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Atmospheric Measurement Techniques Discussions

Interactive comment on "In-operation Field of view Retrieval (IFR) for satellite and ground-based DOAS-type instruments applying coincident high-resolution imager data" *by* Holger Sihler et al.

Anonymous Referee #2

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The paper presents a method for in-operation field of view retrieval (IFR). The method is based on the correlation of the instrument with a higher resolution accompanying instrument. Three applications of this LR/HR system are studied: GOME-2 and AVVHR; OMI and MODIS; MAX-DOAS and an SO_2 camera.

I recommend the paper for publication, the main reason being the usefulness of the method: IFR seems good in monitoring in-flight changes in the FOV.

However, some aspects, in particular in the method section, could be improved.

General comments

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Discussion paper

1) Number of measurements m. The several usages of m throughout the paper are confusing:

On the one hand, m is described as the minimum number to achieve a square linear system: m = n+1, where n is the number of HR pixels in one LR pixel. On the other hand, m is allowed to be smaller than n (p13 l5). Further, m is also a parameter that is easily varied in a study to obtain an error (Appendix). And m is often chosen very large, probably to get a massive over-determined system to cancel out noise.

Does m need to be large because a lot of LR measurements are not independent?

In p13, l1-11 two cases are mentioned. It seems to me, at first sight that in general, m » n as desired by the authors. An matrix with many more rows than columns has a good chance of having adequate rank, and thus of giving a well-defined solution. And, giving the number of HR pixels in a LR pixel (GOME-2: 30x40=1200, see fig 7); OMI: 60x40=2400, see fig 14), an m=1E5 seems sufficient.

However, the second case (p13, r5) is then confusing: m < n will, given enough measurements, not be the case. Perhaps it is meant that rank(H) < n+1 ?

Please clarify.

Specific comments

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p4, l 13: the along-track size of 40km is not explained (I assume 100 minute orbit, so 40km in 6s)

p4, l 33: Does this technique of 9 measurements use (partly) the same HR signals? In what sense does this provide 8 extra, original measurements, or is there a large dependence between them?

p6, l 13: Here pixel size is defined as along x across, while for GOME-2 it was across x along.

p11, l 1-10: A bit confusing. It seems that (A,C) and (B,D) also have the same y-offset. But I am happy to believe that the resampling has been done in an sensible way, and that some constraints (whether and when ABDC is a perfect rectangle, for example) do or do not hold and that adequate (bi-)linear interpolation of the transformed HR pixel-centers is done. I suggest to be either more specific or less specific.

p11, l 15: The constraint that the HR pixels are square and equidistant seems harsh in this respect. A simple global stretching in x-direction (so a varying delta-x) may be an idea; this will not make the remainder of the text/method more difficult.

p12, eqs 2,3,4: the meaning of h changes two times. Confusing.

p12, l 19: linear independent refers to the rows in the matrix. In what way does it translate to the similarity of two or more HR measurements? This seems difficult, and only a brute force approach of defining a massive overdetermined system (more measurements) will ensure that the rank of the matrix will be at least n+1. See also general comment.

p13, l 13: Nothing is said about the expected behavior at the boundary. Is it enforced that the c_ij at the boundary approach zero or noise? From the remainder of the text I see that the grid is chosen large enough and that the noise is a good proxy for an empirical standard deviation.

p15, fig 7: Here, and in other figures, it becomes clear that the grid has been chosen sufficiently large. In what sense does this extent influence the needed number of measurements?

Related remark: If the number of coefficients c_ij becomes too large, it may be an idea to group the c_ij near the boundaries, thereby effectively grouping the square pixels (which are expected to be zero anyway).

[p19, l 8: The question about large m and having an underdetermined system as in the general comment refers to this OMI case.]

p19, l 21: The skewed FOVs (as in OMI) cannot be described by the super-Gaussian. Is that suggested here? Note that only a small extension (a mapping of x and y) is sufficient to have skewed super-gaussians.

p27, l 26: possible explanations: The FOV can be re-computed with another grid size. Does it also occur in that case? If not, it seems a numerical artifact.

p29, l 6: Is the wind speed threshold robust? If the threshold is lower, then m decreases. The 15 m/s is a maximum; is that representative (instead of using a median)? Maybe the trade-off can become a bit better.

p30, l 8 : Again, the m < n case is confusing.

p32, fig A2: The lower boundary of the lines is expected at 1200 (30x40), which seems not the case in the graph. Further, pairs of filtered and all have the same m, which seems incorrect.

Technical and minor corrections

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p5, l 9: channel 2 -> 4 ?

p7, fig 3: sake [of] clarity.

p13, l 27: three additional parameters

p13, l 27: two-dimensional

p14, l 5: seven parameters

p14, l 5: c_k is now described as one-dimensional, while c0 is discarded? p14 l 14 Energy: Then I would have expected a z_bˆ2 to be integrated. Unless it is a common expression.

p15, fig 7: the color scales in a and b are different. The 1D-graphs with magenta lines have no vertical scale.

p15, l 10: in a slightly

p20,l 9: approximates at?

p21, l 2: An effect found ...: not a good sentence.

p30 l 11: are now equal the

p31, l 7: ocean filer. Line 7 repeats sentence line 6.

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