Atmos. Chem. Phys. Discuss., 3, S2531–S2534, 2003
www.atmos-chem-phys.org/acpd/3/S2531/

Atmospheric Chemistry and Physics Discussions

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

Received and published: 6 February 2004

I would like to add more comment in response to the authors' reply.

1. The terrestrial atmosphere may or may not be transparent, it depends on the atmospheric optical depth. As I suggested to the authors, please check the 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.

2. The authors disagree that (2.7) is derived based on the pressure broadening of the extinction coecient and they provided a proof in their reply. Here, I first show how to prove this relationship from the broadening of the extinction coefficient (if the physics is correct), but different approaches should give the same answer. Assume the extinction

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper

coefficient being pressure broaden as

$$k \sim k_s \frac{p}{p_s}$$

where k_s is the extinction coefficient at the surface. Then

$$\frac{\tau}{\tau_s} = \frac{\int_{\infty}^{z} k\, dz'}{\int_{\infty}^{0} k\, dz} = \frac{\int_{\infty}^{z} e^{-z'/H} dz'}{\int_{\infty}^{0} e^{-z'/H} dz} = p_s$$

where H is the scale length.

In (4) of the reply, the physics is actually the same as above. The extinction coefficient is proportional to $N\Sigma \sim p\Sigma$. The authors must make an assumption that the concentration of the substance at height z being proportional to the total pressure, otherwise I don't see how they complete their proof.

My main question was that in (3.5), the relation is changed to the ratio of water vapor optical depth, which is proportional to the ratio of water vapor partial pressure,

$$\frac{\tau_L}{\tau_{L0}} \approx \frac{p_L}{P_{sL}} \tag{R1}$$

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

In the reply, the authors said this is obtained on the basis of the observation. To me, this is not convincing at all.

3. I questioned

$$\frac{p_L}{p_{sL}} \approx \left(\frac{p}{p_s}\right)^{\beta_s}$$

since the relation between water vapor pressure and total pressure generally can not be so well defined. In the atmosphere, the change of water vapor is dramatic with time and location; while the change of pressure is relative very small.

ACPD

3, S2531–S2534, 2003

Interactive Comment

4. I mentioned "water vapor pressure at the surface, p_{sL}, is the partial pressure produced by the accumulated water vapor above a surface, and it has no necessary connection to the saturation vapor pressure corresponding to a certain surface temperature". There is nothing wrong in this statement. The surface temperature could affect the water vapor profile in some extent, as the authors mentioned Raval and Ramaranthan (1989) obtained a 20% agreement in determining the surface vapor pressure by using Clausius-Clapeyron equation, however, in that work the surface vapor pressure is 100% determined by Clausius-Clapeyron equation. This could easily lead a wrong conclusion.

5. Generally OLR will increase with the increase of surface temperature as shown (2.1). However, since the water vapor could increase with the increase of surface temperature, it is possible that more thermal energy could be held by the atmosphere as the surface temperature increases. This is the so-called super green house effect. However, so far this kind water vapor feedback effect is far from understood. What response of large scale dynamics and cloud have to this effect are not very clear to us at present.

From (3.10) the OLR will **exponentially decrease** with the increase of surface temperature. To me this conclusion is too strong. What kind of greenhouse effect should this be called!

Authors repeatedly mentioned the work of Raval and Ramaranthan (1989), but I don't find any data shown in that paper supporting the authors' conclusion. I also suggested to the authors to test their results through ERBE results for EL Nino and non-EL Nino years.

5. Finally the authors expressed their disagreement to the words of "toy model", generally this word means the simple model in atmospheric modeling, like one-dimensional or even two-dimensional energy balance models. Perhaps "highly idealised model" would be more appropriate.

ACPD

3, S2531–S2534, 2003

ACPD
3, S2531–S2534, 2003

Interactive
Comment

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper