Responses to Dr. Peter Caldwell's review comments

First of all, we would like to acknowledge Dr. Peter Caldwell for his careful review, insightful comments and valuable suggestions. Please see below for our responses to Dr. Caldwell's comments. Our responses are marked by **<RE>.**

Major Comments:

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?

<RE>: First of all, you are right about the text description on line 11 of Page 18473. There should be no "Eq." before the numbers in parentheses. They are simply two conditions for dT/dP at a given level to determine whether it is the inversion. Please see our revised manuscript.

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.

<RE>: We don't mean that Eqs. (1) and (2) are different. We simply mean that Eq. (2) is, in general, an equivalent form of Eq. (1). With Eq. (2) we are trying to further justify the way we use the CTEI criterion---not as a local parameter measuring the possibility of buoyancy reversal but a large scale stability parameter like the low troposphere stability (LTS) to determine whether a stable layer of stratus can be maintained below the inversion. Eq. (2) is also used in Lock (2009, QJRMS, Eq. (1)). To avoid confusion, we removed Eq. (2) and revised text accordingly. Please see our revised manuscript.

I'm also confused why your inversion criterion involves eq 2 changing sign across layers (p 18473 line 12).

<RE>: This is due to the accidental "Eq." wrongly put before "(2)".

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.

<RE>: A justification is given in the revised manuscript.

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 inver-sion, reinforcing the inversion layer. Perhaps I don't understand your parameterization.

<RE>: Whether CTEI happens or not depends on the relative strength of the temperature and the moisture jumps across the inversion. We think part of your confusion is due to our error on line 11-12 on Page 18473.

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."**

<RE>: Text has been re-written.

2. **p. 18473, l. 10: why is the inversion constrained to be <0.65 Ps when convection can't even reach that high? Why not simplify by using the same upper bound for shallow convection and inversion height?**

<RE>: Thanks for your careful review. In the GFS, inversion is not constrained to be below 0.65 Ps. But we are looking for low-level inversion below 0.65 Ps. The upper bound of shallow convection (0.7 Ps) is also an arbitrary number. Since the two are not so different it is highly possible that unifying the two will not cause any significant difference in model results.

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

<RE>: remove 'Eq's before '(1)' and '(2)'**.**

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

<RE>: Eq. (2) is removed.

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**

<RE>: Text has been modified to add a brief description of the PBL scheme in NCEP GFS.

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

<RE>: Our simulations started before July 1st. But, we did not use in our analysis the simulation results before July 1st, 2008. These are not data assimilation runs.

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

<RE>: Eq. (2) is removed and not used in this version of the GFS.

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).**

<RE>: This was done in the first place. But when cloud overlap is considered we think taking the max of the two is more suitable than taking the sum. Even if the sum of the two is considered the total cloud in CZ is still much more in most of the region.

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 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).**

<RE>: There are some observational data available after VOCALS-REx. But it seems there is no long data series available for us to get monthly means of some vertical properties and compare our experiment results with.

We will add estimated inversion in Figure 10 in the manuscript as shown in Figure 1 below. GFS does not give inversion height as an output. This inversion is calculated based on 2008 July monthly mean temperature at pressure levels in our post-processed files, which have fewer levels than in GFS model outputs.

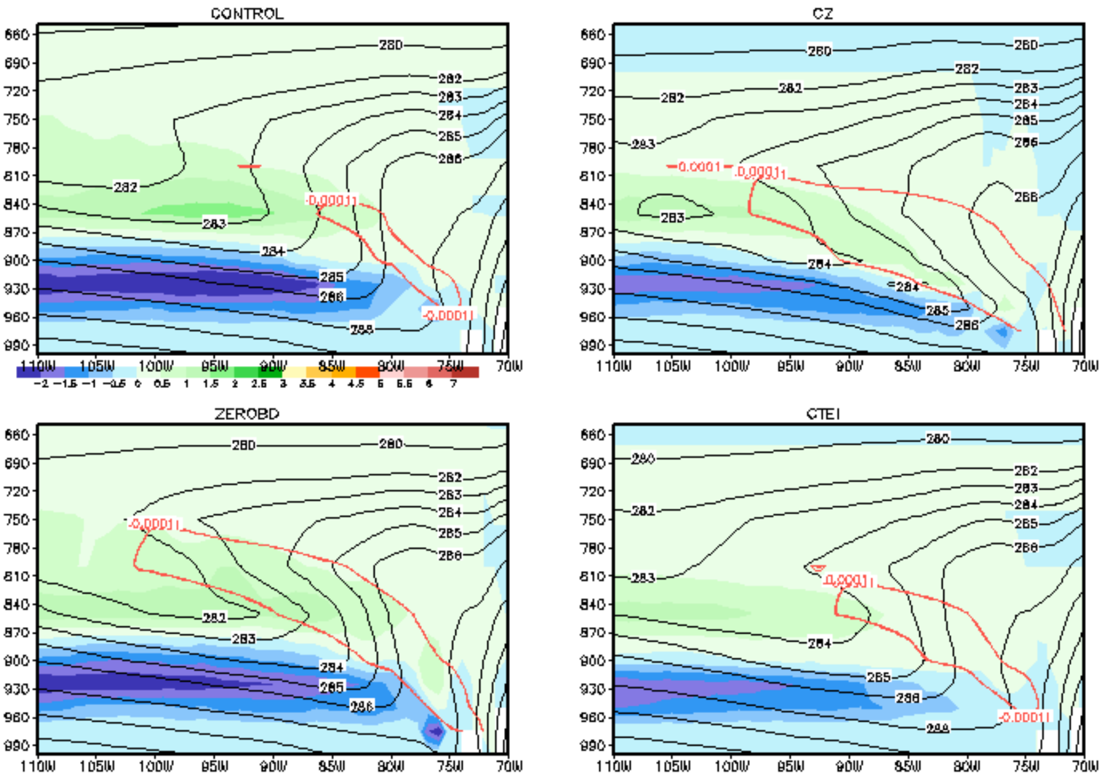

Figure 1. Vertical cross-section of ensemble monthly mean moistening rate (g/kg/day) along 20S (shaded) due to shallow convection in the four experiments. Black contours reprepent the vertical cross-section of temperature. Red contours represent inversion.

As shown in Figure 2, pbl heights are consistent with turbulent mixing in the boundary layers. CZ has the deepest boundary layer off shore and CTEI has the smallest. This suggested the possibility that the increased turbulent mixing by radiative cooling increased entrainment in CZ. However, the entrainment and drying processes might not strong enough to overcome the moistening processes.

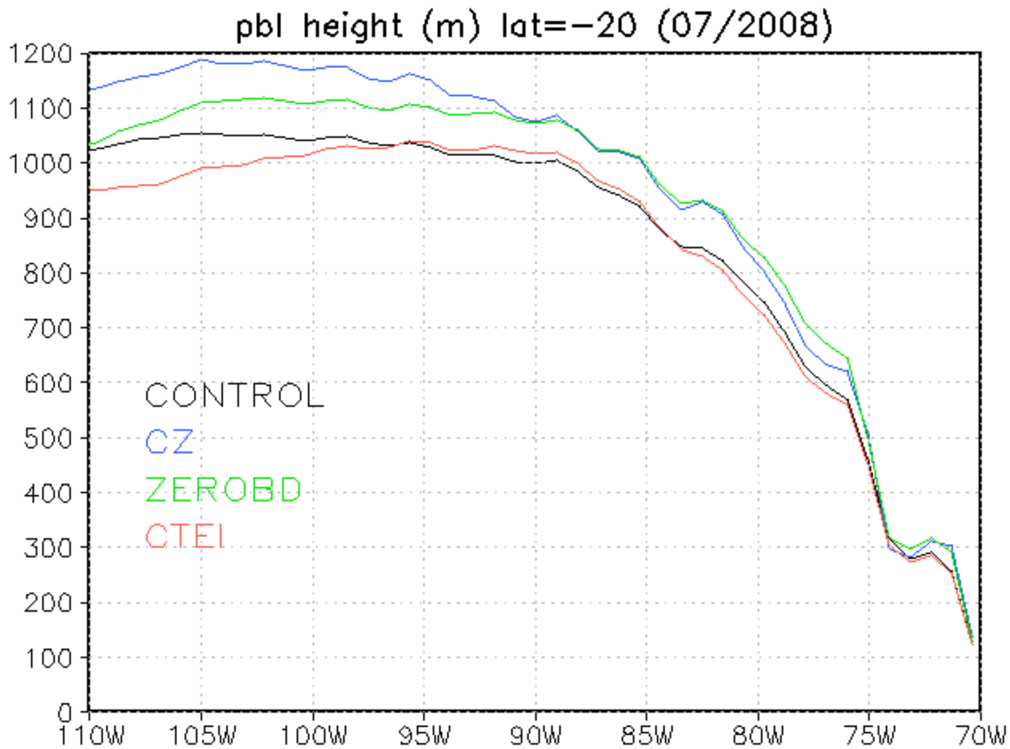

Figure 2. July 2008 mean PBL height (m) along 20S.

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?

<RE>: Text has been modified accordingly**.** Thanks!

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?

<RE>: RAS is modified from AS. It can account for a spectral of convection including deep and shallow convection. But as in AS, RAS is not effective enough to take care all the effects from the shallow convection. That is why we need an explicit shallow convection scheme. We currently do not have a way to quantify the frequency it is triggered. In the model output, there is a diagnostic variable called convective heating rate (K/day) due to RAS to quantify RAS activity. It is shown in the following figure.

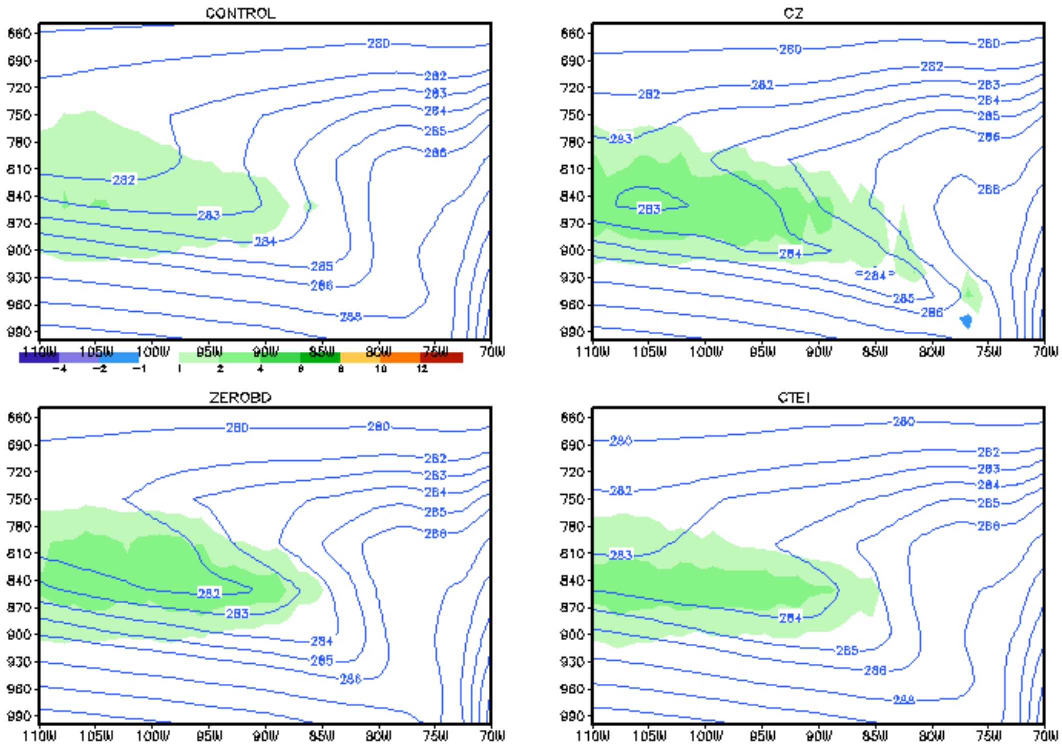

Figure 3. Vertical cross-sections of ensemble monthly mean moistening rates (g/kg/day) along 20S in shades due to RAS in the four experiments

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

<RE>: "greatly" and "much" are removed.

13. 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?

<RE>: Relaxed Arakawa-Schubert (RAS) and Simplified Arakawa-Schubert (SAS) are both modified from Arakawa-Schubert (AS). But, they are different. In this study we used RAS. References are given for RAS and SAS in the manuscript.