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Geoscientific Model Development Discussions

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.

J. He et al.

yzhang9@ncsu.edu

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Reply to Comments by Reviewer #1

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.

Reply:

We thank the reviewer for constructive comments. Please see below our replies to general and specific comments. The line/page numbers refer to those in the track mode version of the revised paper.

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'.

Reply:

To address the reviewer's comment, we have revised the paper to make it better structured and presented. We have enlarged those figures along with the font size. We also cut the lateral boundary off for the domain average calculation in the revised paper, although the domain average values remain similar to those without cutting the lateral boundary. Some sentences are also revised to avoid repeated words in the revised paper.

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.

Reply:

We appreciate the reviewer for recognizing that it is not an easy work to make the regional coupled model work well. We, however, respectfully disagree with the reviewer on the comments that "the simulations are not successful". The main reason that the reviewer drew the conclusion "the authors' final conclusion " the significant impacts of air-sea interactions on chemical predictions" is not robust." is "the authors didn't active the shallow convection scheme in the WRF/Chem model?". This speculation is unfortunately not right. In all our past WRF/Chem simulations including those performed in this work, the shallow convection scheme has always been activated. To our best knowledge, nearly 100% of WRF and WRF/Chem simulations reported in the literature use prescribed SST. Our work represents the first that couples WRF/Chem with a dynamic three-dimensional ocean model. This success of model coupling has been well recognized by the other two reviewers, both suggested acceptance of this paper with very minor revisions.

There are several additional points worth mentioning.

First, the WRF/Chem model configurations used in the baseline simulation (BASE) in this work have been demonstrated to give an overall good model performance over continental U.S. (Wang et al., 2015; Yahya et al., 2015). The simulation to test cumulus parameterization and as a control run for air-sea interaction testing (SEN1), uses the

same configuration as BASE except for the cumulus parameterization (see Table 1). SEN2 (SEN3) has the same configurations as SEN1 except using 1D (3D) dynamic ocean coupling As shown in Table 3a, comparing to BASE, SEN1 outperforms substantially for precipitation and CDNC over both land and ocean, and LWP and SHFLX over ocean; it gives similar performance for most other variables. However, to our best knowledge, the model evaluation of WRF/Chem over ocean has never been performed until this work. The poor performance of the control run (SEN1) over ocean is not surprising, and the fundamental reason is obvious- lack of appropriate air-sea treatments. The low skill of WRF/Chem on the meteorological predictions over ocean showed in the control experiment supports our motivation to couple WRF/Chem with ROM and attests the importance of our work and significance of our major findings.

Second, error/bias compensations often occur in model simulations. In this work, the control run (SEN1) shows 20% underprediction in SWD against CERES, but overpredictions by 60% in latent heat flux (LHFLX) and by 140% in sensible heat flux (SHFLX) against OAFlux. Note that the observations for SWD and heat fluxes came from different sources, each may contain some uncertainties, which may affect the extent of the compensation effect. WRF/Chem-ROM reduces the underprediction in SWD to 14% and substantially reduces the overpredictions in both LHFLX (by 19% comparing to 60% in the control run) and SHFLX (by 50% comparing to 140%). Although there remain some bias compensations in the WRF/Chem-ROM simulation, the magnitudes of biases for all those variables are all reduced, indicating improved model performance.

Third, with regard to the reviewer's comment "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", we'd like to point out that our focus in this study is on examining the importance of dynamic ocean coupling in meteorological conditions and air-quality prediction. This is a first order physics that has been missing in previous air-quality modeling work. We hope the reviewer could provide details on different WRF schemes he/she was referring to, so we can examine them in future work. To address the reviewer's doubts on our work, we conducted student's t-test to demonstrate that the impacts of the explicit air-sea interaction treatments are statistically significant. Those results are summarized in section 3.2.2, see lines 569-580, page 25-26.

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.

Reply:

We are aware of various methods used to calculate/estimate PBLH in both field studies and also model simulations. To address the reviewer's comments, we have included some discussions on the uncertainties in PBLH evaluation in the revised paper, see lines 270-275, pages 12.

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.

Reply:

We did not use T2 and Q2 to represent the conditions in the whole troposphere. As shown in the paper and Tables, we have evaluated the model performance using data from all available surface networks and satellites. Many satellite variables are column variables, e.g., SWD, OLR, SWCF, CF, COT, and LWP. CDNC is for all cloudy layers. The fact that we have carried out a comprehensive evaluation throughout atmospheric columns does not support the above statement made by the reviewer. To further address the reviewer's comment, we have additionally included temperature and specific humidity profiles evaluation in the revised paper, see lines 258-263 and page 12. As

shown in our evaluation in Figures 4a and 4b, the biases of temperature at lower troposphere are nearly the same as those at surface at all sites; both are very small. For moisture, the biases in lower troposphere are actually not very different to the biases at surface at most sites.

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.

Reply:

As the reviewer pointed out earlier, the coupled model system is quite complex, so we did not claim that we have understood all the interactions and feedbacks the system produced. The scope of this manuscript, which is in line with GMD publication requirement, is to document our efforts of coupling WRF-Chem with a dynamic three-dimensional ocean model using an advanced model coupling toolkit. Our premise is that the air-sea interaction is a first order physics that is missing in the WRF and WRF/Chem modeling studies to date. Through systematic model sensitivity studies (baseline, SEN1, SEN2 and SEN3), we have demonstrated that accounting for air-sea interaction has some significant and positive impacts on predicting the meteorological and air-quality state variables. Now this powerful coupled modeling system is in place, we will continue using it to explore the underlying mechanisms, and will report our findings in future communications.

To address the reviewer's comment, we have now included additional in-depth discussions to address the above reviewer's comments in Sections 3.1.1 and 3.2.1, see lines 227-232, 397-403, 427-433 and pages 10-11 and 18-19 in the revised paper. For the reviewer's convenience, we believe that GF scheme generates less precipitation than G3D is because of the differences in autoconversion mechanism used in both schemes, SEN3 gives a cold bias in SST because of initial conditions from global HYCOM, the use of a coarse vertical resolution in ROMS, as well as strong simulated surface wind and possible weak southward currents from ROMS. SEN3 predicts lower LHFLX because difference in sea-air specific humidity (i.e., Qs – Qa) and lower surface wind speeds in SEN3.

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.

Reply:

This has been added in the revised paper.

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

Reply:

It is 4.8 mm day-1. We have corrected this in the revised paper.

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

Reply:

Yes. We agree this is a large change. We have also discussed the reasons in Section 3.2.1, lines 420-433, page 19.

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

Reply:

They have been added in the revised paper.

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.

Reply:

We have revised the sentence and added the references in the revised paper, see lines 41 - 43, page 2.

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

Reply:

We have added the references in the revised paper.

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

Reply:

In this sentence, the coupled model is referring to the coupled atmosphere-ocean model but without chemistry/aerosol prognostic treatments in the atmospheric component. This has been clarified in the revised paper.

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

Reply:

We have added the references in the revised paper.

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

Reply:

There are some updates in G3D since Grell and Devenyi (2002), which is indicated in Grell and Freitas (2014), we have included both Grell and Devenyi (2002) and Grell and Freitas, 2014 as G3D references in the revised paper.

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

Reply:

The coupling time is a free parameter in our modeling system and practically, it is determined based on model physics and computational load. In this study, we focused on the synoptic, meso-scale dynamics of both ocean and atmosphere, which require the coupled modeling system to resolve air-sea flux exchange process over a wide frequency spectrum. In this regard, the coupling interval should be as frequent as computationally possible. The choice of 10 mins currently presents a good balance between physics and computing overhead associated with system coupling. We are examining the sensitivity of coupling time and will report our findings in a future correspondence. To address the reviewer's comments, we have now included above information in the revised paper, see lines 158-162, pages 7-8.

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

Reply:

We have corrected this in the revised paper.

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

Reply:

This has been corrected in the revised paper.

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

Reply:

To address the reviewer's comment, we have included profile evaluations in the revised paper, see Figure4b and lines 259-263, page 12.

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.

Reply:

To address the reviewer's comment, we have included additional discussions in Section 3.1.1, see lines 228-232, pages 10-11 in the revised paper.

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

Reply:

To address the reviewer's comment, we have included profile evaluations in the revised paper.

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.

Reply:

To address the reviewer's comment, we have included additional discussion in Section 3.1.1, see lines 240-241, page 11 in the revised paper.

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

Reply:

The changes in SWCF are due to the combined effects from changes in CDNC and COT. We have included this discussion in the revised paper.

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

Reply:

It is an increase. We have clarified this in the revised paper.

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.

Reply:

We believe that lack of representations of the air-sea interactions is a main reason for such overpredictions. This is supported by a substantial reduction in NMBs in SEN3 (27% for LHFLX and 77% for SHFLX), comparing to those in BASE (59% for LHFLX and 195% for SHFLX) and SEN1 (60% for LHFLX and 140% for SHFLX). The uncertainties associated with the WRF/Chem's representations of convection-cloud-radiation may also contribute to the overpredictions. For example, Alapaty et al. (2012) reported that lack of the model treatments of subgrid cloud feedbacks in radiation calculation in WRF can explain overpredictions in shortwave radiation and precipitation. Even though the convection-cloud-radiation feedback is included in G3D/GF in WRF/Chem v3.6.1, uncertainties exist in the model representation of such feedbacks.

To address the reviewer's comments, we have now included additional possible reasons in this part in the revised paper, see lines 278-284, page 13.

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

Reply:

We have corrected this in the revised paper.

21. Page 9975, Lines 4-6: The authors claimed less precipitation results higher CDNC associated with smaller cloud effective radius? Are there evidences to support this conclusion?

Reply:

The cloud effective radius (CER) is unfortunately not included in the model output. But

LWP is proportional to both the COT and CER, since COT in SEN1 is higher than BASE, the decrease of LWP in SEN1 can be due to the smaller CER in SEN1. Also, the increase of CDNC is usually associated with a decrease in CER. Precipitation is reduced in SEN1, resulting in more aerosols that can be activated to increase CDNC. To address the reviewer's comment, we have now included additional discussions in this part in the revised paper, see lines 292-299, pages 13-14.

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.

Reply:

To our best knowledge, our paper represents the first comparing the results from WRF/Chem-ROMS with those standard versions of WRF/Chem and also the first showing quantitative performance statistics of the simulations using those model versions over ocean.

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?

Reply:

To address the reviewer's comment, we have changed "more convection" to "vertical mixing" in the revised paper.

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.

Reply:

To address the reviewer's comment, we have rephrased the sentence as follows (also see lines 117-121, page 6, in the revised paper).

The 1-D OML model represents a simple balance between the local rate of change of ocean temperate and the net surface heat flux. Negative (positive) net heat flux leads to a cooling (warming) trend of ocean temperature, which in turn affects marine boundary layer stability, air temperature and surface wind distributions (e.g., Chelton et al., 2007).

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.

Reply:

Compared to observational data, SWD in SEN3 is lower, and LHFLX is higher. But compared to SEN1, SWD in SEN3 is higher, and LHFLX is lower. Here we are comparing SST in SEN3 with SEN1.

To address the reviewer's comment, we have clarified this and also included additional discussions in Section 3.2.1 in the revised paper, see lines 391-394, page 18.

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

Reply:

To address the reviewer's comment, we have included additional discussions, see lines 397-405, page18 in the revised paper.

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.

Reply:

In SEN3, lower SST results in less evaporation and less convective instability, therefore, less precipitation over ocean, which is consistent with previous studies (e.g., Keeley et al., 2012). The lower precipitation in SEN3 than SEN1 is probably due to the changes in moisture flux convergence through large-scale changes in the circulation field and SST predicted by ROMS. As a result, the performance of precipitation (see Table 3a) is improved in SEN3 significantly, with NMBs of 29.5% (land)/119.2% (ocean). To avoid confusion, we have removed this sentence and also included additional discussion in the revised paper, see lines 456-461, page 21.

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

Reply:

Yes, the intensity and duration of precipitation can influence the surface predictions of chemical species, we have included this factor, see line 531, page 24 in the revised paper.

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

Reply:

Among all possible datasets, HYCOM is the best one in terms of grid and temporal resolutions. HYCOM is a high resolution global analysis dataset (1/12o) with data frequency on daily basis. The resolution is very close to the grid resolution of 12-km used in this work. For other datasets, they either have relatively coarse grid resolution or their data frequency is on a monthly basis, which can result in large biases when they are interpolated into the model grid resolution or time period.

To address the reviewer's comments, we have now explained why HYCOM was chosen, see lines 147-151, page 7 in the revised paper.

30. Page 9989, Line 10: 2015a.

Reply: This has been corrected in the main text in the revised paper.

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

Reply:

NMB and NME do carry some meanings for T2, although we understand that many meteorologists would prefer other metrics such as MB and RMSE. To address the reviewer's comment, we have now included MB and RMSE when discussing T2 in the revised paper.

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

Reply:

We have corrected this in the revised paper.

Technical comments:

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

Reply:

We have made the suggested change this in the revised paper.

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

Reply:

We have enlarged the font size in the Figures 3, 5, and 6 in the revised paper.

References cited in this reply:

Chelton, D. B., Schlax, M. G., and Samelson, R. M.: Summertime coupling between sea surface temperature and wind stress in the Californi Current System, J. Phys. Oceanogr., 37, 495-517, 2007. Large, W. G.: Surface fluxes for practitioners of global

ocean data assimilation. An Integrated View of Oceanography: Ocean Weather Forecasting in the 21st Century, Chapter 9. Springer, 2006.

Wang, K., Yahya, K., Zhang, Y., Wu, S.-Y., and Grell, G.: Implementation and initial application of a new chemistry-aerosol option in WRF/Chem for simulation of secondary organic aerosols and aerosol indirect effects, Atmos. Environ., 115, 716-732, 2015.

Yahya, K., Wang, K., Zhang, Y., Hogrefe, C., Pouliot, G., and Kleindienst, T.: Application of WRF/Chem version 3.4.1 over North America under the AQMEII Phase 2: evaluation of 2010 application and responses of air quality and meteorology-chemistry interactions to changes in emissions and meteorology from 2006 to 2010, Geosci. Model Dev., 8, 2095-2117, doi:10.5194/gmd-8-2095-2015, 2015.

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