First of all, thanks to Prof Rob Wood, and an anonymous reviewer for taking the time to read and review our manuscript. Both reviews were constructive, and highlighted different areas which will substantially improve the paper. Below is an itemised list of responses to the comments.

RESPONSE TO REVIEW #1

Pg. 9772, ln 16
Reviewer Comment: This is a little damning. Does ECMWF assimilate COSMIC data?

Author Response: The ECMWF does assimilate GPS RO data. This statement is not a reflection on the COSMIC product as a whole, just on it's ability to determine moisture and temperature in the lowest 2.5 km in the vicinity of Macquarie Island. The GPS RO data has the largest positive impact on model performance in the upper levels in the tropics.

Author Changes to Manuscript: Added in section 2.2: 'In addition to the radiosonde data, the ECMWF also assimilates a variety of satellite observations which including GPS RO data.'

Pg. 9772, ln 21
Reviewer Comment: In what way does the occurrence of a seasonal cycle provide confidence about COSMIC's ability to detect PBL height? Is it because COSMIC's seasonal cycle is consistent with that from the sondes?

Author Response: A seasonal cycle is known to be present in PBL heights over the Southern Ocean from sparse radiosonde observations. Since COSMIC can detect this feature, and it is consistent with the radiosonde data, this provides confidence in the ability of COSMIC to reliably identify this PBL feature.

Author Changes to Manuscript: Added: '…similar to that observed in the radio sonde data, …'

Pg. 9773, ln 14
Reviewer Comment: Why does surface contamination impact passive cloud retrievals?

Author Response: CloudSat and CALIPSO both offers active satellite observations. MODIS provides passive observations. Hence, the CloudSat detection algorithm can not reliably separate returns from low clouds and returns from the ocean surface.

Author Changes to Manuscript: None.

Pg. 9775, ln 1
Reviewer Comment: What is it about clouds that causes a problem here?

Author Response: Using the humidity to define the PBL top is most sensitive to a decrease in humidity which commonly occurs over cloud top, leading to typically higher PBL heights when compared to other methods. See figure 3 from Seidel et al. (2010).

Author Changes to Manuscript: Added: 'There are large differences between different methods to calculate the PBL top in the presence of clouds, due to large humidity lapses over cloud top which can commonly lead to higher PBL heights when calculated from the humidity gradient.'

Pg. 9776, ln 19
Reviewer Comment: I wasn't aware there is a higher resolution dataset available. Is the actual resolution of the data as good as the resolution at which it is presented (3000/802 m)? Or is there considerable oversampling to reduce noise?

Author Response: The higher resolution COSMIC data, the 'atmPrf' data set is the full resolution vertical profiles. These are the raw measurements from COSMIC. In this study, no processing has been done to reduce noise.

Author Changes to Manuscript: None.

Pg. 9776, ln 24
Reviewer Comment: In some papers, this is presented as 200 km, others 100 km, and here 50 km. Which is it? The transects are only approximately horizontal with respect to the surface of the earth.

Author Response: This value is calculated from data within the lowest 3 km.

Author Changes to Manuscript: Added: '…which cover the lowest 3 km.'

Pg. 9777, ln 9
Reviewer Comment: but the boxes are significantly smaller than the resolution of the COSMIC profiles, so do you need to average more than one box together to appropriately scale match? Does this make a difference?

Author Response: Only one box from ECMWF was used. Hande et al. (2012b) indicate ERA-I data is consistent over large horizontal sections of the Southern Ocean. This would certainly be true for neighbouring grid boxes, therefore averaging over several grid boxes would make a negligible difference.

Author Changes to Manuscript: None.

Pg. 9777, ln 20
Reviewer Comment: Is this explored?

Author Response: The effect of the different measurement techniques is not explicitly explored in this work.

Author Changes to Manuscript: None.

Pg. 9777, ln 24
Reviewer Comment: But later, you show that ECMWF is better than COSMIC at producing the moisture profile.

Author Response: Correct, according to the analysis presented here, ECMWF is better than COSMIC at producing the moisture. However, when compared to other locations on Earth with better observational coverage, the exact location of cold fronts is prone to larger errors.

Author Changes to Manuscript: Added: '… compared to other regions with better coverage of observations…'

Pg. 9778, ln 1
Reviewer Comment: fig. 1 is rather low resolution (like it has been faxed).

Author Response: The image is taken of the Australian Bureau of Meteorology website (http://www.bom.gov.au/cgi-bin/charts/charts.view.pl?idcode=IDX0033&file=IDX0033.200705021200.gif), and there is no higher resolution available. But the point is to notice the location of the cold fronts, which can be identified.

Author Changes to Manuscript: None

Pg. 9778, ln 21
Reviewer Comment: Actually, this is fairly typical for most marine boundary layers globally. See e.g. Remillard et al. (2012) who analyzed lots of soundings over the Azores and found that most are somewhat decoupled with a clear transition layer below the main inversion.

Author Response: Thanks, the extra reference has been added.

Author Changes to Manuscript: Added: '… and is also commonly observed in other marine environments (Remillard et al., 2013).'

Pg. 9779, ln 14

Reviewer Comment: This is an important consideration. How often do these inversion structures occur?

Author Response: Statistic from Hande et al. (2012b) show that this 'well mixed ABL' structure only occurs around 18% of the time in radiosonde data. Most of the time it is a decoupled ABL.

Author Changes to Manuscript: Added: '… showed that this structure only occurs in around 18% of radiosonde data analysed…'

Pg. 9779, ln 27

Reviewer Comment: Might need to accept that COSMIC in these regions is more a detector of hydrolapses instead of the PBL itself.

Author Response: Yes, I agree. This is effectively what Chan and Wood (2013) showed, and this is the reason it may be troublesome over the Southern Ocean, where one could expect to find multiple significant hydrolapses in the lowest couple of kilometres due to multiple cloud layers.

Author Changes to Manuscript: Added: '… meaning multiple significant gradients in moisture may be present in the lowest few kilometres.'

Pg. 9780, ln 5

Reviewer Comment: Are the ECMWF data similarly averaged?

Author Response: Similar to the comment on pg. 9777, no averaging of ECMWF was done in this study.

Author Changes to Manuscript: None.

Pg. 9780, ln 8

Reviewer Comment: Is the small resultant number more constrained by the small box or by the lack of profiles extending below 500 m?

Author Response: There's three things that limit the number of cases: the time window (+/- 1 hr), the small box (2x2 degrees) and the requirement that profiles extend below 500 m, and they all work to limit the number of samples. We originally required soundings to occur within the same hour, however extending it to +/- 1 hr increased the number by more than double. Similarly, doubling the box to 2x4 degrees results in about another doubling of samples. And the requirement to penetrate to below 500 m further reduced the number. But this is obviously necessary if one wishes to study the ABL.

Author Changes to Manuscript: Added: 'The small sample is a result of three constraints: the time window, the size of the box, and the requirement to penetrate to below 500 m. Relaxing these constraints makes the sample bigger, but less relevant to the local conditions around Macquarie Island.'

Pg. 9780, ln 13
Reviewer Comment: I am confused because I thought that ECMWF temperature is used to constrain COSMIC WETprf temperature. So in panel (b) - note that panels should be labeled - why is the red line so far from the ECWMF temperature profile that is used to constrain T?

Author Response: According to the CDAAC data centre (http://cdaac-www.cosmic.ucar.edu/cdaac/cgi_bin/fileFormats.cgi?type=ecmPrf), the ECMWF profiles '…are used by the CDAAC post-processing system as a first guess to determine moisture below 10 km…'. This implies that the ECMWF profiles are not used to constrain the temperature in the 'wetPrf' COSMIC product. This has been clarified in the 'ECMWF Analysis' section.

Author Changes to Manuscript: Added: 'The process of 1–D variational analysis uses the estimates of moisture from the ECMWF data to produce the COSMICwet measurements.'

Pg. 9780, ln 22
Reviewer Comment: earlier, you implied that the well mixed layer is not the typical case for Macquarie.

Author Response: That is correct, and I believe there is no contradiction here. The well mixed layer is a typical boundary layer structure, however over Macquarie Island it only occurs around 18% of the time in radiosonde data. Most of the time (about 66%), there is a decoupled ABL with multiple low level inversions.

Author Changes to Manuscript: None.

Pg. 9780, ln 25
Reviewer Comment: Why is Figure 4 not mentioned here?

Author Response: The discussion relates to the stabily stratified profile shown in Figure 5. Figure 4 shows the case with multiple layers, and it was discussed in the previous paragraph.

Author Changes to Manuscript: None.

Pg. 9781, ln 15

Reviewer Comment: Were the profiles chosen to represent poor agreement, or were they chosen randomly? There is very poor agreement in all chosen cases. Based on this, I would conclude that there is little skill in COSMIC's PBL detection in this region. Hydrolapses yes, but PBLs no. I guess this is the main point of the paper.

The results also suggest that ECMWF has little skill at representing the PBL temperature or moisture. The differences in temperature between ECWMF and the soundings are 3 K or more for every profile shown (except the last one).

Author Response: The profiles were choses to show the typical structures found over the Southern Ocean, not to show poor agreement. On pg 9780, line 10, we state: '… however to emphasise the typical ABL structures encountered over the Southern Ocean, only the results from four profiles will be shown.' It just so happens that in the four examples chosen, COSMIC show poor skill in identifying the ABL height. I believe you state it nicely, COSMIC can identify hydrolapses, but not necessarily the ABL top.

Author Changes to Manuscript: None.

Pg. 9783, ln 17

Reviewer Comment: I'm not sure what is really gained from presenting tables as well as profiles for the four cases. It would be better to statistically analyse all the data together (all 35 profiles).

Author Response: Table 2 combines the best of both options. The statistics are shown for the four profiles, Fig 2 – Fig 4, as well as the RMSE_total calculated from the 35 co-located profiles. This way, one can get a visual indication of how representative these statistics are of the individual profiles, as well as an idea of the whole sample of 35 profiles.

Author Changes to Manuscript: None.

Pg. 9784, ln 24

Reviewer Comment: These should be the best. Are the authors convinced that they have matched the profiles appropriately?

Author Response: These well mixed ABLs should be the best for the ABL top detection, which appears to be true if one considers the excellent agreement between the ABL tops detected for Figure 3. Here, we are considering the reproduction of the temperature and moisture profiles. Considering the well mixed ABL in Figure 3 again, one can see the temperature and moisture profiles are not captured by COSMIC, mostly because the main inversion is not resolved. Compare this to Figure 5 with no significant inversion, and COSMIC performs very well in reproducing the temperature and moisture profiles.

Author Changes to Manuscript: Added: '… the four worst performing profiles in reproducing the moisture and temperature mostly represent…'

Pg. 2785, ln 15

Reviewer Comment: Figures 6 and 7 paint a completely different picture, suggesting good skill at identifying both primary and secondary inversions. There is a real disconnect in the paper between the profiles and the larger statistics.

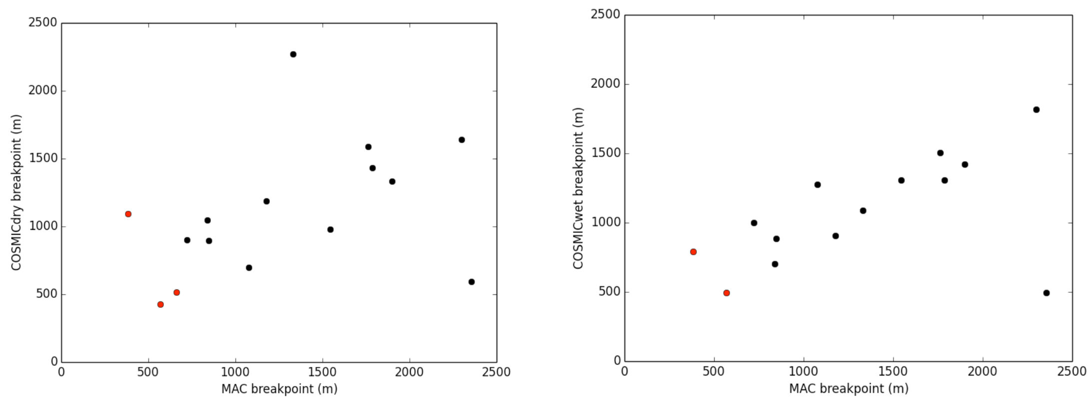
BTW, COSMIC is not detecting the inversions themselves, so this is probably not the appropriate terminology.

Can you make a scatterplot of one vs the other? Or is there no attempt to temporally match the profiles?

Author Response: The differences between the individual profiles and the larger statistics has already been alluded to in the conclusions on pg 9788, last sentence: 'This analysis shows that the COSMIC data product is most useful when analysed statistically on seasonal, or longer, timescales'.

Thanks, the other reviewer pointed out the same thing with the terminology. This has been cleaned up throughout the manuscript. In regards to the scatter plot of MAC vs. COSMIC main break points and secondary break points, the figures appear below (Black dots: main break points, Red dots: secondary break points).

The first thing to note is that there are very few profiles where the inversions temporally and spatially match, but from this you can say that there appears to be no significant bias in measuring the break point heights. This has already been stated on pg 9782, line 21: 'and there appears to be no systematic difference in height between the two COSMIC data products'. This sentence has been modified to explicitly include the MAC data.



Author Changes to Manuscript: Added: '…and there appears to be no systematic difference in height between the two COSMIC data products, or the MAC data set.'

Pg. 2786, ln 3

Reviewer Comment: Why is ECMWF not included in Table 3?

Author Response: The vertical resolution of the ECMWF product is too low for a 300 m sliding window to reliably compute the break points.

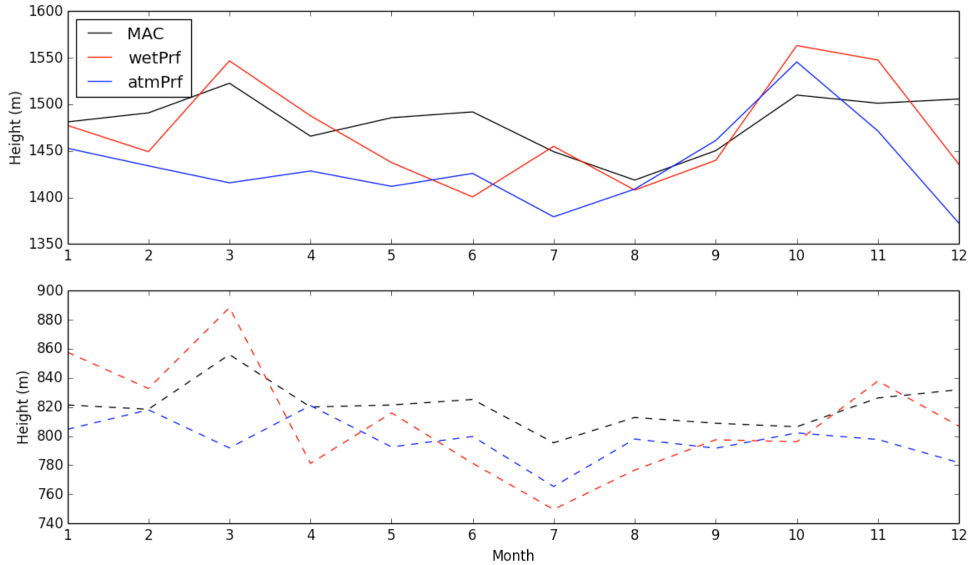
Author Changes to Manuscript: Added: 'Note that the ECMWF product is not included here because the vertical resolution is too coarse to reliably compute break points.'

Pg. 9786, ln 28

Reviewer Comment: Table 4 would be better presented in graphical form (seasonal cycle).

Author Response: No problem. But that means we can't include the diurnal cycle information, but we have already briefly described it in the text.

Author Changes to Manuscript: Removed: Table 4.
Added: Figure 8:
Added: 'The cycle is less notable in the secondary break points. December is a notable exception for main break points in both COSMIC products, but nevertheless, the three month average still preserves the seasonal cycle.'



RESPONSE TO REVIEW #2

Section 2.1:

Reviewer Comment: The authors need to review Kursinski et al. (1997) section 2.2, Atmospheric Bending and Refractive Index Profile Retrieval: Theory. In particular, Kursinski et al. (1997) eqn (1) and/or eqn (2) which show that the measured refractivity profile is derived as a vertical integral (starting from the top of atmosphere) from the observed RO bending angle. The RO bending angle is locally (in height) sensitive to the vertical change in refractive index. This is the basic RO concept which the COSMIC network exploits. The equation shown by the authors as eqn (1) on page 6 is misleading because it shows how the measured refractivity (on the left) should relate to atmospheric thermodynamic variables. The authors then skip over the "dry" temperature product to discuss the "wet" 1D-var product. There are too many issues being glossed over quickly without making the important points. The interpretation of the results later suggest that the authors are not clear themselves about the data they are studying and how they are derived.

I suggest that the section on COSMIC data (section 2.1) needs to address three basic (and relevant) topics;

1) The actual equation used by COSMIC to measure raw refractivity from bending angle as given in Kursinski et al. (1997) eqn (1) and (2). Here the point needs to be made that the RO observation is of bending angle (which is known precisely from the satellite orbital knowledge) and that a refractivity profile is derived using an integral computed over the vertical coordinate. This explains why the authors method of computing the vertical derivative of the COSMIC "raw" refractivity works as well as it does. The local

vertical derivative of the COSMIC refractivity gives the change in bending angle at that height, which is the fundamental COSMIC measurement. That is, the authors are essentially undoing the COSMIC refractivity calculation to get back to the local bending angle profile. Presumably, when the atmospheric boundary layer has characteristic thermodynamic shape that bends the RO signal, then the integral equation used to derive COSMIC refractivity shows this as a change in height. When the boundary layer does not produce a vertical gradient in refractive index, then the COSMIC RO signal will not be bent and thus the top of the BL will be invisible to the RO detection method. I think this explains the good height results show in the last section curiously titled, "Local statistical evaluation" and also the reduced frequency. The RO method is only sensitive to profiles that "bend" the radio waves, no bending means no RO sensitivity. This suggests that it might be even more accurate to look at the bending angle profiles themselves.
2) The theoretical dependence of N thermodynamic variables on p, T, and e (given in this paper as eqn (1)). Following the current eqn (1) there needs to be a brief discussion of the ambiguity between Temperature and Water Vapor in the refractivity equation. It is not possible to derive the temperature without independent knowledge of the water vapor or vice versa. This is a fundamental limitation of the current RO method. The "dry" temperature is only valid in the stratosphere and upper troposphere where the water vapor partial pressure can be neglected in eqn (1). There are many references that could be given on this point and this motivates why the 1-D var method is necessary.
3) The discussion of the "wetPrf" can then be made. The current discussion is inadequate in that it does not specify which ECWMF fields are used in the 1-D var analysis and in particular if the MAC soundings were included in the background field through data assimilation or not. A paper reference to the 1-D var COSMIC method should be included as well as the COSMIC data version used. Typically the ECMWF temperature is used in the troposphere as truth and the COSMIC data is used to retrieve the water vapor concentration profile, but it's not clear if that is what was done in the dataset being analyzed. Some clarification of this point would be helpful to the later discussion.

Author Response: The discussion in the 'COSMIC' section was reordered so the discussion of the COSMICdry product is before the COSMICwet product, therefore being more logical. Some details about the data sets have been added.

Author Changes to Manuscript: The new section is a follows:
'The Constellation Observing System for Meteorology, Ionosphere, and Climate (COSMIC)/Formosa Satellite 3 (FORMOSAT-3) is a constellation of six identical micro satellites each carrying a GPS RO receiver (Anthes et al., 2008). This allows for around 1500–2000 soundings per day around the globe, from 2006 onwards. The COSMIC measurement process is outlined by Kursinski et al. (1997), and summarized below. The primary measurement is of the Doppler shift of a radio signal that is emitted from the GPS satellite, occulted by Earth's atmosphere, and received by the Low Earth Orbiting satellite on the opposite side of the atmospheric limb. From this, a bending angle is derived, then the refractivity can be computed as a vertical integral of the bending angle from the top of the atmosphere down. The limb scanning geometry, either rising or setting occultations, is produced by the relative motion of the two satellites.

The raw measurements of the refractivity are used to identify the ABL height using the common technique of identifying break points in the refractivity profile, to be discussed below in section 3. This technique amounts to computing the vertical derivative of the refractive index, essentially reverting back to the bending angle measurement. This "atmPrf" data set contains approximately 802 data points in the lowest 3 km, and will be referred to as the COSMICraw data set, where appropriate.

In the neutrally charged atmosphere, the measurements of the refractive index (N) can be related to the pressure (p), temperature (T), and water vapour pressure (e) by:

N = 77.6p/T +3.73x10^5 e/T^2

where T is in Kelvin, and p and e are in hPa. With the aid of the hydrostatic equation, (1) can be used to estimate vapour pressure if temperature is specified, and vice versa. In this study, the "wetPrf" product was also used, which combines the raw observations with moisture information from the ECMWF TOGA 2.5 analysis using 1-D variational analysis. The ECMWF analysis product is used as a first guess for moisture below 10 km. The resulting profile is interpolated onto 100m levels to produce the "wetPrf" profiles. Therefore in this dataset, information on moisture and temperature is available at the expense of the high vertical resolution available in the raw refractivity measurements. The refractivity used in the "wetPrf" data set is the analysed refractivity, not the raw measurements. There are, on average, 31 data points in the lowest 3 km of the profiles over the Southern Ocean, and 400 levels available for the whole sounding. For the sake of clarity, this data product will be referred to as the COSMICwet data set. The profiles are constructed from 50km long horizontal transects. Therefore, the profiles from both COSMIC products would represent an average of the conditions over this line.'

Section 2.2

Reviewer Comment: My main complaint in the description of ECMWF analysis as "data" and the use of the word "independent" in this section and elsewhere in the paper. I am not familiar with the details of the ECMWF data used in this study of the Southern Ocean, but I would be very surprised if the MAC soundings were not already assimilated into the ECMWF analysis, especially given the sparse sampling in the southern hemisphere. This point needs to be clarified. Two options are possible; 1) MAC soundings have been denied the ECMWF analysis somehow, or 2) MAC soundings are included in the ECMWF analysis. In either case the issue of independence of ECWMF needs to be clarified. If the MAC soundings are already included in the ECMWF analysis then the comparison back to ECMWF is not an independent check at all, rather it's a measure of the "goodness of fit" of the ECWMF data assimilation methodology which attempts to fit smooth temperature and moisture fields to point measurements. If the MAC soundings are not in the ECMWF analysis fields then the authors should state what data is going into ECMWF assimilation to clarify what is meant by "independent measurments". I recommend that this section be removed from the "data" section 2 and placed into a separate NWP section with some references and discussion of the relevant issues for this paper.

Author Response: The discussion here has been added to in order to clarify details about the analysis product and its independence from other data used in this study.

Author Changes to Manuscript: The new section is as follows:
'The European Centre for Medium-Range Weather Forecasts (ECMWF) has a number of data products providing global coverage of various atmospheric variables. The ECMWF TOGA 2.5 global Upper Air Analysis data set (ECMWF, 1990) is used here to understand the influence of the background data in the 1-D variational data assimilation. The process of 1-D variational analysis uses the estimates of moisture from the ECMWF data to produce the COSMICwet measurements. This analysis data set is not independent of the other data sets considered here. The Macquarie Island radiosonde profiles are used in the assimilation process, however their contribution to the reanalysis data is weighted depending on the error characteristics of this data set. These error characteristics are determined by comparing these observations to others in this regions, mostly from satellite based platforms over the Southern Ocean. When there is agreement between the various observations, the radiosonde data receives a higher weighting, and vice versa. These radiosonde data will be used for the evaluation of the thermodynamics for a limited number of cases. In addition to the radiosonde data, the ECMWF also assimilates the GPS RO data used in this study. Atmospheric profiles from ECMWF represent box averaged quantities. Thus, these profiles represent the regional conditions over a larger portion of the ocean than the radiosonde profiles.'

<u>**Section 4.2**</u>
Reviewer Comment: Thermodynamics Are MAC profiles assimilated into the ECMWF analysis? If so, explain why the ECWMF-MAC error is not zero. Does COSMICwet use the same ECMWF analysis as it's background, or is it using something else such as a reanalysis? The statement that ECMWF analysis gives the smallest RMS error is likely explained by that fact that MAC soundings are already assimilated into the ECMWF and thus should not be used as independent data when compared to MAC. Added to this is the extremely small sample set of 35 COSMIC matchups over the course of eight years. This section is by far the weakest in the paper but it does illustrate the potential problems of deriving thermodynamic profiles from COSMIC measurements. Rather than drawing any conclusions however, I suggest that this section be combined with section 4.1 and the combined section be described as "case study investigations and discussion".

Author Response: Section 4 has been renamed to 'Case Study Evaluation of COSMIC'. The ECMWF-MAC error is not zero since the assimilation process uses more than just the MAC data. The COSMICwet product uses the TOGA 2.5 analysis product, the same as the one presented here and described in section 3. The authors recommend keeping section 4.2 separate from section 4.1, in order to highlight that the analysis of the thermodynamics is completely separate from the analysis of boundary layer heights.

Author Changes to Manuscript: Added: 'This is likely due to the fact that the MAC soundings have been assimilated into the ECMWF data set used here.'

In the conclusions: ‘This illustrates the potential problems of deriving thermodynamic information from COSMIC data.’

<u>Section 6</u>

Reviewer Comment: I have no objection to the conclusions as stated however I think that some of the questions raised about the results could be lifted if my explanations are adopted by the authors. The results actually seem quite sensible if you think about what the RO product is measuring and the strengths and weaknesses of the COSMIC measurement. Surely it’s pretty important that COSMIC can to obtain accurate BL heights over the Southern Oceans even if it is limited to cases where that BL top causes measurable bending.

Author Response: In light of the reviewers comments, some minor clarifications in the conclusions have been made.

Author Changes in Manuscript: Added: ‘However, when co-located profiles from COSMIC and MAC both identify break points, the heights of the break points mostly agree.’
‘The favourable agreement in terms of height is likely due to the break point detection algorithm using the vertical gradient of refractivity to identify break points. This essentially reverts back to the change in the bending angle, which is the fundamental COSMIC measurement.’