Atmos. Chem. Phys. Discuss., 5, S3259–S3265, 2005
www.atmos-chem-phys.org/acpd/5/S3259/
European Geosciences Union


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Convective damping of buoyancy anomalies and its effect on lapse rates in the tropical lower troposphere" *by* I. Folkins

**Anonymous Referee #2**

Received and published: 17 October 2005

Review of "Convective damping of buoyancy anomalies and its effect on lapse rates in the tropical lower troposphere" by I. Folkins

In general this is a nice paper which seeks to try to deduce properties of the tropical lower tropospheric lapse rates. It is partially successful at this task and probably needs to be clarified significantly to make the conclusions stronger. I had trouble understanding some of the discussion. I also do not think that as used the data (particularly the reanalysis data)reflect the conclusions. The conclusions could be made more concise. On the whole this paper seems to be a useful contribution, but not much more than speculation at this point, and I think it should undergo significant revisions if it is to be published in ACP. I provide detailed comments below.

In general the paper could benefit from a few more examples to support the theory advanced. I note a few places where more data could help.

One of the major problems that this paper addresses only tangentially is the disconnect between radiodonde observations of single profiles, and analyses consisting of model solutions over quite large domains. The paper touches on this, but doesn't really note the disconnect here. There is some averaging in the radiosondes over multiple profiles, but these likely miss major convective cores. In addition the model (re-analysis) results presented are probably very model and even resolution dependent, on 2 of the most uncertain parmeterizations: gravity waves and convection. Some brief discussion of these uncertainties is attempted, but more should probably be provided.

This paper addresses relevant scientific questions within the scope of ACP. It takes a novel perspective on the maintenance of tropical lower tropospheric lapse rates, and provides some conclusions. The scientific methods and assumptions are valid but could in many cases be better presented. The conclusions could perhaps be made more concise to directly reflect the results. The description of experiments and calculations is sufficiently complete and precise to allow their reproduction, and proper credit is given to related work. The title clearly reflects the contents of the paper. The language is fluent and precise, and mathematical formulae, symbols, abbreviations, and units are correctly defined. The number and quality of references are appropriate. Several parts of the paper need to be clarified as described below.

The abstract provides a concise summary. However, the statement in line 13 (about SST's above 27C) is never referred to in the text (the 27C number never appears).

pg 7730

line 23: 'layers' is used twice, this seems awkward.

pg 7311

line 3: the example is not clear to me ('heating by induced decent' from buoyant



ACPD

5, S3259–S3265, 2005

parcels?) these seem secondary, not primary effects. Perhaps another sentence or two is necessary.

line 13: to what extent is the deviation from moist adiabatic ascent in observations a bias due to the lack of sampling within or adjacent to convection (radiosondes do not survive in deep convective clouds)?

pg 7312

line 10: is there a distinction that needs to be drawn between shallow and deep convective regions here, or not?

line 23: In Equations 1 & 2, the coupling would seem to imply rigid lids to the region of updrafts and downdrafts. While a bottom certainly exists, is there really a top lid? the tropical tropopause layer is a pretty porous region. Or is the density fall off with height enough to constrain it? Also, the existence of a hadley cell implies that the convective and non-convective regions can be widely separated, and balance only on global and seasonal scales does it not?

pg 7313

line 2: see comment above. I do not think delta_t bar ˜ 0 on large scales, due to the existence of the hadley-walker circulation. It seems to me these constraints might be highly model dependent.

line 23: Do these gravity waves propagate at every level? What waves are sustained at various levels?

pg 7314

line 7: the 'dotted' line is dashed in figure 1 and needs to be fixed.

line 14: what happens outside of the warm pool?

line 15: below 2km isn't very good either? why? same reasons or another one?



ACPD

5, S3259–S3265, 2005

Interactive Comment

line 28: why saturated pseudoadiabat for the background atmosphere? is the background atmosphere saturated? even in regions of high humidity it is not saturated (RH=100% or more).

pg 7315

line 12: are you suggesting a stagnation level at the melting level? Why?

pg 7316

line 3: I was very confused by 'radiative mass flux' having constraints on it imposed by convection, until I got to equation 3. You probably should define the radiative mass flux as dp/dt (gamma_r) here (with equation 3 and 4). This makes it clear you are not talking about constraints on the heating rate (dT/dt or dTheta/dt) but the heating rate divided by the static stability: (dT/dt) / (dT/dP)

line 10: What about trying this with higher resolutions to see if the results are similar. I would be interested also to see this explored with different models (analyses and free running), to make sure that this is not a model issue.

Why is there a big difference between 300 and 400hPa? this is a bit scary, and without explanation. Is this the melting level? Is this the same in all models?

line 25: the fact that Gravity Waves are potentially doing the coupling is also scary, because it implies that there may be a resolution dependence to the response.

pg 7317

line 4: Yikes! Could 900km also just be ~3 * model resolution of 2.5 degrees? Can you show this with different resolutions? (E.g., use a year of the ECMWF analyses at different resolutions).

line 6: does higher wavelength = larger wavelength?

line 7: this last paragraph is good. It would make your case better, and be more robust,



ACPD

5, S3259–S3265, 2005

Interactive Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

EGU

if you could use different models, or different resolutions.

line 18: I don't buy this. It only appears to go to zero because it is 1/10th of the peak value. Is this significant? Why is the value always positive?

pg 7318

line 2: I think you mean figure 4 here, not figure 5 (this is a problem in several places).

line 4: again, this assumes a rigid top lid, which I am not sure is true to the convective region (e.g., Sherwood & Dessler overshooting convection). or is the 'TTL' enough of a cap (Folkins 1999 paper for example).

line 20: This paragraph is a bit unclear to me. Again, it seems to assume pretty tight coupling at most levels. I'm not sure I buy this for deep convection, but if you are dicussing shallow convection it makes more sense, and you could clarify this.

pg 7319

line 20: how is a downdraft nearly saturated? Are you assuming significant condensate loading and continual evaporation?

pg 7320

line 5: as noted, it would be clearer to define this explicitly in section 3. I was very confused until this point.

line 23: The reference to figure 4 here I think should be to figure 5.

pg 7322

line 27: how is Qr (radiative heating) strongly dependent on temperature? I think this is explained at the bottom of the next page, but it should probably go here.

pg 7323

line 3: again, I think the reference here is to figure 4 not figure 5.



ACPD

5, S3259–S3265, 2005

Interactive Comment

pg 7325

line 22: Why is the divergence required to go to zero again at this radius? I still don't think this is well demonstrated in this work.

line 23: Aren't all the stations here in convective regions? Or does the annual cycle (convective seasons, non-convective seasons) matter. What do the subtropical dry regions look like?

pg 7326

line 13: I do not see this difference at all. The differences between the other stations and Ponape or Majuro are less than the difference between them. omega_r seems very similar to figure 5.

pg 7327

line 3: why is there a rapid decrease in condensate loading above the melting level? due to the latent heat of fusion or something? Although the melting level (0C) is not equal to the freezing level (-20 to -40 C).

pg 7328

line 12: Have you demonstrated this? Where? Or is the overall divergence just small?

line 23 & 28: as noted above, melting level (used in the rest of this paper) is not equal to the freezing level. Which do you mean.

line 29: this is really interesting: the difference between deep and shallow convection. But you don'r refer to it at all. could this be responsible? Does shallow convection stop at the melting level? Does it stop at 400hPa (difference in e-folding distance of ECMWF analysis)? Could you see this in OLR data for example?

pg 7329

line 15: This seems pretty speculative to me.



ACPD

5, S3259–S3265, 2005

Interactive Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

EGU

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 7309, 2005.



ACPD

5, S3259–S3265, 2005

Interactive Comment