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Response

I am happy that Betts concurs that the water vapor budget is a crucial part of the climate system. As Betts notes, the fact that water vapor, closely followed by water in the form of stratiform clouds, is, by far the most important greenhouse gas, is rarely stressed. Moreover, the positive feedback that arises from using convective parameterizations which tend to moisturize the atmosphere at all levels when there is warming is the most important positive feedback in current large scale climate models—despite the popular emphasis on feedbacks from cloud cover (Arking 1990). What is frequently misunderstood is the special role of water vapor in the upper troposphere. As noted in Lindzen (1990), convection carries heat away from the surface to the upper troposphere; it is at these levels that the trapping of heat by greenhouse gases assumes its greatest importance. It is easily shown that, on a molecule for molecule basis, water vapor above 6 km is much more important than water vapor near the ground in determining the surface temperature (Arking 1990). Thus, even though water vapor near the ground increases with increasing temperature, a smaller absolute decrease at upper levels can lead to a negative feedback. This point was completely misunderstood by Raval and Ramanathan (1989).

Betts suggests that this failure to stress the role of water substance may arise from the fact that these substances have short relaxation times compared to the minor greenhouse gases. I find this remark incomprehensible. The relaxation time of water substance has nothing to do with its infrared properties. Indeed, it is its short response time that peculiarly enables it to play a possible regulatory role--a role it could not play if its response time were slow.

Betts's contention, that the role of deep convection in the moisture budget of the upper troposphere is not simple, is hard to contest. However, his discussion unnecessarily confuses the situation. First, Betts misrepresents both my approach to cumulonimbus parameterization (Lindzen 1988; Geleyn et al. 1983), and that of Arakawa and Schubert (1974). He asserts that both approaches deal with clouds that originate near the ground and detrain near the tropopause. In both approaches, cumulus clouds are decomposed into infinitesmal elements, each of which is defined by its detrainment level (essentially the level where its temperature equals the environmental temperature). However, the cloud itself detrains continuously. Over how broad a region the cloud detrains depends on the moist enthalpy distribution of the air entering the cloud, but commonly the region extends from heights of 2-3 km up to the tropopause. Betts further points out that cloud detrainment is a major source of water vapor in the upper troposphere. The inference would appear to be that this is not the case in my approach. I can assure Betts that detrainment is indeed the major source of upper-level moisture in my parameterization--a point that I will return to shortly.

Betts argues that clouds should only be considered in the context of the larger scale systems that they are part of; in particular, he emphasizes clusters. My own feeling is that it is preferable to attempt to parameterize the isolated clouds, and calculate the flows that the clouds are embedded in as a response to the clouds (as well as other aspects of the forcing). In most cases, if the calculations are carried out correctly, the results should be the same. For example, if one considers the Hadley circulation generated by latent heat release in cumulonimbus clouds, then one finds that, if the heating is centered at the equator, the upward moving part of the Hadley circulation is largely occurring in the cloud updrafts themselves (Schneider and Lindzen 1977; Schneider 1977). Should one then consider the downward moving part of the Hadley circulation as part of the cloud system? One might, but there appears to be little advantage to doing so. Moreover, doing so forces one to look at clouds involved in the Hadley circulation as somehow being different from clouds embedded in squalls. This seems to me unappealing--especially since the same clouds are embedded in several systems simultaneously. In connection with the cluster systems mentioned by Betts, it might be worth noting that Lindzen (1974) and Stevens and Lindzen (1977) anticipated the multi-cell vertical structured—though for different reasons than those suggested by Betts. Betts emphasizes the slowness of large scale subsidence; this is, in some ways, irrelevant. All subsidence, by adiabatically warming air, leads to drying.

Most of Betts's remarks concern the normal cloud balance. As he correctly notes, this does not address the real question of what happens when we have warming. What we need to know is how warming affects this balance, and how the altered balance affects upper level specific humidity. My approach to this question is relatively simple. Understanding this approach is facilitated by considering the equations for the interaction of cumulus clouds with the water vapor field (Arakawa and Schubert, 1974).

In the presence of cumulus convection—but above the trade cumulus boundary layer—the budget for specific humidity can be written

$$\rho\frac{dq}{dt} = D + M_c\frac{\partial q}{\partial z} + E - C \qquad (1)$$

where

D=detrainment of water vapor,
M_c=cumulus mass flux,
E=reevaporation of water vapor,
C=condensation of water vapor, and
$\frac{dq}{dt} \approx \frac{\partial q}{\partial t} + u\frac{\partial q}{\partial x} + v\frac{\partial q}{\partial x} + w\frac{\partial q}{\partial z}$.

(t is time, x and y are horizontal coordinates, and z is the vertical coordinate; u, v, w are the velocity components in the x, y, z directions.) In regions of concentrated convection there is approximate compensation of cumulus heating and adiabatic cooling; i.e.,

$$pw\frac{\partial \Theta}{\partial z} \sim M_c\frac{\partial \Theta}{\partial z} \qquad (2)$$

(θ is the potential temperature.) which implies that $pw \sim M_c$; and, therefore, in these regions the major drying term, $M_c(\partial q/\partial z)$, is approximately cancelled by $\rho w(\partial q/\partial z)$. In these regions we are left with

$$\rho\left\{u\frac{\partial q}{\partial x} + v\frac{\partial q}{\partial y}\right\} \approx D + E - C \qquad (3)$$

Note that in these regions there will clearly be cirrus formation limited by horizontal dispersal; if there is precipitation from these cirrus then this will contribute to net drying. Reevaporation of liquid water will, on the other hand, supplement D.

Averaged over major closed circulation systems (such as the Hadley circulation) advective terms approximately cancel. For simplicity, we will assume that the closed circulation exists in a homogeneous environment where the cancellation is complete. Upon horizontally averaging, we have the following approximate relation:

$$\overline{D} + \overline{M_c\frac{\partial q}{\partial z}} + \overline{E} - \overline{C} \approx 0 \qquad (4)$$

In this average picture, warming causes D to become smaller. The reason is that each cloud element now originates with a higher moist enthalpy, and, hence, detrains at a higher, colder level where the saturated specific humidity is less. Ongoing calculations show this to be a potentially major negative feedback factor. Betts focuses on two other terms in Equation 4. He points out that *E*, the reevaporation of condensed ice and water (closely related to precipitation ineffeciency) might be a significant moisture source. I agree that this is a possibility. I also agree that we don't know, at this stage, whether it contributes positively or negatively to the climate feedbacks. There remains the contribution from the "drying" term, $\overline{M_c(\partial q/\partial z)}$. In our current work we find that M_c is approximately independent of warming. To be sure, M_c originates in evaporation which increases with warming; however, M_c depends on evaporation divided by the specific humidity of surface air (Lindzen 1988), and the latter quantity also increases with warming in such a manner as to largely cancel the increase in evaporation. It is possible that M_c might increase as a result of an increase in boundary layer turbulence forced by the increased evaporation (Sarachik 1985); this would produce a negative contribution to the feedback. However, preliminary estimates suggest this effect is small. I believe it is this term that Betts is referring to when he discusses the Hadley and Walker circulations. I am puzzled by his line of argument (I didn't refer to either the Hadley or Walker circulations in Lindzen, 1990). While I don't see its relevance to the present discussion, I find the contention that net cooling of the troposphere follows the Planck function at the surface peculiarly at odds with the well known near independence of outgoing longwave radiation on surface temperature (i.e., figure 24 in Lorenz 1967).

Despite the above technical disagreements, Betts and I both agree on the fundamental importance of the upper tropospheric water vapor budget to the question of global warming. I am personally more optimistic that, by keeping arguments specific, we can come to a satisfactory (even if imperfect) resolution of the question in time to rationally consider policy. Betts's wish for society sounds sufficiently noble, but is, in fact, disconcertingly vague. At least as concerns policies that purport to deal with warming, it seems only honest that these policies be required to quantitatively estimate the effect of the policies on the timing and amount of warming. Clearly, we have no accurate way of doing this at the moment. However, if such policies, even in the context of present climate models which predict substantial warming, fail to produce noticeable amelioration, we may plausibly regard it as imprudent to tie such policies to warming. To do otherwise would render the mundane, but not unimportant, matter of governmental accountability mute. It might also endanger what might otherwise be perfectly reasonable policies.

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