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Interactive Comment

Interactive comment on "Impacts of air–sea interactions on regional air quality predictions using WRF/Chem v3.6.1 coupled with ROMS v3.7: southeastern US example" *by* J. He et al.

Anonymous Referee #1

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The authors first compared two different cumulus parameterization schemes (e.g., G3D and GF schemes) in WRF/Chem V3.6.1. Basically, the GF scheme is an improved version to the G3D scheme as documented by Grell and Freitas [2014]. No surprise the meteorological predictions are better by GF scheme.

The authors then use GF scheme combined with 1-D ocean mixed layer model (WRF/Chem-OML) and coupled with a 3-D Regional Ocean Modeling System (WRF/Chem-ROMS), respectively. The authors concluded that WRF/Chem-ROMS improves the predictions of meteorological variables and surface concentrations of some chemical species, hence the regional air quality forecasts can be improved from the coupled atmosphere-ocean models.

General Comments:

1) The paper is not well structured. The figures are too small, especially for the font size in the figures. When making the domain average, the authors should cut the points along the lateral boundary off. The language is occasionally inappropriate, and there are too many repeated words, such as ‘as a result’, ‘due to’, and ‘likely due to’.

2) It is not an easy work to make a coupled atmosphere-ocean regional model work well. In this paper, the authors presented a control experiment (SEN1), which obviously over-predicted the CF, LWP, COT, LHFLX and SHFLX over ocean. The positive feedback from ocean surface to atmosphere leads to the over predicted precipitation, which is 128% more than TMPA observations over ocean. The spatial correlation for the basic meteorological variables, such as precipitation and WS10, are very low. Basically, the control experiment shows the current settings for WRF/Chem has low skills on the meteorological predictions over ocean. The WRF/Chem-ROMS weakens the positive feedback by cooling the SST through under-predicted SWD and over-predicted surface heat fluxes. It is the scenario that one bias offsets another bias. In fact, there are other physics schemes in WRF model available to make the influence of the prescribed SST on the atmospheric simulations much smaller. It seems the authors didn’t active the shallow convection scheme in the WRF/Chem model? The vertical mixing by shallow convection scheme is very important. Overall, the simulations are not successful. Based on those reasons, the authors’ final conclusion “ the significant impacts of air-sea interactions on chemical predictions” is not robust.

3) The authors should be careful when evaluate the PBLH. There are different methods to calculate PBLH [Seidel et la. 2010, 2012], and the method from NCEP/NARR and YSU PBL scheme could be different. The recent findings by Schmid and Niyogi [2012] show the PBLH calculated by radio soundings are not well matched to the PBLH from NARR, and has a relatively better agreement in cold season.

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4) The authors used surface T2 and Q2 to represent the conditions in the whole troposphere. This is not acceptable because the biases of moisture and temperature at lower troposphere could be very different to the biases at surface. The authors should check the temperature and specific humidity profiles instead of only at surface layer.

5) It seems the authors are not able to explain the mechanisms clearly from the model results. For example, why GF scheme could generate less precipitation, why SST has a cold bias in SEN3, and why LHFLX is changed, etc.

Specific Comments:

1. Page 9966, Line 6: Add 'two' between 'with' and 'different' because in this paper the authors only used two different cumulus schemes.

2. Page 9966, Line 10: Is it 4.8 mm/day instead of 4.8 mm?

3. Page 9966, Line 15: Domain averaged SST change of 1°C is considered large in a regional coupled atmosphere-ocean model.

4. Page 9967, line 7: It needs references.

5. Page 9967, lines 9-10: "However, SST patterns can impact precipitation patterns and therefore affect atmospheric heating through latent heat flux". This sentence is strange and need references.

6. Page 9967, Lines 10-12: This sentence also needs references.

7. Page 9968, Line 7: change 'coupled' to 'uncoupled'.

8. Page 9968, Paragraph 1: the whole paragraph needs references.

9. Page 9969, Line 16: The G3D scheme should refer to Grell and Devenyi [2002, GRL].

10. Page 9971, Line 10-11: The authors used 10min as the coupling time. Are the results sensitive to the coupling time?

11. Page 9971, Line 19: the National Climatic Data Center has been renamed to NOAA's National Centers for Environmental Information (NCEI).

12. Page 9973, Line 13: missing 'to' between 'due' and 'an'.

13. Page 9973, Lines 15-16: Q2 cannot represent the whole troposphere. Please see item 4 in general comments.

14. Page 9973, Lines 16-19. The sentence "GF scheme is designed to be less active as the grid size reduces to cloud resolving scales." is correct, but it is not the cause of the decreases of precipitation in SEN1 over most of domain. The cause could be more active convection in GF scheme can dry the troposphere by compensatory subsidence, and reduce grid-scale precipitation.

15. Page 9973, Lines 24-25: Please see item 4 in general comments.

16. Page 9974, Lines 3-5: The explanation to the under-predicted low CF and LWP could be inaccurate since these are two different cumulus schemes. The G3D scheme has a very large portion of grid-scale precipitation, which is usually associated with saturated grids and large LWP and CF at those grids.

17. Page 9974, Lines 10-11. The increase of CDNC over the remote ocean is not significant at all in Fig.3.

18. Page 9974, Line 16: Is it increased or decreased for 1.4 W m-2?

19. Page 9974, Lines 24-26: The authors concluded " Both LHFLX and SHFLX are overpredicted in BASE and SEN1, which is mainly due to lack of representations of the air-sea interactions". The lack of representations of the air-sea interactions can be one of the reasons for the over-predicted BASE and SEN1, but I think the ill-represented convection-cloud-radiation in the model is the major reason.

20. Page 9974, Line 29: exchange 42.2% to 218.8%.

21. Page 9975, Lines 4-6: The authors claimed less precipitation results higher CDNC

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associated with smaller cloud effective radius? Are there evidences to support this conclusion?

22. Page 9976, Lines 4-6: Yes, The large overpredictions of SWCF and LWCF over ocean are attributed to the inaccurate predictions clouds over ocean, indicating the model uncertainties in the cloud dynamics and thermodynamics. But this is definitely not a new conclusion.

23. Page 9976, Lines 13-14: The authors said " ...due to the increase of more convection over ocean (e.g., higher PBLH)". This is confusing. Could the authors explain more?

24. Page 9978, Lines 2-3: The authors stated " This cools the mixed layer, which reduces the SST and hence surface fluxes." In face, ocean surface fluxes are determined by ocean surface temperature as well as near-surface air temperature, humidity and wind speed.

25. Page 9978, Lines 14-15. The authors concluded " The decrease of SST in SEN3 is mainly due to the lower SST from initial conditions from global HYCOM." But it is clear in Fig. 5 that SWD in SEN3 is lower, and LHFLX is higher. The ocean surface is loosing energy.

26. Page 9978, Line 18: "T2 and SST decrease in SEN3, resulting in less evaporation." Again, evaporation is also controlled by other variables.

27. Page 9980, Lines 1-2: The sentence "SST anomalies can induce opposite atmospheric changes in coupled atmosphere-ocean simulation SENS (Wu and Kirtman, 2005,2007) is confusing.

28. Page 9984, Lines 20-23: Do the intensity and duration of precipitation influence the performance of surface predictions of TC, PM2.5, PM10?

29. Page 9986, Lines 5-11: If so, why the authors still chose the current ICs and BCs for ROMS.

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30. Page 9989, Line 10: 2015a.

31. Page 9994, Table 1: It is meaningless to use NMB and NME for T2.

32. Page 10000, Figure 3: 'SWDOWN' to 'SWD' and 'mm' to 'mm/day'?

Technical comments:

1. Page 9973, Line 3: change 'Corr.' to 'Corr'.

2. The font size is too small in Figure 3, 5, and 6.

References:

Paul Schmid and Dev Niyogi (2012), A Method for Estimating Planetary Boundary Layer Heights and Its Application over the ARM Southern Great Plains Site. J. Atmos. Oceanic Technol., 29, 316–322.

Seidel, D. J., Y. Zhang, A. Beljaars, J.-C. Golaz, A. R. Jacobson, and B. Medeiros (2012), Climatology of the planetary boundary layer over the continental United States and Europe, J. Geophys. Res., 117, D17106.

Seidel, D. J., C. O. Ao, and K. Li (2010), Estimating climatological planetary boundary layer heights from radiosonde observations: Comparison of methods and uncertainty analysis, J. Geophys. Res., 115, D16113.

Interactive comment on Geosci. Model Dev. Discuss., 8, 9965, 2015.