The role of a low-level jet for stirring the stable atmospheric surface layer in the Arctic

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This paper presents novel observations of the atmospheric surface layer over northeast Greenland in March 2018, using ground-based and balloon-borne probes. The vertical profiles over a 15 hr period show the occurrence and disappearance of a low level jet. The observations are generally clearly presented and are of interest to ACP readers. The authors go on to propose a hypothesis, that the LLJ can act to advect pollutants above the surface, which can then be mixed back to the surface when the LLJ ceases. The data presented make this a reasonable hypothesis, but the paper tries to use a model calculation to examine the hypothesis further. I did not find this part of the paper convincing: the model is very crude and I could not understand the presentation of the results (fig 10). I can see the value of trying to compare the observations to the 1-D model, but given the disparities I do not see the value of using the model to examine the hypothesis. For sure, if you reduce the diffusion coefficient such that tracer released into a stable layer cannot reach the surface, advection will move the tracer downstream. But I don't see how this adds value to what can be deduced from the observations themselves, because the LLJ conditions are manifestly not 1-D.

Detailed comments:

l. 28. Richardson number (not Richards)

l.90, eqn 2. Suggest you write $(\Delta u(z))^2$ since $\Delta u^2(z)$ could be confused with taking the difference in u^2

l.94. Please provide appropriate references for equation 3. Siebert et al (2006) makes no mention of structure functions, and Wyngaard's book does not present the t*u formulation used in this paper. Also, it is not clear to me why you use the same averaging period (2s) for the structure function and for the mean velocity.

l.101, equation 4. Again, please give a suitable reference – I cannot find it in Wyngaard's book but if it's there please provide the page number. There is on p. 16 the expression $\varepsilon \sim u^3/\ell$ referring to the largest (energy-containing) eddies, so I presume this is what is meant (this is also implied by l. 110). Turbulence is a phenomenon with a cascade of motions from the energy-producing scales to those of viscous dissipation. One could even say that a defining characteristic of turbulence it that it doesn't have a typical scale!

l.102. You say that ε is derived over 5 s scales but on l.96 you say 2 s. In any case, why are you performing a regression of two quantities evaluated over different scales (σ is smoothed over 30 seconds)?

L.105, 108. figure caption says Ri_g, not Ri_b. Which is it?

l.106. The only length scale that can be sensibly deduced from fig 2 is 4 m, from the regression line. I presume that by 'local length scale' you mean the ratio of individual σ^3/ε values, but this will be severely affected by stochastic noise.

l.112. analogously

fig.3 caption: ascents and descents

l.185. Looking at fig. 6 I can only see two maxima, one at the surface and the other at $z/z_i \sim 1.5$, not three as the paper claims. Likewise, there isn't a minimum at $z=z_i$.

l.288. The paragraph as written implies that the model (fig 8) and the observations (fig 9) agree, but they clearly don't. The observations show peaks in the TKE production at 130 and 240 m arising from the shear-induced peaks at 125 and 225 m (the inversion height is, I presume from fig.5, at 100 m), but the model shows a peak near the ground. The red, green and blue curves in the two plots are identical so have clearly been plotted twice – from which source I can't tell. The correct versions of these curves need to be plotted and the paragraph re-written to describe what is actually shown on figs 8 and 9.

Section 5.4. It would help here if the profile of K_m from the observations (derivable from equation 10) were shown and compared with the assumed value in the model. This will show whether the diffusion calculation in the model is at all useful as an estimate of diffusion in the real case.

l.306. As far as I can make out, K_m in the LLJ simulation should be $kK_{m,0} = 0.098*3.7 = 0.362\ m^2s^{-1}$ (l.260). Then a value of Pr=2.59 must be assumed to get the quoted K_H. Why did you choose this value? The paper should justify this choice. What is the equivalent K_H in the pre-LLJ atmosphere? With the lower stability in this case Pr should be around 1 so would $K_H \sim K_{M,0}$?

L.307 At what height is tracer released in the model? Is there a continuous source at x=0 and at some height (or height range)? Without this information it is not possible to understand fig.10.

Fig 10. A much more informative caption is needed for this bewildering figure. What is the colour scale? Is the dark blue that dominates the top panel a low or a high concentration? Do the two plots show maxima in concentration descending towards the ground? Why are there only four colours on the upper plot and 23 on the lower plot? Where is the source of tracer? Is it the same in the two cases?

L.340 katabatic