

Anonymous Referee #1

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:

REPLY:

Thank you very much for this general comment. With respect to the "Synoptic variability" section, we agree with the reviewer that the synoptic analysis is not central to this paper, and it has been described elsewhere. Therefore we restricted the section formerly titled "Synoptic scale variability" to the analysis of the day-to-day variability in the MBL depth, surface winds and sea-level pressure, and changed the title accordingly. A brief context for the large scale and synoptic variability is provided at the beginning of Section 3.4 based on Toniazzo et al. (2011) and Rahn and Garreaud (2010b). On the other hand, we have expanded our description of the diurnal cycle with a diagnostics of the local temperature change and a 3-D trajectory analysis in order to quantify the transport properties of the mean circulation.

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

REPLY:

An explicit reference to the effect of the upsidence wave in the coastal vertical velocity has been included now in section 3.3 when describing Figure 9 (coastal time-longitude diagram of vertical velocity at 800 hPa computed by the model). We added the phrase: "At around 20 LT there is a sudden change in vertical velocity from downward to upward motion connected to the positive phase of the "upsidence wave" depicted in Fig. 9 in Rahn and Garreaud (2010b)." Also, in section 3.2 the differences in timing of the maximum subsidence among different locations along the coast were postulated to reflect the "upsidence wave" propagation.

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?

REPLY:

The main conclusion of new Section 3.2, partially based on modeling results, is that the diurnal cycle of subsidence in the coastal strip is driven by the flow over the Andes slope (Andean pumping), see Fig. 8. On the other hand, we performed a trajectory analysis (new Fig. 11, Section 3.3) showing that this strong Andean pumping prevents any possibility of continental sulfur sources reaching the free troposphere above the Sc cloud deck in at least a one-day time scale, under mean conditions. Conversely,

coastal sources could contribute with sulfur aerosols preferentially in the morning, when the weak inland flow becomes partially blocked by the coastal terrain. The dynamical connection of the atmospheric circulation over this coastal strip with the open ocean within the VOCALS REx area has been discussed in several backward trajectory analyses (e.g. Chand et al., 2010; Toniazzo et al., 2011; Allen et al., 2011; Rahn and Garreaud, 2010b).

Previous papers describing the meteorological variability during VOCALS-REx were not cited (e.g. Toniazzo et al. 2011).

REPLY:

Done. A brief context for the large scale and synoptic variability is provided now at the beginning of Section 3.4 based on Toniazzo et al. (2011) and Rahn and Garreaud (2010b).

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.

REPLY:

We have improved the labeling of most figures. In Figures 2, 3 and 5 contours are not explicitly labeled, in order to make them clearer. However, the captions provide enough information to deduce specific contour values.

The writing is not particularly clear in many places and does not help to provide a succinct and readable synthesis of the findings.

REPLY:

We significantly re-wrote the paper in an attempt to improve the readability of the new version and have reduced the final section with the summary, following the reviewer's suggestions.

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.).

REPLY:

We have eliminated the reference to the DMS. Based on previous work by Chand et al. (2010) and Allen et al. (2011) the analysis of aerosol transport has focused now in anthropogenic sulfur from coastal sites (power plants) and inland emissions from copper smelters. To this end we performed 24-hour 3D forward trajectory analyses in order to describe possible aerosol transport paths from coastal and inland emission sources (see results at the end of section 3.3 and Figure 11). The abstract and summary sections have been modified accordingly.

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

REPLY:

We thank again Reviewer 1's constructive criticism and suggestions. We are confident that by being more focused, the new version of our manuscript will be of interest to the VOCALS's scientific community.

Specific Points:

1. P22784: Line 24. "Contribute" to what? And why at dawn? I don't understand.

REPLY:

On the basis of our new trajectory analysis, this phrase in the Abstract has been reworded to "Conversely, coastal sources could contribute with sulfur aerosols preferentially in the morning..."

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.

REPLY:

The relevant literature on aerosols composition and transport is now cited in the Introduction. They guided our added trajectory analysis. The Abstract now includes the paragraph "From 24-hour forward trajectories issued from significant points at the coast and inland at the extremes of the diurnal cycle, it can be concluded that the strong mean daytime pumping of the Andes in combination with a much weaker return flow, together with a mild seasonal nocturnal easterly flow, prevent any possibility of continental sulfur sources to reach the Sc cloud deck in at least a one-day time scale, under mean conditions."

3. P22785: Line 21. What exactly is delayed?

REPLY:

This word and the entire sentence were deleted and replaced by the paragraph quoted in the reply to the previous question.

4. Aren't volcanoes too high to be a significant source to the SE Pacific region?

REPLY:

In the new version of the paper, volcanoes are not considered relevant as a significant source to the SE Pacific region under mean conditions.

5. P22786, Line 7. What does "aloft" mean when there are at least three distinct levels of action?

REPLY:

The word "aloft" has been replaced by "just above them".

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.

REPLY:

We performed a second simulation from 1-4 November 2008 (at the middle of VOCALS-REx) with the same setup of our original simulation. All figures were recreated using the new simulation. Results do not change much owing to the rather stable conditions at northern Chile during this season. Thanks for pointing this out.

7. Line 22788. "not only is PA: : :." would be better

REPLY:

Done, thank you.

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.

REPLY:

We removed the detailed description of the orographic features. All this coastal orographic description was intended to justify some degree of blocking of the inshore flow within the coastal MBL in a previous version of the manuscript seeking to explain strong morning northerly flow at PA in late winter.

9. Put radiosonde times in a table.

REPLY:

Done. We have put specific information of radiosounding times in the new Table 2.

10. Fig 1. Should show latitudes of the stations.

REPLY:

Done.

11. What is SCFA?

REPLY:

We replaced SCFA by Antofagasta.

12. P22789: Line 20. Only one model was a factor of two too low. Most models do better.

REPLY:

The underestimation of the coastal MBL height is quite marked (by a factor 2) in regional numerical models: MM5 (Garreaud and Muñoz 2006), WRF (Rahn and Garreaud 2010a), COAMPS (Wang et al. 2011), and MetUM (Abel et al. 2010). We have added these references in the new text.

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

REPLY:

Results from the model simulation now include 24-hour 3D forward trajectories, validation against radiosonde observations at Iquique and Paposo, and diagnostics of the diurnal mean cycle in heating/cooling within the subsidence inversion.

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.

REPLY:

We have sharpened the description of Figure 3 and have added in it the general location of the subsidence inversion base according to Muñoz et al., 2011.

The "northerly (NE)" confusion was solved by rewording the phrase "Within the MBL, Fig. 2 shows that south-southwesterly winds peak between 16:00 and 18:00 LT, alternating with weaker northeasterlies at dawn at Iquique and partially at Michilla."

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?

REPLY:

A quantitative assessment of the effect of vertical velocity on coastal 800 hPa warming and inversion base height depression has been added in the before-last paragraph of section 3.3 of the revised manuscript. In both cases the sole vertical advection leads to changes larger than observed, requiring additional compensating mechanisms (horizontal advection and entrainment, respectively), lending support to the hypothesis.

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

REPLY:

The word replicated was eliminated and the sentence was reworded.

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

REPLY:

Done. We modified figure title.

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

REPLY:

It refers to terrain-parallel near-surface flow.

19. Line 10: What is "P"?

REPLY:

The column P is an average coastal profile between 19-22°S as shown in Fig. 8. We clarified this in the new text.

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

REPLY:

The vertical velocity is shown in new Figs. 9 and 10. We do not show divergence of the zonal flow explicitly.

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.

REPLY:

To better describe the synoptic-scale modulation of the coastal MBL in Section 3.4, a week-long sample of key meteorological variables is now described in the main text by means of a subset of time series presented in the Appendix (new Figure 12). The complete time series are left as supplementary material for further documentation and analyses by other authors.

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

REPLY:

Thank you. The word "wave-like" was removed.

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

REPLY:

We eliminated the last paragraph describing MBL depth excursions previous to the VOCALS-Rex period. The other paragraph is kept with the idea of demonstrating that the co-variability between MBL depth, surface winds and atmospheric pressure follows the synoptic-scale patterns found in connection with coastal lows in central Chile (Garreaud et al., 2002), episodic easterly wind events in northern Chile (Huneeus et al., 2006) and those characterizing the climatology of extremes in the MBL depth at Antofagasta (Velásquez, 2010). To make this clearer we added the analysis described in response to comment 21.

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).

REPLY:

We agree with the reviewer. The summary has been substantially shortened. Conclusions regarding aerosols now follow from the arguments given above (reply to specific comment 2) to concentrate in anthropogenic sulfur emissions and the resulting trajectory analysis.

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 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.

REPLY:

This paragraph was completely changed to be based now on the new trajectory analysis performed. This analysis shows that both morning and afternoon forward trajectories within a 24-hour time span are due E-SE except for the coastal ones (including PA) in the morning that are due north while staying near sea-level (last paragraph of Section 3.3).