**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Observations of the boundary layer, cloud, and aerosol variability in the southeast Pacific coastal marine stratocumulus during VOCALS-REx" *by* X. Zheng et al.

Anonymous Referee #3

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This study documents the state of the stratus-topped boundary layer (STBL) near the coast of Argentina/Chile from ∼1 month of aircraft, radiosonde, and reanalysis data during the VOCALS campaign. It is a useful contribution to the literature because near-coastal conditions in the SE Pacific haven't been studied in detail before. However, the paper reads more like a catalog than an attempt to answer any particular scientific question - which greatly reduces its impact. The text is generally clear and well-written with the exception of the points noted below.

Major suggestions:

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1. The paper currently reads like a disconnected set of BL properties. I think it would be much better to give us a description of the atmospheric state for various circumstances. For example, understanding that the synoptic disturbance on Nov 1-2 brings warm, moist, and high-aerosol free tropospheric air which causes the BL to decouple is much more useful than knowing that sometimes in the month each of these individual conditions held. Reorganizing the sections based on regime or defining several regimes at the start and tracing regime properties throughout the existing sections would improve things hugely.

2. Temporal and spatial variability of quantities (e.g. synoptic situation) are put in separate sections. This again prevents us from understanding the situation in a holistic way and leads to some redundancy (e.g end of section 3.1 says what is shown in sect 3.4.

3. You could improve the impact of your paper by using your obs to test whether reanalyses are doing a good job.

Minor Comments:

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1. You may want to include "near-coastal" in your title since this is what differentiates your results from previous work.

2. The NCEP-NCAR reanalysis is really old Why didn't you use something more modern? More generally, there are few observations in this region to constrain reanalysis, so it would probably be good to compare results from several reanalysis products to ensure you're getting the right picture. You could include these results as a comment response and put a sentence in your paper saying your result is unchanged by changing the reanalysis product.

3. Abstract says conditions were well-mixed, but you say later that this isn't always the

case.

4. p. 15427 L 10 "Re TO a horizontal" not AT.

5. p. 15428 top paragraph: I'm confused about the timing of the various data sets. If I understand correctly, flights took place in local morning and you used radiosonde data for the morning, but satellite data for all times of day? You should at least put in a few sentences clarifying this and optimally you should only use data for the morning time in all of your analysis.

6. p. 15428 L 22 "from broader regions" -> "and came from a wider variety of directions".

7. sect 2.3: Can you say that relatively homogeneous conditions in Sc make HYSPLIT accuracy less important? In any case, I think you only use HYSPLIT data back to 48 hrs, so I don't think the concern in L 25 is relevant.

8. p 15426 L 20: Shouldn't the subtropical high be building in austral spring? I'm confused about this and the next sentence, which seem to give contradictory explanations.

9. Sect 3.4: SLP is calculated from 30m legs... is this SLP or pressure at 30m?

10.Sect 4: you present vertical profiles from the aircraft as if the cross-section has no horizontal component. How far is the aircraft traveling in the horizontal as it does these profiles? Do you think this will affect the results?

11.Fig. 7: there's an interesting high-frequency temporal oscillation in these plots. Is this the diurnal cycle?

12.p 15431 L8: decoupling is different than breakup. What exactly do you mean here?

13. p 15431: you list 3 decoupled days on line 12, then later talk about how Nov 1-2 are also decoupled. Confusing.

14. sect 4.2: you mention in-BL wind shear for 24th Oct and 4th Nov here, but in other

places in the text you also mention Oct 27th as having shear. Also, I'm a bit confused how the Oct 24 and 27th BL can be well mixed yet support strong wind shear. Wouldn't momentum mixing quickly destroy this structure? Are you sure you're not mis-sampling the BL top here? Does the cloud structure consist of shallow Cu here? This is a good example of a case where fully describing the BL structure for a regime would be useful..

15. p 15432, L22: "the $*z\_i-*$normalized cloud base"

16. p. 15433: I'm confused what assumptions you make to get the values on line 4.

17. Sect 4.3, end: Your argument that w'w' is weaker here than in nocturnal studies because the BL is decoupled during the day doesn't make sense in conjunction with your assertion that the BL is well-mixed.

18. p. 15434 L7-10: the wording of your entrainment zone explanation could use work.

19. p. 15434, bottom: steadiness of BL depth at pt Alpha could be due to anti-phasing between solar heading and diurnal cycle of subsidence rather than weak coastal influence.

20. p. 15435 L 12 (and other places): be consistent in using local time or UTC (I prefer local time).

21. p. 15437, L 5: I think you mean seasonal rather than annual cycle. Also, getting the standard deviation of monthly anomalies would in addition to the typical change across that month would put the SST jump in better context.

22. eq 8-10: Aren't eq 8 and 9 essentially equivalent to eq 3 and 4? Shouldn't you be able to read the resulting info off Fig. 8? I see this section as redundant. I also don't think the 30m buoyancy flux is particularly interesting since moisture doesn't have a big effect on buoyancy flux (as you show) in unsaturated conditions.

23. p. 15438 L 14: 1-2 Nov is 2 days, not 3.

24. p. 15438 L 14: I don't see why a negative moisture jump invalidates the flux-jump

calculation of entrainment.

25. p. 15438 L 20-21: You should not include data points where you expect the method to fail. Along these lines - shouldn't you not use flight legs that are 80m below the inversion? On a similar note - don't use the cirrus corrupted GOES data (p. 15442, L 8).

26. p. 15439, L 19: why the exclusion of cases with Nd<15cm-3? This arbitrary choice will bias the mean Nd results.

27. p. 15440, L 6: your correction method for converting 0.5% supersaturation to 0.2% is not well explained. What was the correlation between 0.2% and 0.5% values when both were available? What is the magnitude of the correction you apply?

28. p. 15440, L10: How do you distinguish between drizzle water content and cloud water content?

29. p. 15441, top: I don't understand this argument.

30. p. 15441, L 13: how did you choose the functional form for your Nd equation?

31. p. 15444, last sentence: weak w'w' contributing to weak entrainment is a good point that you should mention in sect 5.4.

32. p. 15445, L 20-23: I don't understand this sentence.

33. Fig. 1: I don't understand what the shaded region means.

34. Fig. 8: What are the black dots?

35. Fig. 10: How did you get LWP error bars?