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Interactive comment on "Bayesian inverse modeling and source location of an unintended I-131 release in Europe in the fall of 2011" *by* Ondřej Tichý et al.

Anonymous Referee #1

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1 Overview:

Review of "*Bayesian inverse modeling and source location of an unintended I-131 release in Europe in the fall of 2011*" by Tichy *et al.*

Tichy *et al.* present an analysis of the I-131 release from the Institute of Isotopes in Hungary during the Fall of 2011. They use a sparse set of atmospheric observations and attempt to infer the source location and magnitude. The paper is reasonably well written but the figures could use quite a bit of improvement. In terms of content, the authors have currently not evaluated the meteorology used for this work which strikes this reviewer as a major short-coming. I would recommend major revisions for this

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Discussion paper

manuscript.

2 Major comments:

2.1 Evaluation of the meteorology

The largest concern I have with this manuscript is the lack of meteorological evaluation. The authors use two different particle dispersion models (FLEXPART & HySplit) but both are driven by the same meteorology (GFS at $0.5^\circ \times 0.5^\circ$ resolution). Obviously, if the meteorology is incorrect then the authors will obtain erroneous results. Surely there are observations of windspeed and direction the authors could use to evaluate . Further, the authors assume that all of the I-131 was release in particulate form and, as such, will undergo both dry and wet deposition. How well does GFS simulate the clouds and precipitation? It seems that this would be crucial to accurately representing the dry and wet deposition. Particle dispersion models are strongly dependent on the accurate simulation of the mixed layer heights because this typically dictates the sensitivity of the particles to surface fluxes, how well does GFS simulate mixed layer heights? A systematic bias in mixed layer heights could easily lead to SRS matrices that are too sensitive (or too weak), this would then manifest itself in a bias in the source magnitude.

2.2 Choice of Inversion Method?

As I mentioned in my pre-review, the authors have intelligently framed the problem by examining just a single source and repeating the analysis for an ensemble of potential sources. However, it's unclear to this reviewer why they need to: (a) use Bayesian Model Selection (see Minor Comment #1) and (b) they they need to use LS-APC. Their system of equations is very small (117 $\times$ 91) and could be solved on a laptop, so why don't they use a more flexible framework like a hierarchical Bayesian with an MCMC, rjMCMC, or CMA-ES?

Additionally, do their results provide an error estimate? It seems like many of their reported numbers are missing error bars which makes it difficult to evaluate. A prime example of this is in the abstract when comparing the reported emission (342 GBq) and their derived emission (490 GBq), if the error bars are large then these numbers may be statistically indistinguishable.

3 Minor comments:

3.1 Bayesian Model Selection

Section 3.2 is confusing and it's not clear what the reader is supposed to gain from this section.

They introduce a new variable ($\mathcal{M}$) then do not use it. They introduce an equation (Eq. 10) with 2 terms and then simplify it to just $\exp(\mathcal{L}_{M_i})$. The authors then introduce a complicated set of equation (Eq. 11) but do not explain the variables or terms (they point the reader to the supplement where they, again, do not explain the variables or terms).

It's unclear to this reviewer why the authors need to use this variational lower bound when they should have posterior probabilities (that was supposed to be the motivation for using this LS-APC). Their system of equations is very small (117 $\times$ 91), why can't the authors just solve for the source magnitude at each potential location and compare the probabilities of those solutions? It seems that that would greatly simplify the problem.

3.2 Figures

None of the figures are particularly well done. Most labels are small and difficult to read. The spatial maps are particularly difficult to read. Red and green are very hard to pick out on a dark gray background. Most of the panels in the line plots and scatter plots could be combined into single panels, this would greatly facilitate comparison between the sensitivity studies.

3.3 Discussion of Previous Work

The authors have omitted to mention a range of other studies that have used made major strides to address similar problems (e.g., objectively determining hyper-parameters and using particle dispersion models to estimate sources). At the bare minimum, most of the citations in the inverse modeling section should include “e.g.,”. There are, quite literally, textbooks written about most of this work. An example of this is their Wotowa et al. citation in the first paragraph of Section 3. This is a review paper that cites hundreds of other papers for that particular application of particle dispersion models to estimate sources (which is already a small subset of the literature on this work). Omitting the “e.g.,” is misleading.

4 Specific comments:

Page 4, Line ~30: How long is the sampling time? It seems that this could be very problematic.