

Interactive comment on “Overview of and First Observations from the TILDAE High-Altitude Balloon Mission” by Bennett A. Maruca et al.

Anonymous Referee #1

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The paper by Maruca et al. describes a sonic anemometer for use on a stratospheric long-duration balloon. 3D high-resolved sonic anemometry has a large potential for examination of atmospheric turbulence. Especially questions of isotropy and intermittency are very interesting and only very few techniques are able to answer these demanding tasks. The authors are strongly encouraged to continue data evaluation and the optimization of the instrument. The paper is well written, and several interesting new and partly historic references are appreciated. I suggest some changes in the structure of the paper, taking into account that only limited data has been achieved during the flight (see below). I understand that the scientific analysis of the data just begun and is outside the scope of this paper. Nevertheless the authors should take additional care for quality assurance as well as for the description of the potential of their method. I recommend a review of these sections, with more details given below.

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General comments:

Throughout the whole publication the authors concentrate on the description of an instrument that flies piggyback with a large payload on a floating balloon near 40 km altitude. Unfortunately there was no useful data obtained above 18 km, and only unproven suggestions are made to improve the instruments to cover higher altitudes (lower air densities). A lot of ambitious plans are presented and only few have been achieved. From my point of view this flaws the paper to a large extend. What gets lost in the impression of the reader is that there is a very interesting new technique to measure turbulence up to at least ~ 20 km, with absolute 3D wind and temperature fluctuations at least in the ascent but potentially also during the floating phase – if the floating altitude is low enough. This is not possible with e.g. CTA measurements or Thorpe analysis.

The paper concentrates on the combination with GRIPS. I have not found any information that TILDAE is in principal limited to GRIPS. Maybe other, smaller payloads are possible that would reduce the risk of wake problems or allow for a double TILDAE, measuring on two sides of the payload.

I suggest revising the structure of the manuscript. The whole instrument should be described without the focus on GRIPS and its floating phase at close to 40 km altitude, where TILDAE failed. Of course, this GRIPS ascent would serve as the only test case, but the achievements could be described more prominent. Floating phase measurements and other carrier instruments (if needed) could be described in kind of an outlook.

The description of first tests of sonic anemometry under stratospheric conditions is still possible with the new focus. But one way or the other, a comprehensive test needs further analysis of the regions where TILDAE was unable to measure (tropopause and > 18 km). A pure reference to the specs sheet (and the -50°C limit) is not sufficient, but should be added by e.g. plots of SNR or other meaningful quantities.

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Further effort should be put on validation of absolute flow velocities derived from the sonic anemometer. They can, e.g., be compared with the ascent rate (for v_z) and with the vertical shear of horizontal winds (for v_x and v_y). Comparative data can be taken from the GPS altitude and position change of the gondola.

Specific comments:

p. 2, l. 8/9: The combination of 3D wind and temperature is very interesting and scientifically interesting. I understand that a full scientific analysis cannot be done within this technical paper. Nevertheless, while this is a core of the scientific potential of TILDAE, I would expect e.g. the spectra for a single case like in Fig. 8 to be shown for all four components instead of the sum of the three wind velocities (see below).

p. 2, l. 17: The GRIPS references say “imager” instead of “interferometer”.

p. 2, l. 19/20: For the reader not being familiar with wind soundings on balloons, you should explain that TILDAE is measuring not the true horizontal wind, but the ambient flow around the instrument. This is essentially the vertical shear of the horizontal wind between the altitude of the payload and some “effective altitude” where the wind pushes the balloon-payload-system.

p. 4, l. 14: Please provide some more information on this humidity correction. Fig. 6 contains a lot of tropospheric data. Furthermore, in the Conclusions (p. 15, l. 2/3) TILDAE is advertised for soundings in the tropopause region.

p. 4, l. 16-18: I do not know about the “long history” of hot-wire anemometry in scientific ballooning. I only know about CTA with its ~ 10 y history and irregular flights. Furthermore I am surprised that this technique is part of standard radiosondes. From the best of my knowledge radiosondes track the position of the payload either by radar or GPS to get the horizontal winds. Hot-wire anemometers do only catch the flow around the payload, see above.

p. 5, l. 1-4: I would like to see another important property of CTA mentioned here: Its

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largest sensitivity at low windspeeds (cf. Whelpdale 1967) provides an advantage for the typically small flow velocities around the payload.

Section 3.1: Could you please provide some further information about TILDAE and GRIPS? For TILDAE the distance between transducers and the accuracy of the wind and temperature measurements (at different pressures) would be interesting to know. For GRIPS the size of the balloon, the length of the whole flight train, and the size of the payload would be interesting for estimation of wake effects. What is the length of the TILDAE boom? “Extending beyond the base of GRIPS” is i) vague and ii) not the most important property to estimate wake effects.

p. 6, l. 13: Is there a special advantage of using v_z for the temperature measurement? During ascent v_z is typically larger than v_x and v_y ; during the floating phase v_z is almost zero.

p. 6, l. 28-30: I am not sure whether the wake problem can be identified from the pointing of the balloon and the GPS information. The balloon-gondola system is floating with a horizontal speed being a weighted average over the cross sections of balloon, gondola, ropes etc. Assuming TILDAE is pointing “forward” it could still be in the wake if the wind vector in forward direction at gondola altitude is larger compared to the “mean” wind. The directional information from TILDAE might not help to get the true wind vectors at gondola altitude because it could be influenced by the wake. A second TILDAE in opposite direction would help if the large GRIPS structure is not affecting both at the same time.

p. 7, l. 11-13: Have you been able to recover the SD cards right after landing? If I understand the GRIPS website correctly, most of the GRIPS instrument has been recovered as late as January 2017.

p. 9, l. 15/Fig. 6: I assume that there is a McMurdo radiosonde for 00 UT. It would be interesting to compare the TILDAE temperature data with this standard method. This could furthermore validate the suspect data points.

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p. 10, l. 21/22: Radiosonde data provides also information on humid layers (especially if relative humidity is calculated with respect to ice instead of liquid water), even if the sonde was launched a few hours before TILDAE.

p. 11, l. 3 / Fig. 7: Please provide some information about the altitude.

p. 12, l. 4/5: This intermittent structure is in good agreement with other balloon observations, e.g. Gavrilov, Ann. Geophys., 2005, and Haack et al., JGR, 2014.

p. 12, l. 12 / Fig. 8: While TILDAE aims to examine the isotropy of turbulence; it would be interesting to see the individual wind components (and the temperature) instead of the general wind speed.

p. 12, l. 16-18: The spectrum indeed follows the $-5/3$ slope very nicely. Could you please calculate energy dissipation rates or some other quantity for the description of turbulence? It would help the reader to classify this layer (and estimate the potential of TILDAE).

p. 14, l. 1: On page 6 I read about the advantages from the GRIPS pointing system, but here I learn that this has not been active for the data shown before. I would like to read this much earlier.

p. 14, l. 4: Is there some information about pointing maneuvers in the GRIPS house-keeping data? Instead of wind direction maybe “flow direction” should be used. Other reasons for the apparent change in flow direction might be a true change in wind direction (vertical shear), or a rotation of the gondola due to inertia or wind (flow) pressure on the gondola. Maybe you could infer the pointing direction from GRIPS housekeeping data and compare with horizontal wind direction. Unfortunately these open questions influence the statement that Fig. 8 describes real atmospheric turbulence.

p. 14, l. 8: Do you see any sign of vibration in the accelerometers?

p. 14, l. 10: I am sorry, but I do not see any pattern at 72 s. Please explain.

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p. 14, l. 19-22: In order to catch the GRIPS floating altitude, a further density decrease by a factor of ~ 20 needs to be compensated. Is there any information from the manufacturer whether this gain can be achieved? Is there an automatic control of the gain or might a gain increase result in saturation in the troposphere?

p. 15, l. 1-2: This is a very nice achievement, but flawed by the intention to measure at GRIPS floating altitude.

Typos:

p. 6, l. 13: double “the”

p. 6, l. 17: “based” should read “base”

p. 8, l. 3: “ensuring” should read “ensure”

p. 13, l. 16: “directions” should read “direction”

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