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Comment on amt-2021-173

Anonymous Referee #2

Referee comment on "Instabilities, Dynamics, and Energetics accompanying Atmospheric Layering (IDEAL): high-resolution in situ observations and modeling in and above the nocturnal boundary layer" by Abhiram Doddi et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-173-RC2, 2021

Overview

The article by Doddi et al. presents an overview of a project named IDEAL (Instabilities, Dynamics and Energetics accompanying Atmospheric Layering) aiming to achieve a better understanding of the vertical structure of the troposphere under very stable conditions. The project relies on observations and direct numerical modeling (DSN) tools. The observations consist of high temporal resolution measurements acquired from small instrumented unmanned aircraft systems (UAS), Doppler radar profiles, radiosoundings, and of meteorological measurements near the surface. A measurement campaign took place in October-November 2017, during which 72 flights of UASs took place, with these flights grouped in pairs or threes. Preliminary results of the field campaign are shocased. Two numerical simulations of Kelvin-Helmholtz instabilities development are also presented.

The IDEAL project is undoubtedly a very interesting atmospheric research topic. The instrumental means implemented on the IDEAL project are relevant and original (in particular the use of fast sensors on guided UAS). However the paper suffers from some shortcomings, particularly in the description of the data analysis methods. Also, the articulation between observations and modeling, although very interesting in itself, is not very well presented, and I think this aspect should be addressed with more precision.

I therefore recommend that this article be published with some modifications, some minor, others more substantial.

Major comments

1) The introductory section (first section) is clear and concise. However, the notion of sheets and layers (S&L) in the present context is not completely clear to me. Does it refer to the alternation of stable and turbulent layers? Or is it strictly limited to the presence of "thin strongly stable non-turbulent" at the edge of weakly stratified layers (presumably turbulent)? It seems to me that the works presented in the second paragraph of the introduction sometimes fall into the first category, sometimes into the second. Can you clarify this S&L notion in the present context? Isn't it necessary to precisely define a sheet (threshold gradients, thickness, location)?

2) Several studies of the stable boundary layers partly based on UAS (not only DataHawk) are already published . Also, some results on the properties of turbulent layers in the troposphere have been obtained by careful application of the Thorpe analysis applied to radiosoudings. I think these works should be mentioned in the introductory part.

3) Table 2: how are estimated the accuracy of coldwire T? hotwire velocity? What are the characteristics of the instrumental noise on T? and airspeed? (white noise? Noise level? Impact of motor vibration you mentioned?).

4) The characteristics of the UASs are described in great detail in section 2. However, almost nothing is said about the data analysis methods.

- With what vertical resolution are the vertical gradients estimated? And why this choice?

- No estimates of uncertainties on N2, Ri, CT2, epsilon are presented. Can you estimate an error bar for these quantities? Or at least show the dispersion of the estimates?

- How are turbulent and non-turbulent regions discriminated? (since CT2 and epsilon estimations are meaningless in a non-turbulent region).

- The profiles of figures 11-16, from DH2 or radiosondes appears very smooth. Are they filtered? If so, with which filter? And why did you choose these filtering characteristics?

5) Figures 5 and 7 are not very useful to describe the strategy of the observations, the description is sufficient. On the other hand, I think that one or two figures showing the power spectral density of T and airspeed to illustrate the estimation method of CT2 and epsilon would have been relevant in the present paper.

6) The link between the fifth part (modeling) and the rest of the paper is not very clear.

Was the choice of parameters for the simulations (characteristics of the gravity wave, the tube and the nodes) guided by the observations previously shown?

Specific comments

Granite Peak (in text)☐ Granite Mountain (in figures): please, use the same notations throughout.

Line 171: top right panel of Figure 6 shows RH, not surface winds

Figure 6, and line 171: wind "from the South" are negative (lower left panel of Fig.6). Is this correct?

Figure 6: the x-axis should show the dates of the soundings rather than their numbers. Also, the profiles should be visualized according to their dates, thus avoiding interpolating between soundings from one night to the next (which makes no sense).

Line 219: you mention 31 multi-aircraft sorties. But in line 109, you mention 14 + 13 sorties. Where does the difference come from?

Line 230: "The background atmospheric column was near-neutrally stable…" Where, and when? (I don't really see this in either Figure 12 or Figure 14)

Line 239: I do not see any sheet at 1300 m on Figure 11 or 12. Do you mean 800 m on Figure 12 and 1300 m on Figure 14?

Line 240: "The oscillating motion exhibited by the sheets…" What evidence of an oscillation ?