Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-205-AC1, 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.

Ondřej Tichý et al.

otichy@utia.cas.cz

Received and published: 1 December 2020

We would like to thank you for providing us a detailed review of our manuscript. We are glad that we can submit a revision of our paper. In the following text, we will respond to all comments.

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 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:

Specific comments:

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.

***Authors response*:** Thank you for this suggestion, we have modified the abstract accordingly.

***Changes made in the paper*:** We added the sentence to the abstract.

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?

***Authors response*:** We acknowledged that more details on measuring systems presented in the paper are need. We extended the description of the AMARA/CEGAM systems significantly and with respect to the reviewer comment. All important parameters are now summarized in Table 1 in the paper. We also added discussion on signal processing.

The activity uncertainty is indeed affected by the previous deposition, we have included this into a text together with mentioning also the influence of radon background.

Regarding the displayed data - the end on the time axis corresponds to the filter change. We have included this info in the figure caption. Consequent samples were taken with a higher frequency and the ruthenium activity decreased, therefore the real time measurement provided less and less useful information. 4-hours intervals were computed only for the first sampling interval capturing the arrival of contamination.

***Changes made in the paper*:** Section 2 was extended on Section 2.3 to provide all above mentioned information. Also, the table with important parameters on AMARA and CEGAM systems was added to the paper. Caption of Fig. 2 was expanded to provide further information.

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.

***Authors response*:** Indeed, we did not intend to focus mostly on modeling in conclusion, which we, however, did. Thank you, we extended significantly the first paragraph to give more attention to the introduced modeling systems.

***Changes made in the paper*:** The first paragraph of the conclusion is extended significantly.

Minor issues:

p. 1, Line 24: "several hundred TBq": This needs one or more references where the source term is estimated.

***Authors response*:** Although these findings are referenced later, we agree that they should be referenced also here and we added relevant references.

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. "

***Authors response*:** The sentence was rewritten accordingly.

P4. Figure 2 (A): The y-scale is missing. The reader needs to know how many keVs are displayed.

***Authors response*:** Agree. The ROI width was specified in the figure caption.

P.5 Line 7: (Hyza and Rulik, 2017) should be Hyza and Rulik (2017)

***Authors response*:** Corrected.

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

***Authors response*:** Agree. The brief discussion of possible influences on MDAC are mentioned in newly extended section 2.

p.6 Line 27: I propose "location of the release" instead of "location of the source of the release"

***Authors response*:** Thank you, we agree.

p.9 Line 11: (Tichy et al., 2016) should be Tichy et al. (2016)

***Authors response*:** Corrected.

p.10 Line 28: I suggest "During the period in question" instead of "In the assumed period"

***Authors response*:** We agree with this suggestion.

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)."

***Authors response*:** We reformulated this accordingly.

p.12 Line 16: The colour code is already described in the caption of Figure 6 and can be omitted here.

***Authors response*:** We removed the color code description from here.

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