

Interactive comment on “A permanent aerosol layer at the tropical tropopause layer driven by the intertropical convergence zone” by Q. Bourgeois et al.

Anonymous Referee #2

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This paper concerns an attempt to explain the origin of an aerosol layer in the tropical tropopause layer over Asian using CALIOP data and model simulation. This topic is an important area of research at the moment as the origin and fate of aerosol in the free troposphere is relatively poorly understood. In general the paper is well written, and compelling. However, there are some areas, particularly in the discussion of the potential uncertainties in CALIOP data, where more evidence is needed before the paper can be accepted for publication.

Major issues:

1. Section 3 contains a comprehensive discussion of the possible issues with CALIOP

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data, including the mis-classification of aerosol and cirrus. The authors state that they weren't able to filter data for high confidence in classification as the CAD score was low in all cases – they attribute this to low concentrations rather than doubt about classification. Section 4 (page 2876 line 2) states that the depolarization of the layer is mostly lower than 0.05 consistent with aerosol and in agreement with the findings of Vernier et al (2011). The argument for this layer being aerosol would be much greater if the depolarisation ratio was shown in Figure 1 in addition to the scattering.

2. Section 3 also discusses limitations in the methodology of Vernier et al (2011) including 1) the averaging of CALIOP data over large grid cells, 2) the use of different region for the free background aerosol and 3) the removal of aerosol extinction for which depolarization ratio was above 0.05. Whilst I tend to agree that each of these differences could provide a reason for the differing results, they are not explored sufficiently fully in this paper. For example, showing the depolarization ratio (as in the point above) would strengthen the argument that (3) is not necessary. More discussion of why the different regions were used for the calibration should be included. Finally, the authors tested the impact of (3) above, but did not test the impact of averaging over large grid cells. I suggest they do this to demonstrate their assertion that it is responsible for the difference in results.

3. Please discuss further how the estimate of the radiative effect due to cloud changes from the aerosol in this layer is made. Is it the difference in reflection back to space by cloud between simulations where aerosol is carried as a tracer and where it isn't? If this isn't the case, then this should be how the effect of the cloud is estimated.

Minor issues:

Page 2864, line 16 “radiations” should be “radiation”

Page 2864, line 17 – the abstract should make it clear that this 6Wm⁻² is over the Asian region.

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Page 2865, line 4 “condenstation”

Page 2867 line 8 “. . . hardly any little scavenging” needs changing

Page 2871, line 10 “hypothesis” should be “hypothesise”

Page 2877 line 5, “. . . due to the cloud. . .”

Page 2877 line 18 “strength”

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 2863, 2012.