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Geoscientific Model Development
Discussions

Interactive comment on "On the model uncertainties in Bayesian source reconstruction using the emission inverse modelling system FREARtool v1.0 and the Lagrangian transport and dispersion model Flexpart v9.0.2" *by* Pieter De Meutter et al.

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This paper addresses a question of utmost importance, namely is it possible to determine (better than it is done today) the uncertainties associated with the features of an atmospheric emission? The features of the emission encompass together the release rate, duration and location. This issue is also called "source term estimate" and its applications are numerous especially when a serious nuclear or radiological accident

or the verification of the compliance with the Comprehensive nuclear Test Ban Treaty are at stakes.

In the method developed by the authors, the source term estimate basically uses environmental observations and the FLEXPART atmospheric transport and dispersion model run in backward mode with ECMWF numerical weather predictions as input. Furthermore, this work relies on the Bayesian source reconstruction. Uninformative bounded uniform priors are used to characterize the emission while a general purpose Markov Chain Monte Carlo algorithm, called MT-DREAM(ZS), is used to sample the posterior distribution.

The Bayesian approach has the advantage to providing a probabilistic description of the source term parameters. Thus, the specification of the observation error and of the model error is mandatory, what is the beginning of great difficulties as there is no straightforward way to find out the error of an atmospheric transport and dispersion model. In this regard, the authors use a ensemble of numerical weather predictions in order to create an ensemble of atmospheric transport and dispersion simulations. Then, they propose different methods to infer the model error using these simulations in particular.

The application is carried out for the Ru-106 detections that occurred in September and October 2017 in Eastern Europe and Russia. As pointed out by the authors, their aim is not to identify the release location what has been presumably done in numerous other papers, but to evaluate and compare methods using an ensemble of weather predictions to account for uncertainties in transport and dispersion simulations, and subsequently, source term estimate.

The paper is very interesting, well structured and well written with very few typos. The results obtained by the authors are credible and it is very likely that these results are correct. The comments by the authors are scientifically sound, relevant and mostly convincing.

However, I have some remarks and questions listed hereafter for the authors.

L 21 – What do the the authors call "anomalous radionuclide detections"? That is something I am perfectly aware of, but it is perhaps not the case of all readers.

L 25 – According to the authors, atmospheric transport and dispersion modelling is "one of the methods" to relate detections and the source of emission. I do not see other methods. Which other methods do the authors have in mind?

L 30 – In backward modelling, the source-receptor relationships are calculated from fixed receptors to potential sources (not the opposite as written in the sentence in L 30).

L 32 – The concept of "non-detection" should be explained (or ignored as it is not used in the paper).

L 44 – In this paper, the model error is considered as a whole. Thus, it does not originate only from the numerical weather predictions, but also from the atmospheric transport and dispersion model. The word "mainly" ("because of the underlying weather prediction data") is questionable. The authors should consider rephrasing the sentence.

L 54 – As for me, it is difficult to create and use a relevant ensemble. The reason is not only (and perhaps not mainly) the computational cost of the ensemble, but the way to constitute it with enough variety, limited redundancy, etc. This complex task should be mentioned in the paper.

L 57 – Ditto. It is complicated and not guaranteed that an ensemble captures "most of the possible outcomes". This should be indicated in the paper.

L 59 – What is a "measurement model"?

L 88 – The description of the detections should be gathered in a table with the collection start and stop times (even if I guess that the authors do not wish to develop this aspect

of the data).

L 96 – The beginning of the sentence is "the above observation times". I do not see any observation times above?

L 101 – It is written that FLEXPART is run in backward mode. I wonder how long the simulations go back in time. Could the authors give information about this?

L 110 – It is not obvious that adding and substracting perturbations from an ensemble mean are a legitimate process. Could the authors comment on this?

L 113 – The authors assert that "the spread between the different members represent the uncertainty". This is undoubtedly a way to account for uncertainty in weather predictions, but are the authors sure that the ensemble perfectly encompasses the uncertainty on the meteorological data? The authors should consider being more cautious and rephrasing this sentence.

L 130 – What are the values of t1 and tm, the first and last time for which source-receptor-sensitivities are available for the source reconstruction?

L 131 – The authors assume that the release rate is constant during the release period. I would like to point out that this is a strong assumption as in principle, the release is not known at all. Could the authors comment on this?

L 138 – The total release is assumed to be between 10**10 and 10**16 Bq. This seems to me somewhat arbitrary as it excludes potential releases respectively further downwind and further upwind. Once more, how to proceed when no preconceived solution is available? Could the authors consider commenting on this point?

L 141 – Ditto. How did the authors choose the time interval of the release (all the more that this time interval is quite short)?

L 148 – This is another strong hypothesis that the observations are independent while there is likely a space and time dependency between them. Could the authors comment on this?

L 160 – Does the index "i" in formula (5) indicate that there are as many applications of this formula (with possibly different values of the s, alpha bar and beta bar parameters) as the number of observations?

L 189 – I wonder if the general-purpose Markov Chain Monte Carlo algorithm MT-DREAM(ZS) is freely available? Who developed this MCMC method?

Figure 2 – I suppose that "MDC" stands for "Minimum Detectable Concentration" and that we have LC # MDC / 2. In the formulae, it seems that only LC is used. Could the authors confirm this point?

L 192 – While popular, MCMC methods have well-known drawbacks like the burn-in period or convergence problems. Could the authors consider commenting on this with respect to the MT-DREAM(ZS) algorithm?

L 197 – I have the feeling that all technical details in the last part of this paragraph (and notably the "snooker step") would need some more explanations as this part of the text is too concise (and a bit obscure).

L 209 – It is written here that "s" is an estimate of "sigma", but "sigma" is not defined, nor introduced before. Should the reader understand that sigma stands for sigma_mod?

L 215 – In formula (17), "sigma_srs" and "srs" are not defined. What do these notations stand for? Moreover, what is the reason for the multiplicative value of 16 (and not another value) in the same formula? Could the authors comment on this?

L 216 – The sentence: "as a consequence, the model uncertainty does not depend on the source parameters" is especially unclear or unprecise. What do the authors call "the model"? Is it the weather prediction or the transport and dispersion simulation or both? As the source parameters are not considered as uncertain, I do not see why and how they should take part in the model uncertainty. Please, consider rephrase this sentence.

L 218 – I wonder how "a part of the plume" can be "subject to more atmospheric transport and dispersion processes". All parts of the plume are subject to atmospheric transport and dispersion processes. Small detections may be obtained at the "edge" of the plume or just far from the source of the release. What does a "small" detection mean? It is just a matter of detection method and device. While I globally agree with the ideas contained in this paragraph, I feel that they should be formulated in a different way.

L 226 – The whole section 4 uses the ECMWF unperturbed weather prediction. This should be mentioned at the beginning of the section.

L 229 – As I understand "s_i" includes the model error and the observation error. I wonder what the respective parts of each kind of errors are. Could the authors comment on this? The authors present the source location probability map for three values of "s_i". Of course, it is difficult to choose this parameter and it is the central question which the paper deals with. Is it possible for the authors to motivate the choice of the three "s_i" values? Finally, it is written that "the same value s_i is used for all observations". I wonder why different values of s_i should be associated to the observations as the observation error is by assumption the same for each observation and the model error should depend intrinsically on the model and not on the observation.

Figure 3 – The figure 3 as the following figures seem to me a bit small.

L 237 – I do not see what is an "unknown error"? There are observation errors, representativeness errors or model errors including among others the atmospheric processes not resolved by the model. What is "unknown" is not the type of error, but the value to be attributed to the error.

L 270 – Increasing the value of the parameter s_i results in a shift and an enlargement of the posterior distribution. I wonder why introducing multiplier only results in a shift of the posterior. I suppose that it acts as another way to adjust the posterior without any increase in the level of model uncertainty. Could the authors comment on this?

L 272 – I presume that forcing the model uncertainty with a high value of the parameter s_i predominates against the influence of the multipliers. Do the authors have the same explanation?

L 281 – As for me, it is not so obvious that the errors arising from the meteorological input data have the “largest contribution” to the total model error. Would the atmospheric transport and dispersion model be a “bad model” (what is probably not the case of FLEXPART), the dispersion model error would not be negligible. The authors should perhaps moderate their assessment in L 281.

L 285 – How the data of all grid boxes is aggregated should be more explained. For me, it is not an obvious process.

L 298 – The probability density function of the SRS members should be presented not only for “an arbitrary observation and an arbitrary time” as in Figure 4, but for other observations and times or all distributions should be considered and their moments computed.

Figure 4 – There is a typo in the caption: “distributed” versus “distribution”.

L 321 – I wonder about the generality of the method presented by the authors, especially in case 4 when the parameters are fitted for each observation and time. As a matter of fact, it means that just adding or removing a detection will not only influence the source term estimate, but also the uncertainty on this estimate (and this with the same meteorological fields). Could the authors comment on this?

L 347 – Considering “observation-specific” uncertainty parameters is an ad hoc (and interesting) way to fit the model (and observation) error, but it should not be forgotten that the model error should be an intrinsic feature of the model and not depend on the set of observations which is taken into account. I suggest that the authors argue on this.

L 350 – That the model uncertainty grows when going backwards in time is somewhat

trivial. At least, the contrary would be surprising.

L 353 – It is worth noticing that the oscillations have a circadian period. Is it possible to relate them with the day and night alternation of the boundary layer?

L 365 – It is quite optimistic to assert that both maps in Figure 7 roughly agree. There are many differences. Would the location of the release be the aim of the study, the authors would be certainly quite embarrassed to designate it using one map or the other.

L 390 – I would like to point out that there is an interesting result in L 390. As a matter of fact, using the ensemble only to fit the uncertainty parameters or running all members of the ensemble to figure out the uncertainty seems to be equivalent.

L 410 – As a conclusion, I would suggest to the authors to apply the different approaches and methods presented in their paper to situations in which the source characteristics (especially the location) is known unambiguously (because in the Ru-106 case the source location was not really recognized). In a situation with a clearly identified location of the emission, it would be interesting to see what results (good or less good) are obtained using the inference in different ways, and also what is the most efficient approach.

L 435 – As argued by the authors, it seems that using the members of an ensemble in the source term estimate gives more robust results with regard to the choice of the uncertainty parameter as opposed to not using any ensemble. It seems to me quite logical as the ensemble introduces a kind of uncertainty (which is certainly not all the uncertainty, but a “rigorously built” uncertainty). This uncertainty may predominate against the uncertainty arbitrarily fixed by choosing the uncertainty parameter.

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