Atmos. Chem. Phys. Discuss., 13, C389–C390, 2013
www.atmos-chem-phys-discuss.net/13/C389/2013/


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Impact of land convection on the thermal structure of the lower stratosphere as inferred from COSMIC GPS radio occultations" *by* S. M. Khaykin et al.

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Received and published: 7 March 2013

This paper presents a novel perspective on temperature variations in the lowermost tropical stratosphere. Potentially it is an excellent addition to the literature on this topic, bringing together the unique observational capability of GPS occultation with process understanding of the physics and dynamics of the TTL. Much of it is excellent, and I have only one major area of concern – but it is a major concern.

The central hypothesis of this paper is that there is a diurnal variation in temperature in the tropical lowermost stratosphere caused by injection of air from overshooting convection. Unfortunately the evidence to substantiate this hypothesis just isn't there.

While fig.5 makes an unanswerable case for the association of late afternoon cooling in this region with overshooting convection, this remains an association – it does not prove cause and effect. Indeed the authors are well aware of the problem as they have dedicated a considerable part of the paper to a thorough (and to this reviewer very much appreciated) discussion of atmospheric tides and in the last part of section 4 they say 'finally, although overshooting of adiabatically cooled air masses appears a consistent explanation, a contribution of non-migrating tides, that is of internal gravity waves to the temperature diurnal cycle cannot be totally ruled out'. Well, quite. The paper doesn't even show that this hypothesis is unlikely, let alone rule it out. From a dynamical perspective this is by far the most likely explanation of the temperature cycle – look at the descending phase fronts in fig. 1 for example, which clearly extend down into the lower TTL. What is the gravity wave response to a regional maximum of convection such as the Amazon or Congo basins? Unless the paper can convincingly reject the gravity wave (or non-migrating tide) hypothesis, they cannot claim that the mixing hypothesis is the most likely.

I strongly suggest therefore that section 4 be re-written, giving equal prominence to the two hypotheses and pointing out the future work needed to distinguish between them. On the basis of the observations presented here, both are plausible - though to my eyes the gravity wave one appears most likely. Perhaps both effects contribute. A convection-permitting modelling study over a very large domain would be needed to investigate this further, and such runs are now possible, so that would be a suggested future direction.

As I would hope that this paper, suitably modified, will be widely read and well cited I recommend that the authors take the opportunity to get it proof-read as there are very many instances where the English needs to be improved.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 1, 2013.