

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.

Anonymous Referee #2

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Review of “Large-scale and synoptic meteorology in the South-East Pacific during the observations campaign VOCALS-Rex in Spring 2008” by Toniazzo, Abel, Wood, Mechoso, Allen and Shaffrey for publication in Atmospheric Chemistry and Physics.

General Comment: The paper presents the synoptic conditions of the South-East Pacific region observed during the VOCALS-REx. Majority of the data used in the article are from the UK Met office forecast model, while some data from the radiosondes launched from the ship and that from satellites like MODIS or GOES are also used. Although the paper provides a general overview of conditions, it lacks scientific merit. I suggest the article to be accepted for publication only after major revisions are done to

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it. The authors have drawn conclusion based on the simulations made by the UKMO model, and have not focused on any single particular scientific issue. The authors have shown too many snapshots from the model, all of which do not show anything scientific (e.g. Fig 6 & 7). I do not still see it to be of a merit to the scientific community even after revisions are made, but maybe the authors can revise the manuscript and make it more scientifically compelling. There are total of 61 panels in fig 7 and 124 panels in fig 6, it is beyond my understanding how all of the panels could be of scientific merit. Below are my comments and suggestions.

Major Comments: 1) The overall writing of the paper needs to be improved. Not all the figures are discussed in the text to the full extent. Especially fig 6 and fig 7, either they need to be removed or some panels removed or lot more text has to be added to describe each panel. Also, there are numerous small mistakes in the text, some of which I have listed in the minor comment section.

2) All the figures need to be redone as most of them are missing axis labels, colorbars etc. Some figures have very small contour labels making it impossible to read them. Since, this is an online journal maybe you can replace fig 6 and 7 with animations.

3) The title of the article says “spring 2008” which can easily mislead the reader, confusing it with the northern hemisphere spring. I suggest the title to be “Large-scale and synoptic meteorology in the South-East Pacific during VOCALS-REx as Simulated by UKMO Model”. Since you do not use much of actual observational data, it is necessary to mention the model in the title.

4) In the conclusion section you have made numerous claims like “link between low cloud cover and depth of MBL” on page 256 line 10, “changes in the vertical velocity were most important” page 256 line 23 etc. As all the conclusions are drawn from the UKMO model data, maybe what you are reporting are just effects of how the cloud cover and boundary layer is parameterized in the model. If you would have drawn these conclusions from observations or from LES models then they would have been

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credible, but they are from a mesoscale model, so maybe you can reword the entire conclusion section. Instead of saying “We have shown a link between the amount of low cloud present and the depth of the MBL, diagnosed as the height where the capping inversion is located” maybe you should say “In the UKMO model there is a link between amount of low cloud cover”. Also, it will be worth to report the parameterizations used in the model to represent cloud cover, MBL depth etc in section 2.

5) Please change the abstract, to mention that most of the data used is from the UKMO model. The attempt made by you to link large-scale conditions to local conditions are all using the model data. As I said before, maybe you are just revalidating how things are parameterized in the model. This is the major weakness of the article, as you have not attempted to focus on any particular scientific issue. It is beyond my scope to ask you to focus on a particular scientific question. But please list all the parameterizations in section 2 and please list your conclusions as 1, 2, 3 . . . This way, it will be clearer.

6) Since you want to focus on synoptic scale features and not use any observations, a summary of how well the synoptic conditions are represented in other models will certainly make the article stronger.

Minor Comments: 1) Page 226 line 14, remove “into”. 2) Page 231, line 5-15: while explaining fig 3 you have discussed wind in terms of mass-fluxes, which is nice. But please give more details of how you calculate mass-flux. According to the classic plume decomposition approach of Arakawa and Schubert (1974), you had to subtract the mean velocity, how did you calculate the mean? 3) Please make the horizontal and vertical scale of wind barbs the same in figure 3 and 4, as the total will be conserved and the figure will be easier to read. Further, if I understand correctly, the shades in figure 3 and 4 show the zonal wind speed, white lines show potential temperature and you have dotted lines separating the contours of zonal wind speed. This is way too much information on a single plot. 4) Add axis label to fig 3 and 4 5) Page 232 line 25, second “where” should be “were” 6) page 234, line 4: please mention the year in the date 7) page 234, line 12: Typo “goiva” should be “give” 8) Check the caption of

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fig 14, (a) and (c) might be reversed. 9) Fig7 caption, it says “for each day of October and November 2008 on 23 October 2008”. Please correct. 10) Page 246, line 21: “is” should be “was”.

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