Thank you for reading our paper in detail and providing such valuable comments and suggestions. We have taken all of these into account and made changes to the manuscript accordingly.

The following section lists each comment/question raised with appropriate replies.

<u>Reviewer 2.</u>

a) General comment 1: We agree that this is an indirect measure of aerosol extinction, but quantitative in radiative effect and in CATH. We believe that an operational aerosol extinction product should be produced for MIPAS and have updated the conclusions accordingly.

b) General comment 2: The variable thresholds prove to be most advantageous in the lower stratosphere and in fact around the tropopause. This is particularly important to capture the presence of optically thinner clouds, or thin cirrus clouds that produce CI values slightly greater than 1.8. We are in agreement that lower in the middle troposphere there is little distinction between clouds detected by the 1.8 fixed thresholds and the variable thresholds. However we do also find increased sensitivity in the upper troposphere to volcanic aerosols and to aerosols associated with Black Saturday fires (see Figure 8) with a range of CI values clearly between 1.8 and 4.0 and are nicely picked up by the variable thresholds in this latitude range.

c) General comment 3: For a cloud filling the tangent layer, one can estimate that the uncertainty is asymmetric and ranges approximately from +0.5 to -1.5 km for reduced resolution mode and +1.0 km to -2.0 km for full resolution mode; Höpfner et al 2009 results indicate the positive bias is 0.5 to 1.0 km. However, Höpfner at al, 2009 also note that the offsets depend on the horizontal extent of the clouds. To account for this we conservatively assigned an error of ±1.5 km across all CATH but we have noted that a bias of 0.75 km in reduced resolution mode for UT clouds can explain some of our results and is broadly consistent with expectations from Höpfner et al 2009 simulations for filled tangent layers. Consequently, we performed some tests on the MIPAS CATH data and obtained the "best agreement" with HIRDLS and CALIOP (figure 6) by shifting the MIPAS data in altitude by -0.75 km (see example plot below to illustrate this). We have added information to this effect in the manuscript.

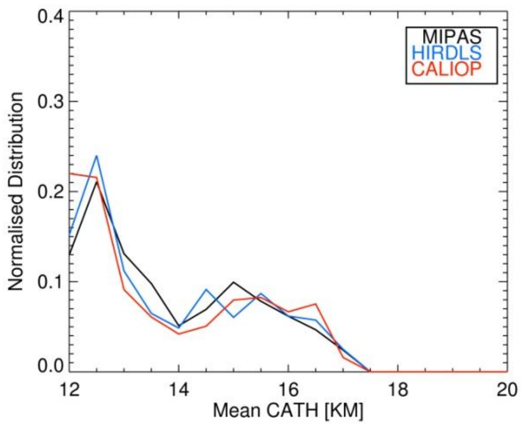

Figure 1. MIPAS data shifted by -0.75 km in comparison to HIRDLS and CALIOP for DJF 2008

d) General comment 4: The value for 30 nW/(cm^2 sr cm^{-1}) was used for the following reason. Histograms of MIPAS NESR (extracted from MIPAS L1b spectra) were analysed for FR and OR MIPAS data (October 2003 and February 2009) in both MW1 and MW2 from 10 to 30 km. In general the NESR was lower in OR mode than FR mode, however, a value of 30 nW/(cm^2 sr cm^{-1}) captured the outer bounds of the NESR variability in both resolutions over the range of altitudes analysed. In essence, 30 nW/(cm^2 sr cm^{-1}) is representative of a conservative NESR.

a) General comment 5:
   a. Since the initial submission, we have revisited this issue in more detail. The effect of ClO is only observed in MW2 where weak ClO features are observed at 832.5 cm^{-1} to 832.8 cm^{-1}, and 833.9 cm^{-1} to 834.1 cm^{-1}, a stronger feature is observed at 833.3 cm^{-1}. We performed some additional tests on the polar index calculations by modifying temperature, O3, ClO and HNO_3 (modifying each separately) and we can deduce that the combined effect of low temperature, low O_3, low HNO_3 (in both microwindows) and enhanced ClO (in MW2) is the dominating factor that determines the resultant index values. This *combined* effect outweighs the impact from the enhanced ClO alone, therefore, masking the ClO lines would not be of any benefit. Generally, we see that low temperature causes an increase in noise with low O_3 and low HNO_3 causing the index values to decrease. We originally did not include any information on the HNO_3 in the manuscript (although in the calculations shown in section 5.2.2. we do modify it according to Manney et al, 2011) and we will now modify the manuscript. We will also clarify in the abstract that it is not high ClO formation but rather, a combined effect from the atmospheric deviations in temperature and the gases above (what we have written is a bit misleading as it suggests that the enhanced ClO is the dominating factor). For this reason, we feel that the figures are not necessary.
   b. In terms of widening the MWs, this is an option as long as the interference from other trace gases is kept at a minimum. Recently, we have started to investigate different spectral windows for cloud/aerosol detection and this is something we will be including in these microwindows investigations.

e) General comment 6: We are not completely clear why the referee wants to see such comparisons other than for general interest. Our intention is not to model the observed CI distributions but rather to ask the question of what is the "gas only" contribution to that distribution. "Modelling" of the distribution would also require noise and other uncertainties to be accounted for and is beyond this paper.

f) P1799 L25: This has been changed to "wideband"

g) p1799: Thank you for highlighting this. The order of the instruments has been modified and grouped as shortwave/near infrared, mid-infrared & sub-millimetre and then a paragraph on thermal infrared instruments.

h) P1800: Yes, this is an important point. We have changed the tense in the manuscript accordingly.

i) P1800 L25: In agreement, this has been changed to "bands" in the manuscript.

j) P1800 L26: Thank you for highlighting this. We intended to refer to radiometric calibration only.

k) P1801 L5: We have changed this in the manuscript to make the FOV description clearer. Our response in point c) should hopefully answer this query.

l) P1802 L14: Due to the extreme viewing angle of HIRDLS, it's local solar time can be different (up to half hour, C. Wright, Pers. Comms.) to that observed MLS, OMI and TES on the AURA platform (as started in the HIRDLS data quality document: http://www.eos.ucar.edu/hirdls/data). Perhaps the reviewer is hinting at some dependence of the datasets on local solar time. Whilst this is certainly possible, it does not seem to be a large effect at these altitudes.

m) P1803 L6: This data selection criteria has been followed as recommended in the HIRDLS data quality document and Steve Massie (co-author). Some extinction data are flagged as bad data by setting the precisions to less than 0 % (due to bad data) and therefore these must be removed.

n) P1804 L7: Yes, baseline offset is much more appropriate. We have changed this in the manuscript.

o) P1804: Yes they do. Specifically they show significant radiance differences when clouds appear in the MIPAS FOV.

p) P1804 L17: Okay, this explanation does seem misleading. We have noted this and will clarify this in the text. The control region does contain CO_2 lines but has some interference from other trace gases.

q) P1804 L21: We think it is important to note that other cloud detection regions exist. In addition we would prefer to stick to CI-A naming convention here to be consistent with other MIPAS cloud papers.

r) P1805 L9: Yes these are the values expected from cloud-free measurements. The manuscript has been changed to make this clearer.

s) P1805 L11: Okay. This point is important. We have modified this in the manuscript.

t) P1805 L19: We agree and have changed this in the text.

u) P1806 L6-7: This means that as the method is simulation based, there is no dependency on MIPAS data to be used for the calculation of thresholds. So in this sense, the thresholds are independent.

v) P1808 L15-25: The water vapour profiles used within each latitude band simulation encompass a combination of saturated profiles (calculated from mean and minimum climatological temperature and pressure) *and* the maximum ECMWF water vapour profiles. (We had missed this from the text but had included it in the table). Separately, we tested the maximum climatological profile in simulations and found that they produced very low index values and therefore would not be able to provide sensible thresholds. We have revised this paragraph to make this clearer and commented on the importance of distinguishing between spectra that may have cloud presence (baseline offset etc) and spectra with high water vapour. This distinction between the two is most apparent at lower altitudes where high water vapour can produce index values (and spectral effects) similar to those produced by clouds.

w) P1808 Eq2.
   a. We realised these terms were missing and have added these to the manuscript.
   b. Thank you for highlighting this. We agree that we showed the fractional error equation in the paper as we were missing a term (note: the absolute error is used in the actual calculations performed). The equation has been rectified and we show the more correct equation even though it could be simplified for constant noise and allowing for different number of frequency points.

x) P1811 Section 5.2.3 Showing extinction figures alongside figure 2 (or tabulating the results) is a good idea. However, we would need to add several figures to show the extinction range for each latitude band. We have decided to show just one example by adding a figure of CI-A vs. extinction for the Northern Hemisphere mid-latitude case. We hope the reviewer finds that this is sufficient.

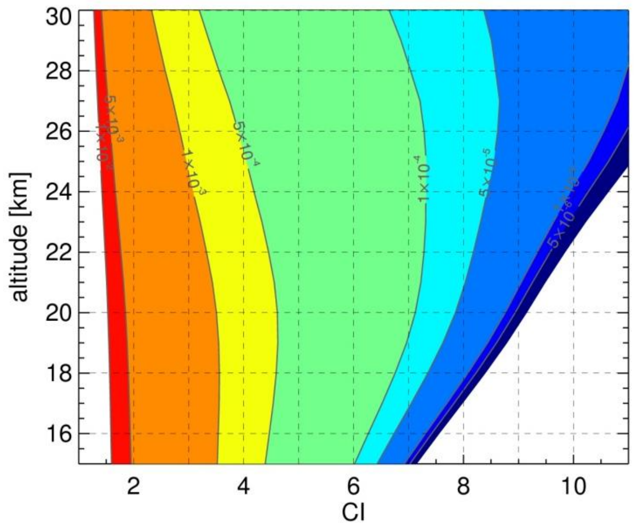

Figure 2. Extinction as a function of Altitude and CI for the Northern hemisphere mid-latitude. Figure shows the extinction limits for various values of CI.

y) P1813 L4: This is true, we have rectified this in the manuscript.

z) P1813 L26: We have decided to more accurately call this the Cloud Top Occurrence Frequency. We choose to do it in 1 km bins because we can also examine the data as a function of altitude although we only report the UTLS frequencies here.

aa) P1816 Fig 7: Yes, we decided to include it later next to the CALIOP CATH figure so that they could be observed alongside each other. However, we have now changed figure 9 to show non-averaged CALIOP data for the 7th to 16th February so that any biases introduced by the averaging can be avoided and to show how well the position of the plume is measured in both MIPAS and CALIOP.

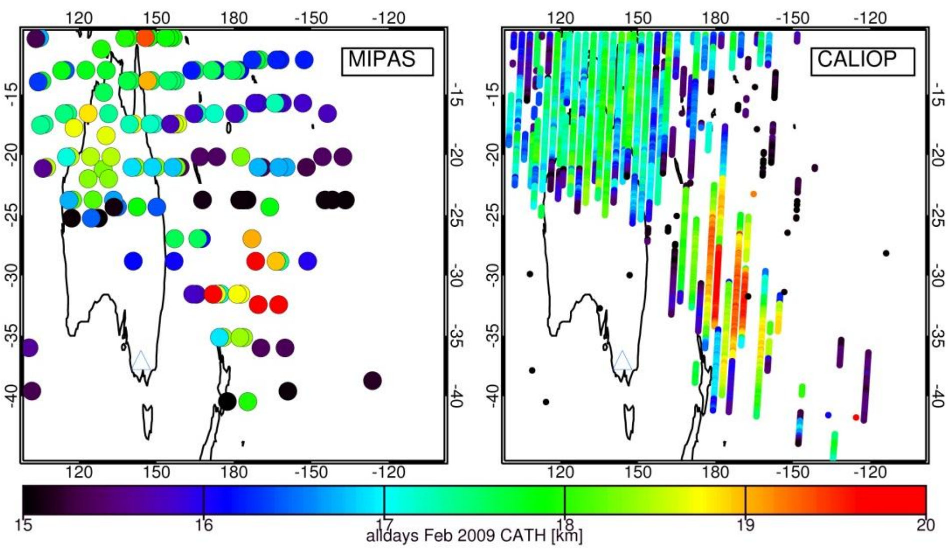

Figure 3. MIPAS and CALIOP CATH (night only) with un-averaged CALIOP data for the 7th to 16th February.

bb) P1816: We have checked this and the points correspond sufficiently closely for this purpose. A figure can be included show the results for a single day if required to demonstrate the positions of the latitude and longitude of the MIPAS and CALIOP measurements.

cc) P1819 L13: 'Yes, we agree, layer is probably not the best way to describe this and we have changed this accordingly

dd) P1828 Table 1: The calculations were performed for the FR MIPAS case using the original MW (as shown in the table). Analysis of MIPAS cloud data was performed for the full instrument mission, however, the case studies shown here are from the OR measurements.

ee) P1829 Table 2: In the RFM model, Water vapour continuum is represented by the "MT_CKD_1.1" continuum model. This information is now added to the table.

ff) P1830 Fig 1: We have taken these points onboard and have modified the figure accordingly.

Technical corrections:

P1796 L11 - expand 'UTLS' on first use (here rather than in L24*). We have expanded UTLS in the first instance.*
P1796 L11 - 'The newly established thresholds improve confidence ... as well as better characterised cloud distributions.'. We have changed this sentence to "*The newly established thresholds improve confidence in the ability of MIPAS to detect particle injection events and plume transport in the upper troposphere and lower stratosphere (UTLS) as well as better characterised cloud distributions."*
P1796 L21 'thresholds of 5 apply ... decrease rapidly...'. If the cloud index value is 5, then this is one (singular) 'threshold'. *This is correct, we have changed this in the text.*
P1796 L26-27 'Comparisons ... establishes' confusion with singular/plural. *This has been changed to avoid confusion*
P1796 L28 'cloudS and aerosol top heights' is changed to *'cloud and aerosol top heights'*
P1797 L6 'into the UTLS, and into the stratosphere'. The 'LS' part of 'UTLS' *is* the stratosphere. We have removed *'and into the stratosphere'*
P1797 L27 'manifest' is now *'manifests'*
P1798 L3 'ejected' is now *'injected'*
P1796 L25-29: 'The [CALIOP] ... offers ... making the most ...' grammatically inconsistent. This sentence has been revised to '*The Cloud-Aerosol Lidar with Orthogonal Polarization (CALIOP) lidar onboard NASAs Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observation (CALIPSO) offers vertical resolution better than 180 m and therefore provides the most detailed spaceborne measurements of clouds currently available (Winker et al, 2007)'*
P1801 L14: 'allow 'has been changed to*' allows'*
P1801 L16: 'upper stratosphere' is changed to *'thermosphere'*
P1802 L18: ';' is changed to *':'*
P1803 L3: recommends' is now *'recommend'*
P1803 L6: '215 and 20 hPa' is now *'215 to 20 hPa'*
P1803 L9: we have inserted *','* after 'lidar'
P1803 L15: 'the CALIOP' is now *'CALIOP'*
P1806 L12: 'As no cloud ... are included' is now *'As cloud and enhanced aerosol are excluded in the simulations..'*
P1812 L20 'CATHs' is now *'CATH'*
P1817 L19: 'partial columns of UTLS' is now *'partial columns of* SO_2*'*
P1820 L28 'statistical based' is now *'statistically based'*
P1829 Table 2: replace 'MIPAS-E' is now *'MIPAS'*

P1831 Fig 2. *We have replace semicolon with comma and overplotted a vertical line indicating a fixed CI=1.8 line.*
P1836 Fig 7 caption: ’Februay’ is changed to *’February*’. We will remove this legend.
P1837 Fig 8. We will remove this legend.