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Atmospheric Measurement Techniques Discussions
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Interactive comment on "Errors induced by different approximations in handling horizontal atmospheric inhomogeneities in MIPAS/ENVISAT retrievals thinsp;" *by* Elisa Castelli et al.

Anonymous Referee #2

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Review of Errors induced by different approximations in handling horizontal atmospheric inhomogeneities in MIPAS/ENVISAT retrievals

by Castelli and collagues.

This is a nice paper that clearly describes a solid study of the impact of horizontal gradients on various approaches to retrievals from the MIPAS instrument. In principal I am happy to see this manuscript proceed to AMT, however, I only have one concern that I'd like to understand beforehand (plus some minor suggestions/comments for the authors to consider). The standard of English is excellent and the figures are very clear.

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Discussion paper

My main concern surrounds the discussion on page 4, lines 10-15. As I understand it, the authors have taken model temperature and pressure profiles on a fixed height grid, and forced the pressures to be horizontally homogeneous while retaining the model horizontal temperature variability. If the altitude grid indeed remains fixed, then surely the model atmosphere that results is not in hydrostatic balance. As such, this becomes an unphysical simulation from which it is arguably hard to draw meaningful "real world" insights. Furthermore, are the lines being observed not subject to significant pressure broadening, making pressure the dominant contributor to the radiance signals? As such, I would have thought that horizontal gradients in pressure are arguably the most important thing to study the impact of (though one would probably ultimately quantify it in terms of impact to the temperature/composition profiles as interpolated to a fixed pressure grid, being the product most widely used in the community). While I understand the authors desire to "focus the analysis on the error caused solely by the approximations in modeling the horizontal variability of temperature and target gas", surely, if the pressure gradients are indeed the dominant term, they should have been included in the analysis.

I'd like the authors to address this point, and consider revising their approach, or at least undertaking a separate quantification of the impact of pressure gradients (but again, the unphysical nature of their atmosphere would limit the usefulness of the result). Perhaps I have misunderstood the description in the manuscript, in which case, greater clarity is required.

==== Minor comments

—- Page 3

Lines 27/28: I don't understand this sentence. By "average atmosphere in a given latitude band" do you mean a zonal mean, i.e., an average over all longitudes? If so, hasn't longitude, by definition, been ruled out. Do you mean averages over a longitude range should not depend on the choice of range (clearly not an appropriate assumption

geophysically). Please clarify.

—- Page 4

Line 5: Please be more quantitative; what is the spatial resolution used in the FM?

Lines 10-15: Please see discussion above.

Lines 17-19: First, please explain why you needed to add any noise at all? Why not simply do a noise free simulation. Also, please state whether, in addition to adding 1/40th of the expected noise, you also quote that 1/40th value as the radiance precision in the retrieval calculation (S_y in the Rodgers formulation), or does the retrieval still believe that the noise is at the 100% level?

—- Page 5

Line 20 (and 22). Your "diff" has been typeset in math mode, you'll want to typeset it in text mode (e.g., \text{diff}, using the amsmath.sty package).

—- Page 6

Line 26: I think this would be better "Polar and mid-latitude winter conditions" if that's what you mean. As it is it could mean "Polar winter" and "mid-latitude all seasons".

—- Page 7

Line 2: As for page 6, line 26. Possibly elsewhere that I didn't catch also.

—- Figures

Figures 2 and on: I would much prefer to see temperature errors quantified in K than in %. K are much more accessible to the general reader.

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