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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessing regional scale predictions of aerosols, marine stratocumulus, and their interactions during VOCALS-REx using WRF-Chem" *by* Q. Yang et al.

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Summary:
This study is, in essence, a validation of prognostic aerosol capability for WRF-CHEM with an interesting and encouraging further validation of interactive aerosol coupled with a dual-moment cloud microphysics (Morrison) scheme. This exercise makes good (but as yet incomplete) use of an enormous wealth of data collected during the VOCALS-Rex field campaign conducted over the South East Pacific in late 2008. Marine boundary layer thermodynamic quantities, aerosol number, aerosol mass and composition, and cloud bulk properties are all analysed and intercompared statistically

over meteorologically and compositionally distinct spatial domains within the wider South East Pacific. The study represents an enormous data analysis exercise, for which the authors should be commended. However, in one or two instances, I think the authors have jumped a little too quickly to confidently stated conclusions that cannot be fully justified (especially in terms of boundary layer structure) – these will be detailed in the general comments below. Also, and perhaps most importantly, it seems that the failure to run the model at sufficiently high spatial resolution so as to capture convective dynamic processes on scales relevant to MBL SCu, or to otherwise include available sub-grid convective parameterisations, may potentially lead to large errors that could have been avoided. Since we are looking here at aerosol-cloud interactions, convective dynamics are critical but not represented as well as they need to be for this analysis to provide useful additional insight for modellers as the study stands at present. In addition, I feel that an opportunity has been missed to make good use of studies already published in the VOCALS special issue (i.e. Allen et al., 2011) and also the BAe-146 dataset. I have chosen to lift anonymity here so that I can help the authors if they would like me to (without asking for co-author recognition).

The work presented here is very important to the atmospheric modelling community given the growing uptake and success of the WRF model and it is timely given the recent improvements to WRF-CHEM. The study also provides a useful distillation of measurement datasets from the VOCALS-Rex campaign, which would be useful to a wider audience (e.g. those interested in composition and dynamics in general). As such, the study is applicable for publication in ACP and will ultimately represent an important and extensive contribution to the VOCALS special issue.

The paper is well presented and well written, the figures and tables are of excellent quality and the study has been expertly executed in the main. It is this reviewer's recommendation that this study be published in ACP subject to some important

revisions, which will now be detailed below. These revisions are hopefully minor relative to the sheer amount of excellent work that must have already gone into this study. This study is so important and already so well executed, that I think it is worth the extra effort to make sure it is the very best that it can be and I hope the authors recognise the effort taken in this review to that constructive end. As someone who is not familiar with the intricacies of WRF-CHEM, I have asked for further advice and comments from colleagues working with the WRF model – these are also included below.

General Comments:

1/ Co-authors: Have the authors carefully checked the status of data protocols on all the datasets used in their study? Good use has been made of a large proportion of the VOCALS-Rex dataset but co-authorship may need to be offered to instrument PIs etc whose data are used here unless data protocols now specifically preclude it. I see several names (several spelled wrong) in the acknowledgements but care needs to be taken that this is sufficient for those (and other) scientists. For this reviewer's part, I am happy that the GOES-10 dataset I spent several weeks creating is being used and I do not require co-authorship, though it would have been proper to have been given the choice. Issues with properly recognising work contributions are very important. The long author list of Allen et al.,2011 is testament to such requirement and I can confirm that getting that author list right was necessarily difficult. This is a growing issue amongst the modelling community, who use measured data as if they were created from thin air (no pun intended). Quality assured measurement datasets are the result of a lot of hard and careful work by many people and future good quality datasets rely on funding that is justified by publications of work which use it and recognise it. The converse is also true – model output should not be used by those primarily working with instruments without recognition of the people who generated such products. Most importantly, co-authorship is important to ensure proper use of the most accurate datasets. I strongly recommend that the authors and the ed-

itor ensure that this issue has been paid due diligence and add co-authors if necessary.

2/ Use of Allen et al., 2011: I feel a little self-important having to promote my own work here but I do feel that there is a wealth of observational statistics and analysis already available in the VOCALS special issue that would have been directly applicable to this study that it would have made sense to make use of or at least discuss. The authors are entirely entitled to repeat my analysis, tailoring it to their needs but I think the authors would have been doing a great service to VOCALS if they had better used (or even more thoroughly intercompared) the existing distillation of data in Allen et al., 2011, which was written for the very purpose of informing studies such as these. Specifically, Figure 3 in this paper is an incomplete copy of panel b in Figure 11 of Allen et al., 2010. This study uses cloud droplet data (Fig.3) from the C-130 only, where Fig. 11 of Allen et al, weights all data from the G1, BAe-146 and C-130 aircraft, presenting a more complete statistical picture. Figure 11 of Allen et al., could have been happily reused in this paper and I recommend that it still is. Furthermore, Figure 4 of this work quotes AMS data divided into coastal and remote zones. I realise that the distinction between coastal and remote are chosen differently for this study so this figure is fully justified but Fig. 8 of Allen et al., also includes AMS data from the BAe-146 - data which are noticeable by their absence here (see below). In addition, Fig. 6 of Allen et al., could have been quoted to demonstrate the measured PDFs of ozone and SO2 (reported there for all aircraft) that are highly relevant to this study, especially when discussing SO2, which appears to be one of the biggest and most important sources of uncertainty in the WRF-CHEM model and this analysis.

3/ Use of BAe-146 data: VOCALS-Rex was an international field campaign. BAe-146 aircraft data have not been used in this study, leading to an incomplete use of the VOCALS dataset. Given the authors' extensive work in this study already, why was this platform not considered? I would suggest that it should be used in a new version of this paper to complete the excellent statistical picture that this work has built. I can offer assistance (without co-authorship) in providing that data. There are important sampling differences between the aircraft noted in Allen et al., 2010 that can only be filled in (as much as possible) by including as much data as possible. Those sampling differences (and the filling in offered by the BAe-146) are absolutely crucial in providing the best distillation of the measured data for comparison to WRF-CHEM output.

4/ Section 3.1: Boundary layer structure: In terms of the quality of the existing conclusions and analysis (issues above aside), this section is where I think there may have been some misinterpretation that leads to one or two later potentially incorrect conclusions. This section is an interpretation of radiosonde measurements from the Ron Brown (RB). A selection of sonde data recorded separately in the remote zone and the coastal zone are presented in Figure 1. The figure is used to show a cold, low humidity bias in the coastal zone relative to the remote zone. Herein lies a potential problem – the sonde data recorded by the RB in the remote zone were heavily weighted to its time on station at 85 W and likewise at 75 W in the coastal case. The variability sampled in the sonde data for each location represents only the variability across the times on station at each location, which were separated by several weeks, both of which are subsets of the period-averaged model data. As noted in Toniazzo et al., 2011, the synoptic conditions between these two time periods when the RB was on station were quite different, with weaker and less variable subsidence after November 1st. This alone could be argued to result in the biases seen in sonde data between the two locations. Furthermore, the explanation that the coast is colder than the remote area due to SST differences is not backed up – were measured SSTs at 75 W colder than those at 85 W I say this as 75 W could be too far west for the spatial extent of the Humboldt current. Perhaps a quick plot of SSTs from the RB when on station at these two locations could answer this problem? Furthermore, this argument is confused by a statement later in the paper that SST-air temperature differences make little difference to MBL depth. The issue of MBL depth is about much more than SST differences, with

the subsidence rate and coastal dynamics all playing an important role which varies in magnitude with distance offshore.

As a solution to solve this question of cold, humidity biases, I recommend using drop-sonde data from the BAe-146 aircraft, which should be added to Figure 1 (or perhaps used instead). These were dropped along 20 S on many flights (and measured at all locations on the same day), which will show if the MBL was systematically colder near the coast than in the remote zone, avoiding the disconnect in temporal sampling in the RB dataset. I raise this important issue because these sonde data are quoted repeatedly in the paper to be the cause of systematic differences in boundary layer structure, as well as potential causes of other chemistry and model biases. If you look closely at Figure 1, the model-model bias in MBL temperature and humidity between the two regions is much smaller (< 1K) than that seen in the sonde data. Also, the authors have rightly noted that Bretherton et al., 2010 did not observe a similar meridional gradient in MBL humidity – together these facts (the sampling disconnect in sonde data used, the small bias in model thermodynamics and the previous findings of Bretherton) make cause enough to question the sonde data used and the current conclusions. I recommend using the BAe-146 sonde dataset and the findings of Bretherton et al., 2010, and the authors' existing analysis of radar intercomparisons (Fig. 8, see item 5 below) to reconsider the analysis of MBL thermodynamics versus the model and the conclusions drawn therefrom. I would risk the hypothesis that the model does an even better job thermodynamically than is currently presented. If left unchecked, I could quite easily see an error value of 150 m in MBL depth error for WRF-CHEM being assumed elsewhere without due justification.

5/ Figure 4: Linked to point 2 above, I recommend adding 146 AMS data to this figure. Also, how are the data weighted in this figure? Have the data been properly weighted for sampling frequency/total sampling time? Also, how did the authors ensure that all the measurements used were in the MBL (i.e. was an altitude constraint or some other proxy used)?

6/ Figure 8 and interpretation on P. 22683: In the final para on this page, the authors used Wyoming Cloud radar data on the C-130 to intercompare cloud thickness and cloud base with model data and conclude that they are in excellent agreement. Certainly, the mean data do compare excellently and as the authors rightly point out, there are important subtleties in the tails and shapes of the three different distributions. However, this conclusion is at odds with the suspect earlier analysis of the RB sonde data – here the means compare very well, whereas the means of MBL depth diagnosed from the sonde data disagree by up to 150 m in the AERO case. If cloud base and cloud thickness agree well when compared to radar data, then by proxy, MBL depth would also agree well in the radar intercomparison. This casts further doubt on the earlier interpretation of the RB sonde data and may serve to suggest that WRF-CHEM is doing even better in terms of thermodynamics than currently concluded.

7/ Section 3.6: Rain rate. The 2DS instrument data from the BAE-146 may be a useful addition to this analysis.

Below is a question from a colleague working directly with WRF-CHEM:
8/ Cumulus parameterisations (or lack thereof) in the model settings: The model is producing an order of magnitude less rainfall than was observed - yet the authors don't seem to have tried to find out if including one of the sub-grid cumulus parameterisations which are included with WRF-CHEM would bring their predicted rainfall closer to the measurements. If the intention is to find out how the aerosol phase affects the model resolved cloud formation only then they shouldn't only be running the model at 9km (which is too coarse to really get the cloud dynamics right as noted by the authors), but instead should be running at a higher resolution instead (say 3 km, or

even smaller). What I think is needed is either a better explanation of why they've not included any sub-grid cumulus parameterisation or/and some sensitivity studies showing what effect including sub-grid cumulus parameterisations would have on their model results. Failure to do so ultimately undermines the ability to make any useful conclusions for the modelling community from the current model output in the context of coupling prognostic aerosol with cloud microphysics.

Specific (minor) Comments:

Abstract: Line 8 – may be worth explicitly stating which satellite measurements, i.e. MODIS, CERES and GOES-10.

P. 22670, line 1: Forgive my ignorance but what is "scalar advection"? Is this an oxymoron - isn't advection a vector quantity by definition? If you mean bulk mass exchange through grid boxes or the like, then please restate or qualify further for non-modellers.

P. 22675, line 29: Can the observed sonde variability really solely be due to open cell dynamics as stated? I would amend to say that ONE source of variability MAY be POCs and quote the other sources of variability (synoptics, SSTs etc).

P. 22676, line 20: Which "large-scale dynamics" are being referred to here and what may be the proposed process link between cloud bulk property changes due to interactive aerosol and changes at the larger scale? I can't see cloud bulk properties influencing mean MBL flow or subsidence rate for example. This idea is used rather loosely and ambiguously as a potential source of reducing MBL depth in the model by a significant 150 m versus the observations. Are there other potential reasons for this reduction in depth that can be diagnosed from WRF-CHEM? This is perhaps one of the most important thermodynamic errors that warrants further discussion.

Section 3.2: Here is where I think the authors really need to fully discuss their results alongside those of Allen et al., 2011 and Bretherton et al., 2010.

P. 22684, line 17: Can you quote the time frequency referred to?

P. 22688, line 4: "horizontal advection may affect tendencies of MBL depth when its gradients exist along wind directions (Rahn and Garreaud, 2010)" – I don't understand this, can you clarify or rephrase?

Below are some further comments from colleagues more familiar with WRF-CHEM and aerosol production:

Page 22679 lines 21-26: The authors say that their model does not include oceanic emissions of organic compounds and yet they say in clean maritime air masses the contribution of organic compounds to organic matter could be as high as 71

Page 22689: modelled DMS is over-predicted by a factor of 3 and yet MBL SO2 is underestimated by a factor of 3. Speeding up DMS oxidation shows little sensitivity to modelled sulfate. There seems to be a huge disconnect here. Perhaps, as they say wet removal of sulfate is too high but I agree with the authors that this needs further investigation. – Perhaps this issue should be restated in the conclusions, given its importance to VOCALS and beyond?

P. 22690, line 10: How does drizzle inhibition lead to high stability? I would have

thought that drizzle acts to stabilize the MBL through cooling. Perhaps there's a chicken or the egg issue to disentangle here in how this para is phrased – do you mean that a background stable MBL leads to less drizzle formation and hence lower CWP? In which case, it is the stable atmosphere that is the controlling factor and not the moisture supply due to drizzle inhibition as suggested.

P. 22690 – this discussion needs to be re-examined in light of the new sonde/MBL structure interpretation.

Technical Corrections:

P. 22668, line 23: "cloud-borne"? You mention interstitial aerosol earlier – does this word relate to activated aerosol? If so, I would just say "activated" as cloud-borne would cover all cloud aerosol.
P. 22670, line 8: Change "...is used that..." to "...is used, which..."
P. 22675, line 12: You define free troposphere as < 3km – does this include the MBL and the inversion as well?
P. 22677, line 9, Change "...the variation..." to "...the true known variation..."
P. 22678, line 15: change "...number, they..." to "...number. They..."
P. 22680, line 21: Change "detect limits" to "detection limits."
P. 22681, line 4: Change "Huneess et al..." to "Huneeus et al..."
P. 22683, line 5: Change "...associate..." to "...associated..."
P. 22681, line 5: It is not clear whether the enhancements referred to are in the model or in the satellite data without looking at the plot. Perhaps amend to "broader band of enhancements near the Peruvian coast in AERO".
P. 22686, line 6: Change to "near-surface".
P. 22688, line 5, Change "...turbulence ..." to "...turbulent..."
P, 22688, line 15, change "limits" to "supresses".

P. 22690, change “numbers at a polluted. . .” to “numbers in a polluted. . .”
P. 22690, change to “. . .found that a simulated. . .”
P. 22690, line 11: change to “leads to. . .”
P.22691, line 4: Change “clearness..” to “clearance. . .”

Acknowledgements: The following names and datasets are spelled wrong: “A Grant” should be “G Allen”. “GEOS-10” should be “GOES-10”. “B Alan” should be “A Bandy”. Please check that this list is complete as per item 1 in my general comments.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 22663, 2011.