

Interactive comment on “Comparison of four inverse modelling systems applied to the estimation of HFC-125, HFC-134a and SF₆ emissions over Europe” by Dominik Brunner et al.

Anonymous Referee #1

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This is a clearly written, informative, and useful paper that adds significantly to our understanding of regional emissions of long-lived trace gases derived from atmospheric data. I had only a few thoughts on clarifications and adjustments to improve the paper:

On the gridded emissions. Some issues could be addressed to make things more robust and clear. It is indeed striking the visual differences in different approaches to gridding the EDGAR emissions in Figure 5. It would have helped me if you had mentioned in this section the spatial distribution of the native EDGAR inventory estimates and, how consistent country totals are after this gridding by the different methods (shown in Figure 11). Given the rather significant and arbitrary variations in the priors, a discussion of emission updates (Figures 6-8) becomes one that is related to two factors: the

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arbitrary errors in the priors because of the imperfect gridding process, and differences in model performance. At this point in the text only the second influence is considered, though it seems necessary to consider how the first factor is influencing the results too. (In other words, if all the models performed exactly the same in their inversion, there would still be substantially different updates apparent in Figure 6-8 because of the different gridding errors associated with the prior.) The better discussion of these issues comes later in the text in the comparison of figures 11 and 12, in my opinion. The authors might consider shortening or revising this earlier section.

Regarding the apparent large differences in adjustments by the different models despite the reasonable similarity in posterior mole fraction time series generated by these models: It would seem that these aren't directly relatable unless you consider the sum of the fluxes shown in Figures 5 and 6, given that the posterior mixing ratios are from the sum of the prior emissions plus adjustments. Given the large apparent differences in the priors because of the different gridding approaches, this seems important to consider.

On background levels. Since the approach for deriving background mole fractions taken by NILU is unique because it involves a subtraction related to the calculated influence of regional emissions on the observations deemed to represent background, it would seem reasonable to suggest that this subtraction might be causing the lower background mole fractions it derives. Is it not fairly easy to determine if this is the source of the offset?

Another minor issue, with regard to backgrounds for the approaches by EMPA2. The REBS approach is mentioned and an optimization is also indicated. Details about the optimization are lacking. Was the optimization applied to the REBS results? And how was that process constrained? Does the text mentioning that “the background is then allowed to evolve slowly with time” mean that it was just another optimized parameter in the inversion who's only constraint was low-frequency variation?

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On section 3.3., uncertainty reductions. The authors seem to succeed in showing evidence refuting the initial statement that this is "a useful diagnostic" since the magnitudes seem primarily dependent on what is assumed as the uncertainty on the prior! In looking for robust conclusions from this section, there is one that I struggle to reconcile: How can uncertainty reductions expressed relative to absolute emission magnitudes be larger for those regions with higher emissions? Some explanation would be helpful here, since it seems not an expected or straightforward conclusion.

Details: Sentence two of abstract, consider adding a word: "but *emissions* have large GWPs and are, therefore..." Also, in the abstract the discrepancy in HFC-125 emissions estimated for the Iberian peninsula is the first point made in the comparison of results vs the UNFCCC inventory emissions, yet the main text mentions that "emissions from the Iberian countries are not well constrained by the current observation network." Some modifications to the abstract seem necessary.

Define "standard deviation (normalized)" in the caption of the figure showing Taylor diagrams. I presume it is the ratio between the observed vs posterior calculated mole fractions—this should be mentioned if true. Any de-trending applied to the results over the year, or is it just the s.d. of the annual data record considered together?

Figure 1 caption, mention that the reduced grid is only associated with the EMPA simulations, if true.

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