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Comment on amt-2022-236

Anonymous Referee #2

Referee comment on "Drone-based meteorological observations up to the tropopause – a concept study" by Konrad B. Bärfuss et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-236-RC2, 2022

Let me begin by saying that I am a strong supporter of the utility and benefit of automated aircraft reports to fill the time and space gaps left between other in situ observing systems. I therefore read the article entitled "Drone-based meteorological observations up to the tropopause" by BarfuÌ□ss, SchmithuÌ□sen and Lampert with interest. Unfortunately, I came away from it feeling disappointed and uninformed. As it stands, the text reads much more like a "Concept Study" or perhaps "A Report on Preliminary Project Activities" than a scientifically supportable journal article and needs to be substantially revised and enhanced before it can be considered for publication. Some of my reasoning for this decision is summarized below.

Firstly, the authors have dedicated nearly 1/3 of the article to the Introduction and background materials. And even then, they have not included any discussion of problems that other authors have discovered with automated reports from commercial aircraft that should provide a far more stable platform for collecting data and providing representative measurements or the atmosphere without being affected by artifacts related to aircraft stability. For example, it has been documented that AMDAR observations from longer-range aircraft provide more accurate wind reports than TAMDAR reports obtained from smaller/lighter regional jets, whose wind reports are much more susceptible to the effects of turbulence than the larger/heavier aircraft. See recent papers by Wagner et al. on this topic. The excessive presentation of background material also detracts from the limited analysis of the flight results.

A fundamental issue of terminology is also scattered throughout the paper related to the phrase "Uncrewed Aerial Systems" (or UAS). According to online references on drones, "a UAS or "Unmanned Aircraft Systems" includes not only the UAV or Drone but also the person on the ground controlling the flight and the system in place that connects both. Basically, the UAV is a component of the UAS, since it refers to only the vehicle itself." As such, why is yet another (possibly conflicting) terminology being added the already excessive meteorological acronym list?

When compared to other sections of the paper, the introduction length (143 lines) seems to be excessive. For example, the important information about instrument accuracy in the section titled Sensor Package uses only 13 lines (and includes a Table that is never referenced in the text and contains an undefined term "prospective"). For other aircraft observing systems, detailed air tunnel tests were performed with the full system to determine whether the installation approaches would degrade the observations further. The reader can only assume that no such tests were done of the LUCA installation approach. The only vague reference notes that some temperature observations were affected by "heat transfer inside the fuselage to the sensor head". The authors also assume that the HMP110 sensor package will work as well on a drone moving through the atmosphere (at a speed not clearly specified in the paper) as it does on a radiosonde package suspended from a balloon moving with the atmospheric flow, not through it. This should not be considered a given. For example, when the TAMDAR system was developed, questions regarding biases produced by instrument lag using capacitive humidity sensors flying through the atmosphere were sufficient to change the instrument design to use dual sensors whose reports were averaged.

I also expected to see some discussion about how wind measurements were derived from the aircraft and how they were affected by turbulence, especially near the level of the jet stream.

Unlike other proven aircraft observing systems which attain speeds much larger than the ambient wind in the upper troposphere, the drone discussed here is likely to be flying at speeds much less than the atmospheric flow. As such, much of the wind vector determination will be dominated by change in geographical location. Without this documentation [or clear knowledge of the system outputs (e.g., GPS locations, drone speed and direction, maximum flight duration, maximum distance from takeoff site to tropopause observation, residence time at tropopause elevation, etc.) that are used to determine the meteorological parameters], it is difficult to assess either the utility of the drone observations or the source of differences between the drone observations and other sources. I would also like to have seen evidence that any object that is 2 m in length and 5-6 kg in mass will not be 'tossed around' in the jet stream. This would have to have a substantial impact of some wind observations, which would than need to be eliminated using specialized quality control. Similarly, since the authors state that the temperature reading may be affected by heat generated within the drone itself, is there a method of determining which readings are likely to be contaminated as a function of whether the drone is flying into the wind (in which case the heat generated by the drone should be moved away from the rear of the aircraft and away from the nose mounted sensors) or with a tailwind (in which case the heat could remain in the proximity of the drone and possible affect the sensors).

Two other sections of the paper provide insufficient support. In the 20 lines of the System Design section, the authors use Figure 1 to provide typical ranges of temperature and wind conditions that were used in the drone design. Panel 1b clearly shows that more than 10% of wind observations at 9-10km exceed 40m/s, yet the instrument is designed to operate in wind speeds < 28m/s. That amounts to a difference in Kinetic Energy of 50%. Significant weather events (e.g., cyclogenesis) are very often associated with very strong jet streaks and substantial ageostrophic circulations that enhance the environment in which he storms form. Accurate knowledge of the sources of Kinetic Energy and its conversion into Available Potential Energy is essential to improve forecasts of these events. If the objective of launching drones is to fill in observations at times when are expected, it is unlikely that the existing design limits of this system will fill that need. This limitation of environments in which wind speeds are < 28m/s makes the use of the phrase "up to the tropopause" inappropriate for inclusion in the title.

Also, the authors seem focused on moisture in the middle-upper troposphere, saying that it is particularly important. I'll grant that moisture can have a number of influences at these levels, but the amount of moisture, its vertical structure and horizontal structure are much more important for forecasting weather events from heavy precipitation and flooding to severe storms.

The section entitled "Potential for covering data gaps with UAS based on atmospheric dynamics" seems mistitled and contains substantial conjecture. Figure 6 provides climatological, not dynamical, information about the rate of change of atmospheric parameters, not the dynamical causes for these changes. It is not new and not particularly relevant to the results presented here, which should be the emphasis of this paper. For example, if the drone system is limited to elevations below ~10km, why to these plots extend up to 35km and how is the reader expected to extract information from them? In addition, phrase like "As one would expect" and "likely results in" are unsubstantiated by the results presented in the paper and detract as conjecture.

My biggest issues with the paper, however, relate to the lack of any quantitative assessment of the drone observations or their possible impact on operational weather forecasting. First, neither Figs 4 nor 5 indicate the units of the wind observations. Are these m/s or knots? Because some LUCA reports in figure 5 show a filled barb on the wind flag, I can only assume that they are knots.

Figure 5 provides anecdotal information from 2 radiosonde matchups that can only be used subjectively. Figure 4, however, indicated that colocation information was available from 5 launches, yet no attempt was made to quantify the differences. In several instances, the authors attribute differences between the two observing systems to time mismatches between the sonde and the drone. Articles assessing the accuracy of AMDAR observations against a longer series of special radiosonde launches prior to and after multiple AMDAR aircraft ascents/descents showed that the time difference effects were substantially less than other factors. When enlarged enough to view the individual wind barbs in Panel 5b, wind speed differences of up to 15 knots are noted at multiple levels around 6km, with erratic directional behavior in the drone observations near 2km and between 4 and 6 km. There is also little evidence in Fig 4 to justify the 2-3 degrees warmer observations near the top of the drone profile in Panel 5b. These differences are crying for detailed quantitative analysis and explanation, but none is available.

The paper is also devoid of any cost/benefit analysis for the use of a system like this in daily operations. After all, the best system in the world will be of little use if weather services can't afford to use it. What is the cost of producing one set of ascent/descent reports? How does this compare to existing radiosonde system costs? (FYI, the NWS in the US has done a detailed analysis of this and find the total cost of radiosonde launches to be in the range or $200 to $250 per launch, personnel, instrument, balloon, and gas.) How timely is observation availability, especially if they are to be used on the mesoscale? Are the observations 'all weather', e.g., can the system fly through condition of aircraft icing? If not, the system will become less attractive. How quickly can the drone be recharged and sent to another mission? What a training is needed both 1) to launch/retrieve the system and process the data and 2) maintain the system? How many systems will be needed to fill the needs of the EU?

As a result of inadequacies like these throughout the text, it read like a preliminary progress report instead of a quantitatively verified scientific contribution to the literature. Although I support concept presented here, the paper is not ready for publication at this time.