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Interactive comment on "Response of Global Surface Ozone Distribution to Northern Hemispheric Sea Surface Temperature Changes: Implication for Long-Range Transport" *by* Kan Yi et al.

Anonymous Referee #3

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This is an interesting modelling study that examines how surface ozone is influenced by warmer SSTs over the Pacific, Atlantic and Indian oceans. With a one degree warming across these basins, the changes in seasonal-mean ozone in the oceanic basin and its surrounding continents are rather large at 1-5 ppb. An increase in SST leads to lower surface ozone over the Pacific and Atlantic oceans but a more mixed response over the Indian ocean. The authors probe the contribution of chemistry and transport processes to these ozone changes. The paper is mostly well written but a number conclusions lack clarity and are not well-substantiated for reasons relating to poor and inconsistent figure quality and interpretation as outlined below. Hence the manuscript, needs much

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improvement before publication.

Major comments:

1) (i) As noted by the other reviewer, the map projections used vary by figure in a non-logical fashion, hence it is extremely difficult to compare results across different figures and hence verify the conclusions in the text. For example, the vertical velocity changes in Figure 6 versus the surface pressure pattern changes in Figure 5 versus the changes in ozone concentrations in Figure 7 (See specific comments also). (ii) In addition, the continental outlines and hence oceanic basins are too difficult to distinguish if they are visible at all. (iii) Finally, most figure panels are too small to be legible- except for Figures S2-S5 which are hugely improved on the other figures (although the continental outlines are still hard to see in Figure S2).

2) The seasonal mean surface ozone changes are quite large. This message could be brought out much more clearly. It would be beneficial to see some discussion of the magnitude of these surface ozone responses through comparison with previous papers even if these only relate to the effects of changes in air temperature or climate on surface ozone, as the further impacts of atmospheric circulation changes can be outlined.

3) (i) The IPR analysis needs to be described more thoroughly and the processes selected would benefit with expanded definitions. In particular, gas-phase chemistry (CHEM) should be defined more clearly as later in the manuscript various other terms are used: net chemical production (Figure 3); photochemistry (line 265). Also vertical diffusion (VDIF) and dry deposition (DRYD) are combined into one term TURB- but these terms act in opposite directions in Figure 2. It would be useful to provide a brief outline as to why these terms are expected to act in opposite directions. (ii) All IPR related figures- Figures 2/S1 are very difficult to read. In addition the relationship between the fluxes and concentrations as plotted on figure 2 is unclear, and appears sensitive to the scaling's used on the right and left hand y-axes. See specific comments

3-8 below. (iii) The text discussing IPR results in section 4.1 is generally confusing and not well substantiated: often the season being referred to is not provided and general statements are sometimes given that only seem applicable to results in boreal summer. The text is section 4.3 also needs to be clarified and tightened in a good number of places- see specific comments. iv) For Figure 2/Table 1, it would be highly beneficial to also have results for the direct effect of a change in SSTs on regional surface ozone in that surface basin before any discussion of upwind or downwind continents. This would aid with interpretation as to the dilution of the ozone response with regional averaging.

4) As noted above for the IPR results, but also in general, the text on the various contributions or roles of intercontinental transport versus that of chemistry is difficult to follow in a number of places and some conclusions appear over-stated. For example, the abstract discusses "suppression of O3 intercontinental transport due to increased stagnation at mid-latitudes induced by SST changes". Stagnation is a localised process largely determined by boundary layer processes and entrainment. Hence, the authors should be cautious in their interpretation based on large-scale changes in wind vectors and vertical velocity to infer changes in stagnation/ventilation. Perhaps clear definitions of what is meant by these terms would be useful. See specific comments below.

5) A number of references in the text are rather old, and some updated references would be highly beneficial. See specific comments below. Also with multi-references the logical order is unclear- chronological order is most commonly used.

Specific comments:

1) As also noted by the other reviewer the frequent use of parenthesis to state a key result dilutes the message of the sentence and makes for a confusing read. Please re-phrase when key points are being made in the abstract and main text (lines 223-230).

2) Line 207/Table 1 – as noted above it would be beneficial to first show a similar table that examines the effect of SST changes within each basin and on other ocean basins.

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3) Line 242- what is meant by atmospheric turbulence intensity and explain to the reader how this relates to VDIF and DRYD.

4) Line 248 “reducing it over North America. For the Pacific W panels in Figure 2 a reduction in VDIF is only seen in in summer in North America; VDIF increases just as strongly in North America in winter and spring.

5) Line 248- “similar increases in VDIF are simulated over North America. Similar to ?

6) Line 253- “the increase of CHEM tends to dominate the surface O3 increase over North America.” This is not obvious from Figure 2 (and it is unclear which season/s are being discussed), and is unintuitive without a clearer definition of CHEM, and how fluxes relate to concentrations in Figure 2.

7) Line 254- “TURB is more important . . . leasing to reduced surface O3 concentrations.” Again the positive and negative fluxes in JJA and SON look to balance so why are there reduced ozone concentrations. Line 257- as above the fluxes look as though they balance (especially in JJA) but ozone concentrations are reduced.

8) Lines 260-263- It would be helpful to define remote versus downwind. Remote is used in this sentence and downwind in the following sentence. If North America is the remote continent in the Pacific W simulation then VDIF is only suppressed in summer, but not in winter and spring.

9) Line 266- “change in photochemistry. . . advection . . . dominates the feedbacks of Indian Ocean warming- CHEM appears as a substantial component in the lowermost right hand panel of Figure 2.

10) Line 275- “Peak changes are confined to the polluted region because of their high precursor emissions”. This is not obviously related. Please explain this statement more clearly. The examples that follow to the end of the paragraph referring to Figure 3 (the regions discussed are hard to see) do not clearly substantiate this.

11) Lines 290 to end of paragraph- “Increase in SST facilitates moist convection. . .

(Lau. . .1997)"- Figure 2 for summer for the Indian W influence on SA- suggests a decrease in deep convection. Please clarify? Here the references used are rather old. This is an interesting find, an increase in SST would be normally accompanied by an increase in surface air temperature directly above the ocean, but yet figure S3 shows cooling but with warming above. Hence it would be beneficial to provide an expanded interpretation of this finding, compare the results with those from more recent papers on how elevated SSTs in the Indian Ocean region or tropics affect surface air temperature and convection.

12) Line 297- The text relating ozone production -temperature relationships to net surface ozone production relationships with temperature should be clarified: it is the ozone not the ozone production that is related to temperature in the references cited, and as the authors note both ozone production and destruction rates will increase with temperature (directly and indirectly through higher humidity).

13) Line 318- As shown in Figure 6. . . surface pressure reduction is closely associated with enhanced upward motion. Please use the same map in Figures 5 and 6 in order to see this association.

14) Line 329- "This effect is confirmed by widespread decreases of upward vertical velocity". Again it is hard to see if vertical velocity reductions are occurring only over the adjacent regions to the regions where the authors suggest enhanced convection may occur.

15) Line 333-end of paragraph. This first sentence of the paragraph discusses atmospheric stability based on zonal mean large-scale temperatures changes between the upper and lower troposphere (a weaker vertical temperature gradient; Figure S3) and stagnation/ventilation which are local processes often related to surface winds. Hence these processes may not be as simply related as suggested. In addition, a differential ozone response over clean and polluted regions seem unlikely to be associated with change in atmospheric stability associated with large-scale increases in upper

tropospheric temperature. The final sentence of the paragraph needs substantiated especially given the link proposed in the previous section between clean regions with reduced net ozone production due to greater destruction.

16) Line 341- please explain how a reduction in low pressure weakens the East Asian monsoon?

17) Line 349/Line 357- if the IPR analysis refers to Figure 2 there seem to be a few inconsistencies – the influence of Pacific W on EA then VDIF appears to have the strongest role, yet advective transport is discussed here? The influence of the Atlantic W on NA then CHEM seems only to have a small contribution in Figure 2 and not be the main contribution discussed here. Furthermore, the logic of the argument that physical transport is not important because of large changes in the upper troposphere but small changes at the surface is unclear.

18) Line 380 to end of paragraph- The results in figures 8 (CO tracer) and 7 (ozone concentrations) look similar and reinforce each other except over the Indian Ocean. Please comment on this.

19) Line 395 to end of paragraph- Is significance plotted in figure 9?- The text cannot be followed well here with the current figure quality. The conclusion on vertical diffusion is hard to follow, given text in previous sections discussing areas of both enhanced convection and subsidence in the ocean basin and downwind.

20) Line 435- “90% of surface O3”- first mention of this in the text.

Minor comments:

1) Line 49- it would be useful to state why ground-level ozone affects food security. Also it would be useful to provide a more up to date reference than 2006 for WHO.

2) Line 54- again can the authors use a more recent reference than Vingarzan et al. 2004.

3) Line 66- an enhanced description of what is meant by atmospheric circulations would be useful e.g. Barnes and Fiore (2013) specifically discuss the effect of the Jetstream in the northern midlatitudes at 500 hPa. Other processes to mention are mid-latitude cyclones and the North Atlantic Oscillation for the N. Atlantic. Some further useful references include: Creilson et al. 2003; Christiadios et al. 2012; Knowland et al. 2014. Line et al. (2012/14) and references therein are useful for circulations relating to atmospheric circulations in the N. Pacific.

Creilson, J. K., Fishman, J., and Wozniak, A. E.: Intercontinental transport of tropospheric ozone: a study of its seasonal variability across the North Atlantic utilizing tropospheric ozone residuals and its relationship to the North Atlantic Oscillation, Atmos. Chem. Phys., 3, 2053–2066, doi:10.5194/acp-3-2053-2003, 2003

Christoudias, T., Pozzer, A., and Lelieveld, J.: Influence of the North Atlantic Oscillation on air pollution transport, Atmos. Chem. Phys., 12, 869–877, doi:10.5194/acp-12-869-2012, 2012 Knowland, K. E., Doherty, R. M., and Hodges, K. I.: The effects of spring-time mid-latitude storms on trace gas composition determined from the MACC reanalysis, Atmos. Chem. Phys., 15, 3605-3628, doi:10.5194/acp-15-3605-2015, 2015.

4) Line 79- what is meant by “SST is an indicator for both marine and terrestrial meteorology”?

5) Line 83- perhaps the reference to the text book is unnecessary.

6) Line 870- some recent references from the IPCC AR5 report will be relevant here.

7) Line 92- it would be more useful to the reader to refer to the specific chapter in IPCC AR5- the science of climate change that discusses SST changes rather than broadly reference the IPCC synthesis report.

8) Line 102- is “according to observations” needed?

9) Line 105 “Emissions of aerosols.. complicate regional SST variability because of their climate effects”- this sentence is unclear.

10) Line 113- besides Lin et al. 2014, Liu et al. (2005) is also a valuable reference here in relation to ENSO and pollution transport from East Asia. Liu, J., D. L. Mauzerall, and L. W. Horowitz (2005), Analysis of seasonal and interannual variability in transpacific transport, J. Geophys. Res., 110, D04302, doi:10.1029/2004JD005207.

11) Line 119- it would be useful to first discuss the surface ozone response for the specific ocean basin relative to the experiment and then discuss effects on surrounding continents. The four continental regions used in Fiore et al. (2009) and elsewhere should be defined here, as they are used throughout the text.

12) Line 157 typo- AEROCOM

13) Line 161 – "scientifically" is unnecessary.

14) Line 251- "similar increases in VDIF" compared to?

15) Line 273- explain how net production rate in this section related to CHEM in the previous section.

16) Line 305 – rephrase "jointly destructs O3 production".

17) Lines 320-323, "Given that ...)." This sentence contains a number of grammar errors. The following sentence starting line 323 seems to state that the pressure difference induced by warmer SSTs would be greater at lower latitude but notes this is not shown here in Figure 5. Please comment further on this or remove.

18) Line 363- Mediterranean?

19) Line 367-"Downward diffusion from the upper troposphere"- please clarify what is meant here as this is not a region of STE.

20) Line 374- why only at mid-latitudes? Figure S3 shows large temperature increases in temperature above all 3 basins.

21) Line 439/line 440 – re-phrase "increasing influence on surface O3 concentrations"

as this is confusing e.g. regional surface ozone over SA decreases under the Indian W simulation.

22) Line 465 – natural variability is not discussed in this paper (although used for significance testing so it is odd to mention here).

23) Figure 6 refers to Figure 7 re surface pressure- should the reference be to Figure 5?

24) Figure 7 – swap panels b) and c) to be consistent with text.

25) Figures 8, 9, S3: the season is omitted from the figure caption.

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