Review about:

Airborne measurements of turbulence and cloud microphysics during PaCE 2022 using the Advanced Max Planck CloudKite Instrument (MPCK+)

By
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First of all, I would like to congratulate the authors on these measurements; I can judge from my own experience how complicated measurements of this kind are. The setup on which these measurements are based is quite impressive.

Nevertheless, I have a few critical general comments first. I would like to divide the presented measurements into two parts: standard meteorological parameters (wind, pressure, temperature and humidity) and, as a second set, perhaps the real challenges such as cloud droplets and high-resolution turbulence which makes the observations definitively unique. The first area is discussed here and data is presented although I have to say that the discussion of the temperature and humidity data in particular raises a few questions, which I have specified below.

The second part is mentioned but taken out of the published data set (holography, PVT, hotwire). This makes the manuscript seem somehow off to me and raises questions. Why are the comparatively simple measurements that every radiosonde can perform discussed in detail, and why are the really elaborate and complicated measurements – which are highly appreciated - mentioned but not published?

Furthermore, I would also expect a quantitative assessment of the data quality which is missing.

The accuracy of the two inertial systems has been discussed in some detail, although I don't really understand the large offset in pitch. What I am missing, however, is an explanation of which measurements really require the Euler angles (roll & pitch) with the corresponding accuracy, as long as a three-dimensional wind vector is not to be determined – which is not planned with a one-component hotwire or Pitot tube.

I think this manuscript needs some more work before it can be considered for publication.

More specific:

Abstract:

Just out of curiosity: why is 'Advanced' capitalized in the full acronym even though the 'A' does not appear in the acronym?

Line 4: I suggest to include “cloudy” before planetary boundary layer to make clear that you sampled boundary layer clouds

Line 6: Well, Pallas is at 68°N and if you consider locations North of the Polar circle as “Arctic” you are right but I suggest to say “Polar regions” or so but this is a personal opinion.

Line 8: the mentioned distance between two cloud droplet size distributions is somewhat misleading; the sampling time and statistical significance is quite complex. Obviously, you store a size distribution every one second and assume less than 10 m/s true airspeed (wind speed)?! Is this a limitation of your system or do you sample individual droplets an estimate a size distribution over one second (later on this is discussed in some detail)? How robust is this estimate? Please clarify!

Line 9ff: To say that aircraft cannot fly in clouds at such low altitude is somewhat to hard. I suggest to soften this statement a little bit, look at the Polar research aircraft of AWI – they fly quite low (over open water) even in clouds and partly also in super-cooled clouds. Maybe you should argue that you can do observations even of low-level clouds/fog where it is often too dangerous for aircraft.

Introduction:

Line 17: Although I know what you are mean: this is only true for a fixed sampling rate – right? In this context, I would also mention that many problems such as adiabatic heating of inlets (consider temperature measurements from fast flying aircraft) and hazards like droplet shattering are minor if using a tethered balloon.

Line 33: is there a reason why not including the imaging data into this data set. It would make the data set much more complete!

Line 38: Please provide details where the spatial scale of less 10 m for size distributions comes from, this statement is somewhat vague (see also my comment about the abstract)

Line 67: Please provide a few more details/arguments why the helikite has an advantage compared to a classical tethered balloon. From your explanation it is not clear for most of the readers (although I know both systems).

Line 71: Please provide one sentence about the advantage to combine two helikites. Is it more stable or just the higher lift?

Line 72/73: I don’t really understand this argument.

Line 82: Do you really think that the nonlinearity between tether length and altitude is an issue? If you have different layers with different wind speeds (for example a low-level jet) you have always the “problem” that you rope is not straight. You measure the height above ground – that should be sufficient – right?

About fig 2: from the description it is not clear for me why the right part of the main tether goes upward? Perhaps you could make a sketch that shows the entire line guide?

Line 112: the flight descriptions such as “20220919.1236” should be explained although one could imagine what it means. Also is the time local or in UTC? Later on, you explain that all times are in UTC but I think a short comment at this point would help.

Line 116ff: Although I like this kind of description of the individual weather conditions during the individual flights, it is a bit confusing and difficult to read. Why not a small paragraph for each of the flights (there aren't too many) instead of a continuous text? Maybe a separated into meteorological conditions and technical issues?

Line 139: the last argument of the description of “b1” is misleading and maybe at the end it includes a typo? Please consider re-wording.

Line 145: you already explained the acronyms in tab 1 so this is somewhat repetitive.

Line 152: tab 1 or 2?

Line 153/4: The editor must decide whether it is common practice to keep parts of the data set under lock and key; in my view, at least a justification should be provided for why this is the case. The fact that the data can be obtained from the authors if there is a ‘justified’ interest is somewhat confusing for a data paper.

Line 174: I am confused by this nomenclature/numbers

Section 3.3: I am a little bit confused here. You didn’t provide any technical information about the sensors itself. However, the Vaisala device for example is based on the classical (heated) humicap – a capacitive sensor measuring the relative humidity with quite high absolute accuracy of 0.8% (as stated in the manual) but only up to 90% RH. So why not make use of the clouds assuming a mean RH = 100%. If you trust the temperature measurements you could calculate the dew point temperature and compare with the readings of the HMP7. I think this would be a more convincing way to describe the accuracy of this device.

Furthermore, I am not quite sure whether I have understood the statements regarding the accuracy of the temperature and humidity sensors correctly. Is it correct that you only get plausible humidity values if you take the arithmetic mean of two different sensors? If that is really the intention, it does not convince me.

For me, the discussion in Sec 3.3 is somewhat vague and I don’t get any feeling about the accuracy of the observations. Even more, I would like to get some numbers describing the accuracy.

Section 3.4.: Does the use of a dual band GNSS really explain the GPS-derived height deviation between the two devices of 10 to 20 m or so? Have you compared to barometric height which should be of high accuracy?

Line 202: I don't quite follow this argument about the temperature below 200 m – especially Fig. 6 don't show data below 500 m ASL – right? And why should a discrepancy with the ground temperature support any statement here? Can a ground-level inversion be completely ruled out? Is there a model for the response time of the sensors? If so, shouldn't it be a simple matter to assume a first-order system and correct for this response time?

I think the time series of a ground-level temperature doesn't really help to understand profile measurements, or have I misunderstood your argument?

At least for the sections shown in Fig. 6, the wind speed is also quite high. If temperature sensors cannot adapt quickly to the environment under these conditions, the design could be a problem, couldn't it?

Everyone is familiar with the problem at low flow velocities, but that is not the case here...

line 213ff: I think there is nothing wrong with the pure description of the cloud drop distribution, but after the formulation it sounds a bit like it would be a continuous process: activation at 5 microns and drop growth at a diameter of 25 microns - but what happens in between? It could almost be a secondary activation - right?

Sec 3.5

The first thing I notice in the profiles are the dew point temperatures, which are above the current temperature in the last two profiles. That is physically questionable – have the colors been mixed up?

Regarding the second last profile and the temperature decrease at the cloud top: without it being explicitly mentioned in the text, I suspect that it is about ascents? How is it prevented that after the cloud passage the sensors have become wet and the drops evaporate when exiting the cloud and artificially cool the sensor?
This would probably only cause a rather short cooling - but should be mentioned as a known problem. But if, as you have speculated, it is a case of evaporating cloud droplets from higher cloud layers, why is the dew point parallel to the temperature? Without being able to prove it with the data: I suspect that it is advection of a cooler layer, but that is just as speculative.

Section 3.7: An inclination deviation of about 10° is remarkable – does anyone have any idea what the cause of this could be? With such a compact system, installation errors should actually be smaller – are there any laboratory tests that can indicate which sensor is correct?

About the yaw angle: As I understand, the yaw angle is the platform heading e.g., the orientation around the vertical axis in an Earth-fixed system. So why is it depending of the course of the helikite? It should be – if the tail makes its job – depending on the wind direction – right? Saying this, I am not totally convinced if I can learn something from Fig 11. Fig 10 tells me something about the system performance and I agree that the payload seems to be quite stable.