. Increasing the number of vertical levels in the cloud region would help here. I think what you are actually parameterizing is the fact that significant radiative destabilization actually occurs within a single cloudy grid cell. You could actually do some offline radiation tests to figure out what the gradient of radiative cooling is as a function of vertical grid dimension rather than arbitrarily specifying an α value. I think α will have to be tuned for different vertical grid resolutions (which is worth keeping in mind). I'm not sure what to think of the fact that you have both resolved and subgrid radiative destabilization occurring. You should think more about whether you're comfortable with this partitioning.

3. I am disturbed by the fact that entrainment seems to be moistening your free troposphere. In reality, entrainment pulls free tropospheric air into the PBL, causing the PBL to deepen, dry, and warm without changing the free troposphere. I think your parameterization acts very differently since you parameterize entrainment as down-gradient diffusion. Is your entrainment parameterization useful if it is operating in a fundamentally incorrect way?

4. It is hard to tell how good your parameterizations are when you are operating on a totally out-of-tune model. In particular, parameterizations behave differently when applied to different base states... and your base state is not the one we care about. On the positive side, I don't think we can conclude that your model does a worse job

on stratocumulus until examining a retuned simulation. The fact that your runs have overestimated cloud top, reasonable TKE, and yet entrainment is underestimated by a factor of 10 suggests that other parameterizations were previously making up for the lack of explicit entrainment and they're still too strong now that entrainment is included.

5. I would like to see separate simulations of the entrainment parameterization and the inclusion of cloud top cooling in the buoyancy flux calculation. Which differences are attributed to which change?

Minor comments:

1. you should mention conserved-variable turbulent transport in the abstract.

2. p1973, L20: you may want to mention that LWP changes are important because they change the shortwave cloud forcing and hence climate feedback.

3. p1974,L22: do you mean that entrainment increases the air-sea temperature gradient, increasing the surface latent heat flux and thus moistening the PBL? This argument can't explain increased regions of RH>100% (ie cloudiness) because as soon as RH increases above its original values the LHF drops below its original value and thus acts to dry the PBL. Perhaps you mean something related to the vertical structure of the atmosphere?

4. p1976,l5: "extend" should be "extent".

5. p1977 L19: your equation is for the liquid-ice static energy rather than the moist static energy. Your definitions and equations for energy quantities are mixed up throughout the paper.

6. p1979, L12: I'm curious why the original value of 10% was chosen. Is this a tuning parameter? Also, your process of tuning it to 0.1% makes me nervous - you've just tuned a global parameter based on data from a single point and time.

7. p1981,L4: Are you applying the whole macrophysics scheme at the beginning and

end of the turbulence scheme, or just doing the condensation? If the latter, your cloud fraction and condensate amount are going to be inconsistent after turbulent diffusion. I'm not sure whether this matters. In any case, what would happen if you just used the before/after condensation calculation to ensure water conservation and left C=10% as before? Perhaps this would give you better results.

8. p1981, L24: MCV improves stratocumulus relative to RED, not to the operational ECHAM, right? And on p1982, L 7, when you say moist conserved variables "decrease the impact of the tuning parameter", you are really saying that cloud decreases due to C and cloud increases due to conserved-variable transport cancel each other, or are you saying that it doesn't matter what C is when moist-conserved variables are used because they widen the PDF to the point where C-limiting never happens? Why would using moist-conserved variables widen the PDF?

9. p1982, last paragraph: You start this paragraph by saying that defining the PBL top is hard, then suddenly switch to a discussion of the geographical regions where you're going to apply your parameterization without ever telling us how you define PBL top. Are you saying you only apply the parameterization in stratocumulus regions because the cloud top is well-defined there?

10. eq 11: your signs are screwed up.

11. p1985,L15: Opposite-signed fluxes in cloudy and clear portions of the grid cell only occur where $q_{clr} <$ free-tropospheric q $<$ saturation q... how frequently does this actually happen? I like the way you partition clear and cloudy regions, but I'm not sure ability to handle opposite-signed fluxes is that important.

12. p1986, L21: TN entrainment does depend on evaporative enhancement through the a term and it depends on TKE generation through w*. I don't understand what you're saying here.

13. p1987, L13: what is the model sensitivity to the saturated/subsaturated ratio?

14. p1988, L11: you say here that rad cooling enhancement of the buoyancy flux is only done in the stratocumulus region, but later you say that radiative cooling enhancement is applied wherever there are clouds. What's going on?

15. p 1990 top: I don't think your justification for the case makes sense. Reword?

16. p1990L20: It is really misleading to say destabilization occurs at cloud base. Really it is happening at the top cloudy layer, which means that including more layers in the cloud region should improve the problem.

17. p1991, L1: I don't think all liquid is rained out in hour 1...

18. p1991, L15: is daily disappearance of cloud realistic?

19. p1991, L28: "not shown" is wrong - it is shown in Figs 7 and 8.

20. p1992, L4: entrainment kills TKE, which could explain the reduced turbulent transport you see in your simulation. Lack of connection between surface and cloud top is disturbing, though, because it is not at all how stratocumulus operate. How can the surface layer decouple without the cloud disappearing? What role is convection playing here?

21. p1992 L15: sink of what?

22. p1993, L18: I think ENTR TKE looks non-constant in the cloud because the cloud is only 1 cell thick, while STD smears cloud over more than 1 layer, enabling it to look more constant. Thus your conclusion is based on the way your plotting software works.

23. Sect 4.2: Bretherton et al (2007) explains LWP decrease with increasing entrainment as due to the small-scale impact of sedimentation on entrainment and provides a correction to the Turton-Nicholls entrainment parameterization to include this effect. You could - but don't - include this effect in your simulation, so I'm not sure what you're really trying to accomplish here. Also, I'm nervous about the fact that your analysis is occurring during the spin-up period. Finally, cloud becomes opaque in the longwave

at around 40 g/m2 LWP, after which point BL radiative cooling saturates and hence turbulence and cloudiness quit depending so much on LWP (and hence on droplet concentration since that's what you are using to change LWP). Your Fig 12 fits this explanation quite well - LWP hits 40g/m2 at around 100 drops/cc and dLWP/dNd and dwe/dNd level off at about that point.

24. Your analysis is really focused on the NE Pacific (both your SCM and cross-section focus on this). I don't think this is sufficient validation for a global model.

25. p1996, L18: I think it's unnecessary to include ERA40 because it doesn't have a worse Hadley cell than ERA-interim. If you want to compare independent validation datasets, use satellite data or something.

26. Fig 6: ^{c} should be the same for clear and cloudy regions... but it has very different x-values. You should fix this.

27. Fig 7: You should really explain or at least acknowledge the fog seen in the right-hand panel.

28. Fig 11: what is the scale of the insets?

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