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Interactive Comment

Interactive comment on "Height of convective layer in planetary atmospheres with condensable and non-condensable greenhouse substances" *by* A. M. Makarieva et al.

Anonymous Referee #2

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This paper studies the relationship of convective height and optical depth for condensible and non-condensible greenhouse substance by using one-dimensional toy model. In the paper, there are many derivations are questionable. The main conclusion is not convincing and suffers from a lack of observation support. I therefore could not agree to publish this paper in ACP.

1. (2.1) is one of physical bases for this paper. However, this linear relation generally does not hold true for the infrared radiation in the atmosphere. The outgoing infrared upward ux not only depends on the surface emission but also depends on the atmospheric emission. If the optical depth is small, the outgoing upward flux is more dependent on the surface emission; if the optical depth is large the upward flux is more

dependent on the local thermal emission at a high altitude. For example in a correlated k distribution model, for about a half number of k (corresponding to the large optical depths), the corresponding outgoing flux is only dependent on the thermal emission inside the atmosphere. In the later part of this paper, the authors emphasize that their theory applies for the case of large water optical depth.

Though the discussion is based on a toy model, it is still not clear what the optical depth means, since the optical depth is highly dependent on the spectral wavelength.

2. In equation (2.7) the second part was derived by the authors, but the more important frst part

$$\frac{\tau}{\tau_s} = \frac{p}{p_s} \tag{1}$$

is based on the assumption of pressure broadening for extinction coefficient (see Chou and Arking, 1980, JAS). How could the authors derive it? This assumption is generally true for regions in the lower atmosphere (less than 100 mb), as in such region the line shapes for gaseous spectrum generally satisfy the Lorenz profile and spectral line widths could be broadened by pressure effect. However the optical depth used in this paper is a kind of broad band mean result, it is not clear if such relation still holds true.

Note in (2.7): the ratio of water vapor optical depth is proportional to the ratio of total pressure. However, in (3.5) the relation is changed to the ratio of water vapor optical depth being proportional to the ratio of water vapor partial pressure,

$$\frac{\tau_L}{psL} \approx \frac{p_L}{p_{sL}}. \tag{2}$$

This is incorrect. The pressure broadening effect is due to molecular collisions from all gases, not only from water vapor molecules. The water vapor mixing ratio is very small in the atmosphere.

Above relation in (2) plays crucial role in the derivation of the main conclusion (3.10).

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By using Clausius-Clapeyron equation the authors derived another relation in

$$\frac{p_L}{P_{sL}} \approx \left(\frac{p}{p_s}\right)^{\beta_s}. \quad (3)$$

This is very skeptical. In the atmosphere the relation between water vapor pressure and total pressure generally can not be so well defined. The change of water vapor pressure has very little impact on total total pressure.

3. Clausius-Clapeyron equation is extensively used in this paper. In (3.9) Clausius-Clapeyron equation is used again to derive the relation between saturation vapor pressures at dierent moments. Then (2) is used to find the relationship between water vapor optical depth and surface temperature. However, in (2) the water vapor pressure at the surface, p_{sL}, is the partial pressure produced by the accumulated water vapor above the surface, and it has no necessary connection to the saturation vapor pressure corresponding to a certain surface temperature. In some extent, the water vapor profile is determined by the large scale dynamic transport, also cloud locally is the most important process to modify the water vapor profile in the atmosphere. The water vapor pressure (optcal depth) is not only determined by the surface condition.

4. Based on above incorrect premises, the authors desired the main conclusion of (3.10), that the OLR will exponentially decrease with the increase of surface temperature. At least for me there is no such evidence in the climate simulations. The authors could test their results through ERBE results for EL Niro and non-EL Niro years.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 6701, 2003.

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