Responses to reviewers, ACP-2010-431 "An aircraft case study of the spatial transition from closed to open mesoscale cellular convection over the Southeast Pacific" by R. Wood et al.

Review comments in black. Responses provided in red

Responses to anonymous Referee #1

This manuscript provides an extensive description of the boundary layer, aerosol, cloud, and precipitation variability across a well-defined boundary between closed and open cells in the south-east Pacific. It extends our knowledge of the physical variations across the boundary between these two types of mesoscale convection, and provides a plausible conceptual based model for understanding this transformation. As such, this study provides a valuable baseline on which to base and evaluate numerical simulations of the spatial transition using models of varying complexity. Although the major elements of this manuscript are acceptable as presented for publication, several issues should be addressed to help clarify the presentation and further support the conclusions. Although the manuscript is generally well written, it is much longer than the attention span of the typical reader (and reviewer). In its current form this contribution would be on the order 25-30 pages in a traditional journal. A reduction of about 25% in the length and the number of figures would help focus some of the very detailed discussions given and force a more streamlined synthesis of the results.

We thank the reviewer for their constructive and thoughtful review. We provide point-by-point responses below. We agree that the manuscript is relatively long, but feel that the detail is warranted since many case-study papers do not provide sufficient information. This is especially important for those papers that are being used by the modeling community (as this one is). The editor appears to agree with this.

Major Issues:

Abstract: A shorter more succinct abstract would provide a better summary of the principal findings of this work. The more speculative conclusions (see below) should be removed unless properly supported in the main text.

Agreed. I removed the final sentence, i.e. "Since turbulent intensity, and presumably entrainment rate, in the overcast cloud layer is much stronger than in the POC, this implies differences in subsidence rate at the top of the MBL that are likely caused by compensating circulation above the top of the MBL.", and instead discuss it in the manuscript.

The possible contributions of the diurnal variability to the differences in the C-130 flight made in the early morning and the BAe-146 observations made in the late afternoon (12 hours later) needs to be more thoroughly addressed. There is some mention of this point in the text, but little in the discussion of the possible impact of any diurnal variability. Is there any variability observed in the GOES satellite images that could address this issue?

This is an important point. However, it is very difficult to determine the impact of diurnal variability and what is non-diurnally related evolution of the POC-overcast boundary region from satellite data alone. There are no studies documenting the diurnal evolution of POCs, although we do know that their formation (first emergence within overcast stratocumulus) tends to occur at night, see Wood et al. 2008, JGR). It is highly likely that there is a diurnal cycle, since thin clouds are more likely to be affected by the absorption of solar radiation during the day. Certainly, we know that over the SE Pacific region in general, cloud cover reduces quite dramatically during the day and increases to almost complete cloud cover during the night (e.g. Abel et al. ACP, VOCALS special issue), but since POCs only occur perhaps 10% of the time (see Wood et al. JGR), it is not clear whether their diurnal cycle is the same as that in the more commonly observed closed cell stratocumulus. I should note that since the considerable high cloud contamination in this particular case precludes an objective study of the diurnal cycle from satellite data in this case.

In response to the reviewer's request, I have included a brief discussion of these points in the revised manuscript, in Section 2.1:

"Despite considerable diurnal variability in the clouds near the coast and in the far SW of the region shown in Fig.~\ref{satellite_time}, in the region of the POC feature itself there is no clear evidence of diurnal variability. There are two key points that make it difficult to determine diurnal variability in this case: (a) high clouds are masking our ability to clearly delineate from satellite the evolution overnight on the 27/28 October; (b) aircraft sampling issues including their location with respect to the POC-overcast boundary make it difficult to separate diurnal variability from evolution of the POC feature independent of diurnal variability."

There are contradictory statements about the possible role of precipitation in the formation and maintenance of the POC. In the abstract the authors state that "Mean cloud-base precipitation rates inside the POC are several mm day−1, but rates in the closed cell region are not greatly lower than this, which suggests that precipitation is not a sufficient condition for POC formation from overcast stratocumulus". But in pages 11-12 the results presented indicate that in the overcast region, cloud base precipitation rates of about 2 mm day-1 are shown to be present in about 25% of the cloudy columns and those with Zmax as high as 10 dBZ (about 10 mm/day) corresponds to 2% of the columns. In the POC, however, these heavily precipitating regions of 10 mm/day occur at a rate that is 3 fold higher than those in the solid cloud and in the transition zone the occurrence of heavy drizzle is reported to be about 15 times higher than that in the closed cell. Thus, the meaning and intent of the statement in the abstract is unclear, since there seems to be substantial differences in the character and the extent of drizzle in the solid, POC, and transition region.

Some clarification in the abstract would be useful, since at the end of section 3.3 the authors state that".. there is a fundamentally different nature to the precipitation inside the POC compared with the surrounding overcast clouds, with a broader distribution of Zmax and locally stronger precipitation." This point is also relevant concerning the discussion given on page16 in the first sentence of first paragraph where the authors indicate that "A striking feature, observed with both in-situ and radar data, is that the mean precipitation rate at the cloud level in the overcast region is significant (3-4 mm day−1) and about three quarters of that in the POC (4-5 mm day−1)." What is the basis for this assertion and the statement in the abstract? This may be discussed earlier in the text, but it appears to be at odds with the discussion on pages 11-12.

The point we are trying to make here is that, since the overcast region is remaining overcast and yet can support mean precip rates of > 2 mm/day, drizzle itself cannot be a sufficient condition for transition to open cells. This statement isn't conditional on knowledge of the rates in the open cell region since this is the result of the transition which almost is from closed to open cells. However, as the reviewer points out, we do quote the rates in the open cells when making the "not a sufficient condition" claim, which is unnecessary. To address this I changed the abstract text to:

"Mean cloud-base precipitation rates inside the POC are several mm\,d^{-1}, but rates in the closed cell region are not greatly lower than this. This latter finding suggests that precipitation is not a~sufficient condition for POC formation from overcast stratocumulus"

Once the transition has occurred, it certainly does generate differences in the nature of the precipitation, with stronger and less frequent precipitation in the POC. However, we feel that this is a response to the transition and is not particularly important for its cause.

Page 13: Although the surface sensible heat fluxes reported as less than 15 Wm-2 are small, the contribution of the moisture flux to the virtual sensible heat flux is substantial and helps give a virtual heat flux of 25-30 Wm-2. Although this is still less than the 70-90 Wm-2 nighttime radiative flux divergence at cloud top, during the daytime it may be of similar importance to the energetics of the boundary layer as the radiative forcing.

This is a valid point, and we thank the reviewer for pointing this out. This virtual heat flux is probably important for maintaining the cumulus clouds that feed the upper stratocumulus layer in the POC. We have included a statement about the potential importance of the virtual heat flux in the POC. We now add:

"Surface estimated sensible heat
fluxes (SHF) are small (${<}15$\,W\,m^{-2})
throughout. However, the estimated latent heat flux (LHF), however, is
large (115--130\,W\,m^{-2} on B409 increasing to
122\,W\,m^{-2} in the POC and 160\,W\,m^{-2} in the
overcast on RF06). This contributes an additional 8-10~W~m^{-2} to
add to the SHF, giving surface virtual heat fluxes of 20-25~W~m^{-2} which
is not an insubstantial surface contribution to buoyant production. "

On page 14 It is unclear how the mean boundary layer depth is determined. It appears to be based on the soundings shown in Fig. 7, since no radar or lidar estimates are available for the BAE146; But the values are reported as averages; thus it is unclear if more that the two soundings (one from the POC and the other from the solid cloud) are used to make this average. If this is the case, the sample size may be insufficient to show that the difference (_ 100 m) between the two flights is significant or that the single sounding in each area for each flight is sufficient to discuss any differences or similarities. Radar cloud tops will only be available from the C-130 flights. If soundings were used, how significant are the differences in the heights between the two flights?

We used soundings (a) to insure consistency between flights; (b) because the cloud top heights occur slightly below the inversion base (see e.g. Table 4 which shows cloud top heights 30 m below inversion base heights in the POC). On RF06, there were considerable profiles through the inversion (sawtooth leg) and they gave little spread, so that we are quite confident of the value. On the 146 flight, as the reviewer points out, there were only three inversion-crossing soundings. These gave values of 1295m and 1270m, hence the estimated value of 1280 m given in Table 3. Thus, since these are within 25 m of each other (as might be expected from behavior of strong inversions which tend to equalize with fast gravity wave responses), we are fairly confident in our conclusion. However, the sampling is not good on B409 and it would be more comforting to have 3 or 4 more profiles.

We changed the manuscript to:

"The most marked difference between the two flights is the MBL
depth which increases from 1280\,m on B409 to 1375\,m on
RF06, with uncertainties in these estimates being some 30~m
determined from inter-profile variability."

Is there any horizontal variability within the POC and closed cell regions? Some clarification of these points is warranted. There is no reference to the soundings or the other observations that might have been used to develop the discussion here. Some of the results from the C-130 radar observations might help in establishing the uncertainty. Thus, it might be useful to move this discussion to a point after the observations are discussed.

There is some variability in inversion base height, roughly 30 m between different soundings. In the POC (and to some extent the overcast region), radar observations show cloud top height variability that is considerably greater than that indicated in the inversion base height from the soundings. This is shown in Fig. 11b. In the POC, cloud top does not always coincide with the inversion base height. There are occasions with clear slots above very thin cloud and still in the MBL. The thick and active cumulus clouds also push up the inversion very locally. I am therefore not convinced that the radar variability informs us about the true inversion base variability.

The analysis and discussion of the entrainment processes includes some speculative aspects that have not been identified as such. Energy and moisture budgets are used to estimate entrainment rates for the combined POC and solid cloud area. It is unclear what these results add to the manuscript. This estimate of the entrainment rate has to do little with the differences in the entrainment rates between the solid cloud and the POC.

The differences in entrainment rate between the overcast and POC regions are very important, but as the reviewer correctly points out, we cannot determine these from the available observations with any certainty. However, we also believe that the entrainment rates for the overcast-POC boundary region are also useful for constraining models that are now being run to attempt to simulate the boundary. These are useful not only as a dynamic constraint but also for a constraint on the rate of supply of CCN from the free troposphere, which has been proposed as a mechanism for replenishing CCN against coalescence scavenging losses. I think the fact that the inversion in the POC and overcast regions rises at approximately the same rate indicates that the difference between entrainment rate and subsidence rate is the same in both the POC and overcast. This may be important, e.g. see paper by Bretherton et al. in JAMES. http://james.agu.org/index.php/JAMES/article/view/v2n14, which is now cited in the revised manuscript.

The vertical velocity variance estimates made in the two regions is used to infer that the entrainment rate in the POC is less than that in the solid cloud. However, the turbulence in the POC may be substantially less homogeneous in nature than that in the solid cloud, since the strong updraft and downdraft elements may occupy a much smaller area than in the solid cloud area, which may affect the significance of the vertical velocity variance reported due to poor sampling statistics. If the significance of entrainment estimates and the inversion height uncertainty for the POC and solid cloud areas cannot be established, then the speculation that the subsidence over the POC may be less than that over the solid cloud should be eliminated.

The reviewer has a valid point. We now point out the sampling errors mean that these variance estimates are somewhat uncertain. However, even with the lack of a good error estimate for the vertical wind variance (estimating these errors requires knowledge of the third moment!), we still think that it is worth speculating about the differences in subsidence, since there is additional model support that this is indeed the case (see refs in revised manuscript).

In Section 6 the discussion of the aerosol characteristics and variations between the POC and the solid cloud is very long and tedious. In the end it is unclear (or at least one looses track) of what has been learned from these observations about the aerosol and characteristic in the POC and the solid cloud. There is very little given in the conclusions on this point. A good synthesis of the results with fewer details would be useful. The speculative statements that are in this section might work better in the conceptual model discussion.

We disagree with the reviewer's assertion, since there are to date almost no measurements detailing the aerosol properties inside and outside of POCs. Whether the description is tedious might reflect the reviewer's own interests, and I am reluctant to change this section given that it took a lot of back and forth with the coauthors to get it into shape. The existence of an ultraclean layer is a rather new finding (although we have seen hints of it in previous as yet unpublished studies).

Despite a very long and detailed section entitled Discussion and Conceptual Model, the Conclusions section is relatively short (shorter than the abstract). In both sections there is relatively little discussion of the aerosol observations that was discussed extensively in the text of the manuscript.

This is a valid point, and we have now added some more discussion about the aerosol in the conclusions section

Minor Issues:

Overall the writing is well done; but since some major reduction in the length of the manuscript is needed, only limited comments are provided on minor editing issues that can be addressed in the final version.

Section 2.2: It might be better to start with a full paragraph that includes an introductory sentence etc. The one sentence paragraph that starts this section is informative, but reflects a style more appropriate for a research report rather than a formal publication. This style is also reflected in the way the results are presented with more detail in some cases than required and th inclusion of some results that are not relevant to the focus of the manuscript.

We incorporated the first sentence into the following paragraph. With case studies, there is a tendency for them to more closely resemble research reports than other types of formal publication. Nevertheless, we believe that this is useful since many “research reports” are never written and will therefore not be available for the community. Model studies based on case studies often bring to light details that may not have been considered important initially. Hence, we opt for the inclusion of detail for completeness. With online publishing, there doesn’t seem to be any real need to insist on short papers in all cases.

Page 8; first paragraph: It would be useful to give the general nature and time of the C-130 flight in the same manner as given for the BAE-146 flight.

This is given, but in the next paragraph. We actually now merge the two together into one paragraph to avoid confusion.

Page 12, Sec. 3.3: The second sentence is unclear, since 60% from the more sensitive WCL is not substantially less than the 55-60% reported from the radar.

This is a valid point, and one we should’ve noted earlier. Indeed, the radar does appear to see most of the clouds captured by the lidar within the POC. We clarify this now.

Page 15; Sentence at bottom of the page is incomplete.

Reworded to avoid confusion

Page 23; last sentence of second paragraph; Sentence needs to be edited to eliminate an extraneous verb.

Done