Atmos. Chem. Phys. Discuss., 3, S2635–S2646, 2003
www.atmos-chem-phys.org/acpd/3/S2635/

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.

**A. M. Makarieva et al.**

Received and published: 7 March 2004

In this commentary we reply to the comments of Referee No. 1.

*1. The referee finds confusing "the idea that past models are incorrect because assume that the air is saturated at $\tau = 1$." He adds that "past radiative convective modelling studies by Ramanathan etc. certainly do not assume this".*

As is explicitly stated in our paper (see abstract and elsewhere), we discuss the behaviour of the outgoing thermal radiation at large values of atmospheric optical thickness corresponding to high surface temperatures. Several previous radiative-convective studies that we explicitly cite in the Introduction pursued the same goal and found a plateau of the outgoing radiation at large $\tau_s$, that is, no change of the outgoing radiation with increasing surface temperature and $\tau_s$.

That this plateau arises namely due to the assumption of the atmosphere being saturated at $\tau \sim 1$ was explicitly demonstrated by several authors of the papers we criticize (see, e.g., Nakajima et al., 1992, p. 2261, right column). In our paper we have just reformulated this statement using the mathematics of our paper, to make clear how this statement relates to our conclusions.

In our paper we show that the assumption of atmosphere being saturated at $\tau \sim 1$ is incorrect, as far as optical depth of the tropopause increases with growing $\tau_s$. At large $\tau_s$ the upper radiative layer $\tau \sim 1$ is located in the stratosphere, which cannot be saturated with respect to water vapor (the saturation is due to the presence of a sufficiently large negative temperature gradient and occurs in the troposphere only, see the paper and below).

While it is true that radiative-convective studies by Ramanathan etc. did not assume saturation at $\tau = 1$, it is equally true that none of these studies stated that the outgoing flux into space is independent of atmospheric optical thickness $\tau_s$ and that there is a plateau of the outgoing thermal radiation at large $\tau_s$.

*2. The referee is "a little more than confused why accurate radiation models coupled to a physically based convection scheme would get this relationship wrong, whereas the more idealised methodology gives different answers".*

The answer to this question is to be found in a detailed analysis of the assumptions that are laid in the ground of the "accurate radiation models". Such models (we presume that here one is referred to modern global circulation and radiative-convective models) are ultracomplex in terms of the number of degrees of freedom and computational power involved, but lack clear underlying physical mechanisms. Instead, they operate with phenomenological relationships which effectively means postulating the unknown dependencies between different variables. Most research today is concentrated on making models even "more accurate" by increasing computer power, while the physical foundations of such models are rarely subjected to a critical analysis. As time goes, such an analysis is becoming increasingly troublesome for those who ultimately decide to undertake it, because the discovered faults will make such people oppose the majority of the scientific community and the long-standing tradition.

Such an analysis is not the topic of our paper either. However, to support the strong statements of the above paragraph, here we give just one example of a physically incorrect parameterization laid in the ground of a large family of both global circulation and radiative-convective models. This parameterization deals with relative humidity.

### *Relative humidity*

Relative humidity is defined as the ratio between the observed partial pressure of water vapour at a given altitude and temperature, $p_L(z,T)$, and the saturated water vapour pressure at this temperature, $p_L^*(T)$:

$$H_{rel}(z,T) \equiv p_L(z,T)/p_L^*(T). \tag{1}$$

As is well-known, partial pressures of atmospheric gases drop exponentially with altitude in the gravitational field of Earth. The concentration of water vapor is also maximum at the planetary surface (two-thirds of which is liquid water). If the atmosphere had a uniform temperature, $T = \mathrm{const}$, one had $p_L^*(T) = \mathrm{const}$, and relative humidity would exponentially drop with altitude proportionally to partial pressure of water vapour, $H_{rel} \propto p_L(z)$.

However, in the real atmosphere temperature drops with altitude too, $T = T(z)$. In this case the resulting dependence of $H_{rel}$ on altitude depends on saturation pressure function $p_L^*(T)$, which is described by Clausius-Clapeyron equation, that is, an exponential dependence of $p_L^*$ on $T$.

Mean relative humidity at the planetary surface is of the order of unity (Stone and Carlson, 1979), indicating a close to saturation state. The Clausius-Clapeyron equation prescribes an about two-fold drop of $p_L^*(T)$ per each ten degrees of decrease in $T$. This is a larger magnitude than the gravitation-related drop of $p_L(z,T)$. Thus, $p_L^*(T)$ drops with altitude more rapidly than would do $p_L(z,T)$ if far from saturation. In other words, if relative humidity at the surface were significantly lower than unity, $H_{rel\,s} \ll 1$, relative humidity would **grow** (not drop) with altitude (at the observed values of atmospheric lapse rate). But as far as $H_{rel}$ cannot be larger than unity and is already close to it at the surface, water vapour is – on average – close to saturation at all heights where the magnitude of temperature gradient is sufficiently large to ensure rapid drop of $p_L^*(T)$ with $z$. (The corresponding increase in $H_{rel}$ which cannot be realised due to the natural limitation $H_{rel} \leq 1$ is compensated by condensation of water vapour in precipitation.) This situation corresponds to **constant mean** relative humidity in the troposphere.

Quantitatively, this effect is in detail described in our paper.

The physically transparent statement of constant mean relative humidity was employed as early as in 1979 by Stone and Carlson (1979). Recently, this statement was demonstrated – quite expectedly – to excellently fit the available satellite data on atmospheric water vapour content (Wentz and Schabel, 2000). The assumption of constant relative humidity $H_{rel} = 1$ (saturated troposphere) was employed in most papers where greenhouse effect was attempted to be studied from the first physical principles (e.g., Ingersoll, 1969; Kasting, 1988; Nakajima et al., 1992).

In the meantime, in the past global circulation (Manabe and Wetherald, 1967) and radiative-convective (Ramanathan and Coakley, 1979) models one postulated a phenomenological parameterization:

$$H_{rel} = H_{rel\,s}\frac{p/p_s - 0.02}{0.98}, \tag{2}$$

where $H_{rel\,s}$ and $p_s$ are relative humidity and air pressure at the surface.

This unphysical parameterization (put forward by Manabe and Wetherald (1967) and based on fragmentary observations dating back to the 50s) implies **an exponential drop (!)** of relative humidity in the lower atmosphere, where $p/p_s \gg 0.02$ and $H_{rel}$ is simply proportional to air pressure $p$ which decreases exponentially with altitude. With no attempt to discuss its possible physical content, this parameterization was adopted in most models (with minor modifications from model to model).

How can one explain the observed water condensation and cloud formation at a considerable atmospheric height (not at the surface!), if the relative humidity drops exponentially with altitude starting from the surface where it is already less than unity! If the above parameterization (or its relative) continues to be employed in modern global circulation models (it is virtually impossible to trace modern ultracomplicated hierarchical models to their underlying physical assumptions), their forecasts are anything but physically responsible.

It is indicative that in later works Ramanathan et al. apparently did not further promote the above unphysical parameterization of $H_{rel}$. In particular, Weaver and Ramanathan (1995) wrote that the observed vertical distribution of atmospheric water vapour can be quantitatively explained from Clausius-Clapeyron equation, using the observed lapse rate and assuming **constant relative humidity.**

It is hardly productive to consider numerical models based on postulated phenomenological relationships and artificially fitted to describe the observed data as "more accurate" or "more detailed", meanwhile labeling any effort to derive a physically sound picture of atmospheric radiation from the first, well-established physical principles as "highly idealised", "toy model" etc.

No matter how accurate, phenomenological parameterizations, that is, parameterizations not based on fundamental physical regularities, are deprived of predicative power, as far as their validity is, by definition, confined to the observed range of values. In this

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respect phenomenological parameterizations differ little from mere tabulation of the observed data. Extending such phenomenological parameterizations beyond the observed range of values does not have any justification and may result in serious errors, including the violation of energy conservation law, in any field of science (Makarieva et al., 2004).

*3. The referee recommends, in order to "to convince the readers that the conclusions are valid",*

*3.1 to explain clearly what assumptions they are making and why these might or might not be robust*

All quantitative results and conclusions obtained in our paper are obtained on the basis of well-established physical regularities:

1) diffusion of thermal photons described by Eddington's approximation (discussed in detail in our third reply "Radiative transfer, greenhouse effect" to Referee No. 2, see also the end of present response);

2) the second law of thermodynamics according to which energy passes from objects with higher temperature (in our case air with temperature $T$) to the objects with lower temperature (thermal radiation representing the ultimate stage of dissipation of all ordered energy fluxes in the planetary system);

3) the existence of a threshold value of the atmospheric temperature lapse rate, above which convection is switched on;

4) hydrostatic equilibrium of atmospheric air;

5) Clausius-Clapeyron equation describing the dependence between saturated concentration of water vapor and temperature.

In our paper we do not use **any postulated relationships that would be lacking a clear physical background and based on fitting an arbitrarily chosen mathematical function to empirical data** (as in the above discussed example of physically flawed parameterization of relative humidity, Eq. (2), in the "more accurate" models). The well-established physical regularities are robust in their nature and do not demand a further analysis of their validity. We are convinced that such well-established physical regularities but not phenomenological parameterizations constitute the only possible background for a quantitatively responsible analysis of climate change (as well as of any other problem of natural science).

In our previous responses, we have answered all concrete comments of Referee No. 2 that pertained the validity of the physical principles that we employed and the related derivation of our major statements.

*3.2 to contrast their results with results of a more detailed radiative-convective code and explain why differences arise (if possible)*

See items 1 and 2 of present response.

*3.3 to look for any observational evidence that could refute or justify their claim, going further than the anecdotal evidence for convection heights on Venus and at the equator of the Earth.*

It would be appropriate to note that the radiative-convective studies of greenhouse effect that we criticize in our paper are not overloaded with observational evidence. The structure of these studies is as follows. They start from several mathematically formulated physical assumptions and derive the behaviour of the outgoing thermal radiation at large values of surface temperature and atmospheric optical thickness. The importance of such type theoretical studies is obvious – they allow to envisage possible scenarios of planetary climate evolution at those values of planetary parameters that are not currently observed on the modern Earth or another planet. (For this reason observational evidence for these studies can be naturally missing.) It is for this importance that theoretical studies of such type are regularly pursued and published in mainstream journals.

In our paper we followed the same overall design of the paper organisation, and showed on the basis of well-established physical laws that one of the assumptions previously employed (i.e. saturation of the atmosphere at $\tau \sim 1$) is incorrect. We also show that this assumption is critical for the behaviour of the outgoing thermal radiation and that the results of the previous studies would have been different were this assumption corrected. At least in theoretical physics this point alone would make the case for publication, as fixing errors in the current state of art.

However, in our paper we go further than that. We show that our approach (with no artificial fitting to observations but standing on first principles alone) yields a quantitatively sound estimates of the height of convective zone on equatorial Earth and on Venus, corresponding to the region of large $\tau_s$ to which our study pertains. The ability to quantitatively reproduce the tropospheric height represents an important check-up for radiative-convective studies (Ramanathan and Coakley, 1979). The studies we criticize in our paper did not use this check-up to test the validity of their basic assumptions (see, e.g., Nakajima et al., 1992).

As we have explained in our first response to Referee No. 2, the predictions of our study with respect to the outgoing flux of thermal radiation (i.e. the exponential decrease of OLR with $T_s$) are not expected to precisely describe modern Earth. This is because modern Earth's climate is profoundly influenced by the ordered processes in the biosphere, which have direct impact on atmospheric humidity (through evapotranspiration and production of surface-active substances in the ocean) and surface temperature (through control of oceanic turbidity) (e.g, Sathyendranath et al., 1991). These processes appear to be the force which stabilises the OLR and terrestrial climate in its present life-compatible state (Gorshkov and Makarieva, 2002).

However, there is increasing evidence that at high temperatures these controlling biotic mechanisms fail and the OLR starts to be governed by physical mechanisms described in our paper. We have already noted (in our second reply to Referee No. 2) that, according to observations (Stephens and Greenwald (1991), at $T_s \geq 299$ K the OLR over cloudy sky drops with increasing sea surface temperature even more radically than prescribed by Clausius-Clapeyron equation.

Furthermore, in a detailed analysis of the dependence of the greenhouse effect on surface temperature in the entire tropical zone of Earth (again the region of larger $\tau_s$), Yang and Tung (1998, p. 2694) showed that "as the surface warms in the area mean, the area-mean outgoing longwave radiation **decreases** [our boldface], which is an indication of the enhanced greenhouse effect due to surface warming." This is in accordance with our prediction of decreasing OLR with growing $T_s$.

Notably, this observable effect, although completely non-emphasized in the current climate literature, is very important for the climate stability problem, as far as decreasing OLR with growing $T_s$ is equivalent to physical instability of climate, which we discussed in detail elsewhere (Gorshkov and Makarieva, 2002).

## *4. Summary*

We have provided as detailed as possible response to all comments of the two referees, to whom we express our sincere gratitude for the time they spent on our paper. We would be ready to incorporate any parts of our response(s) into the paper to make the text more clear and compelling.

However, we would like to point out that this is our second submission of the paper to ACPD. The initial text we submitted contained very detailed explanations of all the physics we employed to derive our statements. In particular, we presented an original derivation of Eddington's approximation and gave a detailed derivation of the relationship $p/p_s = \tau/\tau_s$, two issues that caused most problems with Anonymous Referee No. 2.

However, at that time an anonymous referee judged that our paper contained too much "well-known introductory material" and explicitly suggested to remove most of it as an indispensable condition for consideration of the paper. We followed this recommendation, the paper was published in ACPD in its present form, but now there is an apparent demand from the referees for additional explanations. This led us to think that, probably, this is a subjective point pertaining more to the attitude of a concrete reader rather than to the paper's contents.

*Note on previous comment*

In our previous response ("Radiative transfer, greenhouse effect" ACPD 2004, 4: S2615-S2625) a misprint should be corrected on p. S2621, 7th line from top: the formula for absorptivity should be $A_\nu(x) = (1 - e^{-\tau_s x})$.

We would also like to note that Eqs. (4) and (5) on p. S2621 are not equivalent to the radiative transfer equation (3) on p. S2620, as far as Eqs. (4) and (5) contain an additional assumption, namely that $J_\nu(\tau_s) = I_\nu(\tau_s)$.

The reason for putting $J_\nu(\tau_s) = I_\nu(\tau_s)$ is the assumption that at the Earth's surface radiation is in thermodynamic equilibrium, i.e. it is assumed that $I(\tau_s) = \sigma T_s^4$, where $T_s$ is surface temperature, which is equal to air temperature $T_{as}$ at the Earth's surface, and that $J(\tau_s) = \sigma T_{as}^4$. Such an assumption represents a very crude physical error. It is valid only for a stationary equilibrium radiation (e.g. radiation in an enclosure), where there is *no radiative transfer* and *no net flux of radiation*. Transfer of radiation is namely determined by the non-zero difference $I(\tau_s) - J(\tau_s) = I_0$. In the absence of radiative transfer, when this difference is zero, there is a uniform radiative field with radiation intensity independent of optical depth and spatial coordinate (like within the enclosure).

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For example, in a planar (stratified) atmosphere there is no radiative transfer in the horizontal direction and radiation intensity does not change with horizontal coordinate.

In radiative equilibrium $I_0 = I(\tau_s) - J(\tau_s)$ at all $\tau$ and, as far as $J(0) = 0$, one has $I_0 = I(0)$, i.e. $I_0$ coincides with the outgoing thermal radiation. In this case the radiative transfer equation solves as $I(\tau) = I(0)(c\tau_s + 1)$ ($c$ is a constant of the order of unity). This means that the equality $J_\nu(\tau_s) = I_\nu(\tau_s)$ may only hold *approximately* at large $\tau_s$, when $I_\nu(\tau) \gg I_0$.

Thus, in all modern calculations of the greenhouse effect that are based on Eq. (5) (p. S2621) and its modifications one effectively puts $I_0 = 0$, thus assuming absence of radiative transfer along the vertical axis from the very start. It is not surprising therefore that, after superimposing various spectroscopical formulae on the originally physically meaningless Eq. (5), one obtains physically meaningless conclusions about, e.g., logarithmic growth of the greenhouse effect with concentrations of greenhouse substances and $\tau_s$, instead of direct proportionality to $\tau_s$ with possible slight spectroscopically derived modifications of this major dependence. Ultimately, if the dependence of absorptivity on concentrations is neglected (as in grey substances), from Eq. (5) one arrives to the paradoxical conclusion that greenhouse effect is not at all affected by growing atmospheric optical thickness. This statement, as we noted, contradicts all foundations of the radiative transfer physics.

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