Atmos. Chem. Phys. Discuss., 9, C5118–C5122, 2009
www.atmos-chem-phys-discuss.net/9/C5118/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The effect of misleading surface temperature estimations on the sensible heat fluxes at a high Arctic site – the Arctic turbulence experiment 2006 on Svalbard (ARCTEX-2006)" *by* J. Lüers and J. Bareiss

Anonymous Referee #1

Received and published: 21 September 2009

This manuscript compares two different flux-gradient models, given inputs for surface temperatures from two different sources. These two sources are infrared measurements, and a 3-layer-model (3LM) that calculates the surface temperature based on the heat flux. The conclusion is that the 3-layer-model gives sensible heat flux estimates that agree best with direct eddy covariance measurements. The topic is highly relevant since the polar regions are the most rapidly changing environment on the planet and since our understanding of crucial atmosphere-surface exchange processes are in this environment is still insufficient.

There are two main issues with which I have trouble in this paper. The first and most troublesome is that using the 3LM to calculate the surface temperature T(0) ,which is a required input for the flux gradient models in order to calculate the sensible heat flux (H), requires a precise measurement of H. Of course the flux-gradient parameterizations can be used to calculate other fluxes (trace gases etc. – if you assume similar eddy diffusivities) in which case this is useful, but regarding H only, this is a circular argument. In the subsequent analysis of H values calculated using the T(0) derived from LM3 as input one needs to be careful to keep this circularity in mind. This should be discussed at least in a few sentences, probably in section 3.3.

The second issue is that the temperature profiles shown are clearly indicative of katabatic or anabatic advection of heat. The "nose" in the profile looks exactly like what you get from the classical Prandtl [1942] balance of advection of momentum and heat along a slope. Even slopes of 0.001 can produce katabatic flows, so even if the flow direction wasn't directly from Zeppelin Mountain, the fact that the measurement site was 11m above sea level, and presumably at most a km or two from the sea, indicates that there was sufficient slope. This calls into question just how representative the shown profiles are of the Arctic in general – out over the ocean, temperatures would be expected to increase monotonically with height in general (excepting transition periods), without the nose, and this has been observed to be the case at Barrow, Alaska (unpublished data). On the other hand one might argue that if the 3LM approach works so well under these relatively complicated conditions, it would work even better in monotonic situations!

Related to this, the penultimate paragraph in the conclusions seems rather speculative to me... one might argue that your eddy flux measurements were at the most complicated level (right at the peak of the nose), and yet you obtained very good agreement between direct measurements and the model (ignoring the circular argument problem for a moment). Since there are no sensible heat flux measurements at other heights to demonstrate how things may go wrong if the height isn't properly chosen, the available information is insufficient to provide much guidance, other than a rather general

ACPD

9, C5118–C5122, 2009

warning. This is a bit of a disappointment after the promise made at the end of the Introduction of recommendations on how to optimize gradient-flux setups.

Finally, the paper would be significantly strengthened by an analysis of the physical reasons for the differences between the models. The 3LM approach is shown to provide the best fit without an analysis (other than it being more sophisticated) of how it moves outliers in the other methods closer to the 1:1 line.

Specific comments

P16914 L19: “enough” is subjective. It would be more defensible to say that the 3LM provides a better fit to EF measurements than other models.

P16916 L 16: I wouldn’t use “disturbed” to describe the observed temperature profile, unless you refer to the peak you see at 2.4m. The strong inversion at the ground is to be expected, it is no surprise – however, the peak you see at 2.4m is due to a katabatic flow disturbance (see above). I would just call this a strong inversion near the ground.

P16918 L19: please state the height of the eddy covariance measurements in this paragraph somewhere – at the moment the first mention of this is on P16926!

P16918 L20-25: this reads like an advertisement. You may want to leave accolades such as “internationally standardized” and “state of the art” to impartial observers.

P16918 L29: at this point, or in 3.3, please include some statistics on what fraction of the flux data were eliminated by TK2.

P16919 L 17: I would include an explicit statement, in the text (doesn’t have to be a numbered equation), of your exact definition of RiB

P16921 L19: how much may the emissivity vary at your site from 0.99 (e.g. standard deviation)?

P16922 L11: if K=0.4 then 4.K isn’t 4. Just eliminate the confusing “assumed as 4”.

ACPD

9, C5118–C5122, 2009

P16926 L18: this almost suggests that with more careful filtering, the IR approach may also yield a closer fit with the flux measurements. Food for thought.

P16927 L23: check the wind roses (direction), or plot a hodograph, to identify katabatic/anabatic flows

P16928 L11: how about calling this an “effective surface temperature”, or “effective aerodynamic surface temperature” (which may well be different from the actual surface temperature)

Fig.1:

1. what is being shown here? Are these two instantaneous snapshots? If they are averages, over what time period?

2. what are the horizontal error bars?

3. please move the z[m] label outside the box; and there is no need to repeat the labels on the right and top axes

4. mark the height of the flux measurements on the figure (a line, or an arrow pointing at the axis. . .)

Fig. 2-4: surely the QC didn’t just eliminate periods with snowdrift or precipitation effects – didn’t it also eliminate non-stationary periods, those with unusual ITCs etc.? Please modify the captions accordingly.

Technical corrections

P16914 L5: change “an Arctic landscape” to “Arctic landscapes”

P16914 L9: Plural of “formula” is “formulae”

P16914 L12: Untypical => atypical

P16918 L10: nominally at 2m and 10m

ACPD

9, C5118–C5122, 2009

Interactive Comment

P16921 L4: asnow => a snow

P15926 L26: this is a confusing sentence. Maybe: “the differences imply that some tundra surface without snow cover (3-10% in May) was in the footprint of the measurements”, or something like that.

P16927 L19: . . . influence on the estimation of . . .

P16929 L23: . . . conflicting task of finding an. . .

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 16913, 2009.