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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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Review of "Height Increase of the Melting Level Stability Anomaly in the Tropics" by Ian Folkins

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.

(b) The author assumes that supercooled liquid droplets will form in the mesoscale updraft above the freezing level. If this occurred in signficant 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.

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

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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.

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.

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

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