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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

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Main Comments:

1. Reliance on low resolution ECMWF analysis and sensitivity to convective parameterization

This concern also comes up several times in the comments below, and is addressed there as well. The interpretation given here for the vertical variation of lower tropospheric lapse rates does, indeed, require that the total divergence become small on scales of 2000 km or larger. ECMWF winds were used to show that this is the case. As the paper discusses, the degree to which one can rely on the accuracy of ECMWF tropical divergence estimates is unclear. We have mainly responded to this concern by repeating the same analysis using horizontal winds from the GEOS-4 model. The results are in general agreement with those from ECMWF winds.

2. Representativeness of Radiosonde profiles:

Yes, radiosondes rarely measure temperature or humidities inside convective clouds, so the virtual temperature lapse rate generated from the radiosondes (and the associated deviations from moist adiabatic lapse rate) may not be representative of the tropical mean. However, the papers by Wei, Lucas, and Jorgenson (among others) show that differences in density between tropical updrafts/downdrafts and their environment are weak, as are (as one would expect) the vertical velocities within convection. It is therefore unlikely that if radiosonde profiles within clouds had been included, the mean density profile of the tropics would have been substantially different. In this case, however, one would have had to have had an instrument which measured the condensate loading inside convection and used density temperature, rather than virtual temperature, as the surrogate for density. Two air parcels at the same pressure, but in general different water vapor concentration, condensate loading, and temperature, will have the same density temperature if they have the same density. The prediction that the tropical atmosphere should adopt a moist adiabatic temperature profile is, more fundamentally, a prediction of the mean density profile since the mechanism which accomplishes the adjustment is gravity waves, which are generated by density anomalies. The deviations in observed density from that predicted by moist adiabatic ascent, shown in Figure 1, are therefore likely to be representative of the tropical mean (at least in deep convective regions). In this paper, I am using the vertical derivative of a clear sky density variable (virtual temperature) to exaggerate the differences between observations and theory.

The radiosonde profiles were also used as input temperature and water vapor profiles in a radiative transfer model calculating climatological radiative mass flux profiles. In this case, one would want to specifically exclude any radiosonde profiles within convective updrafts/downdrafts, since one wants to find the mean vertical radiative mass flux of the background atmosphere.

ACPD

5, S3798–S3805, 2005

Other Comments:

pg 7310:

line 23:layers used twice - fixed

pg 7311

line 3: heating by induced descent. This has been rephrased.

line 13: issue of radiosonde representativeness addressed above.

pg 7312

line 10: now explicitly state that I am referring to deep convection regions only. ‘‘active" is changed to ‘‘deep" throughout the paper.

line 23: I have explicitly noted that the pressure velocities are functions of pressure, and are not tropospherically averaged. I am assuming this is where the notion I was assuming the existence of a rigid lid originated. Eqs (1) and (2) are just definitions, and don't make any assumptions, other than that one can make a meaningful physical distinction between the convective and background domains. Yes, I agree that the existence of the Hadley cell does imply that the upper tropospheric divergence of the convective regions is balanced by the upper tropospheric convergence of the less convective regions only on a tropics-wide scale. I would argue, however, that this thinking is more appropriate in the upper troposphere. This issue is discussed in more detail above, but Figure 3 (and to a lesser extent the new Figure 4) support the idea that mass circulations in the lower tropical troposphere have a shorter horizontal scale. I also now explicitly refer to this issue with a new paragraph in the Conclusions section.

pg 7313

line 2: I would view the Hadley-Walker circulation as primarily an upper tropospheric circulation, whereas I am referring here to divergence patterns in the lower troposphere. The issue is how much faith one can have in ECMWF ERA-40 horizontal winds in the

ACPD

5, S3798–S3805, 2005

Interactive Comment

lower tropical troposphere, and is addressed in detail above. I have also added a new paragraph addressing this issue in the conclusions (second last).

line 23: I added a ‘‘throughout” to indicate that the vertical displacements would occur throughout the troposphere.

pg 7314

line 7: dotted changed to dashed

line 14: In examining the SPARC and SHADOZ high resolution radiosonde lapse rate climatologies, I have never encountered a location which exhibits reversible moist adiabatic lapse rates over any significant height range. However, I would hesitate to include a discussion of regional lapse rate variations since it would lengthen the paper, and is outside its main scope.

line 15: Lapse rates in the subcloud layer approach (below 950 mb) approach dry adiabatic. Presumably, rapid vertical mixing give rises rise to a layer in which virtual potential temperature is roughly constant. The layer between 950 mb and 2 km is usually considered part of the marine boundary layer, in which mixing is strong compared to the free troposphere above. Rapid decreases in RH above 2 km certainly support this view. However, I would rather not speculate on what determines lapse rates in this interval.

line 28: Yes, I had neglected to add that I was assuming the air parcel had risen above the lifting condensation level, and could be assumed to be saturated. In this case, the relevant density comparison is between the saturated equivalent potential temperature of the rising air parcel, and the saturated equivalent potential temperature of the background atmosphere. I have also added a comment on the circumstances in which saturated equivalent potential temperature can be regarded as a density variable.

pg 7315

line 12: near the melting level, or minimum in saturated equivalent potential temper-

ACPD

5, S3798–S3805, 2005

Interactive Comment

ature, the buoyancy of an updraft/downdraft would be temporarily independent of altitude, but no, I would not refer to this level as a stagnation level.

pg 7316

line 3: I didn't fully understand this comment, especially the role of the gamma_r. I took it to mean that it was potentially ambiguous to use a pressure velocity to refer to a mass flux, and have tried to clarify this. In response to this comment, I have also moved (3) and (4) much earlier in the paper, now before (1) and (2), and I think this makes the paper clearer, especially with respect to how a constraint on the radiative mass flux gives rise to a partial constraint on the static stability.

Just as a side note (I am not exactly sure how relevant this is to the referee's comment), whether one accepts the specific arguments of the paper or not, it is certainly true that the vertical variation of the radiative mass flux imposes constraints on convection (and also vice versa, of course, since inferring causality with tropical convection is so problematic), since the radiative mass flux has to be equal and opposite to the mean updraft + downdraft mass flux. There is nothing new about these types of thermodynamic arguments.

line 10: Although I have not tried a different resolution with ECMWF (it is my impression the higher resolution ECMWF reanalysis data is not publically available), I have explored this issue using the GEOS4 horizontal winds. Of course, there is still no ultimate assurance it is not a model issue, but exploring different models does give one an impression of model-related uncertainties.

I do mention that the difference may be due to vertical motions in the upper troposphere being more dominated by faster traveling (larger vertical wavelength) gravity waves. I would be reluctant to speculate further. The big difference between 300 hPa and 400 hPa seen in ECMWF is not seen to the same extent in the GEOS4 model.

line 25: I agree that investigating the issue using higher resolution ECMWF analysis

ACPD

5, S3798–S3805, 2005

Interactive Comment

EGU

data would be desirable, though even then not be a conclusive resolution, since one could argue that there is still likely to be a sensitivity to convective scheme/gravity waves. I now mention these issues in the paper. I think it is more useful to investigate this issue using a different model, as is now done.

pg 7317

line 4: Discussed above. 2.5 degrees at the equator is about 280 km, or 3 times smaller than 900 km. In addition, in a new paragraph in this section, we discuss the issue of grid resolution, how it compares with the expected coherence length of the total divergence, and how one would expect the areally averaged divergence to respond.

line 6: meant higher vertical wavenumber. Fixed.

line 7: I do now use a different model, and mention sensitivity to gravity waves.

line 18: Why does the value remain positive? Individual area mean divergences could be positive or negative, independent of the sign of the divergence at the center of the area. Presumably, in this case, however, where you have averages over many different areas, there persists a small statistical bias toward positive values out to large distances. However, the 500 mb average divergence does actually go negative, and I now show this in the figure.

pg 7318

line 2: fixed

line 4: I think this would assume rigid sides. i.e. that the outward horizontal pressure forces of an expanding plume ultimately force the air of the surrounding atmosphere down to a lower pressure level as a response to the air being compressed horizontally. The earth's rotation precludes accommodating horizontal motions in the far field. This comes back to the finite scale of the Rossby radius (off the equator).

line 20: As in the previous comment, one would expect the relationship between vertical changes in buoyancy, and entrainment/detrainment to be generally valid for all types of buoyancy anomalies, and is a widely accepted consequence of the Bretherton and Smolarkiewicz paper.

pg 7319

line 20: It is now clearer that I am referring to a hypothetical downdraft that was nearly saturated. Yes, this would be more likely in cases with larger condensate loading.

p 7320

line 5: These equations are now given much earlier.

line 23: fixed

pg 7322

line 27: Qr (radiative heating) is strongly dependent on temperature because longwave emission goes as the fourth power of temperature, but I don't think its necessary to discuss this explicitly.

pg 7323

line 3: fixed

pg 7325

line 22: This issue is discussed above, and of course, comes down to how much one believes ERA-40 lower tropospheric tropical divergences. Although they may not be perfect, they are probably the best one has to go on.

line 23: As shown in Fig 8, rainfall is less extensive over Ponape and Majuro than the other 3 stations.

pg 7326

line 13: The mass fluxes are similar in magnitude but they are more variable between

ACPD

5, S3798–S3805, 2005

Interactive Comment

2 and 5.2 km in Fig 9 than Fig 5, though I admit the differences are not large. I have modified the wording to make it clearer that I am referring to the variability with height, which I think is fairly obvious.

pg 7327

line 3: Although this question is beyond the scope of the paper, it is believed that the weakness of the vertical wind shear in tropical oceanic convection, and the slow upward velocities, allows either enough time for supercooled water to freeze, or for ice formed at higher levels to fall through updrafts and freeze the supercooled water. The resulting relatively quick appearance of ice above the melting level in oceanic convection favors precipitation, and condensate removal.

pg 7328

line 12: The background radiative mass flux divergence is weak since the radiative mass flux is weakly dependent on height. Added a reference to the appropriate figure.

line 23 and 28: I always mean melting level and this is now fixed.

line 29: Yes, I think the change in the lapse rate structure could be related to the change in cloud outflow regimes and this is discussed in the Folkins and Martin 2005 JAS reference. There is some evidence that outflow from shallow convection (congestus cumulii) does diminish above the melting level (papers by Johnson et al.), though other papers (Trenberth et al., 2003, J Clim) suggest that it reaches 8 km. Inferring mass outflow from OLR cloud tops is problematic, unless one has some reason for assuming that outflow is preferentially distributed near cloud top.

pg 7329

line 15: That these comments are speculative is clear from the context, and it is common practice to give some speculation toward the end of a conclusions section.

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