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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Southeast Pacific atmospheric composition and variability sampled along 20 S during VOCALS-REx" *by* G. Allen et al.

G. Allen et al.

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Final author response for Allen et al., 2010, ACPD

We thank both reviewers for taking the time to provide a constructive and thorough review of our study of atmospheric composition properties in the South East Pacific during the VOCALS project and for recognising the scientific merit to our work in the context of providing new insight into longitudinal gradients of key atmospheric pollutants to link to complimentary studies investigating the role of such pollutants in modulating cloud bulk properties in this climatically important and complex region. We would like to take the opportunity to address these useful suggestions in turn below after discussing more general revisions to the manuscript.

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General revisions: 1/ Since submission of the ACPD article, we have been made aware of several additional co-authors who should have been rightly recognised for their contribution to this study through provision of datasets. These additional co-authors, who now appear in the revised author list, are: Lynn Russell, Jamie Trembath, Mark Bart and Jian Wang (BNL). These co-authors all contributed data from instruments they either owned, operated or quality assessed for provision to this study and we apologise for not including these scientists at the ACPD stage. We also correct the name G. McKeeking to G. McMeeking for a typographic error.

Response to Reviewer 1:

Reviewer 1 - General comments:

1/ As suggested by the reviewer, we have further emphasized the role of our identified longitudinal zones and the reasons for choosing them. We have added sentences to the abstract, discussion and conclusions to make this set of results more obvious as the key quantitative product of this study which is of use to modellers.

2/ The reviewer raises the issue of synoptic variation as a source of variability in our reported data. This most certainly is a potential source of variability and one which was investigated thoroughly by the reviewers. Before arriving at the currently reported results, the authors first diagnosed 3 moderately different synoptic "regimes" during the VOCALS study period, derived according to an analysis of surface pressure anomalies and synoptic features in reanalysis data. To investigate potential variability induced by these different synoptic regimes, which were typical of the intra-seasonal variability in the SEP region more generally (Toniazzo et al., 2011, ACPD, in press), we initially binned our measurement data according to those three periods to look for significant systematic biases in pollutant concentrations versus longitude. Such differences were significantly less than the natural sampling variability (currently reported using median, quartile and decile extrema) and we therefore decided to bin all data across the entire campaign, rather than by synoptic regime and we can therefore conclude that any

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signal in atmospheric composition due to typical synoptic variability does not dominate over other sources of natural background variability. We have included exemplar plots below (Figure 1) which illustrate longitudinally gridded sulphate aerosol mass concentration (which might be expected to be one of the more variable quantities in the atmosphere) between 15 Oct to 1 Nov (Period 1) and 2 Nov – 16 Nov (Period 2) measured by the Aerosol Mass Spectrometer (AMS) weighted across all aircraft platforms (with Ron Brown data shown as purple bars). The figure clearly shows that there is no statistically significant bias between the two periods and that median values at all longitudes in either period fit well within the inter-quartile range of corresponding concentrations in the other period. We also believe that the current reporting of upper and lower deciles to our binned data gives a useful representation of the minima and maxima in our dataset and captures this background variability. This result, in terms of the negligibility of the synoptic influence on composition, is actually an important conclusion and one that is not given due prominence in the manuscript. We have therefore added this conclusion to the abstract and conclusions and provided a more thorough discussion in our short meteorology overview section.

Reviewer 1 - Specific Comments:

1/ pg 685, ln 16: As suggested, we have now included better referencing to studies which diagnose the limitations of various models in representing marine SCu in the SEP. These include papers now published in ACPD (e.g. such as Abel et al., 2010) as part of the VOCALS project with examples of specific problems that models have had in representing SEP cloud – e.g. boundary layer depth etc. We have also detailed how this paper and the VOCALS project more generally will move this issue forward as the reviewer has suggested.

2/ pg. 685, ln2: Excellent suggestion – we have now referenced Fig. 1 from Hawkins et al., 2010 to aid our discussion of the source locations and gradient in aerosol away from the S. American coast.

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3/ pg. 691, lns 8-10: We have referenced Hawkins et al., 2010 to give further weight to our use of unity collection efficiency for AMS data.

4/ pg. 692m lns 27-28: We now discuss the Hawkins et al., 2010 IC/AMS intercomparison and discuss their use of an alternative collection efficiency due to increased organic aerosol mass observed on the Ron Brown research vessel. We maintain that a uniform collection efficiency of 1 for the aircraft AMD dataset is appropriate to measurements of lower concentrations of organics above the near-surface layer. We also note an ongoing study led by Linsay Shank investigating potential sources of bias between organic measurements by the aircraft platforms and RHB vessel during VOCALS, which may yield further insight and suggest here that the elevated RHB organic measurements remain an open question.

5/ Pg. 702, lns 8-10: The reviewer has interpreted our results correctly, however we recognise the potential confusion that this could create so we have rephrased this paragraph for better clarification.

6/ pg. 702, lns 14-15: It is interesting that the RHB measurements see lower loadings in November relative to October at concurrent locations. This is not an observation that is manifest in the broader aircraft statistics and may represent a sampling artefact. We have, however noted this observation in the revised manuscript so as to alert the reader to the existence of such outliers and that our reported statistics only capture the broader picture across VOCALS and that there may be incidences where short-lived extrema exist. We again note the work underway by Lindsay Shank et al investigating potential sources of bias between aircraft AMS organics and those measured on the RHB.

7/ Pg. 702, lins 26-29: Again, this comment rightly refers to point location variability which might be expected to vary. From longitudinal profiles of aircraft data for individual flights, we do also see a variable spatial “reach” of continental tracers between flights, consistent with ship observations the reviewer highlights. However, we do already note this inter-flight variability and we pose our definitions of broad zones precisely to capture the broader variability. As such, our statistics create a generalised resource, inclusive and representative of this natural point variability for use by those who need a set of simple representative data for use in models etc. Clearly, for highly specialised, localised studies, the researcher would be better informed by in situ data recorded as close as possible to their case study, but for a general analysis, our statistics are more suitable.

8/ Pg. 708, ln8: It is unclear what the reviewer means by this statement but we suspect that the reviewer is suggesting that land tracers (radon) are correlated to organic matter and that organic matter is therefore land-based, whilst sulphate aerosol is not. We believe this is still an open question as our analysis by back trajectories has shown that airmasses rich in organics tend to come from long-range upper tropospheric transport of airmasses uplifted over the maritime continent to the west whilst airmasses rich in sulphate tend to come from the South American Coast. Both airmass types appear to mix into the MBL in the transition and remote zones making this a complex area in which to attempt to determine dominant landmass sources. We would suggest that the Ron Brown data might be better interpreted with this issue in mind. However, we note the collection efficiency issue that has already been discussed in comment 7 above.

9/ pg. 709, ln 20: This is useful information. The reviewer is correct to note that the AMD detection limit means that we cannot discern meaningful spectra from which to determine organic functionality. We will reference the Hawkins paper, highlighting their important finding that marine OM dominated in the remote zone. We also point to the long-range transport of OM however in the remote zone. We would suggest that further information on oxidative ageing of the OM could be useful in diagnosing the relative importance of local marine sources versus long-range transport.

Technical comments: We agree and have auctioned all technical comments, including larger labels on our figures and consistent colouring for our zone distinctions between all figures.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 681, 2011.

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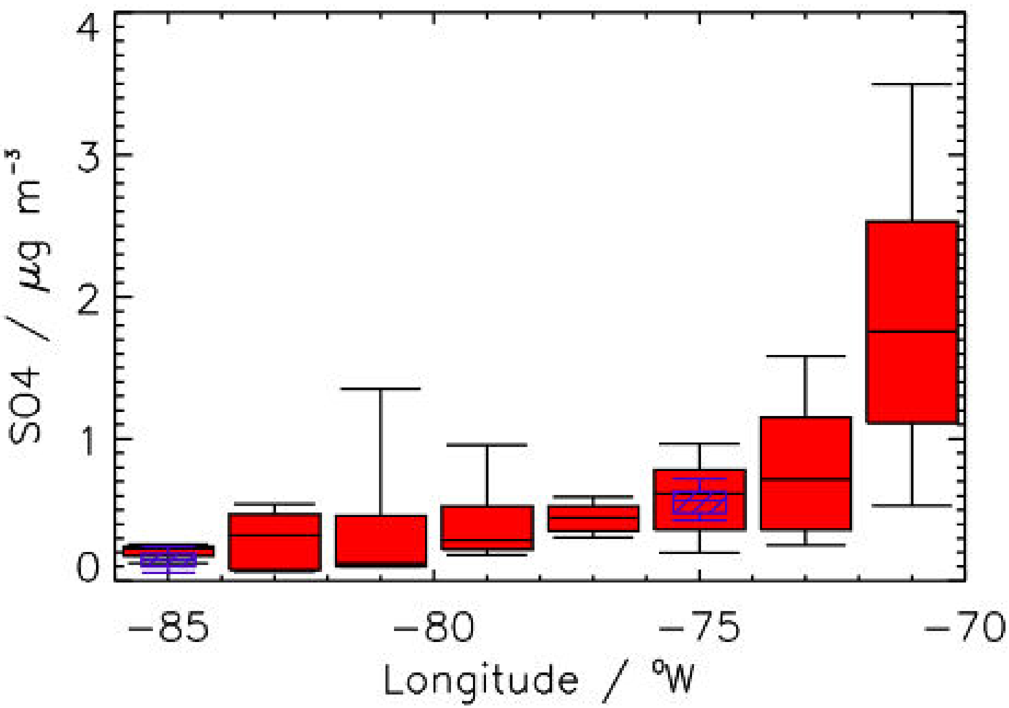

Fig. 1. AMS Sulphate mass concentration in Period 1. The box and whiskers represent quartile and decile extrema in each longitude bin respectively and solid central bar

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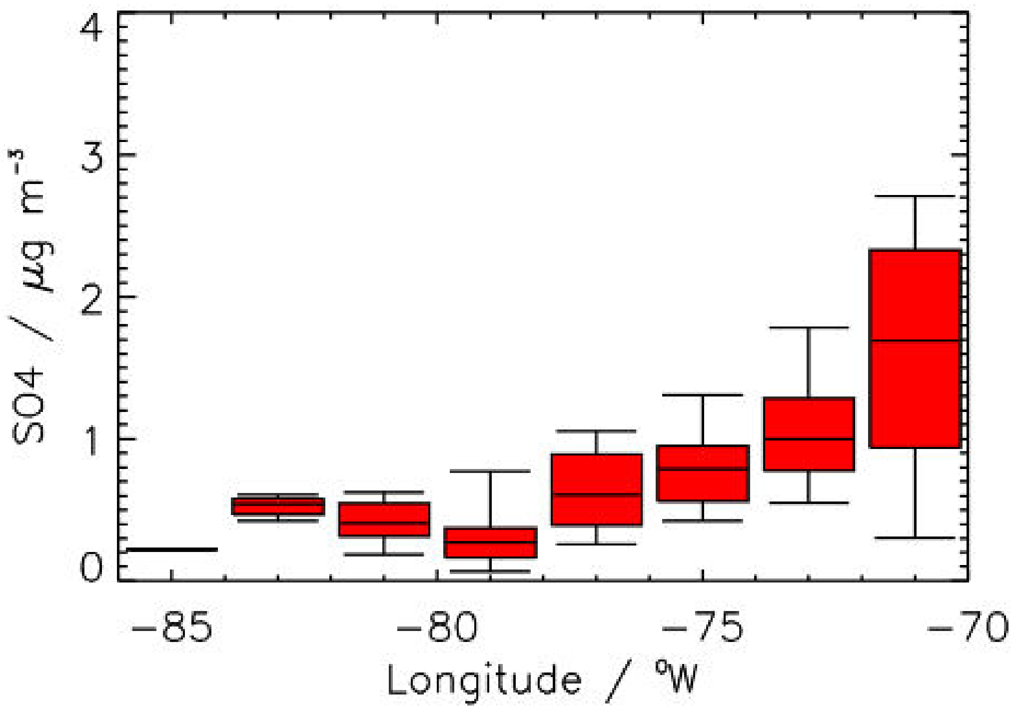

Fig. 2. AMS Sulphate mass concentration in Period 2. The box and whiskers represent quartile and decile extrema in each longitude bin respectively and solid central bar

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