

## ***Interactive comment on “Meteorological observations in the Northern Chilean coast during VOCALS-REx” by J. A. Rutllant et al.***

### **Anonymous Referee #1**

Received and published: 14 October 2012

This manuscript describes some of the characteristics of the diurnal cycle and synoptic meteorological variability observed at coastal sites along the northern Chilean coast during the VOCALS Regional Experiment during September–November 2008. The emphasis on the diurnal cycle is the strongest and most coherent part of the work (although it needs some work), whereas the description of the synoptic variability is rather unfocused and does not contribute much to our understanding. I was hoping that the manuscript would be stronger than it is currently, in keeping with the many excellent papers that the University of Chile groups have produced over the last 15–20 years. I think that much can be achieved in revising the manuscript to achieve the following:

1. Better connection between this work and previous studies. I came away from read-

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ing the manuscript not knowing if there is any connection between the observations presented here and the upsidence wave that two of the coauthors themselves discovered. All the findings here seem focused upon an extremely narrow (50 km wide) coastal strip. Is the implication that measurements made here are essentially not useful for understanding the broader flows affecting flow adjacent to the Andes and over the broader SE Pacific? Previous papers describing the meteorological variability during VOCALS-REx were not cited (e.g. Toniazzo et al. 2011).

2. More clarity in the presentation. Almost all of the figures are too small to read, have inadequate contour labeling, wind arrows without a legend, axis labels that are way too small. I became frustrated rather quickly at not being able to read a single quantitative value from most of the plots. The writing is not particularly clear in many places and does not help to provide a succinct and readable synthesis of the findings.

In addition, the authors need to think more seriously about their conclusions regarding aerosol transports, which are all rather speculative and do not discuss where the primary sources are located with respect to the flows they are describing. Their discussion of the role of DMS in adding to the coastal CCN would have been fine in 2007, but post-REx understanding has moved beyond this stage. DMS increases with distance from the coast (Allen et al. 2011), so cannot be the source of the elevated CCN and cloud droplet concentrations. The aerosol measurements at Paposo described in Chand et al. (2011) are not even cited. In addition, their conclusions regarding transport from elevated smelters seem inconsistent with their own findings (Fig. 7) which suggest rapid upslope transport of said emissions during the afternoon without strong downward transport at night. This means that these smelter emissions are unlikely to be carried to the ocean immediately above the MBL, but probably higher up. Trajectory analyses with their high resolution WRF run would help here. In addition, there are two recent studies in ACP that address the regional aerosol sources and transport (Yang et al., Saide et al.).

So, in summary, I think there are some interesting results here, but work needs to be

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done to improve the analysis and make the work more readable by and relevant to the broader VOCALS community and beyond. Otherwise, I fear that the paper will not receive the attention that I believe some of these observations deserve. I describe more specific points that may help the revisions below.

#### Specific Points:

1. P22785: Line 24. “Contribute” to what? And why at dawn? I don’t understand.
2. Please draw on available aerosol measurements and papers describing them (most of which are in the VOCALS ACP special issue), or remove discussion of relevance to aerosol transports.
3. P22785: Line 21. What exactly is delayed?
4. Aren’t volcanoes too high to be a significant source to the SE Pacific region?
5. P22786, Line 7. What does “aloft” mean when there are at least three distinct levels of action?
6. Line 26. The dates for VOCALS-REx here are not consistent with their WRF simulations which took place before REx. Why not perform simulations in the relevant time window? This seems a little sloppy.
7. Line 22788. “not only is PA. . .” would be better
8. Line 11-16. These “important” features are not referred to later. How important are they? This is an example of the lack of focus.
9. Put radiosonde times in a table.
10. Fig 1. Should show latitudes of the stations.
11. What is SCFA?
12. P22789: Line 20. Only one model was a factor of two too low. Most models do better.

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13. Having a short, high-resolution WRF simulation is very helpful, but the simulation seems highly underexploited.

14. P22791: Lines 5-21. This is confusing. I'm not sure what we're learning from this. What does "northerly (NE)" mean? Is it northerly or northeasterly? Show mean MBL top in Figure 3 since many readers will not be familiar with previous studies.

15. Line 25. This hypothesis is not clear. No test seems to be carried out about the part where it lowers the MBL top height in response to the afternoon subsidence. Does this happen?

16. P22792: Line 8. I don't consider a 5 hour delay in the timing to be consistent with the term "replicated".

17. Fig 6 title says cloud frequency, but what is shown is a difference.

18. P22793: Line 2. Does a "5 m/s upslope flow" mean 5 m/s updrafts?

19. Line 10: What is "P"?

20. It would be more illustrative if Fig 7 showed the vertical velocity not the divergence of the zonal flow.

21. P22794. Much of this cites figures in the supplementary material. It just seems as though the authors gave up including figures at this point.

22. Line 28. In what way are the oscillations wave-like other than being oscillations?

23. P22795. This is wordy and rambles without reaching any real conclusions.

24. The summary and discussion is more or less just a reiteration of the results section. This is not sufficient. Conclusions regarding aerosols are not informed by drawing on published observations (see my comment above).

25. P22798: Lines 1-10. I don't understand this. I don't see a 5 m/s offshore flow in Fig. 7 (the only figure that provides flow information at the location of the key smelter

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being discussed (Chuquicamata)). I see instead a strong, broad upslope flow extending out to 30 km from the Andes during the afternoon but only a weaker downslope much closer to the slopes during the night. It seems that the primary direction of the emissions would be upslope, from where the flow might take them to the east or west depending upon the synoptic flow. Can the authors comment on this? Also, if the effluent does end up just above the MBL then it would be quite quickly entrained into the MBL rather than being transported far offshore as the satellite observations indicate.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 22783, 2012.

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