Reply

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As often happens in science, discoveries are made independently by two or more groups at roughly the same time. In our case, we had made substantial progress toward a theory for velocity and buoyancy scales in moist convective clouds when one of us (Emanuel) received the paper by Rennó and Ingersoll (1996; hereafter RI) for review. As there were substantial differences in our approaches, we elected to publish our own work, acknowledging both the similarities and differences with the approach taken by RI; we did not intend our work to be primarily a critique of RI and certainly did not dismiss RI's conclusions. Klein's (1997) comments on the two works, while instructive in some ways, are misleading in others, and we wish to take this opportunity to clarify the difference between RI and our own work (Emanuel and Bister 1996, hereafter EB).

As Klein points out, there are two major elements in the construction of velocity, fractional area, and buoyancy scales by both EB and RI. The first is the recognition that in radiative–convective equilibrium, radiation is absorbed at a higher temperature than it is emitted at, thus producing a reversible entropy sink that must, in equilibrium, be compensated by an irreversible entropy source. Rennó and Ingersoll and EB differ only on implicit or explicit assumptions about the mean temperature at which mechanical dissipation occurs, and EB point out that one cannot assume a priori that mechanical dissipation is the dominant entropy source. That having been said, the estimates of mechanical dissipation in EB and RI differ by, at most, 20%. Having an estimate of dissipation rates also directly yields the integrated buoyancy flux in convective clouds, which is the rate at which potential energy is converted to kinetic energy. A separate closure for the mass flux then yields a scale for buoyancy.

It is in this second key element, the closure for the mass fluxes, that RI and EB differ substantially. We believe that Klein has confused matters by introducing his own closure for the mass fluxes, claiming (falsely, we believe) that it is equivalent to RI's and then showing that the resulting scaling for buoyancy is substantially the same as EB's. We would like to take up a discussion of Klein's closure for mass flux after first demonstrating that it is not at all the same as RI's.

Klein's closure for the mass flux carried by convective clouds is given by his Eq. (3):

$$M_{\rm rad}(s_t - s_b) = -\overline{Q}_A. \tag{1}$$

Had RI actually used (1), they would have obtained a result similar (but not identical) to EB because the radiative cooling rate of the atmosphere would largely cancel the radiative energy input that appears in the expression for buoyancy flux, as Klein points out. Instead, RI use an approximation for the radiative subsidence rate given by their Eqs. (32) and (33), whose dependence on the rate of radiative cooling enters through a temperature, T_c, which RI take to be an entropy-weighted mean temperature. (Later, RI evidently approximate it as a constant in examining sensitivities of convective buoyancy scales to thermodynamic efficiency, rate of heat input, etc.) Thus their final prediction for the convective buoyancy scale (the total convective available potential energy, or TCAPE) is given by their Eq. (40):

$$\text{TCAPE} \approx \left(\frac{c_p}{8\epsilon\sigma_R T_c^3}\right)\eta F_{\rm in}. \tag{2}$$

Here c_p is the heat capacity at constant pressure, ϵ is the emissivity, σ_R is the Stefan–Boltzmann constant, η is a thermodynamic efficiency, and $F_{\rm in}$ is the rate of radiative heat input.

Because $F_{\rm in}$ is not proportional to T_c^3, (2) predicts that TCAPE depends on $F_{\rm in}$,[1] which is inconsistent with modeling results, as pointed out by EB. This is a direct consequence of RI's closure for the mass fluxes, which we believe to be substantially incorrect, owing to an

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[1] RI's assumption that the vertical entropy change across a radiating slab scales as the ensemble mean entropy excess over the slab's local radiative equilibrium entropy leads to TCAPE depending on $F_{\rm in}$.

incomplete description of radiative cooling rates. Rennó and Ingersoll leave little doubt about their interpretation of (2), stating that ''The predicted TCAPE and *w* values are strongly dependent on both the heat engine efficiency *and the heat input*, which determine the energy available for mechanical work'' (italics ours).

Klein correctly points out that the use of his own mass flux closure (1) yields an expression for CAPE that is nearly invariant with the rate of radiative forcing and is thus consistent with the scaling derived by EB. We had considered using (1) ourselves, but elected not to. It is worth discussing the reasons why. Essentially, (1) is an expression for the total upward mass flux at any altitude. By comparison, EB's closure, based on subcloud-layer energy equilibrium, gives an expression [their (23)] for only the upward mass flux at cloud base. Emanuel and Bister point out that for a dry entraining plume, the cloud base mass flux multiplied by undilute buoyancy is unaffected by entrainment. Since CAPE is defined for undilute buoyancy, our expression would be more exact in the case of a dry entraining plume. Of course, real convective clouds are not dry, and EB discuss at some length the differences that can be expected owing to the production of downdrafts and buoyancy reversal.

As a result of different closures for mass flux, EB and Klein obtain different expressions for CAPE that, however, share an independence of the rate of radiative forcing. Both differ substantially from RI's closure.

We stand by our assertion that, in equilibrium, the net absorption of radiation by the surface is equal to the mass-integrated radiative cooling of the atmosphere. Rennó's (1997) assertion that this implies zero thermodynamic efficiency is correct as long as it is recognized that our system is closed and does no work on any external system. All the work conversions are internal and thus there is no inconsistency in our formulation.

Finally, Klein points out that EB's reference to the observation by Ramage (1971) that more humid atmospheres have less CAPE may be misleading, because the observed atmosphere may be far from radiative–convective equilibrium. We concur with this point and also agree that better comparison with observations is highly desirable. But we feel that existing numerical experiments rather conclusively rule out RI's prediction, as entailed in (2) above.

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