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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Height increase of the melting level stability anomaly in the tropics" *by* I. Folkins

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I found it awkward to reply to the reviewer comments without being able to refer to a revised manuscript. I have therefore uploaded it as a suplement in response to both reviewers. This response also contains a summary of the main changes to the manuscript and a response to the editor comments.

Major Changes to the Manuscript

(1) Figure 2 now shows the radial temperature response to high rain events, and the effect of this response on the atmospheric stability.

(2) Many of the months in 1998 had a small number of radiosonde profiles. We therefore removed 1998 from our analysis, as well as the discussion of the effect of the 1998

El Nino event. The amplification profiles are essentially unchanged.

(3) Figure 4 now shows the dependence of the slope of the regression on the rain rate. The increase with rain rate is physically expected; a larger rain rate should more effectively transport near surface temperature anomalies to the upper troposphere. We also use this relationship as part of our argument that the amplification factor should not be considered an "intrinsic" aspect of moist convection.

(4) Figure 6 shows the sensitivity of the amplification factor to regional averaging, and to the vertical depth over which the surface temperature is defined.

(5) We have removed references to the pseudoadiabatic amplification factor. Given the sensitivity of the amplification factor to how it is defined (at least given the limitations of real datasets), it is not helpful to think of an "ideal" reference amplification profile.

(6) We have removed Figure 2 from the paper. The main point of the figure was to show that there existed correlations between the monthly mean temperature anomalies of the radiosonde stations, so that it was appropriate to add the anomalies together. However, the fact that there are likely to be long distance correlations between the temperature anomalies of radiosonde stations is obvious from Figure 2.

(7) We have removed the hydrostatic response from the previous Figure 6 (now Figure 8). The fact that the monthly mean pressure anomalies are hydrostatic is to be expected.

(8) When calculating the "warmed" temperature profile in which the surface temperature is assumed to increase by 1 K, we now simply add the amplification factor to the background temperature at every height. This procedure is much simpler and generates the same "warmed" lapse rate profile (Figure 7).

(9) Title shortened.

There have been many other small changes. Many of these are discussed in the responses to the reviewers below.

Response to Reviewer 1

Title: Review of The Melting Level Stability Anomaly in the Tropics

The author uses a homogeneous set of radiosonde data from 5 west Pacific islands to show the relationship between monthly temperature changes in the boundary layer and aloft. The vertical structure of this variability is quite complex and the author focuses on changes in the profile at and below the freezing level. These changes are consistent with a model previously published by the author. The changes at all levels are not consistent with what would be expected if the atmosphere maintained a moist adiabatic structure through the changes. The author is careful to distinguish his results from others that purport to show how global warming might affect temperature profiles. The results are expected to be different because the boundary layer temperature changes in his record are more likely to be local rather than global. However, the author uses some CMIP model runs to show that somewhat similar results are found when temperature changes in climate models. These results are interesting but not very conclusive due to the fact that details of temperature profiles in models are almost completely the result of the models - cumulus parameterizations, in which we have little confidence. The results of this paper are interesting and I see no significant errors in method or conclusions inconsistent with the results. I recommend publication subject to minor revision.

Minor comments:

1. I think that the discussion in the first 3 paragraphs of the introduction is a bit off the mark on two points:

(a) In the first paragraph the author states that "The resulting acceleration in fall speed [at the freezing level] increases the downward flux of ice condensate." This is not correct; the increase in fall speed is matched by a corresponding decrease in particle density, which means that the flux, which is the product of the two, remains constant. Precipitation mass continuity insures this.

This sentence has been removed. More generally, we have removed almost all discussion of cloud microphysics in the text. It is simpler, and more directly observationally justified, to attribute the existence of the melting level stability maximum to the effect of the stratiform temperature response to deep convection, now shown in Figure 2.

(b) The author assumes that supercooled liquid droplets will form in the mesoscale updraft above the freezing level. If this occurred in significant amounts, the precipitation particles would rime, producing graupel rather than snow. If there is any growth of ice particles in stratiform regions above the freezing level, my guess is that it would be via vapor deposition on ice. The mesoscale updraft probably isn't strong enough to cause the vapor pressure to reach liquid saturation values in the presence of a high concentration of ice crystals.

These microphysical comments in the original draft were also outside the scope of the paper and have been removed.

2. Page 11569, line 29: My impression is that the MLSA doesn't shift upward; it deepens. (Does the bottom rise also?)

From looking at Figure 7, I think it is appropriate to describe the first order change in the MLSA as an upward shift. However, it is true that the height of the top of the boundary layer occurs at 2 km in both the warmed and background lapse rate profiles, and that this does give rise to an effective deepening of the MLSA. We have added a paragraph in Section 3.4 noting that the MLSA does also deepen in response to an increase in surface temperature.

3. Section 2.3: I think that the discussion of climate model results needs to be tempered by the realization that temperature structure is governed largely by cumulus parameterizations, which are anything but reliable when it comes to such details.

We now mention in Section 3.6 that the use of convective parameterizations is the most likely reason for the lack of a secondary maximum in the amplification factor of

the climate models. However, we also note that a cloud resolving model appears also to not exhibit the secondary maximum.

4. Page 11577, line 18: I don't see a dashed gray curve in figure 6 just a solid curve and a curve defined by little circles.

This figure has been modified. We no longer show the little circles, because the fact that the pressure response above the surface is hydrostatic is unsurprising.

5. Figure 3 caption, line 4: Shouldn't 10 be 10 km?

This is fixed.

Response to T. Dunkerton (Editor)

Radiosonde data from the western Pacific are used to examine monthly mean temperature and pressure fluctuations in relation to near-surface temperature fluctuations, taking care to separate out the occasional months when precipitation was atypically low at the station locations. The results yield vertical profiles of "temperature amplification" factor analogous to those that might be derived theoretically from a moist pseudo-adiabat.

The response of the tropical atmosphere to changes in tropical near-surface temperature is a complicated problem owing to the fact that part of the response will be dynamical, and therefore likely to be communicated from one place to another. Supposing for the sake of argument that zonal variations are inconsequential, and that the tropics are entirely self-contained, we must still consider meridional variations that drive the tropical branch of the Hadley circulation.

The problem is reasonably simple only if the imposed surface fluctuation is independent of latitude, AND is imposed on an initial temperature distribution that is also independent of latitude. In this case, the observed response should correspond to the theoretical amplification factor.

In reality the initial distribution is not uniform, and there exists a Hadley circulation (Held and Hou, 1980 JAS). For an updated perspective on this problem the author may consult Fang and Tung (1996 JAS). The dynamical response complicates the interpretation significantly. Nevertheless I would be willing to entertain the notion that, because these radiosonde stations are located in the warm pool, where meridional gradients of SST are weak, one might expect something akin to the theoretical amplification factor to be observed at this location.

For the above reasons, and others, we have removed any reference in the paper to a pseudoadiabatic amplification factor. As stated above, there are theoretical reasons why it may be unobservable in the real atmosphere, as well as ambiguities in how an observationally based amplification factor should be defined.

As editor and reviewer, my gravest concern is with the accuracy of the amplification factors as derived from the regression analysis (see comment below). Please address this potentially major issue, and all minor comments, prior to submission of a revised version for ACP.

Discussion on this issue given below.

MINOR FIXES AND IMPROVEMENTS:

(page.line) these numbers were derived from the submitted pdf, version 2

2.102: 11 year record

Fixed, except now changed to 10 year record everywhere.

2.120: changes in surface temperature

Fixed.

2.150: ordinarily

Fixed.

2.150: suggest for clarity "...dry season. Dry months nevertheless occur at these locations occasionally, as during an El Nino event when precipitation shifts to the central and eastern Pacific."

We no longer use 1998 in the analysis, so have removed references to the 1998 El Nino event.

2.157: suggest for clarity "...below a particular threshold as observed, for example, during an El Nino event in the first four months of 1998."

See above.

With these two clarifications it will then be clear that dry anomalies can occur at these locations due to inter-annual, but not seasonal, variability.

2.157, 3.241, 3.250: Minor clarification is needed regarding the data removed under light-rain conditions: were these data included in Figure 2 and in Figure 3's best fit? Or are the gray dots in Figure 3 excluded from both? (also see 4.289)

In the caption to Figure 5, we now clarify that the light rain months were removed.

3.216: boundary layer

Fixed.

3.257: suggest for clarity "reductions of sea surface temperature around the radiosonde stations"

This reference to El Nino was removed, due to removal of 1998.

4.318: (possibly major comment)

I noticed in Figure 3 that the regression of y upon x yields a slope significantly less than one might infer from the black dots themselves. This problem is common to such regression: minimizing the variance of y per se does not yield the correct slope that minimizes the "moment of inertia" of the black dots about the sloping line. Regression

of x upon y might have the opposite problem. The moment of inertia depends on distance from the sloping axis, not from x- or y-axes individually.

*The moment of inertia is not defined uniquely when units on these axes differ (since their scaling is arbitrary) but fortunately this is not an issue in Figure 3. You will need, first, to scale the axes identically in order to judge the quality of fit by eye. After making this assessment at 1:1 you can then **display** the results at*

whatever aspect ratio you want.

A steeper slope, as suggested by the black dots, would put the data in better agreement with the pseudoadiabat, which would make quite an interesting result.

I interpret this comment as similar to the comments given by reviewer 3 questioning the appropriateness of a regression in which the near surface temperature anomaly is defined as an independent variable (forcing), and the free tropospheric temperature anomaly is defined as the dependent variable (response). For example, one could normalize construct standardized temperature anomalies by dividing each anomaly by the standard deviation at each height, and then define a regression line by minimizing the perpendicular distance between each point and the best fit. This type of regression is more appropriate if trying to avoid any assumption of causality between the variables. In general, this approach does generate steeper slopes which can be considerably larger than the slopes calculated here. We have not adopted this approach here, however, since have we decided to frame the question as trying to determine the effect of local changes in boundary layer temperature on the local temperature of the free troposphere. In terms of the radiative convective interaction between temperature anomalies in the boundary layer and free troposphere, this question is, in practice, easier to address since the boundary layer temperature anomaly can be much more easily expressed in terms of a single value

than the free troposphere (though the paper now notes that ambiguity in the definition of depth of the boundary layer does generate variability in the temperature amplification

profile). In other words, there is in practice an asymmetry in causality, because the forcing that a 1 km interval of free troposphere exerts on the boundary layer should be much smaller than the reverse forcing.

We have tried to clarify the assumptions in out statistical approach in a paragraph in Section 3.4, starting with line 221.

4.323: "warmed" temperature profile would be better, in my opinion

This suggestion has been adopted.

Also at Section 3.5 header, 4.372, 4.376, 5.378, 5.380, Figure 7 caption

Fixed.

4.327: suggest for clarity "...2008, the observed pressure anomaly at a given (geometric) altitude can be plotted against the surface temperature anomaly, as done for 10 km temperature in Figure 3. The change in pressure..."

This section has been re-written.

4.336-340: The argument assumes that all mass stays in the column, hence the hydrostatic surface pressure is unaltered.

The assumption that the surface pressure is fixed has been added to the start of Section 3.5.

4.338: warming of the atmosphere below

Yes, now use "underlying" atmosphere in second line after start of Section 3.5.

There is no constraint on how temperature aloft changes in this argument. Elsewhere we have noted that middle atmosphere cooling implies a contraction of the atmosphere (Dunkerton et al., 1998 GRL).

4.374: Figure 5

Fixed.

5.383: That the lapse rate change has a "node" near 4 km is consistent with the temperature amplification factor having a secondary maximum there.

This is noted.

5.405 vs 5.411: My apologies in case I have missed the point. Since you use monthly means from the longer model dataset, it is the same monthly mean – not long-term climate – variability that is being assessed, as in the radiosonde analysis. You simply have a longer record of data. For climate variability, the temperatures would be averaged over a much longer period, prior to regression and calculation of the amplification factor. That is a separate question, which you could address also, using the model data. (The radiosonde data might be problematic owing to calibration issues.)

Yes, the only difference with a longer term dataset is that the anomalies are defined with respect to a longer term baseline. However, as the baseline period gets longer, you would expect the anomalies to include more of a climate component, and that this climate component would be more strongly coherently expressed (spatially and temporally) both at the surface and in the free troposphere. In this case, the correlations and amplification factors would increase with an increase in the length of the baseline time period (provided you had a stable homogeneous observing system).

Rather than defining the temperature anomalies of a particular month, we could define them for a year, or some other time period. However, since as mentioned, this would be problematic to attempt with the radiosonde data, it would go outside the current scope of the paper, which is mainly that the model comparisons are used as an aide to interpreting the radiosonde data.

We now discuss some of the differences associated with using a different baseline period in Section 3.6.

5.474: also at 4.294, Figures 5 and 8: Some error bars on the observed amplification

factor would be helpful, if you can generate them (after resolving the best-fit issue above). The low values of r^2 may not suffice to prove significance (e.g., at 95MLSA signal is the more important thing.

We now show error bars on the amplification profile shown in Figure 9.

Figure 3 caption: mean 10 km temperature anomaly as a function

This is fixed.

By the way, do the black circles represent a binned average of black dots, or something else?

Yes, this is now discussed in the caption to Figure 5.

Figure 4: Sum of gray dot probabilities = 1 ?

This figure has been removed.

Figure 6: The comparison to hydrostatic balance may or may not be meaningful, depending on (i) whether the radiosonde measurements are accurate enough to assess balance, and (ii) whether they were corrected a priori to satisfy hydrostatic balance automatically, prior to archival. I don't know the answer... perhaps you do.

We have removed from Figure 8 the values corresponding to hydrostatic balance.

All figures: Whether the duplicate annotation on axes left/right and above/below is "trend-setting" or simply an "unforced error" it might be removed for simplicity of style. Annotation of the opposite axis is normally done when another quantity with different units is shown also (as in Figure 4,5).

This has been corrected.

Thanks for your interest in ACP and for submission of a succinct, well-written paper.

Please also note the supplement to this comment:

http://www.atmos-chem-phys-discuss.net/12/C10855/2012/acpd-12-C10855-2012-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 11567, 2012.