Thank-you for the careful and thorough reading of my manuscript and for all of the helpful comments. I very much appreciate the time and effort you've both taken!

Responses to Referee #1

Specific Comments:

1) Information upon the water vapour continuum should be contained within the clear singular vectors, as estimated from the RFM simulated spectra – and thus one would expect that the SVD method to avoid the issue of misinterpreted large water continuum as cloud. However: this study has not done extensive testing of how the method responds to large water vapour continuum – and hence I am uncomfortable making strong statements to the effect that the method is 'immune' to such issues. I have, however, stated that the SVD method should, from a theoretical standpoint, behave consistently, for the reasons presented above at the end of Section 3.2.

2) Have changed the order of Fig. 2 and Fig. 3 – and restructured paragraph to reflect this, as well as made changes to clarify the phrases indicated.

3) Correct, the RFM is non-scattering. This shouldn't be an issue for anything but the thinnest of cloud – in a part of Hurley (2008), a study using KOPRA simulations (scattering simulations, see Spang et al., 2008 for details on simulations) was carried out to quantify the discrepancy between non-scattering (ie. RFM) and scattering (KOPRA) simulations, and there was found to be very little difference for clouds having extinction larger than 10^{-4} km^{-1}. I have added a brief summary of this to Section 4.

   Horizontal homogeneity is assumed – I've made this more explicit in the text.

4) Yes, you are correct – it should be 80% instead of 90%. This has been changed in the text.

5) I've rewritten the two paragraphs at the end of Section 7, and given the units for '10000'.

6) I agree that this would be a very useful approach in order to detect PSCs, and in fact perhaps to distinguish between PSC types (although I've only done very preliminary work on this in Hurley, 2008) – but the current work presented is for tropospheric clouds only, as it was felt that the RFM could not provide spectra representative of PSCs – and at the time which this SVD detection work was carried out, there were no available PSC spectra. There are now PSC spectra available through the MIPclouds project (Spang et al., 2008 for details), upon which analysis could be carried out – however I'm happy to have just written this up as a potential application of this method in a brief section before the conclusions.

7) Yes, you're correct. It isn't necessarily better to use a small spectral range as opposed to a large one. The reasoning was that one would expect that the 960-961 cm^{-1} range to have the highest chance of being well-fit, as it is the most atmospherically (gaseously) transparent part of the larger spectral region – and hence is most likely to have the gaseous component well-isolated. The choice of a smaller spectral region is also useful in practicalities of application, as inputting large portions of spectra into IDL etc for analysis can be time consuming.

8) Yes, the PDFs of Integrated Radiance Ratio at the lowest altitudes do look different – but that is surely to do with the proportion of cloud-to-clear measurements at those altitudes (meaning that there will be a much higher proportion of cloud at those lowest altitudes than anywhere else). To counteract this, we've fitted only the left-most 'half' of the cloudy peak in the PDFs, so there should be minimal inclusion of clear points – but I do register your concern. I have noted this in the text – but in practice, it is unlikely that the lowest altitudes will not be obscured by higher cloud, which will be well dealt with by the thresholds thus determined.

9) Yes, mea culpa – the '0.0025' quoted comes from applying the SVD method to the RFM simulated spectra (for which the EF is known) and I'd not mentioned that in the text. I have now explained where this comes from in the text.

Minor Comments:

p1189, l19: Have put in the spectral windows.

p1190, l22: Have put in some details.

p1191, l18: Have changed the reference.

Technical Corrections:

Have swapped order of Figs. 2-3.

Fig.3 units are incorrect – should be in W, not nW, and now fixed.

Figs.10-11 now have appropriate information in the legends.

I'd accidentally attached the wrong version of my bibtex file, and not thoroughly checked the typeset proof, which is the issue with many of the references. Have attached the correct one now, and have checked that this has dealt with the specifics you've mentioned.

Typo 'misdianosed' now fixed.