Response to reviewer #1

We thank the reviewer for the constructive comments that have helped to improve the paper. Below is a list of responses to each of the reviewer's comments.

The only main concern is with the title. This is a very general title but with a degree of specificity that may not attract a wide range of readers who are not familiar with either POCs or VOCALS REX. Not sure that the number of POCs studied adds much information. Also, would it be better to give a geographical location? The addition of some key words like aerosols, precipitation, clouds, and boundary layer would be useful in making a more interesting title that might be more enticing to a larger audience.
We agree with the reviewer and have modified the title to ***Aircraft observations of aerosol, cloud, precipitation, and boundary layer properties in pockets of open cells over the southeast Pacific***.

Minor Points:
Page 8292; line 23-24; What is the length (or duration) of the level legs? From Fig. 1 this looks to be about 150-250 km. This information is important for the vertical velocity variance estimates given later. A few words could be added here to clarify.
The following sentence has been included in Page 8292, line 25 after "cloud (~1500 m).": Level flight legs ranged in length from 120 to 240 km.

Page 8297; line 23-24: Confusing sentence construction. As written it reads as "evolution" caps "the cloud top height"'
The sentence has been modified to: "A critical parameter in marine boundary layer (MBL) evolution is the inversion height, which determines the cloud top height and affects the likelihood of boundary layer decoupling (Jones et al., 2011).

Page 8297, line 25: Although the difference in temperature and water vapor can cause some bias in the retrieved SSTs, this effect should be minimal when using the Heimann since it makes measurements in the window region (9.6-11.5 microns). But another potential reason for a cold bias would be the small IR reflection from the sky, that would be less in the POCs than in the overcast areas. For an IR sea surface reflectivity of 0.01 (emissivity of 0.99) and a 50% cloud fraction, there could be a cold bias in the SST of about 0.5 ^{o}C compared with a completely overcast case, since the clear-sky downward emission is less than that from a low solid cloud. More can be found in the literature about this point.
Thank you for suggesting this explanation. We have tested the reviewer's idea that the Heimann retrievals of SSTs are affected by the reflection of IR from the clear and cloudy areas by examining how well the retrieved SSTs correlated with the downward IR irradiance retrieved by the upward pointing radiometer. Indeed, the SSTs do correlate somewhat with the downward IR irradiance. We have also found that precipitation also affects the SST retrievals. However, given the data available to us, we are unable to easily quantify the effect of the downward irradiance on the SST retrievals. Therefore, we have replaced the sentences on Page 8297 L5-10 with the following sentences: "However, the apparent SST decrease in the POC is likely a sampling artifact. First, the approximately 1K decrease in the subcloud temperature and 1 g kg^{-1} increase in the water vapor mixing ratio in the POC can lead to a low bias in the SSTs retrieved over the POC. Second, the decrease in cloud cover in the POC can decrease the amount of reflected infrared radiation, also leading to a low bias in SSTs retrieved over the POC. Indeed, when the retrieved SST and downward infrared irradiance are compared within the POC segments of the flight legs, they somewhat correlate with each other."

Page 8297, line 25: Temperatures should not be described as hot or cold (just like vapor pressure would not be characterized as moist or dry). Air or other substances can be given attributes such as hot, cold, heavy, light etc. but temperature, pressure, mass etc. should not. In this sentence it would be better to write the lowest temperature rather than the coldest.
Done.

Page 8300; It is unclear how the vertical velocity variance is calculated. In Table 3 there is a quantity $CL\ w'^2$ that is not defined in the table description. This appears to be cloud layer vertical velocity variance described in the text. Some clarification should be included in the text and the table descriptor. Are the lengths (or duration) of the legs the same? Since there may be substantial mesoscale variability in the POCs, sampling times may be an issue. Are trends removed? Or is just the mean removed in the calculation? Some clarification would help. In Fig. 4 a vertical velocity variance from a 21-second running (mean?) is plotted.
It is indeed unclear at this point. We have just removed the mean to calculate the variance in w. To clarify, the following sentence is inserted in P8300 L5, after "measured within cloud-layer legs.": "The cloud-layer leg w'^2 is calculated by calculating the variance in the vertical velocity in each of the POC and overcast segment of the cloud-layer flight leg."

Page 8303; lines 8-10: Awkward sentence construction.
The sentence has been revised to: "Because the increase mainly occurs in the size range of smaller drops, the larger concentrations in the POC do not contribute to large differences in the precipitation rate."

Page 8303; line 16: Still unclear how the vertical velocity variance is calculated. Here this may be the 21-second running mean describe in the Table 4 descriptor.
We agree with the reviewer that it is unclear what is mean by w'^2. Therefore, we have added the following sentence in line 19 after "0.03 $m^2 s^{-2}$ is used.": "Here, we calculate the w'^2 as the 21-s running mean w'^2."

Page 8306; lines 14-17: Long sentence is not very effective.
The sentence has been modified to read: "Whereas the processes that determine the sources and sinks of accumulation-mode aerosols appear to have a clear vertical structure, the Aitken-mode aerosols appear to be modulated by a variety of processes that are not as well constrained across different POC cases."

Page 8306; section 6.3: The aerosol equilibrium solution that the authors are seeking follows the simple treatment of cloud-aerosol-precipitation interactions made by that Baker and Charlson (1990) to give bimodal solutions. Their study should probably be cited.
We have included the following sentence in line 22 after "relatively constant concentration.": "Such a stable equilibrium at low aerosol concentrations is suggested by the modeling study of Baker and Charlson (1990)."

Page 8307; lines 7-9: Could remove "at the surface" from this sentence to simplify. Also are the references to the surface layer in this section really references to the subcloud layer? In boundary layer meteorology there is usually a distinction made between the surface layer and the sub-cloud (wellmixed)layer.
We have removed "at the surface". We have also replaced "surface layer" with "subcloud layer".

Page 8307; paragraph starting at line 10 is very long and would be much more effectively if it were split into two or three paragraphs.
Agreed. Paragraph breaks were included after Page 8307 Line 18 "(see Table 6)." and after Page 8308 Line 2 "2.6 cm s^{-1}."

Figure 3; a word or two is apparently missing in the last sentence of the caption.
We apologize for the omission. The sentence now reads: "Instead of the inversion base height, the mean height of the maximum vertical gradient in potential temperature is chosen for the reference height so that the temperature changes in each area are better aligned."

Response to reviewer #2

We thank the reviewer for the comments to the paper. Below are our responses to the reviewer's comments.

Comments
To me the main remaining difficulty in explaining POCs is understanding the aerosol budget. Section 6.3 attempts to solve this, but is unsuccessful. The main problem is that the source term from the modeled sea-to-atmosphere flux is much more variable than the sink terms. Ultimately that means that the extremely simplified approach is not suitable to describe the many processes leading to the hypothesized equilibrium aerosol state.

We agree with the reviewer that the proposed budget equation appears to be too simple to describe the various processes involved in determining the equilibrium aerosol state. We have expanded on the sentence on Page 8308 Line 3-4 such that it now reads: "There are several possible reasons for this discrepancy in addition to the simplicity of the budget in Eq. 1."

The LES modeling study of POCs by Berner et al. (2014) also exhibits an equilibrium aerosol state, so further tests with LES will likely be necessary to diagnose whether a feedback mechanism exists that keeps subcloud aerosol concentrations relatively constant across different POCs. This is included in our Discussion and Conclusions section.

pg. 8307: Integrated over the size distribution of the PCASP: I think the authors mean integrated over the same size range as sampled by the PCASP.
Done.