Atmos. Chem. Phys. Discuss., 12, C10867–C10873, 2012
www.atmos-chem-phys-discuss.net/12/C10867/2012/

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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Received and published: 20 December 2012

The current state of the manuscript has been uploaded as a supplement. The response to the other reviewer also gives a summary of the main changes to the manuscript, and a response to editor comments.

Response to Reviewer 3

The subject of this paper is the layer between 3-5 km in regions of active tropical convection in which atmospheric stability deviates markedly from that produced by simple moist ascent. Johnson et al [1999] had drawn attention to a level of enhanced stability at the melting level in the COARE intensive flux array, and related this to the

height of cumulus congestus echo tops. In Folkins [2009], the author developed a one dimensional model in which mesoscale downdrafts forming below precipitating stratiform anvils produce stability and relative humidity anomalies similar in structure to those observed in a tropical radiosonde climatology. In the present paper, the author uses a radiosonde dataset from the western Pacific to examine the response of this "melting-level stability anomaly" (MLSA) to changes in near-surface temperatures, the goal being to show that the response is consistent with the hypothesis that the MLSA is due to a rapid increase of stratiform downdraft mass flux below the melting level.

We now emphasize that the stability maximum at the melting level is due to the stratiform temperature response of the background atmosphere to high rain events on short timescales. It is likely that the stratiform temperature response is generated, to a large degree, by mesoscale downdrafts. However, the paper, by itself, can't make this connection, so this part of the paper has been removed and replaced with Figure 2.

The author's approach is to examine profiles of monthly mean of temperature, pressure and relative humidity over the period from 1998-2008 at a set of 5 sonde stations in the western Pacific gridded at 200 m intervals. In Figure 1, the time-averaged sonde data show a deep layer from 2 to 7 km within which lapse rates exceed a representative pseudoadiabatic lapse rate, with a relative lapse rate minimum at 4 km and relative maximum at 5.5 km. The latter defines the upper edge of the MLSA. Temperature anomalies at each grid height are then scattered against the temperature anomalies below 1 km (called near surface anomalies) to derive the change in temperature at each height for a 1-degree near-surface anomaly. The resulting response profile in Figure 5, which only begins to go negative at 16 km, has some subtle structure within the MLSA - i.e. a relative maximum in the response near 3.5 km and relative minima near 2 and another near the 5.5 km.

(Error bars on the temperature response profile would also seem to be in order, particularly as the r2 values throughout the free

troposphere plotted in the figure are on the order of a third.)

Error bars have been added to the observed amplification factor shown in Figure 9.

The author states that these changes are consistent with an upward displacement of the MLSA. This is evident from the lapse rate plot in Figure 7, though the "warm" lapse rate differs from the background in other ways besides a simple upward shift. For example, one could alternatively interpret the warm lapse rate layer above 4 km as more stable and deeper than the background lapse rate.

The other reviewer also made the point that the MLSA appears to become deeper, due to the depth of the boundary layer apparently remaining fixed, and the height of the melting level stability maximum moving upward. I agree that this is a potentially important point, and is now mentioned.

However the change to the "warm" lapse rate and its effects on the MLSA are interpreted, the results do make physical sense, in particular the pressure response shown in Figure 6. Nevertheless, the fundamental approach used here is to calculate the "warm" response of the atmosphere at each level independently (as stated in the first paragraph of §3.3) ; coupling of anomalies in the vertical is not taken into account. A more complex analysis might not be warranted

here ; after all 11 years of monthly anomalies does not make a large dataset, but I think a more thorough physical justification of the simple approach that was adopted would benefit the argument.

I agree that it is simplistic to think of the variability in the atmospheric temperature profile to be driven, independently at each level, by changes in near surface temperature. In particular, the calculation of the slope (or amplification factor) assumes that the surface temperature is the independent variable (or forcing), and that the temperature anomaly in the free troposphere is a response. Deviations from the regression are considered "noise". In reality, temperatures in the free troposphere partially determine

CAPE, so also play a regulating the rainfall rate and temperatures in the boundary layer. During the approach to radiative convective equilibrium, it is more appropriate to think of free tropospheric and boundary layer temperatures as converging into balance with each other, rather than a one way causality in which temperatures in the free troposphere respond to near surface temperature anomalies. However, we have retained our previous analysis. This is partly because, in the real atmosphere, it may be appropriate to think of free tropospheric temperatures as more strongly subject to dynamical noise (nonlocal influences) than temperatures in the boundary layer. Second, this approach has been widely used, in climate perspective, when attempting to determine the response of temperatures in the free troposphere to changes in temperature near the surface. Third, there is a kind of asymmetry between the boundary layer temperature anomalies and the free troposphere, in that the temperature anomaly of the boundary layer can be more easily characterized

as a single variable (due to turbulent mixing). We have added some discussion of these assumptions in Section 3.4.

In Section 3.6 the response of CMIP models to a near-surface warming is calculated and compared to the response in the five-station radiosonde set. As stated in the final sentence of the first paragraph in this section, the goal is to show that the amplification factors calculated with the models are similar to those with the radiosonde data. Based on Figure 8, the author concludes that they are similar.

The motivation for introducing the models has been re-written. The main purpose was to determine whether the 10 year amplification factor of the models was similar to the 50 year amplification factor of the models. If so, there would be some basis for believing that the 10 year amplification factor of the radiosondes would be similar to a radiosonde amplification factor calculated from a longer term record. This argument is made at the beginning of Sections 2.3 and 3.6. We do not want to argue that the amplification factors of the models are necessarily in good agreement with the radiosonde data. The differences between the models and the radiosondes are emphasized in the last two sentences of the paper.

I would agree that they are similar to the extent that the amplification factors are substantially lower than the moist adiabatic. Nevertheless, the models response in the upper troposphere is much smaller than the sonde response, and taken as a group, the six climate models fail to reproduce the enhanced temperature response in the MLSA seen with the radiosondes. (Although one, the NCAR CCSM3 model, does exhibit a stability minimum at 4 km.) Given that the focus of the paper is on the MLSA and its physical basis, it is not made clear to the reader how the amplification factor calculated for the upper troposphere is relevant in the first place. This is especially so considering the clear difference in amplification factors between the models and the sondes in the lower atmosphere. A link is not made explicitly until the sentence beginning on line 28 in the Conclusions section. Here the lack of the large upper tropospheric amplification factors of a moist adiabatic response in the radiosonde and model data sets is attributed to the "complex response" in the lower troposphere. How this happens is not discussed however, but it certainly cannot be related to the response of the MLSA, since that is missing in all but one of the six CMIP models examined here.

Yes, the upper tropospheric response of the models is not the main focus of the paper. However, we did want to show the model comparisons to see if the 10 year and 50 year amplification factors of the models were similar to each other. Once we show the model results, we are obliged to comment on them. However, I have removed the argument given in the original draft for why the large adiabatic amplification factors are not observed (complex lower tropospheric response). This argument was inconsistent with the model results.

To summarize this point, it seems to this reviewer that the CMIP model analysis and the tie-in to the larger controversy about large-scale response the atmosphere to a warmer tropical sea surface are not clearly relevant to the question posed in the abstract, that is whether or not a warmer sea surface temperature shifts the MLSA upward. The answer to this question is clearly yes, although the simple methodology used to address the question does relatively little to reveal the physical significance of the result. Once a response was established in Figures 5-7, this discussion thread is more or less abandoned and the author shifts to a rather different discussion with a focus on the amplification factors. There are hints at various places throughout the text to the vertical coupling of the tropical atmosphere through convection, rainfall, large-scale subsidence and other processes, but they are not brought together in a clear statement of how these would influence the MLSA or the more general context of the trimodal structure of the tropical atmosphere. This is a disappointment, as I was hoping for rather more from this author, who has a justifiably admirable record of original contributions.

I agree the paper does little to reveal the physical significance of the upward shift in the MLSA, or the origin of the trimodal structure of tropical convection. These issues would be difficult to address in the absence of a model. However, we do now show that the MLSA originates from the stratiform temperature response of the atmosphere to high rain events. If one accepts (1) that the stratiform heating profile originates from precipitating stratiform clouds, and (2) that

the lower surfaces of these clouds are fixed near the melting level, one then does have an explanation for the upward shift. This paper hasn't demonstrated (1) or (2), but they are reasonable assumptions.

Errors in the text:

I found two errors in the References:

1. Johnson et al appeared in J. Climate, 12.

Fixed.

2. Redelsperger et al appeared in J. Atmos. Sci., 59.

Fixed.

Recommendation:

Despite the diffuse and at times cryptic nature of the argument, the author clearly has a grasp of the complexities of the issue at hand. It could even be said that the paper in its present form pursues two separate issues. My suggestion is that the paper be accepted for publication with the recommendation that the text be revised so as to more clearly subordinate the amplification issues to the question of the physical underlying processes controlling the MLSA.

The paper has been re-written and should now be more clear. Figure 2 shows that the physical origin of the MLSA is the short timescale stratiform temperature response of the atmosphere in the neighborhood of high rain events. I am not sure how much further one could go without using a model, and the paper was intended to be a relatively short observationally based paper. The amplification issues are central to the paper, since the amplification profile is needed to calculate the shift in the lapse rate. However, the comparisons with the models have been downgraded. We have also removed comparisons with the moist adiabatic amplification factor. Given the numerous reasons why, in principle, it would be difficult to ever observe an “ideal" moist adiabatic amplification factor, these comparisons were an unnecessary distraction.

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