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Comments on 'Budget studies of heat flux profiles in the convective boundary layer over land' by H. Cattle and K. J. Weston (*Q.J.* 1975, 353–363)

By A. K. BETTS

Cattle and Weston have presented interesting boundary layer heat flux profiles derived by the budget method from the time change of the atmosphere θ-profile. However there seems to be some confusion over the role of clouds, and the meaning of the downward heat flux at the inversion base. In the 16 June 1972 case they state that extensive cloud was present increasing from 4/8 cumulus to stratocumulus during the budget period. While the inversion base remained constant at 780 mb, cloud base rose from 910 to 840 mb. The authors discussed the sensible heat sink associated with the evaporation of cloud water between 910 and 840 mb as cloudbase rises. However they do not discuss the role of clouds in the vertical transport of heat. In the cumulus layer, clouds transport heat through the process of condensing water at low levels, transporting it upward (because the release of latent heat makes them buoyant) and evaporating the liquid at the inversion level (Betts 1973, 1975). The budget method gives a 'heat transport' which is the difference $C_p\overline{\omega'\theta'}-(L\theta/T)\overline{\omega'q'_l}$ between the sensible heat flux and the 'latent' heat flux associated with the vertical flux of liquid water (q_l). This was called a flux of liquid water potential temperature $C_p\overline{\omega'\theta_l'}$ in Betts, 1973. Thus, apart from the storage change of liquid water which was estimated as equivalent to an additional flux convergence of 20 W m^{-2} their Fig. 6 should be interpreted as $C_p\overline{\omega'\theta_l'}$ which reduces to the sensible heat flux $C_p\overline{\omega'\theta'}$ below cloud base.

This raises a question concerning the meaning and magnitude of the parameter k: the ratio of the maximum downward flux below the inversion to the surface heat flux. The arguments which suggest $0 < k < 1$, based on kinetic energy dissipation, become invalid in the presence of clouds. With the release of latent heat through the condensation of water, the clouds become buoyant ($\overline{\omega'\theta'}$ positive) although their associated flux $\overline{\omega'\theta_l'}$ is negative. In other words the energy flux $C_p\overline{\omega'\theta_l'}$ (which is derived from a budget analysis) can go on increasing negatively above cloud base up to the inversion base because it has been uncoupled from the buoyancy heat flux during the convection. In the case of the trade-wind cumulus layer analysed in Betts (1973, 1975) the ratio of the flux at the inversion base to the surface heat flux is about −1·6.

Provided the mass flux associated with the convection decreases sharply at cloud base (meaning that much of the dry convection is 'trapped' in the subcloud layer) it is still likely that the ratio of the *cloud base* flux to the surface sensible heat flux is constrained to a value ~0·3. However this ratio is a little difficult to determine in the 16 June case analysed by the authors, since cloud base rises 70 mb during the budget period, so that it is unlikely that the time-averaged profile (Fig. 6) represents the situation throughout the budget period.

REFERENCES

Betts, A. K.	1973	Non-precipitating cumulus convection and its parameterization, *Quart. J. R. Met. Soc.*, **99**, 178–196.
	1975	Parametric interpretation of trade-wind cumulus budget studies, *J. Atmos. Sci.*, **32**, 1934–1945.

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REPLY BY K. J. WESTON AND H. CATTLE

We would like to thank Dr Betts for his valuable comments and we have no disagreement with any of the points he has made. However there is, of course, no difference implied in our interpretation of the sensible heat flux profile on 24 March occasion since there was almost no cloud present, and it is interesting to note (but of questionable significance!) that the profile is similar to that on 16 June and the value of k is almost the same despite considerable cloud on the latter occasion.

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Corrigenda

Berson, F. A. and Baird, G., October 1975:
A numerical model of cumulonimbus convection generating a protected cone.

In our paper on cumulonimbus convection it was stated that pseudo-adiabatic temperature lapse rates tabulated in Smithsonian Meteorological Tables and incorporated in pseudoadiabats on aerological diagrams, appear to be in error. This conclusion is wrong. We are grateful to Mr P. Goldsmith of the Meteorological Office for having clarified the matter in correspondence. His colleagues, Drs Bennets and Cuddington, using identical equations and box values, calculated temperatures along the relevant pseudoadiabat to within 0·5 degC agreement with the tephigram while we had found an apparent positive discrepancy of 1 to 2 degC on Refsdahl's skew T *v.* $\log p$ diagram. On further examination our correspondents located an error in a programme subroutine, viz. substitution of environment temperature in the place of cloud temperature, where the latter enters into saturation vapour pressure terms. As a result of this error the gross buoyancy underlying our results was too small, one effect of which is diminished updraught velocities. However, the main results remain qualitatively valid, in particular that significantly lower and more acceptable updraught velocities ensue from the version of the structure model in which buoyancy is defined as local.

Allowing for the error in gross buoyancy, the correct moist adiabat in Fig. 1 will be SA* while the curve SA is to be ignored. It should however, be pointed out that in the presence of lateral turbulent mixing of heat and momentum a substitution of environment temperature in the saturation vapour pressure term would nevertheless make sense. For by doing this, the moisture content in the cloud is being diminished while at the same time saturation is assured. In turn this is equivalent to allowing crudely for effects of moisture exchange between cloud and non-saturated environment. It might account for the satisfactory fit of observational data of updraught and ceiling height (section 8).

We feel obliged to explain why the mentioned discrepancy in the pseudoadiabat, which at the time we believed to be real, was attributed to an approximation of the expression for the dependence of saturation vapour pressure on temperature as given in the Smithsonian Tables. Prior to locating it on p. 372 of the 6th edition we had derived it by differentiating the formula for saturation vapour pressure on p. 350. Using exponential functions, rather than powers to the base 10, made for dissimilarity to start with. Moreover, not recognizing the last term in the published version of that expression on p. 372 for what it stood for, because it is ambiguously printed to say the least, the notion of there being an approximation became fixed. In fact, as it turned out all the terms in the respective expression are identical. Consequently the portion of appendix 1 beginning with the words "In the Smithsonian Tables . . ." at the bottom of p. 927 should be ignored.

Mason, B. J., July 1976: Towards the understanding and prediction of climatic variations.

Page 496: line 7: 35% should read 15%

Warhaft. Z., July 1976: Heat and moisture flux in the stratified boundary layer.

Page 706: Eq. (11) should read

$$\frac{K_H}{K_q} = 1+\frac{1}{2}\frac{g}{\overline{w'^2}}\frac{\overline{\theta'^2}}{\bar{\theta}}\left(\frac{\partial\bar{\theta}}{\partial z}\right)^{-1}\left(\rho_{\theta_q}\frac{\rho_{w\theta}}{\rho_{wq}}-1\right) \quad . \quad . \quad . \qquad (11)$$

Pittock, A. B., July 1973: Global meridional interactions in stratosphere and troposphere.

Page 437: last line of table gives the 30-year mean values for each month of the year, not the 1971 values.