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Atmospheric Measurement Techniques Discussions

Interactive comment on "ChAMBRe: a new atmospheric simulation Chamber for Aerosol Modelling and Bio-aerosol Research" *by* Dario Massabò et al.

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The manuscript provides a characterisation of the new ChAMBRe atmospheric simulation chamber at Genova. The importance of the quantification of chamber-specific influences and interferences is not routinely recognised and the authors are to be commended on their attempts to provide such a characterisation. This manuscript provides the first part of "a user manual" for such a chamber and a demonstration of its fitness for purpose. As such, it is highly suitable for publication in AMT. The range of important questions that can be addressed in ChAMBRe is succinctly and concisely summarised

in the introduction, which thereby highlights the need for such a facility. Key characteristics: Size dependent particle lifetime; Preservation of bioaerosol viability through injection and extraction; Ability to explore bioaerosol viability under variable controllable and measurable changes in atmospheric composition; Repeatability under clean conditions demonstrated for 2 bacterial strains; one each of gram positive and gram negative groups, showing that it is possible to investigate viability under changing environmental conditions. The chamber and instrument description section is sufficiently detailed to provide the reader with the information to evaluate the suitability of the facility. It appears that the facility is very appropriately designed and well-appointed. The chamber characterisation is one of the main foci of the paper and has been carried out and described well - certainly adequately for a reader to evaluate the characteristics of the facility. The protocol for preparation and execution of the bacterial experiments has been well developed and is described appropriately. As a non-specialist, I am not qualified to comment on the appropriateness of the biological handling protocols or e.g. choice of bacteria; however, for the purposes of the manuscript, the details provided are sufficient to reproduce the protocol. The description of the first experiments was informative and provided a good illustration of the suitability for purpose of the facility. The repeatability within Poisson statistical expectation was convincing evidence for this. The fact that an empirically-determined dilution was required for E. coli (and that CFU was extremely RH dependent) is interesting and these experiments demonstrated the great care that will need to be employed in future investigations using the facility. Generally, I find the publication very suitable for publication in AMT. However, I think there is one very important omission that can be addressed with modest discussion.This relates to the closeness to ambient conditions that is achievable within the facility A key challenge in investigating PBAP in general and airborne bacteria in particular is their extremely low ambient abundance (from 0.01 to 1 cells/cc). Such low concentrations present difficulties with particle sizing and counting instrumentation relating to the counting statistics, often physically limited by the instrument sample volume. I believe it is important that the authors discuss such limitations and the implications of

necessarily studying under higher than ambient concentrations within ChAMBRe (such as differences in amount of reactant per cell). In this regard, it should be noted that the facility description indicates a maximum particle number measurement threshold of 10ËĘ7 /cc, but now lower detection limit and sensitivity. Indeed, the only mention of the instrument sensitivity for the lower limit is on line 229 where the value is still not provided. A general discussion of the accessible ranges of concentrations, the challenges with instrumentation and the comparison to ambient conditions should be provided. One final point on this subject - one might have expected that a key instrument for PBAP experiments would be one of the recent online fluorescence instruments (e.g. WIBS, UV-APS or more recent developments). Could the authors discuss whether these factor in their plans. It would appear to provide an ideal opportunity for comparison of online and offline techniques and perhaps address some of the concentration concerns I have raised above.

Authors reply: We thank the Reviewer for the extremely useful suggestions/criticisms and comments. We have carefully taken into account each point to improve the quality of the paper. In the text, we have added some information on typical bacteria residence time and concentration in the atmosphere as well as about minimum detection levels and sensitivity of used instrumentation. During our experiments, the typical bacteria concentration in the chamber is about 10ˆ5 cells/mˆ3 –see lines 369 of the original manuscript- (since 10ˆ7 is the concentration before the aerosolization process). This value is actually very close to the values of 0.01-1 cells/cc reported in literature (and by the reviewer) of typical ambient abundance. As the reviewer correctly notes, recent florescence instruments can help a lot in our experimental activity. In fact, the installation of a WIBS unit is expected in the next 6 months.

line 98 and elsewhere: Genova or Genoa? Consistency should be ensured throughout the document.

The format will be consistent in the revised manuscript.

line 108 and 118: I presume this should be 5 * 10ËĘ{-2} mbar, not 510ËĘ{-2} (presumably for AMT, SI units should be used - the editor can advise). All subsequent pressure values also suffer from this notation and it should be corrected.

The corrected notation will be used in the revised manuscript.

line 121: I presume the 2 step procedure is to ensure the HEPA and zeolite efficiency is not challenged by operating from 5 * 10ËĘ{-2} mbar to 5 mbar. Is this correct? If so, it would be useful to state it.

Actually, the two-step process comes from a long expertise with vacuum systems: in the very first phase of the venting, the use of nitrogen from a cylinder guarantees that no water vapor (or others) enter the chamber and penetrate in the walls. When the walls are “coated” with nitrogen, ambient air is used through the HEPA filter to bring back the chamber to atm. Pressure.

line 142 - 147: I presume the 253 nm lamp is to allow sterilisation without ozone generation. If so, it would be useful to state it for the reader’s benefit.

Exactly, a clarification was added to the text.

line 174 - 179: It is interesting to note the dehumidification system in the OPC. This will be very useful to avoid droplet ingress into the instrument and prevent too much hygroscopic growth. It will also ensure a good chance that non-spherical "solid" particles do not assume spherical geometry by water uptake. What is the implication of this for OPC sizing of e.g. rod-shaped or other non-spherical bacteria or dust? Don’t OPCs rely on refractive index and shape assumptions?

The “artifacts” in the OPC response related to particle shape are known and need proper correction (see for instance Caponi et al. (2017). Spectral- and size-resolved mass absorption efficiency of mineral dust aerosols in the shortwave spectrum: a simulation chamber study. ATMOSPHERIC CHEMISTRY AND PHYSICS, vol. 17, p. 7175) however, in our experiments we do not try to measure the bacteria size-distribution by

OPC (since the counts are totally dominated by the salt particles of the physiological solution) and the OPC is just used to check the global quantity of the injected material.

line 191: I'd suggest the upper limit to typical reaction times should be days rather than hours (e.g. the gas phase oxidation lifetime of SO2, OH oxidation lifetime of methane, ageing of organic aerosol and increase in O:C ratio etc...)

The statement will be corrected in the revised manuscript. .

line 198: I think the reference should be to 4.2 not 4.3

Done.

line 205: It is stated that "Aerosol dilution due to the air flow through the two counters (in total: 1.6 L min-1) was taken into account and properly corrected". Does this mean that pressure is held constant and the same amount of clean scrubbed air is supplied to the chamber? This should be stated.

The chamber is designed to ensure that the pressure is kept constant: the same amount of clean air is introduced into the chamber through the input from the HEPA filter.

This statement will be included in the revised manuscript.

line 208 - 209: "nicely reproduced" - please provide a more scientific description – a goodness of fit metric ideally.

The Lai & Nazarov model can of course predict the life-times in a approximative way (but we also must underline that what it is really important to know is the typical lifetime values for particles in a given dimensional range). As a matter of fact, "nicely" here corresponds to a mean discrepancy between calculated and measured lifetimes of about 50%. This figure will be included in the revised text changing the sentence in:

"...nicely reproduced (i.e. the mean discrepancy between measured and calculated values is around 50%)".

Trivial: I believe the Pasteur reference was originally from Annales des sciences naturelles, Zoologie, 4th series (1861), followed by its publication in Annales de chimie et de physique, 3rd series, 64 (1862), not 1890

Citation will be corrected in the revised manuscript.

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