Response to Comments from reviewers dated (approximately) 10 January 2020
and
Line-by-Line Summary of changes to the manuscript

RESPONSE

My thanks to all reviewers for their comments. They were helpful. In response to Dr. Petty's comments I spell checked the manuscript and made all necessary (three) corrections. The Anonymous Referee #1 is satisfied with my changes. No further response is necessary to either of these two reviewers. Dr. Kowlaski (ASK) suggested several minor revisions. My response to ASK follows.

ASK: This revised manuscript is much improved in comparison with the initial submission, and I suggest that it be published subject to minor revisions. I have tried to organize my recommendations in order of importance. **RESPONSE:** *OK.*

ASK (1.) Line 245: The author claims to identify an error in equation (2.66) of Curry and Webster (1999), but I see no error in that equation. The coefficient 0.2 modifying q_v derives from a binomial expansion and approximation, multiplying both numerator and denominator by the same factor (1 - 0.87q_v) and then neglecting quadratic terms to simplify the result (since $q_v^2 \ll 1$). This can be found in other texts as well (e.g., Rogers and Yau, 1988, A Short Course in Cloud Physics, Pergamon, Oxford). In equation (15) and all subsequent equations that contain the factor 0.33, I believe that this should be changed back to the coefficient 0.2. This may also change the percentage that appears in the conclusions (line 298).
RESPONSE: *I agree. I misunderstood c_{pv} in Equation (2.66) of Curry and Webster (1999). Sorry for the confusion. All necessary corrections were made and the sentence pointing out the putative error has been removed.*

ASK (2.) Line 284: The 'displacement assumption' of Paw U et al. (2000) can hardly be brought into question, since it falls directly out of the Ideal Gas Law for the conditions that they assumed. The context of the Paw U et al. paper is evaporation that is both isobaric (as assumed by the Webb et al. paper under consideration) and isothermal (excluding temperature effects, i.e., the WPL vapour correction). In such a context, equation (4) of Webb et al. (1980) is a version of the Ideal Gas Law that adequately justifies the relevance of water vapour displacing dry air. I appreciate the author's argument that evaporation is truly neither a constant volume, nor a constant pressure process (line 103), but I do not think that it justifies the wording used here.
RESPONSE: *No Change. I disagree. Nowhere in the derivation of the WPL terms, whether by an isobaric or isothermal process or both, is the assumption made that an evaporating molecule (mol) of water vapor exactly replaces a molecule (mol) of dry air. Yet that is precisely what Paw U et al. do (i.e., $\varrho_a' = -\varrho_v'$). Furthermore, (1) I don't think Paw U et al. would necessarily agree that their derivation assumes that evaporation is an isothermal process (it clearly is not: see my Appendix) and (2), and I think the reviewer would agree with me, that it makes no physical sense to assume that the process of evaporation is an isothermal isobaric process. Finally, I am not justifying my wording on the basis of my assertion that evaporation lies somewhere between a constant volume and a constant pressure process. I justify my statement on the fact that the result proposed by Paw U et al. did not give a result that is consistent with*

what I obtained using an Equation (15), which basically is an equation of state for a moist atmosphere. It seems to me that Paw U et al.'s displacement assumption leads to a contradiction and specifically contradicts what one might expect from the appropriate equation of state for moist air.

ASK (3.) Lines 230-240: There is something inconsistent about beginning an argument for defining the heat flux using potential temperature (rather than the temperature) with an equation that is valid only for an incompressible atmospheric process. It may be preferable to use the proper definition of the material derivative as $d\theta/dt = \partial\theta/\partial t + u \bullet \nabla\theta$ and so be able to remove the word 'incompressible'. Perhaps even simpler would be to simply state that the potential temperature is the key variable for discussion, and cite an appropriate reference (e.g., Kowalski, A. S. and Argueso, D., 2011, Tellus, 63B, 1059-1066).
RESPONSE: *Fair point. The following changes were made: Equation (14) now includes the term associated with compressibility and I have removed the word 'incompressible' from the preceding sentence. I have also added the following sentence: "Here the term associated with compressible effects, $\theta\nabla \bullet \boldsymbol{u}$, is included for completeness, although it is not important for the present purposes."*

ASK (4.) I find the author's use of temperature ranges to be inconsistent and frankly inexplicable, resulting in an unnecessary distraction from the message of the paper. Line 57 suggests examination of the surface energy budget near STP (i.e., not far from 0C), which seems appropriate if somewhat vague.
RESPONSE: *The sentence following one that the reviewer cites states 'Here "near STP" will be understood as pressures between about 70 kPa and 105 kPa and temperatures between about 0 C and 100 C or so – or an atmospheric state typical of near-surface conditions on earth.' I am not sure why the reviewer finds my use of the words "near STP" vague.*
CONTINUING ASK (4.) However, temperature ranges are later defined variously throughout the manuscript (all converted to C here) as:
a. Line 59: 0 - 100C; **RESPONSE:** *At this point of the text this range of temperatures is appropriate since all calculations are done between these temperatures and all graphs cover this range of temperatures.*
b. Line 155: 3 - 42C; **RESPONSE:** *Changed to 3 - 52C*
c. Line 185: 7 - 77C; *Changed to 3 - 52C* and
d. Line 298: 12 - 52C. **RESPONSE:** *Changed to 3 - 52C*
CONTINUING ASK (4.) I believe that a more appropriate range of temperatures commonly encountered with micrometeorological techniques (line 156) would be something like -35 to 45C. If the extrapolation of Dr. Massman's results to such a range in any way changes his calculations, then some revision may be required that might not classify as minor.
RESPONSE: *The following change was made: The sentence on line 156 now reads 'With the exception of sublimation of ice or snow, these results suggests that surface energy fluxes associated with ET measured at temperatures commonly encountered with micrometeorological techniques (i.e., between about 275 and 325 K) . . . '; where the underlined portion of the text has been added to the original text.*
ADDITIONAL RESPONSE: *The reviewer is correct I did am not considering sub-zero C temperatures. But it is interesting that the energy balance closure tends to be worse over ice and snow. I am left to wonder now if it might be worth investigating the influence of the thermodynamics of sublimation on the surface energy balance.*

ASK (5.) The use of both mass- and molar-based definitions of the specific heat is similarly distracting. I see little point in defining the molar specific heat when its use complicates the 'final result' (as in equation 13 which, if I am not mistaken, has disguised the mass specific heat as the ratio c_v/μ).
RESPONSE: *No Change. The reviewer is correct about μ being buried in the final result. That is because I wanted my final result to be directly comparable to the expression developed by Paw U et al. (2000) and that takes careful management of the units. It is also important that the end result be consistent with my reworking of Paw U et al.'s derivation and their mol-per-mol displacement assumption, which is in molar units. Sorry if this causes confusion; but, switching units does not concern me that much because the conversion is linear and therefore straightforward.*

ASK (6.) Throughout the manuscript, units are specified with no space separating them. So for example at line 16, I think that 'Wm^{-2}' should be changed to '$\mathrm{W\ m}^{-2}$', and likewise in many subsequent instances. This is particularly egregious at line 43, where the characters 'kgm' appear in succession.
RESPONSE: *This seems to be a journal formatting issue. I am not sure how to change this.*

ASK (7.) At line 69, delete the first instance of the word 'pure'. ***The manuscript has been changed.***

ASK (8.) At line 72, 'unnecessary to consider' ***The manuscript has been changed.***

ASK (9.) At line 75, 'components of the specific enthalpy' ***The manuscript has been changed.***

I hope that some fraction of these suggestions will be helpful when producing the final version of the manuscript. ***They were.***

SUMMARY

The following lists the line numbers of the revised manuscript where changes were made to the manuscript.

lines 30, 32, 69, 72, 75 – Minor editorial (spelling and grammatical) changes – **All Reviewers**
lines 154-156; line 186 – **ASK (4.)**
lines 231-236 – **ASK (3.)**
lines 243-245; line 252; lines 256-259; lines 265-266; line 272; line 284 – **ASK (1.)**
lines 299-301 – **ASK (1.)** & **ASK (4.)**