

Interactive comment on “The determination of highly time resolved and source separated black carbon emission rates using radon as a tracer of atmospheric dynamics” by Asta Gregorič et al.

Anonymous Referee #2

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The manuscript by Gregorič et al. is an attempt to estimate Black Carbon emission rates on high temporal resolution over two contrasting environments. The manuscript presents a unique method of using Radon as a tracer to determine the mixing layer heights. The application of mixing layer height information and Spatio-temporal decay of Black Carbon concentration was further used to calculate the BC emission rates. The authors also compared their results with the BC emission estimates over some other regions of the world. The manuscript provides a valuable substitute for bottom-up approaches of estimating BC emission rates which are having high uncertainties in their activity, emission factors, and technology divisions. Although new, and limited over a few locations only, the pioneer method used in the manuscript could be useful

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for other regions also. I recommend to accept the manuscript after resolving a few issues which are as follows:

1. The introduction section is unusually long with full of irrelevant information. I would suggest the authors modify the introduction section and re-write. Instead of describing the general impacts and roles of BC in the atmosphere (which are widely available in the literature), focus your description on the existing emission estimates, their problems, and the need for the methods which have been described in the manuscript. Two pages would be more than sufficient for the introduction.
2. Section 2.1 is also full of unnecessary information. Page 6, line 13-31: please reduce the content. There is no need to describe the population, growth, and implementation of various plans by the municipality. Please merge the ‘measurement locations’ and ‘geological setting’ together.
3. Page 8, line 3-30, already available in the literature, not needed specifically. Just cite the literature and remove the theoretical information.
4. Page 9, line 12: Please add the full form of FFT in the list of abbreviations in Table 1.
5. Section 2.4: It is recommended to provide a scatterplot of modelled-MLH with GDAS also in supplementary file.
6. Page 14, line 23: Authors mention that the Average Radon activity concentration was similar at both measurement locations, i.e., 15 ± 11 Bqm-3 and 14 ± 10 Bqm-3. At the same time, the authors also mentioned that it was slightly lower in the spring 13 ± 9 Bqm-3 and 12 ± 8 Bqm-3. Consider the standard deviations in the data, I do not see any difference in the data. Authors should check whether these differences are statistically significant or not and add a line on it.
7. Page 15, line 14: Despite significantly higher.25% higher in LJ than in AJ. What is meant by only 25% higher? And how population is a factor here? It looks

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highly ambiguous statement. The authors should consider removing it.

8. Figure 1 should be modified significantly. The background map items are almost invisible.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-911>, 2019.