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Interactive comment on "Harmonization and comparison of vertically resolved atmospheric state observations: Methods, effects, and uncertainty budget" *by* Arno Keppens et al.

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The response to the Referees is structured in a clear and easy-to-follow sequence: (1) comments from Referees; (2) author's response; (3) author's changes in manuscript.

GENERAL COMMENTS (1) The paper, written by a team of authors who have much experience of comparison of various types of satellite data product and reference measurements, represents a worthy attempt to present a unified framework for all such comparisons, while also giving due recognition to earlier work by other authors who have dealt with more specific aspects. Unlike the authors, most readers will only be familiar with limited applications of the intercomparison problem, so the challenge is

to write a paper general enough to cover the whole field, while also making it understandable to those only working on particular aspects. It's not an easy read, but I don't see how it could be done better, and the flow chart and tables offer useful summaries. Hopefully this paper will serve as the reference document for future discussions of validation methodology. (2) The authors highly appreciate the reviewer's positive view on the manuscript, and are very grateful for the encouraging words. (3) No changes have been made based on the general comments.

MINOR COMMENTS I have only minor suggestions for clarifications and grammar:

(1) P4 L6: 'which are' instead of 'who are' (2) Agree. (3) This suggestion has been implemented.

(1) P5 Eqs. 9, 10: Should subscripts in the bracketed terms be 'L' rather than 'N'? (2) Thanks for noting this erroneous remnant of a previous notation. (3) Lower indices N have been replaced by L in Eqs. (9) and (10).

(1) P7 Eq. 13: This equation made no sense to me. (2) It is agreed that equation 13 is not fully clear in its present form. In fact, the nested max and min in the two terms indicated the layers' upper and lower bounds respectively, and not the mathematical max or min (as for the first occurrences). This has now been made clear in the equation and the text. (3) The second max and min in each term have been replaced by an upper height index U and L, respectively, and the first sentence following the equation has been extended with the following: "and the upper indices U and L indicating the layers' upper and lower height bounds, respectively"

(1) P8, L21: Assuming 'prior constraints in terms of prior covariance matrices' is to be taken as a single entity, I suggest removing the comma after 'constraints' otherwise it may appear that measurement weights and prior profile shapes are also in terms of prior covariance matrices. (2) The authors agree that the current formulation including the comma is misleading. Removing the comma however suggests that prior constraints are always expressed in terms of prior covariance matrices, which is not

the case. The authors therefore suggest to just remove the text part between the commas, to keep the discussion as general as possible at this point in the manuscript. Later in the text, below Eq. (15), the following text part (already there in the discussion paper) sufficiently specifies what is needed for further discussion: "whereby the prior constraint is typically, but not necessarily, given by the inverse of the prior covariance matrix" (3) The text part "in terms of prior covariance matrices" has been removed from the first sentence of Section 4.3.

(1) P8, L23 (and elsewhere): I suggest 'retrieval artefacts' rather than 'retrieval effects' to emphasise that these are unintended rather than intended consequences of the retrieval process. (2) The authors agree, also in order not the confuse with the 'effects' of the harmonization operations. (3) Occurrences in the text of "retrieval effects" have been replaced by "retrieval artefacts".

(1) P8, L24 (and elsewhere): I suggest 'eliminates' rather than 'annihilates' 'Annihilate' has violent connotations (although perhaps the authors do have particularly strong feelings on this matter?). (2) Although "annihilation" has appeared in the literature before, it is not a fixed or generally applied expression. The authors therefore agree with the reviewer's suggestion. (3) Occurrences in the text of "annihilation" or variations thereon have been replaced by "elimination" or variations thereon.

(1) P11 Eq. 23-24: Getting from Eq. 23 to Eq. 24 doesn't seem obvious. Does this require some trick along the lines of getting from 4.11 to 4.12 in Rodgers? (2) In order to obtain Eq. (24), it is sufficient to apply the matrix algebra property $(AB)^* = B^*A^*$ multiple times, indeed as in Rodgers (2000) 4.11 to 4.12, or 10.49 to 10.50, as referred to in the paragraph below Eq. (24). The authors however agree that the current suggestion of inserting the least squares definition of the pseudo-inverse regridding matrix is not fully straightforward. Eq. (24) is obtained more directly if one neutrally inserts the transposed regridding matrix and its pseudo-inverse directly. This suggestion has now been adopted in the text, and Eq. (24) has been extended with an intermediate step. (3) The sentence between Eqs. (23) and (24) has been rewritten: "By insertion of the

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transposed regridding matrix and its pseudo-inverse, one obtains:" Eq. (24) has been extended to explicitly show this insertion as an intermediate calculation step.