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Comment

***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**

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Answers to Referees

Referee 01

The first question stated by Referee 01: The circular argument regarding QH.

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Author's statements:

We agree that the calculation of surface temperatures using the 3LM to derive the sensible heat fluxes is a circular argument. The authors are aware of this.

The major objective of this paper is to show the problems of using incorrect input data (in this case surface temperatures) for widely used heat flux parameterization such as the ones of Ebert and Curry (1993) or Launiainen and Cheng (1995). These approaches require only little input data and are therefore widely used by Non-Meteorologists for glaciological and permafrost studies as well as sea-ice physics.

During ARCTEX 2006 we had the chance to directly measure surface temperatures using an infrared thermometer and to derive those temperatures from radiation measurements of the Baseline Surface Radiation Network situated nearby. Both methods showed enormous measurement errors in the data sets. Hence, it was intended by the authors to present an IR-radiation independent method to determine the surface temperature $T(0)$ using eddy-covariance data and to create an independent and consistent data set for surface temperature to run the parameterizations of EC93 and LC95. This was the only possibility to compare the parameterization results with the direct eddy-flux measurements of sensible heat.

As recommended by the Referee 01 this issue is now included at the end of Sect. 3.2.

The second question stated by Referee 01: Origin of a disturbed vertical air temperature profile or sharp inversion layer close to the surface.

Author's statements (see also statement regarding Fig 1 below):

Of course the explanation of the phenomena as a horizontal advection effect or as a katabatic drainage flow is obviously and we never excluded this possibility in our paper.

On the other hand similar effects were also found over water in the coastal region and over the open ocean and over snow.

For references see:

Black sea:

Chundshua G.G., Andreev E.G. (1980): O mechanizme formirovanija inverzii temperatury v privodnoim sloe atmosfery nad morem. Dokl AN SSSR, 255, 829-832.

Caspian Sea:

Foken, Th.; Kuznecov, O. A. (1978): Die wichtigsten Ergebnisse der gemeinsamen Expedition "KASPEX-76" des Institutes für Ozeanologie Moskau und der Karl-Marx-Universität Leipzig. Beitr. Meeresforsch., 41, 41-47.

Antarctica ice shelf:

Sodemann, H. and Foken, T. (2005): Special characteristics of the temperature structure near the surface, Theor Appl Climatol, 80, 81-89.

The Neumayer station is nearly not affected by advection (Sodemann and Foken 2005) and in stable nights the wind velocity goes to zero.

For Ny Alesund advection may be a reason but we could proof that this is most of the time NOT the case because only for 2 events drainage wind flows down the slopes of the nearby Zeppelin Mountain range were observable, the strongest one on May 9 between 18 h CET and midnight CET. During this few hours the wind speed at 10 m some times dropped below 1 m/s but the wind speed in 1.4 m height remained between 1.2 and 1.6 m/s with wind directions (2 m a.g.l.) from the south or southwest > Zeppelin mountain (Fig a).

(see graph at last page)

Fig a: 30min mean wind and temperature profile based on 1 min measurements May 9, 2006, 23:00 h to 23:30 h CET

But this is an exception!

We could see (proof) this based on the vertical wind speed and wind direction profiles measured with our gradient tower close to the eddy-flux station.

For nearly all other cases the wind profile is "undisturbed" and more or less neutral. But the origin or reason for a "disturbed" profile (like a strong inversion, Fig 1 of the paper case A or something in-between, Fig 1 case B) is not of any importance for the objective of our study. This phenomenon is just an error source.

The main point of this - regarding the atmospheric exchange - is the decoupling of the first 1 to 2 m from the rest of the Prandtl-Layer above. For an experienced boundary layer meteorologist this - often very rapid changing, not neutral, strong temperature gradients in the first 3 m above ground forcing coupled or decoupled exchange conditions above a more or less smooth surface (tundra or snow or water) - may be familiar but for a lot of researches coming from the soil or permafrost community interested in energy exchange processes this phenomenon is maybe not known.

It is not our intention to dwell on this issue (origin) too much in this paper. As we said, the main point is the decoupling effect as a possible error source to determine the "real" surface temperature.

We added the above issue and the requested information about the percentage of case A and B into the text at page P16923.

Referee's 01 remark: 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!

Author's statement: Sure, we agree with this statement.

Referee's 01 remark: 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. . . Since there are no sensible heat flux measurements at other heights to demonstrate how things may go wrong

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if the height isn't properly chosen, the available information is insufficient to provide much guidance, other than a rather general warning.

Author's statement: That is a good point. It seems that we are lucky with our eddy measurements during May 2006 and the height of 2.4 m was acceptable.

As stated in our conclusion, the measurement height of the eddy-sensors must (should) be above the decoupled layer to capture the full turbulence spectra. But no one knows the height of this "disturbed" layer at any possible time. Therefore we must find out as much about such effects for each monitoring site.

A combination of eddy and gradient tower systems is of course the best solution. But to run a full vertical profile of let us say at least 5 ultra-sonic-anemometers plus 5 fast responding gas-analyzers doesn't make sense, because turbulent Eddy-flux-measurements with fast responding sensors below 2 m above ground will be incorrect due to the relationship of the sensor's path-length (typically 10 cm to 15 cm) to the size and lifetime of the turbulent eddies. Because, if your eddy measurement is too high you will get some trouble with the representative footprint, if you are too close to the surface you will get in trouble due to the decoupled internal layers and due to the incomplete spectral distribution of the turbulence elements. Another aspect must be considered (as written in our conclusions): to find a compromise between the effect of the disturbance of the temperature profile and the conflictive task to find an acceptable fetch and the desired footprint area.

Therefore a combination of a single eddy-flux station plus a classical gradient tower with ventilated thermo-hygrometers and cup-anemometers seems to be rational.

Referee's 01 remark: 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.

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Author's statement:

We do not think that this is surprising. The reason is a good quality check of the turbulence data (Foken and Wichura 1996; Foken et al. 2004), which excludes all data with intermittences (typical under stable conditions) and non steady state conditions for which the eddy-covariance method cannot be applied. The gradient-three-layer approach has better results (lesser scatter) because this includes not only the turbulent layer (like the bulk approach with extrapolation of the turbulent profile to $T(0)$) but also the buffer and molecular layer that means it is physically more exact (See: Bjutner 1974; Foken et al. 1978, Foken 1984; Mangarella et al. 1972; 1973; Oertel 2004).

It is not our intension to dwell on this issue in our paper to much, because this is already done by the cited publications.

We have changed and extended the text on P16926 accordingly.

Specific comments by Referee 01:

P16914 L16 (abstract): "enough" is subjective. It would be more defensible to say that the 3LM provides a better fit to EF measurements than other models.

Abstract text changed to:

The results of a comparison of different sensible heat-flux parameterizations with direct measurements indicate that the use of a hydrodynamic three-layer temperature-profile model achieves the best fit and reproduces the temporal variability of the surface temperature better than other approaches.

P16916 L16: ...use "disturbed" to describe the observed temperature profile...

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Author's statement: We think the term "disturbed" profile is well chosen. Compared to the "normal" or better neutral, non-linear (log.) vertical air temperature profile, any significant variation like this strong inversion is "disturbed", not meaning "atypical". You can compare this to the effect of mechanical internal boundary layers forced by a discontinuity in surface roughness disturbing the vertical wind profile.

Please, see our statement above.

Remark: The original phrase "thermocline inversion layer" was already replaced by "disturbed temperature profile close to the surface" to exclude misunderstandings due to the remarks of Referee 02.

P16918 L19: Please state the height of the eddy covariance measurements in this paragraph.

Text changed:

The UBT eddy-flux measurement complex EF was equipped with a Campbell Scientific CSAT3 ultra-sonic-anemometer to measure the turbulent variation of all three wind vectors as well as the sonic temperature at 2.4 m above ground.

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.

Author's statement: This is certainly not an "advertisement"! The software TK2 is internationally standardized and using state of the art post-processing methods accepted by the majority of the Eddy-Covariance community. This software package is used as a standard routine by a lot of internationally projects like CEOP-AEGIS, COPS, LITFASS, FLUXNET, VERTICO, ECHO etc.

See: Mauder, M., Foken, T., Clement, R. Elbers, J.A., Eugster, W., Grünwald, T., Heusinkveld, B., and Kolle, O.: Quality control of CarboEurope flux data - Part 2:

Inter-comparison of eddy-covariance software, Biogeosciences, 5, 451-462, 2008.
(Please, see also the following statement)

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

Author's statement: You are right. The long QA/QC part was deleted, because it is (will be) published in the second ARCTEX-2006 Paper in detail:

Lüers, J. and Bareiss, J.: Direct near surface measurements of sensible heat fluxes in the arctic tundra applying eddy-covariance and laser scintillometry - The Arctic Turbulence Experiment 2006 on Svalbard (ARCTEX-2006), Theor. Appl. Climatol., submitted, Dec 2009.

ALL state of science correction methods are applied to the ARCTEX-data, like detection of spikes, Planar-Fit-Rotation, correction of spectral loss, correction for density fluctuations etc.

The Steady State test applied to the ARCTEX-2006 data proves high quality conditions (classes 1 to 3) for 92% of all u^* and 73% of all buoyancy fluxes. As expected, most of the low-quality classes 7 to 9 occur at periods of very stable atmospheric stratification and very weak values of the friction velocity like on May 9 or during the night from May 11 to May 12, 2006.

The Integral Turbulence Characteristic (ITC) test results in more or less low-quality flags, especially regarding temperature. This is mostly due to the marked intermittence pattern of the sensible heat flux typical for polar regions in the spring or autumn transition season (longer neutral periods without turbulence interrupted by rapid and acute turbulent events) and due to limitations regarding the relation of standard derivation and flux caused by neutral conditions (Thomas and Foken 2002). We decided to ignore the ITC test, because it is not "designed" or adapted yet in the TK2 software to polar conditions.

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We added the relevant statistics regarding the Steady state test to the text on P16918.

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.

Author's statement: Text added on P16919:

The Richardson-number is the ratio of shear production to the buoyancy production or destruction of turbulence energy using the characteristic vertical temperature and wind gradients.

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

Author's statement: The variability is unknown. And that's one of the main problems if recalculation a temperature out of IR-radiation. A simple or easy way to measure the Kirchhoff's emissivity directly for heterogeneous soil/snow/ice/vegetation surfaces doesn't exist.

P16922 L11: if $K=0.4$ then $4.K$ isn't 4. Just eliminate the confusing "assumed as 4".

Text changed:

The term $4 * k$ represents the normalized temperature difference of the integral of the buffer layer (Foken, 1984).

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.

Author's statement: We agree. And if we do not have any eddy-flux data the IR-method

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is still the best way. If you eliminate all "falsifying" periods and if you have a "good" assumption about the emissivity of the surface, you could gain a better fitting.

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

Author's statement: see our statements above.

Wind roses (daily): See our experiment documentations online at:

<http://www.arctex.uni-bayreuth.de/>

P16928 L11: how about calling this an "effective surface temperature", or "effective aerodynamic surface temperature"

Author's statement: We are not sure if this is probably more confusing than helpful to "introduce" such a term.

Fig.1: 1. Are these two instantaneous snapshots? 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)

Author's statements:

1) Of course not. The shown profiles case A and B are MEANS of selected profiles based on half hourly air temperature values of the whole time series between May 7 and May 19. For case A we selected every 30 min profile fitting to the shape of a strong inversion forced by rapid, strong surface cooling. We found that this happens mostly around 5 and 8 o'clock or 17 and 22 o'clock CET. For case B we selected times with a strong surface warming, thus a sharp temperature decrease in the first 1 m and than a sharp increase until 2.4 m.

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Case A occurs at 36% of all 30 min profiles, Case B at 50% of all 30 min profiles between May 7 and May 19, 2006.

We have changed the text at page P16923 accordingly (see above) and the caption of Fig 1, including the percents.

2) We have tested (paired two-sample t-tests) for both cases A and B if the temp. means e.g. between 0 m and 2.4 m a.g.l. (case A) or 0 m and 0.7 m (0.7 and 2.4 m) case B are equal or not. All t-test results assure that this means are different with 99% confidence.

We have added the t-test results to the text, thus error bars in the plot are not necessary.

3) Fig changed accordingly.

4) We think that is not necessary.

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.

Author's statements: We changed the captions accordingly.

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.

Author's statements: You are right. This sentence was confusing. We have corrected this passage.

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

P16921 L4: asnow => a snow

P16927 L19: influence on the estimation of

P16929 L23: conflicting task of finding an

Author's statements: Text corrected.

Referee 02

The first main remark stated by Referee 02: Firstly I would ask the authors to dwell on what the aerodynamic roughness length for temperature, zT , and the surface temperature, T_0 , actually are.

Author's main statement:

The paper by Sodemann and Foken (2005) discusses the case when the Monin-Obukhov similarity theory (Monin and Obukhov 1954) fails over ice surfaces (references for similar cases over water surfaces are given in this paper), or more related to the reference comment the "dynamical sublayer" in the lowest meter (Foken 2006; Monin and Obukhov 1954; Oertel 2004). Second, the roughness temperature was mainly included by Louis (Louis 1979; Louis et al. 1982) or Garratt 1992 (Garratt, J. R.: The Atmospheric Boundary Layer. Cambridge Univ. Press 1992) or Jacobson 1998 or 2005 (Jacobson, M. Z.: Fundamentals of Atmospheric Modelling. Cambridge Univ. Press) into the modeling practice. The method has many disadvantages because it is an extrapolation of the temperature profile from the turbulent layer into a layer with mainly molecular exchange processes to a "near" surface temperature.

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Therefore several parameterizations could be used.

One - very simple - is given in our paper just as an EXAMPLE and applied on the measuring data but not directly determined.

We mentioned this whole subject only to give a simple recommendation for the case that the tundra surface conditions in spring time changed from full snow cover to partly snow cover or even snow free cases, where the roughness length z_0 will increase.

The referee 02 wrote:

”...what the aerodynamic roughness length for temperature, z_T , and the surface temperature, T_0 , actually are. z_T is analogous to the roughness height, z_0 , the latter being the height at which the wind speed extrapolates to the surface wind ($=0$ m/s). z_T is the extrapolated temperature at this height.”

The last sentence is not true.

Read page 161 of Andreas, E.L.: A theory for the scalar roughness and the scalar transfer coefficients over snow and sea ice, Bound-Lay Meteorol, 38, 159-184, 1987.

That z_T and z_0 are equal is for most cases wrong.

See: Andreas, E.L.: A theory for the scalar roughness and the scalar transfer coefficients over snow and sea ice, Bound-Lay Meteorol, 38, 159-184, 1987.

He wrote one page 162:

“We shall see shortly that, contrary to the common assumption (e.g., Paulson, 1970; Businger et al., 1971; Lettau, 1979), z_T and z_q rarely equal z_0 .” ...“it is clear that predicting CH and CE requires also finding z_T/z_0 or z_q/z_0 .”

But of course it could be that z_0 and z_T are in the same range so that the resistance ratio $\ln(z_0/z_T)$ is around zero or z_0/z_T around one (see Garratt 1992 page

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89ff or Jacobson 2005 pages 233 to 235). This could be valid for or will
". . .serve as good approximations for the rather smooth surfaces of $lnz_0 \sim -lnzT$ i.e.
with the Reynolds analogy valid, such as over water and snow surfaces in light and
moderate winds."

(Launiainen, J.: Derivation of the relationship between the Obukhov stability parameter
and the bulk Richardson Number for flux-profile studies, Bound-Lay Meteorol, 76,
165-179, 1995.)

But in general the conditions vary between z_0 from 10^{-5} to 10^{-1} m and z_0/zT from 0.5
to 7.3 (Launiainen 1995, or Andreas, 1987, Fig. 8, or Garratt, 1992)

Even the authors cited by the Referee 02 (King JC and Anderson PS 1994: Heat
and Water-Vapor Fluxes and Scalar Roughness Lengths over an Antarctic Ice Shelf.
Boundary-Layer Meteorology. 69(1-2):101-121) wrote in the abstract:

*"The variation of heat and water vapour fluxes with stability is well described by
Monin-Obukhov similarity theory but the scalar roughness lengths for heat and water
vapour appear to be much larger than the momentum roughness length. Possible
explanations of this effect are discussed."*

The relevant part in King's paper regarding zT confirming our view could be found at
page112 and 113.

What we have done is to run the LC95-parameterization

- a) with a distinct z_0/zT ratio (=7 at the ARCTEX-site) IF we assume snow free tundra
surface conditions (rougher surface) and
- b) with a z_0/zT equal 1 (fixed in the LC95 program-algorithm) for a full snow covered
(smooth) surface.

For the comparison in Chap 3.3 (and Fig 2 and 3) we chose method b), thus assuming
a snow covered surface to run the LC95-parameterization.

We decided to use method b), thus a very smooth surface, because during May
2006 the footprint area of the eddy measurements was either full covered by snow
or ice or later predominantly covered by snow (only very small snow free patches).

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Consequently, the LC95 run with method b) results to a better quantitative agreement than the runs using method a).

We have changed the text and added a passage on page P16921 and on page P16926 accordingly and we have included a passage at the end of Sect. 3.2 to clarify the whole issue (see also our comments to Referee 01).

The referee 02 wrote: The paper is not clear on this point, but I believe it effectively attempts Method 3, using a number of assumptions to estimate z_T and T_s , which are then used to validate a more complex "3LM" model. The paper needs to be very clear as to what is being measured and what is being tested or validated.

Author's statement: The text changes mention just above makes it now hopefully clear what our intentions are. During ARCTEX 2006 we had the chance to directly measure surface temperatures using an infrared thermometer and to derive those temperatures from radiation measurements of the Baseline Surface Radiation Network situated nearby. Both methods showed enormous measurement errors in the data sets. Hence, it was intended by the authors to present an IR-radiation independent method to determine the surface temperature $T(0)$ using eddy-covariance data and to create an independent and consistent data set for surface temperature to run the parameterizations of EC93 and LC95. This was the only possibility to compare the parameterization results with the direct eddy-flux measurements of sensible heat.

The second main remark stated by Referee 02: I still have concerns regarding experimental and instrument error. ...measuring z_0 using profiles of cup anemometers generates large error. These estimates should be compared to that derived from the sonic anemometer under neutral conditions. Further

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concern results from the sentence at the top of page 7 starting: "Based on the on-site observed geometric roughness...", which implies that z_0 was estimated from what the surface looked like.

Author's statement:

About the quality check of the eddy data we have added some statistics (see our comment to Referee 01) on page P16919.

To use a vertical wind profile obtained by cup-anemometers logarithmic arranged on a gradient tower is still a valid method e.g. to detect internal boundary layers due to roughness changes in the footprint area. But we agree that especially above snow/ice or water this method can be inaccurate, because small errors in the wind measurements can cause large changes in the roughness height (Foken, 2008).

Thus, we have of course calculated z_0 from the sonic data using the integrated profile equation for momentum at neutral conditions. The result: (0.12 mm for the whole period May 5 to May 19).

The on-site determined geometric (real) roughness of the snow surface of 0.02 meter (2 cm) was indeed a visual observation of the authors itself. But that is NOT z_0 ! z_0 was determined independently as described.

We have changed the text slightly on P16921.

Remark stated by Referee 02: ...circularity or self-correlation

Author's statement: please, see comments to Referee 01.

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Acknowledgement

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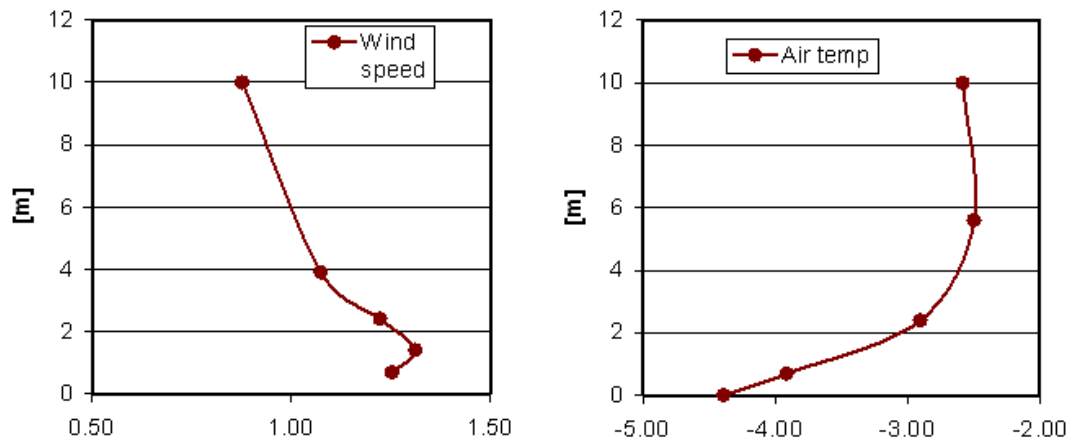


Fig. 1. Fig a: 30min mean wind and temperature profile based on 1 min measurements May 9, 2006, 23:00 h to 23:30 h CET

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