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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.

Anonymous Referee #3

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Summary

This paper uses the VOCALS-REx field observations and coincident satellite data in the southeast Pacific to demonstrate the utility of the regional WRF-Chem model in representing marine stratocumulus and its interactions with aerosols. In addition to comparing model output to observations, this study also compare runs with and without interactive aerosols to show the improvement made by modeled aerosol-cloud interactions. The authors focus on average thermodynamic, cloud and aerosol variables over the month of observations, as well as quantities related to boundary layer structure and energy balance. This is a good first step in model evaluation and motivates further

use and evaluation of WRF-Chem to studying aerosol effects on marine stratocumulus. The authors find that the simulations including aerosol-cloud interactions compare fairly well to observations, and perform better than simulations without aerosols and a fixed droplet concentration. Biases in some meteorological and aerosol variables are noted and potential impacts on other simulation variables are explored. Temperature and humidity biases may influence errors in other variables. Although much more complete and detailed comparisons making use of more VOCALS observational data and more simulations focused on isolating aerosol-cloud interactions should be done in the future, this paper succeeds in demonstrating WRF-Chem is a good tool for reasonably representing the VOCALS-REx mean characteristics. The significant improvements seen by including interactive aerosols seem overstated (discussed in comment section) based on the specifications of the simulations used to make the comparisons. However, the conclusions drawn from the results, such as 'including spatially varying aerosol characteristics is important when simulating marine stratocumulus over the southeastern Pacific", don't overstep the boundaries of what one could determine from the simulations used. It is clear a large amount of work went into producing these results, and these results should be published before more complicated metrics are used to evaluate WRF-Chem. For these reasons I recommend this paper be published following minor revisions.

General Comments

1. The authors address the limitations of comparing the specific AERO and MET cases in the summary/conclusion, but restrict the impacts to the variability of microphysics. Some of the improvements in the AERO run could be due to simply representing the spatial gradient in droplet concentration, as implied by the last sentence of the abstract. Reproducing the mean droplet concentration does not require interactive aerosols. Fixing the droplet concentration to a constant in time, but with a gradient representative of mean southeast Pacific conditions would likely also show improvement over the MET simulation, not necessarily just in microphysics. The way this section is written now

implies that the improvements seen in MBL structure, energy fluxes at the TOA and surface, and macrophysical cloud properties would be the same if the MET case included a spatial or temporal gradient in the fixed droplet concentration. The statement addressing this issue should be made more inclusive of potential impacts of using a different MET simulation.

2. The 'MET' simulation fixes the droplet concentrations at a very high value that causes a lot of the biases seen (addressed in the paper) in this simulation. It is not emphasized enough (though is briefly in the summary) that this may cause the improvements noted AERO simulation to appear larger and more significant than they might be if a different constant droplet concentration were used.

3. It would be helpful if the paper would describe the 'MET' simulation in more detail, specifically what it represents physically. What does 'rain' mean if droplet concentration doesn't change? If the aerosol module is turned off, why is it necessary to prescribe droplet concentration to a fixed value? One possible explanation is if droplet concentration can change, droplet loss to rain would represent a loss of aerosol, even though there is no aerosol module. A cloud-aerosol interaction would therefore still exist. Although this is perhaps intuitive and obvious, adding a description would keep readers from guessing what the MET simulations represent. In the MET simulations is there a fixed aerosol impact on radiation?

4. It is stated in the abstract that the Morrison microphysics scheme is used because it allows for two-way aerosol cloud interactions, and the paper mentions that it is newly connected to the aerosol code, but specifically how and why this scheme is preferable is not explained. The Morrison scheme is well described, but it is not clear in the paper why it is better than the past used Lin scheme. The Lin scheme also allows for 'two way' aerosol cloud interactions, and although was originally single moment, has been modified to behave as a double moment scheme. What is the benefit to using this new double moment scheme? Have there been runs or studies comparing the Lin scheme to the Morrison scheme in WRF-Chem? Since the reasons for/benefits to using the

Morrison scheme aren't described or cited in the paper, either an explanation should be added to section 2.1 or the fact that the Morrison scheme is being used shouldn't be the leading line in the abstract. What is different/better about this microphysical representation, especially with regard to aerosols?

5. In the Lin scheme the autoconversion of rain has a different parameterization if droplet concentration is prescribed than if it is predicted. In the Morrison scheme implementation are there any differences in the cloud physics based on whether Nd is constant or varying? This doesn't need to be explained in the paper as there is likely not. But if there is, it is important when comparing the 'AERO' to the 'MET' simulations to explain all possible differences between the two.

6. Most of the figures comparing VOCALS data to model output model use one platform, either RB or C-130 without explaining a reason for the choices, or incorporating data from the other platforms that could add a significant amount of observations. RB data is biased by date, and different synoptic conditions could impact the boundary layer structure over the REx period. The first paragraph of section 3.1 describes the coastal MBL as being '$\sim$2K colder and $\sim$2gkg-1 less humid' than the remote MBL, based on Table 3, which only uses ship data. While the qualitative statement may be correct, using data that is recorded on different dates in different places is not representative of the mean geographic contrast on average. A multi-platform mean utilizing the flight data to fill in some of the missing data would be a better observation to use to make such a quantitative statement. In addition, there is no mention of number of samples or statistical significance on the results, which would greatly enhance the arguments made. Also, aircraft and ship data are used for comparison of different quantities to model simulations without reasons explaining the choice of comparison platform. With aerosol variables (e.g. Table 4 and Figure 4) comparisons with several platforms are made and it is clear 'observations' refers to any of the platforms, but the discussion of cloud variables needs to be modified to make the reader understand why RB is used in some cases and C-130 in others.

Specific Comments

a. Table 3: It is unclear why the SST is in units of Celsius, while the temperature is in units of Kelvin. If one wants to compare the two, they need to do arithmetic. I suggest changing the SST values to SI units.

b. The Table 3 caption should explain at what vertical level dqv/dh and dthetav/dh refer to. Based on the text, I assume this is across the inversion level (section 3.1), but the numbers cited are not the same as the table, as they are not divided by dh. Does it refer instead to the average dqv/dh over the MBL?

c. Table 3: Why are droplet concentration observations from RB, while aerosol concentrations are from C-130 data? In Figure 3 both aerosol and droplet concentrations are from C-130 observations, but Section 3.2.1 describes Nd from aircraft '(Nd, Fig.3 and Table 3)'. It also describes Nd from RB to compare with Bretherton et al. 2010 values (from MODIS and aircraft). This is confusing and it is not clear why the discussion uses different observation platforms to make different points. First, '(Nd, Fig.3 and Table 3)' should be changed to '(Nd, Fig.3)' because Table 3 doesn't show aircraft Nd. The reason for switching the discussion from aircraft data to 'near surface' Nd from the RB needs to be explained. Perhaps Table 3 could include comparisons of model Nd with both RB and C-130.

d. Page 22675 Line 28-29: It seems likely that the larger variability over the remote region is due to open/closed cellular dynamics, but other factors could influence this: Other good possibilities are mentioned, but what about the decreased subsidence away from the Hadley cell (allowing for deepening of the boundary layer), or the distribution/sparseness of available data? The paper should mention the number of observations used for the 'coast' and 'remote' averages.

e. Page 22675, lines 1-3. While it is true mixing data from multiple sources can obscure a real signal only observed from one of the sensors, it also possible that the humidity contrast noted in RB data is artificially created by the sampling distribution in time

and space. Also, even with systematic differences between flight and ship sensors, both flight and ship should observe some magnitude of zonal humidity contrast if it exists and is detectable. One would need to look at the flight and ship observations separately, and combined to determine if the humidity contrast is indeed obscured in the Bretherton et al. 2010 paper. Is the humidity contrast statistically significant based on the degrees of freedom from RB observations?

g. Page 22677, Line 4-5: Is the larger variation in MBL depth during the daytime something noticed in model output, observations, or both?

h. Page 22680, Line 10: The text says "... treating sea salt as NaCl in model implies an overestimation of sodium and chloride emissions by 25% and 10%." What is the source of this information? After accounting for this effect, there is still an overestimation by factor of 1.9. Does this number apply to both sodium and chloride separately by the same amount?

i. Page 22681, Line 18-20: While this may be indicative of the first aerosol indirect effect, strictly speaking the first AIE is described in conditions of constant CWP. While not obvious from the color scale used in Figure 6, there is a longitudinal gradient of CWP in the region that complicates attribution of the first AIE. I suggest removing the mention of the AIE as it is not discussed further here.

Technical Comments

i. Figure 1 caption: include '(red)' after 'RB ship' to be consistent

ii. Page 22684, Line 3: The word 'identical' is too strong of a word for this situation.

iii. Page 22684, Line 9: Refer to Fig. 5 after "AOD"

iv. Page 22684, Line 10: Break this into two sentences, e.g "...biases in TOA SW. For example, ..."

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