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Reply on RC2

Abhiram Doddi et al.

Author 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-AC2, 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, radio soundings, 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 showcased. 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)?

[Author Response]: The stable atmospheric column consists of deep homogeneous, but perhaps weakly turbulent layers (where gradients of various properties are negligible) bounded by relatively thin “sheets” with sharp gradients of temperature and humidity. Hence, the “Sheets and Layers” terminology. Such structures are ubiquitous in Doppler Wind profiler radar images and radiosonde soundings. This will be clarified in the revised paper.

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.

Few contemporary UAS platforms can provide reliable measurements of TKE dissipation rates and temperature structure function parameter from weak, small-scale turbulence events. Some notable works include Van der Kroonenberg et al 2008, Wildmann et al 2014, Altstadter et al 2015, Baserud et al 2016.

We concur with the reviewer’s comment that Thorpe analysis is a reliable technique to infer turbulence characteristics from radiosonde data. A few notable works include Clayson and Kantha 2008, Gong and Geller 2010, Wilson et al 2011, and Kohma et al 2019. However, the pioneering work on the application of Thorpe analysis to radiosonde observation data (Clayson and Kantha 2008) in studying turbulent layers in the troposphere is acknowledged. The authors will include the above-mentioned studies to strengthen the literature presented in the introductory section. Please note Dr. L Kantha is a coauthor of this paper.

Reference: Clayson, C. A. and L. Kantha, 2008. Turbulence and mixing in the free atmosphere inferred from high-resolution soundings, J. Atmos. Oceanic Tech., 25, 833-852.

Reference: Wildmann, N., Ravi, S., and Bange, J. 2014. Towards higher accuracy andbetter frequency response with standard multi-hole probes in turbulencemeasurement with remotely piloted aircraft (RPA).Atmospheric Measure-ment Techniques, 7(4):1027–1041.

Reference: van den Kroonenberg, A., Martin, T., Buschmann, M., Bange, J., andV ¨orsmann, P. 2008. Measuring the Wind Vector Using the AutonomousMini Aerial Vehicle M2AV.Journal of Atmospheric and Oceanic Technol-ogy, 25(11):1969–1982.

Reference: B ¨aserud, L., Reuder, J., Jonassen, M. O., Kral, S. T., Paskyabi, M. B., andLothon, M. 2016. Proof of concept for turbulence measurements with theRPAS SUMO during the BLLAST campaign.Atmospheric MeasurementTechniques, 9(10):4901–4913.

Reference: Altst ¨adter, B., Platis, A., Wehner, B., Scholtz, A., Wildmann, N., Hermann,M., K ¨athner, R., Baars, H., Bange, J., and Lampert, A. 2015. ALAD-INA – an unmanned research aircraft for observing vertical and horizontaldistributions of ultrafine particles within the atmospheric boundary layer.Atmos. Meas. Tech., 8(4):1627–1639.

Reference: Wilson, R., F. Dalaudier, and H. Luce, 2011: Can one detect small-scale turbulence from standard meteorological radiosondes? Atmos. Meas. Tech, 4, 795-804, doi:10.5194/amt- 4-795-2011.

Reference: Gong, J., and M. A. Geller, 2010: Vertical fluctuation energy in US high vertical resolution radiosonde data as an indicator of convective gravity wave sources. J. Geophys. Res., 115, D11110, doi:10.1029/2009JD012265.

Reference: Kohma, M., K. Sato, Y. Tomikawa, K. Nishimura, and T. Sato, 2019: Estimate of Turbulent Energy Dissipation Rate From the VHF Radar and Radiosonde Observations in the Antarctic. J. Geophys. Res., 124, doi.org/10.1029/2018JD029521.

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?).

[Author Response]: Coldwire temperature (sampled at 800 Hz) is calibrated against a commercial sensor (slow – 100 Hz), so this retains the accuracy specified for this reference sensor. Similarly, hotwire velocity is calibrated against the Pitot-static sensor. In turn, the Pitot-static airspeed is calibrated against GPS speed over each loiter circle as the average of maximum and minimum ground speed.

The turbulence parameters like the TKE dissipation rate and the temperature structure function parameter are estimated by employing spectral analysis of high-cadence CW temperature, HW and pitot airspeed measurements.

Motor vibrations produce periodic artifacts (sharp peaks at specific frequencies) in the HW and pitot airspeed spectra that are excluded in the spectral fitting procedure when estimating the turbulence parameters by an iterative technique that is beyond the scope of this paper. Also, care is taken during spectral analysis to exclude the data close to the sensors' (white) noise floor.

Several (~100 samples) 'quiet' (non-turbulent) spectral samples (calculated using 1s time series of 800 Hz data) were analyzed from CW temperature and pitot airspeed measurements to determine the sensor noise floor. The CW sensor noise floor was estimated to be 1.25×10^{-8} K^2/Hz. The pitot and HW noise floor was estimated to be at 1.5×10^{-7} m^2/s^2/Hz.

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

[Author Response]: The authors agree that this is a shortcoming. The procedures employed to compute turbulence parameters of TKE dissipation rate and temperature structure function parameter, and the estimates of horizontal wind vector components are novel. These estimation algorithms were developed as part of the lead author's (Abhiram Doddi) doctoral thesis and are yet unpublished. The authors are currently working to describe turbulence and wind estimation procedures in upcoming and follow-on research articles. Reference will, however, be made to lead author's thesis.

However, Lawrence and Balsley 2013, Luce 2019 (citations given below) describe the general framework of the estimation procedures utilized for computing wind and turbulence parameters presented in this article. We will include a subsection (in section 2) that briefly describes the estimation procedures.

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

[Author Response]: The vertical gradients of winds are estimated using pressure altitude data (sampled at 800 Hz) which is filtered and subsampled to 10Hz. This is used to calculate the vertical gradient of horizontal winds. The potential temperature is calculated from the measurements of CW temperature (at 800 Hz), but the buoyancy frequency, and Richardson numbers are calculated using subsampled data (just as above) at 10Hz. For our preliminary analysis, this resolution provided a reasonable compromise between vertical resolution and overly noisy estimates. Other studies using this dataset may make different choices.

- 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?

[Author Response]: Uncertainties in estimating epsilon and C_T^2 are best described by the variance in the spectral fits to the measured spectra. This information was omitted in the figures presenting these quantities. The manuscript will be revised to include the uncertainties for epsilon and C_T^2.

It is not common practice to present uncertainties for N2 and Ri estimates. Instead, the distributions of these quantities are typically found in the literature. We intend to do so in the revision.

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

[Author Response]: Our method of estimating epsilon and C_T^2 result in these parameters quantified at every data analysis interval (altitude or time).

In deriving epsilon and C_T^2, the measured power spectral density (PSD) calculated over a short interval of time is fit against a model Kolmogorov spectrum (e.g., Tatarskii 1961, Frehlich et al 2003). In case of the measurements obtained from sampling in non-turbulent regions, the measured PSD exhibit very poor fits to the model Kolmogorov spectra (do not exhibit an f^-5/3 slope in PSD vs f). This results in large fit errors, enabling these intervals to be excluded from subsequent analyses, or flagged for more detailed scrutiny of the spectral data, as needed for the analysis at hand.

- 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?

[Author Response]: We concur that a brief description of the estimation procedures for the parameters presented in these figures is warranted. For instance, the resolution of turbulence parameters depends on the time series intervals employed during spectral analysis (here, it is 1Hz because 1s intervals have been used to estimate epsilon and C_T^2), and the resolution of wind estimates depends on the interval over which the estimates are averaged (here, it is 1Hz because averaging is conducted over a duration of 1s).

We will include a subsection (in section 2) to briefly describe the procedures used to estimate wind and turbulence parameter in addition to potential temperature, N^2, and gradient Ri.

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.

[Author Response]: As described in the previous comment, a subsection explaining (briefly) the estimation procedures will be included with spectra each for CW temperature and pitot (and HW) airspeed.

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?

[Author Response]: Section 5 presents results from two previously conducted DNS to study the formation of S&L structures arising from superpositions of convectively stable gravity waves (GW) and dynamically stable mean shears. The first DNS experiment featured a GW of amplitude 0.5 (relative to vertical gradient of local potential i.e.,) and an intrinsic frequency of N/10 (where N is the Brunt Vaisala Frequency) at Reynolds Number of 50,000. The second DNS experiment designed to study the Kelvin Helmholtz Instability (KHI) assumed a Reynolds number of 5000 and a minimum Richardson Number of 0.1 with a random white noise background velocity field (superimposed to stimulate instability growth leading to KH billows). Figures 17 and 18 present relevant results from these two DNS.

These DNS of multi-scale dynamics (MSD) suggested that the resulting KHI tubes and knots (T&K) dynamics are likely major contributors to the S&L structures which ubiquitously occur in the atmosphere. Thus, these DNS studies presented in section 5 provided the motivations for the IDEAL observation program. In phase II of the IDEAL project, we plan to expand such DNS studies to explore the implications of IDEAL measurements.

This section, as the reviewer points out, is misplaced. It serves better purpose to present the motivations provided by DNS upfront – within the introduction section. We will restructure the implications of these initial DNS studies and present them as motivations within the introduction section.

Specific comments

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

[Author Response]: This will be changed to "Granite Mountain" everywhere in the revised manuscript for consistency.

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

[Author Response]: Yes, we will make the needed change in the revised manuscript.

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

[Author Response]: Yes, southerly winds are negative. However, we realized that the range on this figure is not helpful. We will replot this figure with a colormap showing better data range for clarity.

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).

[Author Response]: This detail was also highlighted by other reviewers. In the revised manuscript, this figure will be replotted with the dates of soundings on the X-axis while omitting data interpolation.

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

[Author Response]: A total of 31 sorties were carried out of which data from 27 (14+13) sorties were processed and analyzed as the other 4 sorties (consisting of 6 flights (1+2+2+1) in total) contained corrupt data. Therefore, these datasets were discarded. This detail is not mentioned in the manuscript. We intend to include a table listing the background conditions, flight location, dates and time of flight, brief description of observed features, and flight strategy for all UAS sorties.

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)

[Author Response]: The background N2 value (away from the turbulent layers and sheets) averaged to 10^{-4} s^{-2}. The dashed red and blue vertical lines in the N2 tiles of figures 12 and 14 represent the 10^{-4} s^{-2} values for each profile. We refer to this miniscule N2 value are near-neutrally stable.

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 239 was meant to comment on figure 14. Not figures 11 and 12. The figure reference will be fixed in the revised manuscript.

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

[Author Response]: The mean height of the sheets (at 800 m and 1300 m) identified from the N2 profiles in figure 14 undulates. We infer from this detail that the sheets are oscillating/ undulating.