

## ***Interactive comment on “The degree of freedom for signal assessment of measurement networks for joint chemical state and emission analysis” by Xueran Wu et al.***

### **Anonymous Referee #1**

Received and published: 28 February 2018

General comments: This paper describes a methodology to determine the information content (in particular, the degree of freedom for signal) of joint chemical state and emission inversions. A detailed mathematical analysis is provided in the first part, followed by numerical illustrations. There are several issues with this article: 1) The method is known. Recent articles, such as, e.g., Bousseres and Henze (qjrms, 2017), Spantini et al. (2015), presented in detail this approach for information content analysis, with similar mathematical developments. Other related methods in the context of ensemble data assimilation are described in the literature, see for instance Anderson (2001) (Ensemble Adjustment Kalman Filter). This has to be acknowledged and discussed by the authors. 2) The grammar needs to be thoroughly checked. In many places the text

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is unclear due to poor phrasing. 3) The main text contains too many equations, which is distracting and makes the reading difficult. Most of the mathematical developments should be moved to an Appendix.

Detailed comments: 1) Introduction: It is too long. Also, there are lots of redundancies. A number of references should be added (see general comments above for some of them) and discussed. In particular, the authors should clearly acknowledge previous works where similar analysis were conducted, and explain what their study adds to the current state of knowledge. If there is no real novelty in the approach, then the article should be presented as a review paper focusing on methods for information content analysis, with a numerical application for the specific problem of joint chemical state and emission inversion. 2) P1, L17-18: Rephrase. 3) P1, L19-24: Shorten. There are many repetitions. 4) P6, L13-23: Could be simplified (or should go to an Appendix). 5) P7 L9-15: Redundancies. Poor phrasing. 6) P7 L19: Good. You should do that simplification earlier. There is no need to split all the operators like in (11). 7) P7, Eq (17): Please define the mathematical terms you use. For instance,  $t_{-1}$  is not defined here. Presumably  $P_{-1}(t_0|t_{-1})$  is the prior error covariance matrix, in which case it should be clearly stated. 8) P8, L1-2: I do not understand this sentence. 9) P8. Eq (19): this equation is well-known and the previous developments are not needed. 10) P8 L13-14: Unclear. Please rephrase. 11) P13, Eq (53): Notation  $V$ ,  $S$ ,  $U$  has already been used in (46) and (48), and the SVDs in (53) and (46) are not related. Please use another notation. 12) Section 6: Again, lots of mathematical developments that should be in an Appendix.

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Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2017-220>, 2017.

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