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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 #2

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The authors report a new real-time measurement of radionuclides in airborne particles that yields a better temporal resolution of low concentration radionuclide measurement. This improved temporal resolution in turn improves the reliability of the inverse modeling of the Ru-106 event in fall 2017, during a period when the new device was already employed. The authors conclude that if applied in multiple locations across Europe, the possible location of a radionuclide release may be identified and its source term estimated more quickly as the inverse modeling is more reliable. Previous experiences have shown that the modeling of radionuclide transport and dispersion in the atmosphere is often complicated by the low temporal resolution of the measurements. However long sampling periods and spectrum acquisition times are required in order

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Discussion paper

to quantify low activity concentrations hundreds or thousands of kilometers away from the location of the release. The authors present a procedure that delivers spectra for analysis during air sampling, thus allowing the retrieval of the temporal evolution during the sampling period. The modeling part of the study confirms that the improved temporal resolution renders the inverse modeling more accurate. I agree that better temporal resolutions of radionuclide concentrations at very low concentrations are highly desirable for environmental radioactivity monitoring and the authors present a new and promising approach in this direction. I thus support the publication of this study in AMT. However I recommend the following revisions before publication:

Abstract: I suggest that the capabilities of the new device are specified more clearly in the abstract, e.g.: p.1 Line 7: ... gamma-ray counting of aerosol filters and allow us to determine the moment when Ru-106 arrived at the measurement site within XXX minutes and activity concentrations as low as XXX can be detected in 4-hour intervals.

Section 2: Measurement methodology and datasets The descriptions of the new AMARA and CEGAM systems are very short. A reader of "Atmospheric Measurement Techniques" might be interested in a some details about the techniques which are omitted in the manuscript. I recommend revising Sections 2.2.1 and 2.2.2 such that they at minimum answer the following questions: 1) What is the efficiency of the HPGe-detector relative to the 3"-NaI 2) How is the detector cooled (electrically or liquid nitrogen) 3) How stable is the temperature of the Germanium crystal on a hot summer day or a cold winter day? 4) Do variations in relative humidity affect the detector? 5) Is the energy calibration affected by variations in ambient conditions (temperature or humidity) ? Is there an automatic recalibration procedure, e.g. with a reference peak? 6) How was the efficiency calibration performed? 7) Was True Coincidence Summation (TCS) considered as it is for the standard sampling and measurement procedure? 8) What is the interval between the spectra in the case of AMARA? Signal Treatment: 1) How accurately can the time of arrival be determined (see my suggestion "within XXX minutes" for the abstract) 2) How was the AMARA reconstruction (black line in

Figure 2B) performed? What are the corresponding time intervals and uncertainties? Are uncertainties of one interval affected by the deposition of Ru-106 during previous intervals? Further, it is obvious from figure 2 that the plume continued for longer than is on display here. Why is the remaining part not shown? Was it also split into 4-hour CEGAM intervals for inverse modeling runs?

Section 5: Conclusions This section almost reads as if the authors have carried out a pure modeling study. In its current form it does little justice to the technical progress that they achieved and which justifies publication in a technical journal. I suggest that the authors provide a brief summary of the new measurement device and its advantages in this section.

Other minor issues: p. 1, Line 24: “several hundred TBq”: This needs one or more references where the source term is estimated. p. 2, Line 9: I suggest rewriting this sentence along the lines of: “Since medical sources and RTG would neither explain the occurrence of Ru-106 nor the large source of several hundred TBq, fresh nuclear fuel is the most likely candidate. “ P4. Figure 2 (A): The y-scale is missing. The reader needs to know how many keVs are displayed. P.5 Line 7: (Hyza and Rulik, 2017) should be Hyza and Rulik (2017) P.5 Line 9: it should be mentioned that the MDAC worsens for one particular interval if some Ru-106 was already deposited in a preceding interval p.6 Line 27: I propose “location of the release” instead of “location of the source of the release” p.9 Line 11: (Tichy et al., 2016) should be Tichy et al. (2016) p.10 Line 28: I suggest “During the period in question” instead of “In the assumed period” p.12 Line 10: I suggest “A release at location 4 in southern Romania would contradict ground-based observations to the east of the location was thus also rejected (see Masson et al., 2019).” p.12 Line 16: The colour code is already described in the caption of Figure 6 and can be omitted here.

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