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Geoscientific Model Development Discussions
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Interactive comment on "A sparse reconstruction method for the estimation of multiresolution emission fields via atmospheric inversion" *by* J. Ray et al.

Anonymous Referee #1

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The authors present a new method for inverse modeling that leverages sparse reconstruction. This new method is intriguing and is developed in a thoughtful way by the authors. I think this paper will make a great addition to the field of inverse modeling. I highly recommend this paper for publication in GMD. Below, I have listed several suggestions for the authors to consider as they revise the paper for publication in GMD.

Overarching comments

1. Accessibility

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Interactive Discussion
Discussion Paper

Much of the paper uses very technical wording, and I worry that much of this phrasing may not be very accessible to atmospheric scientists. Most existing atmospheric inversion studies use a single framework for inverse modeling – Eq. 1 listed in this article. Additional frameworks, like the one presented in this article, could be incredibly useful. However, I suspect that most atmospheric scientists will be unfamiliar with this type of sparse reconstruction in the same way that most are familiar with Eq. 1. I might focus on making this article more accessible to that audience. The authors could do this in a number of ways: (1) by removing technical phrases or terms of art that are not strictly necessary, (2) by providing more descriptive explanation of some of the methods, or (3) by more explicitly guiding the reader through some of the equations.

In particular, I might focus on re-wording the abstract and introduction in a less technical way – in a manner that provides more physical intuition for a reader who may not be familiar with this type of sparse reconstruction method. This re-wording would help broaden the article's appeal to a wider audience and will clearly motivate the subsequent sections that, by necessity, are more technical. To this end, I might focus on giving the reader a holistic, descriptive overview of why under-constrained problems can be challenging, what sparse reconstruction methods are, and how those methods can provide an attractive solution.

2. Choice of synthetic data study

I am somewhat concerned about the choice of synthetic data study. A recent paper by Shiga et al. (2014, doi:10.1002/2014GL059684) indicates that existing atmospheric measurements have difficulty identifying ffCO_2 fluxes above biospheric fluxes. As a result, I wonder if ffCO_2 is necessarily the best species for a synthetic case study. In the real world, these emissions are often obscured by fluxes from the biosphere. The authors might instead want to consider a gas with both natural and anthropogenic emissions that are largely non-negative. For example, methane, nitrous oxide, or one of several fluorinated greenhouse gases could make for a good synthetic case study.

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I think this issue may actually be cursory to the central objective of the paper – to present a new inversion method. However, it may nonetheless distract the reader or detract from the perceived applicability of the method.

Detailed comments

1. The beginning of the abstract is somewhat technical and may be challenging for the reader to follow. For example, the terms "wavelt-based random field models" and "non-rectangular geometries" may not be familiar to the reader. The authors could instead open with a non-technical sentence that communicates how this sparse reconstruction scheme is advantageous or how it represents an advancement.
2. 5625, Lines 6-8: Would it be possible to cite a reference that illustrates an example of this?
3. 5625, Lines 16-17: Could one theoretically use a model selection method (like AIC, BIC, or DIC) to decide whether a parameterization is too simple or too complex?
4. 5625, Lines 25-28: These examples add a lot of extra technical detail to the description of sparse reconstruction methods. I wonder if this level of detail is necessary when giving the reader a broad, holistic definition of sparse reconstruction.
5. 5626, Lines 2-6: What is an l_1 and l_2 norm? Furthermore, what is the "offline construction of a spatial parameterization" and why would we want to dispense of it?
6. 5626, Lines 19-20: What do you mean by "choice of the proxy used for spatial disaggregation"?

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7. 5627, Lines 12-13: What is a wavelet-based random field model?

8. 5627, Line 22-23: What kind of spatial parameterization does this paper develop? I.e., it is not entirely clear what the term "spatial parameterization" refers to here.

9. 5629, Line 4-11: How different are EDGAR and Vulcan? This difference would help the reader understand whether the estimated emissions shown in Fig. 1 more closely resemble the prior (EDGAR) or the true fluxes (Vulcan). The authors may want to consider adding a plot of EDGAR fluxes to Fig. 1.

10. 5630: It may be useful to the reader to define the dimensions of each matrix or vector.

11. 5630, Line 26: What are orthogonal bases with compact support? Some readers may understand this term, but I worry that many atmospheric scientists may not fully understand this technical term.

12. 5631, Eq. 3: It might be useful to the reader to explain in words what the components of this equation mean. I.e., it may be helpful to guide the reader through this equation. The current text does not provide much explanation of what this equation means. In addition, does μ refer to the mean here?

13. 5631: Are "incoherence" and "mutual coherence" the same thing? It may be useful to explain the relationship of these terms to the reader.

14. 5632, line 11: What is the l_0 norm of $\vec{w}$ and what is the l_2 norm of the measurement-model discrepancy? Do they refer to the "1" and "2" subscripts in Eq. 4? If so, it may be useful to clarify here.

15. 5635, Eq. 7: Is there any way to guide the reader through this equation? I know that this equation is, to some degree, an extension of Eq. 6, but I worry that the authors may lose the reader here.

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16. 5637, lines 3-6: What is the ultimate effect of fine versus coarser scale wavelets? I.e., what effect would these differences have on the estimated fluxes or what desirable quality would these properties confer?

17. 5637 Approach C: Could you potentially describe in more qualitative terms how this approach differs from B?

18. 5638 Eq. 11: Have these variables already been defined elsewhere in the manuscript? I may have missed this definition. If not, it would be helpful to explicitly define these variables for the reader (or explicitly state how they relate to the equations in Approaches A-C).

19. section 4.1: The authors do a great job of leading the reader through this section in a structured and informative way.

20. conclusions: The conclusions section is well-written. The authors are adept at summarizing their method and its advantages in a way that is likely to be accessible to many readers.

21. 5648, lines 3-8: The authors may want to remind the reader which sections discuss each step. The reader may not remember each step exactly, and a reference to individual sections would help the reader jump to this information quickly.

22. 5648, line 11: How would a Kalman filter rectify this problem? I would either clarify this logic or omit the statement.

23. 5648, lines 17-20: I think this Matlab code will make the inversion method much more accessible to most readers.

24. Figure 1: I might set zero values to white or use a color scale that uses light colors or shades for low emissions values. This could make the figure more visually appealing and easy to see.

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25. Figure 5: It could be more informative to list actual dates on the x-axis in panel a. The current label ("observation number") is not very informative to the reader.

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