

Interactive  
Comment

***Interactive comment on “Large-scale and synoptic meteorology in the South-East Pacific during the observations campaign VOCALS-REx in Spring 2008” by T. Toniazzo et al.***

**T. Toniazzo et al.**

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We appreciate the constructive suggestions made by Reviewer #2.

This is a first reply by the authors made to addresses the general contents of this review and attempts to clear potential misunderstandings. A more detailed response including corrective actions on the manuscript will follow.

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## General.

In terms of its main conclusions, this review raises three main general points.

1. The first point refers to the large number of Figures, judging them unnecessary. It is suggested to “remove” Figure 6 and 7, on the basis that they contain too many panels (124 and 61, respectively) which are not discussed one by one in the text.
2. The second general point is that much of our work is based on the analysis of operational analyses and 21-hour forecasts with the UKMO operational forecast model. The reviewer doubts the quality of this dataset and its relevance for the actual meteorological conditions. He argues that conclusions from “observations” or from LEM modelling output would have been more “credible”.
3. The third and final point concerns the scientific merit of the paper, which is deemed to be lacking.

In the following, we address each of these three points in turn. We argue that the aims and scope of this paper justify our use of Figures; that our use of the UKMO data, besides not being exclusive, is legitimate; and that there is new scientific content in this paper.

We have some concerns that the reviewer may not have taken the Authors’ Comment (AC C2) into account (see minor comment #8). We also note that virtually all of the reviewer’s comments address the first part of the paper only. No remarks have been made about the contents or the text beyond page 246, i.e. much of section 5, and of all of sections 6, 7 and 8 of the manuscript. The reviewer does not seem to acknowledge our use of data other than the UKMO system, made in that part of the manuscript.

1.

Here we need to stress again the main aims and the scope of this paper, which controlled much of the structure, organisation and contents of the manuscript, and which do not seem to have been properly appreciated by the reviewer. The paper serves as an integral part of the documentation of the meteorology during VOCALS-REx and as a reference to it, for the benefit of all the community who are or will work with VOCALS-REx data. Its aim is to put those data into their meteorological context, both in terms of the weather experienced during the campaign, and of its connections to the large-scale circulation and thus its significance and representativeness in climatological terms.

Such a wide scope is a direct consequence of the wide scope of VOCALS itself, which is focussed, but not limited to PBL dynamics or clouds, and includes their implications for the large-scale circulation. Two concrete examples of such connections and implications concern the areas or origin of tracers sampled during VOCALS-REx, and the relationship between the state of the regional PBL and cloud and the wider circulation around that region.

Such documentation per se is mainly descriptive and does not have the character of an original scientific contribution. Rather, it is about applying current meteorological knowledge to the specific context of the south-eastern Pacific sub-tropical anti-cyclone and of the VOCALS-REx campaign. In particular, Figures 6 and 7, and the discussion around them in Section 4, have this primary aim. The “postage stamp” panels (ordered by day and time of the day in Figure 6, and by day only on Figure 7) serve to convey the synoptic conditions found in the area for each day of the campaign, for the perusal of all scientists that were involved in operations or those who have an interest in case studies. In that respect, each day deserves consideration, and we do not think that an animation could replace the contour plots. However, putting larger versions of these Figures on-line could be a good idea. The text discusses the main synoptic-scale features that recur prominently in those charts.

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Our motivations for preparing such material is our belief that this kind of documentation a useful resource for VOCALS-scientists, and that it should be readily available and easily accessible to all those working with VOCALS data, along with the VOCALS operations paper (Wood et al. 2010). The best place for such a publication is therefore in the ACP special issue covering VOCALS-REx.

Finally, we wish to point out that the reviewer's point 2) in his "major comments" is incorrect. All Figures have axis labels and colour bars, with the exception of Figure 1, where unfortunately the bottom has been clipped in the Copernicus type-setting. This should be easily remedied. For obvious reasons, labels and colour bar are not repeated for every single panel in Figures 6 and 7. They are shown in the last panels only. We're happy to make changes where required. In some cases the small size of some features might be easily remedied by simply printing the Figures on a size that fills the page (especially Figures 3 and 4, but also 14, 18, 19) where the space is currently not optimally exploited.

2.

The reviewer repeatedly refers to "simulations made by the UKMO model", and appears to dismiss our results as maybe relevant for the model world but not for the real-world meteorology. We find this a rather puzzling stance. First of all, this is not data from a "simulation", but a forecast system with 4-D variational data assimilation, and for most of the paper we use the operational analysis fields at 00z. This is clearly explained in Section 2, and in other relevant parts of the manuscript. Second, later in the paper we make extensive use of ERA-Interim reanalysis data. We found a very high degree of similarity between the two data-sets. Oddly, the reviewer's qualification of our data sources would apply more pertinently to previous work addressing the VOCALS-REx meteorology, e.g. Rahn and Garreaud (2010a,b), which has been accepted and published.

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Moreover, the UKMO analysis and forecast performance for the specific region and the specific period of time has been thoroughly validated against the field observations, first of all by Abel et al. (2010), but also by this paper specifically with regard to PBL depth and cloud cover. This is shown in Figure 9 and 10. If this were not enough, an independent assessment of this forecast system has been made by Wyant et al. (2010) in the PreVOCA paper. Therein it is explicitly stated that the UKMO system (one of only two among 6 operational models) has skill in cloud forecasts over a time-span even longer than that we consider here. It is on the strength of such assessment, and on the general quality of the forecasting system, that we base our results - not on specific mechanisms of the model parametrisations.

Obviously, we agree that the model-generated fields are not “truth” and indeed discrepancies are visible e.g. in Figure 10. However, it is a fairly good representation of it, especially in terms of variability. The relationships between fields in the operational analyses and *both* model-generated and observed cloud robustly indicate the presence of balance conditions (however they may be generated) presented and discussed in Section 5.

The links and relative influence of horizontal vs. vertical advection reflect the analysis fields, there being no other source of data (other than, equivalently, reanalysis products) to establish such a connection. These results might be model-dependent; however, we find that they are consistent with previous results based on completely different modelling system and parametrisations (e.g. Rahn and Garreaud 2010b). More recent analysis with the ERA-Interim product also shows consistency. As we know, one cannot have confidence in the total rates of subsidence represented in different products. The *changes* however appear to be consistent across them.

Finally, the reviewer’s suggestion that LEM-simulation output would be more “credible” is not only vague (what simulations?) but seems again to misrepresent the scope and significance of our study. First, LEM output is obviously not “truth”, either, with known large sensitivities and biases that depend on model formulation (Stevens et al. 2005).

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More to the point, however, here we are exclusively discussing relationships that occur, and indeed may be plausibly expected to hold, in a quasi-equilibrium sense on synoptic and larger spatio-temporal scales. We would not attempt to go further with forecast or reanalysis data, but existing LEM results, as far as we are aware, do not address these questions. Nevertheless, LEM simulation that could address the *processes* that lead to the “observed” synoptic- and large-scales behaviour would be very valuable, and in particular they could be used to falsify our hypothesis of cloud-top stability control (a long-standing controversy in the LEM community, e.g. Kurowsky et al. 2009; Xiao et al. 2010). Such work is well outside the scope of this paper.

3.

Given the scope stated in 1., the manuscript covers a wide range of aspects of the atmospheric circulation in the sub-tropical anti-cyclone. In much of this, known science is applied to the specific context of VOCALS-REx. However, the paper does also contain new scientific results and insights, specifically on the linkage between the circulation at different scales and on how the effects cascade down to influence the regional cloud-cover. This work strengthens and qualifies previously hypotheses (e.g. George and Wood 2010) that the temporal variability of tropical marine  $Sc$  is controlled to a very significant degree by the meteorology alone. As the reviewer does not address any of these points, and does not refer to the relevant sections and Figures, our impression is that the conclusions of “lacking scientific merit” is mostly a result of the criticism on the two previous points.

### Major comments

We address here points 1.-6. made by the reviewer from page C373 onward.

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1. We will correct the mistakes in the text. As for the Figures and their discussion, please see point 1) above.
2. See above (point 1. in the general reply); the reviewer's statement concerning the Figures is incorrect. We acknowledge that legibility might be an issue in some cases. Many efforts have already been made by the authors to address this during preparation. In several instances in the present manuscript it may be simply a matter of increasing the size of the plot to fill the page. Otherwise, we can easily increase the size of the labels. Clearly a more precise feedback by the reader is required.
3. We disagree - see point 2. in the general comments above, but also point 6. below. We thought that "Spring" in the "south-east" Pacific would be understood correctly by the reader, but it's easy enough to change that.
4. We are happy to add references to the relevant model parametrisations in Section 2. For the rest, please see point 2, in the general comments above.
5. We are happy to specify our use of UKMO operational data in the abstract, and to itemize our main conclusions. We would say we have attempted to focus on a few science questions, as re-iterated here in point 3. of the general comments. We should endeavour to make this clearer in the conclusions.
6. We have already made use of other data, in particular ERA-Interim reanalysis. We are happy to include a brief cross-validation between the two products - we have already noted a very strong similarity.

### Minor comments

**1,5,6,7,9,10:** OK, thank you.

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**2:** this isn't a parametrised mass flux; it is the product of the (mean) wind vector and the air density. Obviously the projections along a section are not solenoidal, the divergence being compensated by the zonal/meridional component.

**3:** the wind barbs are already scaled to give a consistent visual impression. They match the scales of the axes which are obviously very different. Yes these are busy Figures - so far most people seem to have liked them as "summary plots" of the circulation. Reviewer #1 suggested to add another field on top... Maybe by plotting them larger they will be more easily readable?

**4:** they are already present, although it seems that the y-label for the left-hand panel (identical to that between the two panels) has been clipped in the Copernicus typesetting. A similar problem has beset Figure 1.

**8:** this comments suggests that the reviewer has not taken the first Author's Comment (AC C2) into account, where the mistake of an old text version used for the typesetting was rectified by adding and correcting the (three) relevant sections of the manuscript, including Figure 14.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 225, 2011.

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