Atmos. Meas. Tech. Discuss., 8, C333–C337, 2015
www.atmos-meas-tech-discuss.net/8/C333/2015/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Vertical level selection for temperature and trace gas profile retrievals using IASI" *by* R. A. Vincent et al.

T. von Clarmann (Referee)

thomas.clarmann@kit.edu

Received and published: 20 March 2015

In this paper, a silent approximation in a paper of mine (von Clarmann and Grabowski, 2007) has been identified, and a more rigorous method to remove prior information from remotely sensed data is proposed and tested. The authors find that their new method is superior over the use of equally spaced vertical grids and grids determined by the method suggested by myself and Grabowski. Although the gain of information with respect to the latter is small, I find the paper useful for three reasons:

1. The new method might not justify the additional effort for the cases tested in this study, but in future there may be other applications where the gain by the new method may be larger. Then it is good to have such a method at hand.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

2. Not publishing this work would imply the risk that other researchers who are not aware of this study try the same approach again and waste their time to find out again that the new approach may not pay off.

3. This study also investigates the benefit of any (theirs or mine) optimized gridding with respect to equidistant gridding. This is a useful result already.

The paper is structured and written very clear and thus is easy to understand. My specific comments are mainly minor technical issues. Since I am not a native English speaker, my language-related recommendations should not be blindly followed. I suggest publication of this paper in AMT and hope that my specific comments will help to further improve the paper.

Please define all acronyms and abbreviations used in the abstract (RTTOV, IASI)

p2592 l24: One can also understand the interpolation scheme of the coarse grid to be a kind of a priori assumption. With this in mind, it might be more adequate to write "...relies less on FORMAL prior knowledge and ..."

p2593 l3: Even if you define the IASI acronym in the abstract, please leave the definition here as it is, because abstract and the main text are two independent entities.

p2593 l11: the acronym GEOS-Chem should be defined.

p2593 l17: the acronym DFS should be defined, and a reference to the Rodgers (2000) book may be adequate, because you refer to the particular technical meaning of 'degrees of freedom' as discussed in this book, not to 'degrees of freedom' as a generic term.

p2594 l3: please define the abbreviations AIRS and CrIS; probably references to these missions might be adequate.

p2594 l15: Here DFS is defined but the abbreviation is already used on page 2593.

AMTD
8, C333–C337, 2015

p2594 l19: Shouldn't it read either "At THIS point..." or "... estimate (Rodgers 2000), at which point..."?

p2595 l5: Here $\vec{x}$ is the estimate of the atmospheric state. In l 11 $\hat{\vec{x}}$ is the estimate. Later, on p 2596, where the averaging kernel is defined, the hat and the non-hat version appear in one equation, and the non-hat x means the true state and no longer its estimate, which is confusing. This ambiguity can be remedied by replacing "$\vec{x}$ is the desired estimate of the true state" by "$\vec{x}$ is the true state to be estimated".

p2595 l7: The non-instrumental errors still can have random characteristic, e.g. when doing an ozone retrieval, the assumed temperature can be too high or too low, and this can vary between measurements. Thus I am not really happy with the term "systematic errors".

p2595 l14: since $m \neq n$, $\mathbf{K}$ usually is not invertible. Thus the term "optimal inverse" may be confusing. What about "optimal generalized inverse"?

p2595 l23: the optimal estimation constraint penalizes not only estimates whose profile SHAPE deviates from that of the a priori but also estimates of the same shape but with other amounts (contrary to a constraint involving a squared difference matrix, because there the amounts are in the null space of the constraint matrix, and only the shape is constrained. I suggest just to delete the word 'shape', then everything is ok.

p2596 l3: Shouldn't it read either "In THIS case" or "...sample atmospheres, in which case..."?

p2596 l6: I am not happy with the term 'ad hoc method', because the optimal estimation scheme is optimal only if the actual atmospheric state is a member of the ensemble used to calculate the prior. This is usually not the case, and to use the past mean state of the atmosphere as prior for the retrieval of the actual atmospheric state makes the silent assumption that the mean atmospheric state does not change, i.e. that the future resembles the past. I think David Hume (1748) was the first one to show that

AMTD
8, C333–C337, 2015

there is no rational justification for this assumption. Of course on can assume that this assumption is adequate but this assumption is no more and no less 'ad hoc' than, e.g., the Aristotelean assumption that *natura non facit saltus* (nature does not make a jump; here applied to the altitude domain) which is implemented by the Twomey-Tikhonov constraint. The optimality of optimal estimation is conditional to the validity of the prior assumption and thus not absolutely true. Thus, I suggest to slightly reword this statement. See also my next comment.

p2596 l7: It is not true that Twomey-Tikhonov regularization (and I understand that it is these the sentence refers to – however, this is not quite clear) ignores the correlations. I think that quite the opposite is true: The squared difference terms in a 1st order Tikhonov scheme correspond to certain correlations in the covariance matrix, while a 1st order Tikhonov scheme ignores (intentionally!) term of the covariance matrix referring to the total amount. Tilman Steck (Steck and von Clarmann, Applied Optics, Vol 40, Nr 21, p3559ff 2001) has shown that an inverse covariance matrix with exponential decay of the covariances can be written as a series of Tikhonov matrices of increasing order. A Tikhonov first order regularization matrix then corresponds to a covariance matrix with negligible diagonal, and truncated after the 1st order difference term. That means that, going from an inverse covariance matrix to a 1st order Tikhonov matrix, only correlations survive, but variance approaches infinity (i.e. the inverse variance approaches zero, which is why the 1st order Tikhonov matrix has a null space). This is just the opposite of what is stated in the paper. I agree, however, that the Twomey-Tikhonov regularization ignores higher order correlations.

p2596 l11: The non-hat x has not yet been correctly defined. Perhaps my comment to p2595 l5 helps to remedy this.

p2599 l11: The fact that there is something to decide about $\mathbf{S}_a$ confirms what I have said before, i.e. that optimal estimation also depends on some ad hoc decisions.

p2603 l10: I would prefer to write out 'logarithm'.

AMTD
8, C333–C337, 2015

p2603 l10: The Jacobians refer to the log of VMR. While Jacobians referring to VMR in a fairly transparent atmosphere depend only weakly on the VMR, this is not the case for the log averaging kernels. I suspect that this leads to more variable averaging kernels than you would have in a retrieval of VMR. Does this cause any problems (or explain any problems already detected) when trying to apply the same optimized grid to different atmospheric conditions (globally designed grid)?

p2603 l23: Has TOV already been defined?

p2604 l4: please define GEMS.

p2605 l16: is 'were' correct? I would have expected 'was' (but I may be wrong).

p2607 Sect 5: There is a further argument for the globally designed grid: E.g. analysis of time series causes major problems if either the grid (or in the case of constrained retrievals the averaging kernels) change with time. See also the introduction of the current discussion paper in AMTD by myself, Norbert Glatthor and Johannes Plieninger.

Table captions and figure captions: I have just learned that according to ISO standard, table and figure captions should read "quantity / unit" rather than "quantity [unit]".

Best regards, Thomas von Clarmann

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 2591, 2015.