

## ***Interactive comment on “Diagnostic methods for atmospheric inversions of long-lived greenhouse gases” by Anna M. Michalak et al.***

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### Response to Referee #1

We thank the Referee for their constructive input. We have structured our response using the following sequence, per instructions: (1) comments from Referee, (2) author response, (3) changes in manuscript.

COMMENT FROM REFEREE: The manuscript provides an interesting overview of the existing diagnostics to evaluate atmospheric inversions of long-lived tracers. The paper doesn't introduce any novelty in the field, but rather, it establishes a list of the existing tools. It is well written, and there is no obvious “wrong” point to comment on. I was quite pleased with Sections 1 and 2, which are a nice introduction to the topic, for non-specialists. I was unfortunately less convinced by Sections 3 and 4: although they are

well written as well, I wonder what kind of reader would actually learn from it. Inverse modeling specialists are already familiar with the concepts that are presented; Non-specialists will get an idea of the diagnostics tools available, but since the paper often doesn't go much beyond listing them, they will have to read the (many) references to actually understand them.

**AUTHOR RESPONSE:** We thank the referee for recognizing the value and intent of the work. With regard to Sections 3 and 4, our goal is two-fold. Although we agree that inverse modeling specialists will already be familiar with some (or many) of the concepts presented, we doubt that any specialist would be familiar with the full spectrum of approaches presented here, unless they themselves had conducted a full review of the literature. Speaking on behalf of the three specialists who authored this manuscript, although each of us was familiar with many of the approaches we describe, we each also learned about some that were new to us. For non-specialists, we agree that they would need to read additional references in order to get a deeper understanding of a particular approach. We consider this a strength rather than a weakness of the work. In essence, in Section 3 we are providing a guided tour of the literature, which would allow a non-specialist to know exactly where to go for a more in depth presentation of any particular approach.

**CHANGES IN MANUSCRIPT:** In the revised manuscript, we will more clearly enunciate the dual target audience for this review in Section 1. We will also implement changes to put the approaches within a clearer context, as described in more detail in other responses below.

**COMMENT FROM REFEREE:** As an example, in Section 3.1.1 (the first in which some diagnostic tools are actually presented and discussed), in 19 lines, the authors talk about: evaluation inversions against observations left out of the inversions; evaluation inversions against observations from aircraft profiles (and as a one-line example, against vertical concentration profiles); evaluation of satellite observations constrained inversions using in-situ measurements; evaluation of in-situ observations constrained

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inversions using satellite measurements; evaluation against “all types of independent atmospheric observations”. Each of these in less than three lines.

This is not useful to the experienced inverse modelers who are already very familiar with all this. This is not very interesting for newcomers to inverse modeling (it can be summarized in one sentence: “evaluate your results against independent data”, the rest is case-specific). Finally, for specialists from other disciplines who would like to get a glimpse at how inverse models are evaluated, it quickly gets boring. Meanwhile, there are important questions that could be discussed here, but that are, in the best case, left to Section 4: comparing observations with their model counterpart is not always trivial (case of satellite observations which may require an important work of data selection, bias correction, and the application of an averaging kernel to the model fields), not always wise (comparing low-resolution model CO<sub>2</sub> fields with CO<sub>2</sub> observations in an urban environment is not so smart), and not always that useful (the implications of a bias vs. independent observations in the upper stratosphere are not the same than that of a bias in the continental boundary layer). On the other hand, not doing it is sometimes catastrophic (incorrect interpretation of inversions constrained by biased satellite data).

**AUTHOR RESPONSE:** The referee’s point is well taken. As described in the first response above, we believe that there is value to presenting a broad ranging set of references and variations on diagnostics approaches, for both specialists and non-specialists. This goes with our vision of this manuscript as a roadmap to the existing literature. At the same time, we agree with the referee that in some instances the desire to be thorough came at the expenses of synthesis and interpretation.

**CHANGES IN MANUSCRIPT:** In revising the manuscript, we will examine each subsection in Section 3 with the goal of keeping detail to the extent it is useful to our goal, but at the same time restructuring the discussion in a way that synthesizes information more effectively, as with the example listed by the referee. The main changes will be at the start of each subsection (3.1, 3.2, 3.3, 3.4), where we will present more context for

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the subset of diagnostics to be presented.

COMMENT FROM REFEREE: Some subsections of Section 3 are better, but overall, the paper would read much nicer with less references, less examples, but more detailed ones (given the pedigree of the authors, I am certain that they can easily find some from their own work, and illustrate them with a few figures). Once again, the key is to define the target readers, and what they should retain: Non specialists don't need to know of tens of examples (they won't remember them all anyway), but they need to understand correctly and completely those that are presented. Specialists might be interested in the many references, but most of them could be moved out of the main text, perhaps to one or several tables (perhaps one for each Section 3.1, 3.2 and 3.3), as it is often done in literature reviews. Before final publication in ACP, I would therefore recommend that the authors consider revising Section 3 and 4, keeping in mind that readers should be able to learn from it without having to read the references and/or the other papers from the special issue.

AUTHOR RESPONSE: We agree with the referee that a review of this sort could fundamentally take one of two forms. The first is, as the referee described, an in depth look at a small selected set of prototypical examples. The second is, as we have done here, a more comprehensive overview of the literature. There are advantages and disadvantages to each. We made the choice to use the second model in part specifically to make it easy for readers to dig deeper if they chose to, by looking up the references included in the manuscript.

CHANGES IN MANUSCRIPT: With the above in mind, we have decided not to restructure the manuscript by limiting discussion to a few prototypical examples and putting the remainder of references in a table. That being said, we take the referee's concerns to heart, and will revise Sections 3 and 4 to provide more context, synthesis, and interpretation, to the extend possible without substantially increasing the overall length of the manuscript.

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