Reply to C. B. Taylor

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We are grateful for C. B. Taylor's critical comments on our paper (Rozanski and Sonntag, 1982), since it caused us to carefully re-examine the problem and find a few new aspects not or not sufficiently emphasized before. To some extent, the comments resume an old controversy on the possible interpretation of Taylor's experimental data, dating back more than a decade, At that time, the suggestion of treating the *average* isotopic profile by a necessarily oversimplified water vapour model rather than trying to interpret the individual profiles had already been discussed.

We still favour the approach of the average profile, and the procedure selected by us to derive an average deuterium profile from the original data was neither arbitrary nor accidental, as Taylor seems to suggest. Straight arithmetic averaging of the individual profiles, uniformly distributed throughout the year is not the only approach possible, but in our opinion, it is the most adequate procedure of constructing a mean deuterium profile reflecting the average climatological situation in the sampling area given. The averaging procedure recommended by Taylor (i.e. weighting by the actual water vapour mixing ratios), due to seasonal variations in the atmospheric water vapour content, obviously gives preference to summer profiles. The mixing ratios recorded by Taylor during summer months are, on the average, twice as high as those observed in winter (Taylor, 1968, 1972). The weighting procedure proposed thus leads to a substantial shift of the average profile towards more positive delta values (cf. profiles A and B in Fig. 1 Taylor, 1984). It is worth noting here that in spite of the wide spectrum of meteorological situations covered by the individual profiles, our average deuterium profile is still reasonably well defined (cf. Fig. 7 in Rozanski and Sonntag, 1982), and we feel, therefore, that contrary to Taylor's opinion, it is not a meaningless approach to discuss this *average* vertical distribution of deuterium in tropospheric moisture, and to search for mechanisms controlling it.

The principal aim in developing a multibox model which includes isotopic exchange between sinking droplets and atmospheric vapour was to study the effect of this exchange in compensating inevitable gradient reduction by diffusion. As we have shown, this droplet/vapour exchange, a process generally being expected (see e.g. Bolin, 1958) to be effective in the formation of rain, is in fact a very efficient process in depleting the heavy isotopes in the water vapour escaping the precipitation process itself and being lifted to the somewhat higher levels in the troposphere. This obvious and easy way of producing depleted water vapour is very welcome, in view of the crucial point in the whole problem, namely: how can the water vapour isotopic profile be maintained over a considerable time period as the experimental evidence by Taylor and by Ehhalt rather clearly seems to show. We now think, that the isotope profile preservation *after* or *around* a rain event (this mechanism was not mentioned in our paper, and to some extent goes alongside with Dr. Taylor's reasoning) is greatly facilitated by the slight subsidence brought about by clear weather, it being the mass balance compensation of the air-lifting processes during rain events somewhere else. This subsidence can keep the clear weather atmospheric humidity below saturation, despite the fact that there is vertical diffusion.

We admit that, to be consistent with our model, the moist adiabatic temperature scale should be used in Fig. 7 of our paper instead of the constant temperature lapse rate scale actually used there. We are grateful to Dr. Taylor for pointing out this confusing mistake.

In conclusion, we maintain that the model discussion of the *average* isotopic water vapour profiles is a useful and elucidating exercise as a step towards an understanding of these profiles even if our paper may not yet have fully succeeded in reaching this goal.

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