Anonymous Referee #2

First of all we like to thank the reviewer for his helpful comments! We try to consider them carefully and address them all in detail below.

SPECIFIC COMMENTS

The design of the new FCPC was mentioned to be targeted at airborne use. Many CPCs flying on aircraft have to be designed to work in the pressurised aircraft cabin. I would like the authors to comment on this aspect regarding their new FCPC.
- Yes, airborne measurements are special applications. However, for our purpose, the FCPC will be operated under ambient pressure, means ACTOS does not provide a pressurized cabin. Furthermore, the maximum height will be around 3 km, thus the ambient pressure is not comparable to aircraft measurements.
The purpose of this paper is the description of the setup of the FCPC and presenting calibrations at ground conditions. Maybe in a follow-up study we will present first airborne measurements in connection with required calibrations. Thus, we have it in mind but think it is not necessary to include it here.

Reading the acknowledgement one could get the impression that different setups of the FCPC have been tested, some of which were probably not fulfilling the requirements. I wonder if it is worth mentioning which setups failed to deliver. This could certainly help to understand critical aspects of the setup finally chosen for the FCPC.
- In fact, we tried several setups and operation conditions before this final one. I think that is the normal way when setting up a completely new instrument. The first versions were operated in the laboratory only. With satisfying results there the last version was transferred to a 'field-version'. Various parts were modified during the development: different sizes and filling mechanisms of the saturator have been tested as well as different inner diameters of the condenser. The first versions did not contain the condenser bypass to remove liquid butanol. Also a number of small parts, such as connectors has been substituted or modified during the development, but I am sure, not all these steps are of reader's interest.
One point was already mentioned in the manuscript, that we tried also a Swagelok cross as mixing chamber like Wang et al., 2002, as well as a Swagelok 'T' and decided to use the second one.

We added to the setup section:
'The presented field version is the result of a few-year process. The development included also a half a dozen laboratory versions which did not fulfill the requirements sufficiently. Thus, for instance a horizontally-oriented saturator and a condenser without butanol-removal did not allow stable operation under atmospheric conditions.'

Page 5910, line 22, "In contrast to . . ." — Why "In contrast"? The purpose of the Wang et al. design hasn't been stated.
- Correct, we removed this phrase.

Page 5911, line 13 — How was the thermal decoupling actually achieved? The Swagelok T being mentioned is a stainless steel type, I assume? Is every part of the flow system connected by Swagelok fittings?
-yes, most parts are of stainless steel or aluminum. A thermal decoupling was reached by usage of a Teflon cartridge which was inserted into the Swagelok fitting to prevent any direct contact between the Swagelok fitting and the following tube leading to the condenser.
We added '..by a teflon cartridge…'

Page 5911, lines 5-6 — How was ensured that the temperature sensor is actually measuring the "right" temperature of the gas flow? In other words: Are there relevant temperature gradients?
- The temperature sensors measure the temperature on the walls of saturator and condenser. Probably there will be temperature gradients. But after some time of operation the system will be stable, i.e. also the temperature profile inside the instrument. It is not important for us, if the temperature is identical to the set temperature at any location inside the saturator, we only need reproducible conditions. Means, we know the set temperatures, measure the corresponding efficiency curve and apply it for exactly these set temperatures.

Page 5913, lines 19-23 — three temperature settings have been evaluated as described (leading to three different cut-offs). What about other temperature settings? Have these not been tested or have they not allowed successful operation?
- We tested also few more settings, but they turned out to be less stable. For this study we decided to present a few operation conditions only. In a next step we are planning to operate two FCPCs in parallel

with different temperature differences. Then we have to test the operation under more extreme conditions, such as maximum and minimum temperature differences.

We added to the text:
'These settings were chosen because of stable and reliable results in the laboratory and in the atmosphere. For much higher temperature differences the measurements became unstable and for much lower temperature differences the counting efficiency went down.'

Page 5916, lines 6-8—I am not familiar with power spectrum analysis. The -5/3 slope: is that standard textbook knowledge? Can a reference be provided here?
- The "-5/3-law" is standard textbook knowledge in the turbulence community; it is one of the fundamental findings of "classical" turbulence (Kolmogorov 1941). We added a reference of a recently published textbook for atmospheric turbulence, which describes these findings and where the landmark papers are cited for further reading. However, it was difficult to find the balance between a short description of the turbulence part and writing a detailed introduction into turbulence – in particular since many assumptions for turbulence spectra are not completely fulfilled for a counting process and many questions – which are going far beyond the scope of this technical paper – are still unsolved. We have partly rewritten this section due to comments from reviewer 3.

Page 5919, lines 21-27 (and corresponding discussion in Section 3.3.2) — I am a bit confused by the view of a CPC measurement addressing a spatial scale (of 60 cm). Can this be better explained? It is obvious that the response time is crucial to capture effects of turbulence in a time series of data. Why discussing a spatial scale?
- We tried to explain the steeper slope of the observed spectrum (compared with -5/3) and it looks like a "low-pass-filter effect" which can be better described in the time/space domain but this explanation was questionable (see comment of reviewer 3). We decided to use the spectral analysis only to distinguish between "coherent structures" and white noise of purely Poisson distributed particles at small scales. The reasons of small deviations of the spectrum from the classical -5/3 slope can be manifold and will not discussed in this paper – we noted that a thorough discussion of "why a counting process should (or not) follow the classical -5/3 slope" is still missing in the literature.

Fig. 5, lower panel. Why is this not a strict 1:1 relationship? What is the meaning of the offset in the FCPC concentration data if compared to the electrometer data? How does this relate to the stated sampling efficiency of the FCPC of 0.97 in the asymptotic branch of the efficiency curve?
- Yes, we agree that there is no reason for an offset in this relationship. Reasonably, the linear fit should pass 0;0, which was not considered in the first version. Thus, the fitting was done again with a fit curve passing zero and this resulted in a slope of 1.002, which is really close to 1. The figure was exchanged to one with new fit included.

TECHNICAL COMMENTS

Page 5909, line 6, "The spectrum . . ." — spectrum doesn't seem to be the right word in this context.
-okay, it was changed to: 'These applications include…'

Page 5910, line 3 — there should be a single tilde to denote "approximately 100"
- done.

Page 5911, line 6 — insert "temperature" before "sensor"
-You probably mean line 16: done.

Page 5912, line 20 — The actual type of the electrometer ("TSI . . .") could be mentioned a couple of lines higher up when the electrometer gets mentioned for the first time.
- Okay, done.

Page 5913, line 12, "the slope" — I think I know what the authors refer to, but a better wording should be found to describe where the standard deviation was actually higher.
- Reviewer 3 suggested 'cutoff region', thus we substituted slope with 'cutoff region'.