Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-205-RC2, 2020

Atmospheric Measurement Techniques
Discussions

Interactive comment on "Real-time measurement of radionuclide concentrations and its impact on inverse modeling of ^{106}Ru release in the fall of 2017" *by* Ondřej Tichý et al.

Anonymous Referee #1

Received and published: 17 September 2020

Review of "Real-time measurement of radionuclide concentrations and its impact on inverse modeling of Ru-106 release in the fall of 2017"

General comments

This paper presents an inverse modelling study of airborne Ru-106 detections made in the Czech Republic in the fall of 2017. An existing inverse modeling algorithm, already successfully applied in earlier studies, is used and the results are compared (and found compatible) with other studies that considered the Ru-106 detections. Furthermore, in this study the authors also describe two new detection systems. The motivation for the new detection systems is that they will provide observations with a higher temporal

resolution, which is obtained by reducing the sampling time (CEGAM system) or by measuring during sampling (AMARA system). Finally, the authors consider different data sets to perform the inverse modelling. They conclude that inverse modelling using data with shorter sampling times (thus having a higher temporal resolution) performs equally well or better than inverse modelling using data having sampling times of a few days up to one week. This paper is relevant and results are compatible with previous studies, although I think the conclusions related to the added value of the new measurement systems are not well supported by the results. Furthermore, the chosen case study – although being a very important and interesting case – is likely not well-suited to fully demonstrate the added value of such systems given the large geotemporal scales of the Ru-106 release (having source-receptor distances of thousands of kilometers).

Specific comments

In the abstract, the authors wrote: "Since reasonable temporal resolution of concentration measurements is crucial for proper source term reconstruction, the standard one week sampling interval could be limiting". Although it is sensible that better temporal resolution will lead to better source reconstruction, I'm wondering how important the limiting effect is. The effect is likely case-dependent, and in particular more pronounced for problems with shorter geotemporal scales. In that light, the Ru-106 case might not fully demonstrate the added value of short sampling times. A test with a fictitious source and fictitious measurements would be instructive (one test at scales of a few hunderds of kilometers, and another test at a few thousands of kilometers). The fictitious experiment could demonstrate and quantify the limiting effect of long sampling times in a more controlled way.

The AMARA and CEGAM measurement system descriptions are not clear to me. Specifically: p 5, line 9: "The achieved MDAC for Ru-106 is at a level of 1 mBq/m3 per one-hour integration time and 12 hours of sampling." Does this mean that an activity concentration measurement is available every hour, and that the filter is renewed every 12 hours? And for the CEGAM system, a measurement is available every 4 hours, and the filter is renewed every 4 hours? What is the philosophy of having two different systems, and will both systems be used and maintained in the coming years? p 5, line 8: the reference to Fig 2 is slightly confusing since no 4-hour averaging is applied for the AMARA system?

p 6, line 5: “Unfortunately, the CEGAM system was not yet operational during the Ru-106 incident but we have simulated its output by integrating the AMARA results in a 4 hours window.” Some additional information would be helpful here. If CEGAM pseudo-observations are used based on the AMARA observations, then they would contain the same information? I assume the simulated output is not used for the inversion, but it would be good to confirm this in the text.

Section 2.3: Dataset description: I think it would be good to add a figure or table that summarizes the different datasets (range of observed activity concentrations, number of observations, number of (non-)detections. After consulting the Supplementary Information, I am a bit worried that the differences between data set “RAW” and “FAST” are too small to be significant. Also, why is the integration window set to be between 3 and 13 hours? From Sections 2.2.1 and 2.2.2, I expected that measurements from the AMARA system would be available every hour (and measurements from the CEGAM system every 4 hours)?

p 9 line 5: it would be instructive to get an estimate of the values used for \sigma_{length}ˆ2 in the calculation of the inverse covariance matrix R.

Section 4.1: Atmospheric transport modeling: Numerical weather prediction data, which is used to drive the atmospheric transport model Hysplit, was available every 6 hours. This is likely sufficient for the geotemporal scales of the problem. However, it might not if one wants to explore the added value of measurements with sub-daily sampling periods.

Table 3: Can the authors think of any reason why the release length is significantly different for the four considered locations when using the data sets “WEEKS” and “CUT”,

but not when using the datasets “RAW” and “FAST”? If one does not assume a priori that a short release period is better, Table 3 could be interpreted as if data set “FAST” gives less information regarding the release duration than “WEEKS”, as it is less sensitive to the location. Also, I guess that the regularization will have a larger impact on the release duration than the choice of the data set. From these considerations, I am not convinced that the real-time monitoring data results in a better temporal specification of the release, as stated in Conclusions.

Figure 7 is important for assessing the quality of the inverse modeling results that were obtained using different data sets, by comparing simulated activity concentrations with the IAEA measurements. However, Figure 7 seems to suggest that the temporal resolution of the observations do not really matter for this case. Perhaps other metrics might reveal an improvement from the use of higher temporal resolution, but I doubt that that will be the case for this specific case study (large geotemporal scales and 6-h meteorological data). In the same Figure 7, data set “RAW” performs slightly worse than data set “WEEKS”. Do the authors have an explanation for that? From temporal resolution considerations, I would expect that “FAST” performs equally well or better than “RAW”, and “RAW” equally well or better than “WEEKS”. Also, from Figure 4 and knowing the true source location, I do not see why the results using the “FAST” data set would be better than the results from the other data sets. Concerning the table in Figure 9, I wonder whether other metrics would come to different conclusions (the NMSE, although widely used, is not unbiased, see Poli and Cirillo, 1993). These considerations make it hard for me to agree with the statement made on p 17 line 5.

p 17 line 1: how are the probabilities of the source location calculated? Is it the evidence / marginal likelihood, but normalized so that its sum over the whole domain equals 1?

In Conclusions, the authors wrote: “It is safe to state that the installation of multiple devices such as AMARA and CEGAM over a larger region (ona European scale) would certainly yield additional improvements in source location and in source term estimation in the eventof a radionuclide atmospheric release." There is a trade-off between detector sensitivity and the sampling length (more observations will have a higher minimum detectable concentration). I suggest to briefly discuss this trade-off also in the conclusions. Also, although I agree that there is potential in using observations with higher temporal resolution, I don't think that its added value is clearly demonstrated in this study.

Technical corrections

p 3, line 1: location –> localisation p 10, line 13: "… and run for the period …" –> "… and release particles during the period ..." p 12, line 16: "The estimated source terms are displayed for the RAW dataset using blue lines, for the WEEKS dataset using magenta lines, for the FAST dataset using red lines, and for the CUT dataset using green lines." –> I suggest to omit this sentence at this is already mentioned in the caption of Figure 6.

References:

Poli, A. A., & Cirillo, M. C. (1993). On the use of the normalized mean square error in evaluating dispersion model performance. Atmospheric Environment. Part A. General Topics, 27(15), 2427-2434.

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