

## Reply to the editor

We would like to appreciate the editor for his constructive comments on our manuscript after his carefully checking. We corrected and modified our manuscript following his comments. Also, we read carefully the manuscript again and made some cosmetic corrections of the manuscript. Please note that all the modified parts are shown by red-color font in the revised manuscript.

### Comments

**Line 51. “CGCMs”?**

Corrected.

**Line 110. “. . available for only a relatively short time, limiting . .”?**

Corrected.

**Line 121. “. . is plentiful literature . .”?**

Corrected.

**Line 122. “. . referred to.” Or “. . referenced.”**

Corrected.

**Line 123 delete final “,”. Line 125 delete “,”**

Corrected.

**Line 130. “and using”**

Corrected.

**Line 158. Delete first “,”. “. . with subscript oml . .”**

Corrected.

**Line 161 (Ref 2 commented). “. . production is linear in u, v, as long”?**

Corrected.

**Lines 168-169. “. . temperature just below the OML . .”?**

Corrected.

**Lines 182-188 (resuming Editor comments). I think the “new” and former text could be integrated better. As I understand it (i) entrainment velocity at the bottom of the OML, (ii)  $Q_b$  and (iii) advection of  $\partial\theta_{oml}/\partial y$  are all in RESD but (i) is small. (iii) is not frontogenetic (I agree) but it could cause incorrect diagnosis of frontogenesis in the Eulerian view.**

Thank you so much for the comment. We combined the new and previous texts for RESD. Please see lines 183-189.

**Lines 192-195. The main text needs to refer to the supplement here (c.f. Ref 2 comment “161” and later). In relation to this and Ref 2 comment “242”, by analogy with time-averaging stratification, it occurred to me that for the climatology sharp gradients would be retained better by time-averaging  $y$  for given temperature (I assume you time-averaged temperature for given location). When there is (inter-annual) variability, time-averaging for a given location spreads out the temperature distribution. This bears on section 4 et seq.**

We added referring the supplemental information. Please see lines 191-192.

**Lines 257-261.** This is clumsy with the additions to the text. Better “. . RESD is estimated from (3.4) where the left hand side  $\partial/\partial t(\partial\theta_{oml}/\partial y)$  is zero for climatology independent of time: RESD = . . . (5.1). Note that all terms . .”

Corrected.

**Line 264.** “at least” is unclear. “at least in part” or omit?

Corrected.

**Line 278.** “temporal” -> “time”

Corrected.

**Line 284.** Omit “respectively” (twice)

Corrected.

**Line 301 (Ref 2 commented).** I think  $\theta_{oml}$  is a function of (respectively)  $t$ ,  $t+\Delta t$ ,  $t-\Delta t$  and the denominator on the right-hand side should be  $2\Delta t$ . (I guess then  $\Delta t = 1$  day). You would need to change line 302 accordingly.

Corrected.

**Lines 350, 377&379.** It is strange to see a (Angola) Dome being identified with low SSH. I am not familiar with the area and associated names but some explanation would help assure similarly unfamiliar readers that there is not a mistake here.

The term “dome” is referred to the vertical structure of temperature in this area that evokes a cold “dome”. The first definition of Angola Dome is by Mazeika (1976) as cold dome. We added this citation and brief explanation of Angola Dome. Please see lines 349-350.

**Line 385.** Better omit “up to”

Corrected.

**Line 399.** “. . points out the negative  $\partial w_b/\partial y$  and the positive stratification . .” to correspond with your response to the referee and the figure?

Yes. We corrected.

**Line 410.** “forcing and wind-driven mixing. Also . .” in accord with Referee 2 comment “360” and your response.

Corrected.

**Line 438.** Omit second “the”?”

Omitted.

## **Reply to the reviewer#1**

We would like to appreciate the reviewer#1 for his/her constructive comments on our manuscript. We corrected and modified our manuscript following his/her comments. Please note that the modified parts are shown by red-color font in the revised manuscript.

**Lines 183-184. (Referee 1 comment). I generally agree with the authors regarding their response to my Major Comment #1, but I do not think entrainment is a higher order term. Thus, I recommend the authors to replace “its contribution is of higher order and it might be” with “it is”.**

Corrected.