Short comment: I.Kovalets

1-It is clear that gamma dose rate measurements are most important for operational real-time source inversion. Therefore it is not clear how to choose automatically (in the real-time) the regularization parameters (1, 2). The hyperparameter estimation method which is cited in the paper may be not suitable in a real-time since according to Winiarek et al. (2012), p. 2949 "the result of this estimation is very sensitive to the context and is also computationally demanding".

The sensitivity study that we carried out on the Fukushima application showed that:
- The choice of the $\lambda 1$ value has a limited impact on the identification of the potential release periods and on their number (see table 1).
- The choice of the $\lambda 2$ value has an even more limited impact on the release assessment and we used a very low value. Several reasons can explain the minimal sensitivity to the $\lambda 2$ value. First, many observations are used and the use of a regularization term is less critical. Second, the barrier function limits the solution space and reduces the requirement for a regularization term. Third, the initial source term set to zero contributes indirectly to regularize the inverse problem.

For an operational application in the event of any future accident situation, a preliminary rough source term could be very quickly assessed by using low values for $\lambda 1$ and $\lambda 2$ assuming the number of observations would be sufficient. At the same time, accurate regularization parameters could be determined with the hyperparameter estimation method proposed by Winiarek et al..

2-I believe the authors should say some more about particular dose rate calculation method which they used. For instance in the work of Tsiouri et al. (2012) results of source inversion appeared to be sensitive to particular dose rate calculation method (see discussion on p. 39 of the quoted work).

The dose rate calculation method we used is simplified. It is based on dose coefficients estimated following the hypothesis of a semi-infinite contamination. Such hypothesis is too simplified to correctly estimate the near field dose rate and explain the high sensitivity mentioned by Tsiouri et al. (2012). Given the spatial resolution of our simulations, the hypothesis is, by contrast, entirely realistic.

3-As far as I understood from the paper the release height had been uniformly distributed through the first 2 model layers (between 0 and 160 m AGL). Were the model results sensitive to this assumption?

The presented results are obtained considering a release distributed uniformly through the first two model layers. The release height has been chosen following the analysis of the release events proposed by Katata et al. (2012) and Korsakissok et al. (2013). The release events are considered happening at 20 m (reactor pressure vessel), 120

m (exhaust stack) for specific venting actions. For the explosions, the source could have been diluted by the heat and momentum of the explosion and Katata et al. and Korsakissok et al. assumed a dilution within 100 m and 300m on the vertical.

Besides, source term assessment had been carried out by assuming a release uniformly distributed through the first three model layers (between 0 and 280 m). Results show that:

- The potential release periods remain unchanged;
- For most of the release events, the assessed release rates are of the same order of magnitude;
- For the uncertain reconstructed release events, when the assessment is based on only one or two measurements stations, release rates can significantly vary depending on the supposed release height. For example, the release rates assessed for event 1 and 2 (see Table 4) significantly differ depending on which release height was considered. They are a factor 2 higher when the release is distributed between 0 and 280 m.

4-It is stated in the paper that "the number of constraints must be at least equal to the number of radionuclides to be used for the evaluation of the release rate". May be there is small ambiguity and the number of constraints should be by 1 less than the number of radionuclides since when there is only 1 radionuclide there are not constraints (on radionuclide ratios)?

The correction has been done.

5-There is very large uncertainty (5 orders) in ratio of the released Xe-133 to the released Cs134 (equation 11). Isn't this explained by the mentioned in paper volatility of noble gases? If this explanation is true don't authors think that it could be possible to use time-dependant restrictions on radionuclide ratios in such case (i.e., the more time after release happens the less could be upper bound of the noble gases)?

There is very few Xe-133 measurements in Japan all located south of the FD-NPP. They show that the Xe-133/Cs-134 ratio vary considerably during the emissions. For example, on March 15, Xe-133 is the strongest contributing radionuclide to dose rate observed in Chiba. Those observed values size the upper bound of the Xe-133/Cs-134 ratio. After March 16, it is likely that most of Xe-133 content in the initial inventory has been released. Still Xe-133 was observed in Chiba after this date. Those observed values size the lower bound of the Xe-133/Cs-134 ratio. The large range of value considered for Xe-133/Cs-134 ratio is not due to uncertainties but to the large fluctuation of the Xe-133 releases. Obtained results considering such soft constraint proved to be satisfactory and realistic, which is very much in favor of the proposed method and show that the problem is sufficiently constrain.

The number of Xe-133 measurements was insufficient to reliably estimate time-dependent restrictions on Xe-133/Cs-134 but was done for the other radionuclides. The time-dependent ratios for I-131/Cs-134, Te-132/Cs-134 and Cs-136/Cs-134 were assessed using air concentrations measurements and taking into account the radioactive decay assuming that each radionuclide ratio follows an exponential decay. The resulting source term was found to be similar to the presented source term (i.e. identical total amount released and similar temporal

evolution of the release rates). Results demonstrate that the important number of dose rate observations taken into account in the inversion process allows using soft constraints on isotopic ratios and that the algorithm is reasonably efficient to assess the isotopic composition of the release.

6-Also in the same paragraph the meaning of the following sentence is not clear: "This result suggests that it is sufficient to reduce the dimensions of the inverse problem, thereby reducing the influence of the regularization term". Wasn't the dimension of the inverse problem already reduced at Step 2 (previous subsection)? What is meant by reducing the dimension of inverse problem here?

The sentence has been changed using the arguments given to answer your question n°1.

7-It is difficult to agree (par. 2 at p. 15574 and in other places) that $\sigma b = 0$ means 'no prior knowledge of the source term'. It is more logically to consider that no prior knowledge of the source term is equivalent to the absence of the regularization term (or what is the same - to infinite diagonal values in matrix B) while $\sigma b = 0$ means presence of prior source term having zero value. This mixture of terminology could be misleading.

The referee is right; we used the deviation from zero to stabilize the solution and our argument to make no use of an a priori source term is not strictly correct. We should have say that we do not need to use a specific a priori source term because the dose rate network is sufficiently dense and well distributed to reconstruct most of the releases events. The text has been changed.