

Interactive comment on “A European-wide ²²²Radon and ²²²Radon progeny comparison study” by Dominik Schmithüsen et al.

Anonymous Referee #3

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General: This paper compares various ²²²Rn activity instruments. The two-filter ANSTRO instruments have a fundamentally different measurement principle than the one filter methods, such as the HRM. Only at full equilibrium between ²¹⁴Po and ²²²Rn, the instruments are expected to give identical results. This study points to calibration difference between the ANSTO and HRM instruments of about 11% (ANSTRO being higher).

The paper is well structured but could better articulate its aim and conclusions in the introduction and abstract. For instance: “here we report on” seems rather a statement from a intercomparison report than a scientific paper. From the abstract the main issues at stake should be immediately clear. Also, a better link with application of the data should be made. Now it seems that validation of models is the main use, while the rest of the paper does not touch upon model activities anymore.

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I have one major point. The disequilibrium remains uncertain. It is claimed that Jacobi and André (1963) provide evidence that equilibrium effects are negligible above 50m a.g.l.. However, this will depend strongly on the atmospheric mixing characteristics. Especially the CBW comparison at 180 / 200 m provides a unique opportunity to sample conditionally based on atmospheric mixing characteristics (e.g. based on the potential temperature gradient along the tower). Under stably stratified conditions one would sample in the free atmosphere at 180 / 200 m, where the equilibrium assumption is safe. For well-mixed conditions this assumption is less certain. Therefore, a strong test would be to separately determine calibration factors for stable and unstable conditions. I do not know how this is related to Porstendörfer (1994), but for CBW unique data for mixing classification are available. Without further analysis I think it is too early to write: “no systematic relation between disequilibrium and meteorological conditions was identified in our data sets” (Page 18).

Anyhow, a simple correction for disequilibrium effects based only on height seems a rather crude approach. Concerning its use as “tracer” to validate atmospheric transport and boundary layer mixing, an option could be to simulate ^{222}Rn progeny in models such that disequilibrium effects are modelled instead of a priori corrected for in a data set.

Minor issues.

Abstract: From the abstract it should be clear why a correction is needed. Different measurement principle? Preliminary $^{214}\text{Po}/^{222}\text{Rn}$ disequilibrium values: this comes out of the blue in the abstract.

Page 2, line 24: it might be nice to mention which instruments are considered to be more accurate.

Page 3, line 4 “It may also occur” unclear what this refers to. ...disequilibrium?

Page 3, line 17: to maximize the number of ^{218}Po progeny ($T_{1/2} = 3$ min)

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collected. . . unclear.

Page 4, line 23: Taking into account the flow rate through the filter, the filter efficiency, and the solid angle of the detector (which depends on the distance of the detector from the filter), enables calculation of the atmospheric ^{214}Po activity concentration.

Rewrite: e.g. from the atmospheric ^{214}Po activity concentration can be calculated, taking into account the flow rate through the filter, the filter efficiency, and the solid angle of the detector (which depends on the distance of the detector from the filter).

Page 8: in both, x and y component, in both the x and y components,

Page 9, line 23: "Owing to the station's elevation, it is rarely reaching the atmospheric boundary layer". Unclear. Probably you want to say that the site will normally sample air from the free troposphere, although this likely depends on the time of day and the season.

Page 13, line 5: here, suddenly "progeny" changes to "progenies". I do not know what is correct, but it should be consistent.

Page 15, line 12: between the two systems; I guess the two ANSTRO systems?

Page 15, line 27: As expected. . . .; please add why this is expected (I guess at 35 m you expect larger disequilibrium effects, but better to articulate this once more).

Page 16, line 16: 4.2 Calibration differences; should be 4.1

Page 18, line 18: (Capuana, 2016) reference missing in list.

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