

Interactive comment on “Ozone and carbon monoxide observations over open oceans on R/V Mirai from 67° S to 75° N during 2012 to 2017: Testing global chemical reanalysis in terms of Arctic processes, low ozone levels at low latitudes, and pollution transport” by Yugo Kanaya et al.

Anonymous Referee #2

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This paper discusses comprehensive shipborne O₃ and CO measurements covering a large oceanic region from the Arctic to the Southern Ocean over the period of 2012-2017. The dataset was thoroughly analysed and was compared to the simulation results from a tropospheric chemistry reanalysis model (TCR-2), demonstrating the usefulness of such dataset in critical model evaluation. The authors also carried out two fo-

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cused analyses assessing the underlying processes causing models to underestimate Arctic O₃ and to overestimate O₃ in the western Pacific equatorial region, respectively, compared to observations. The paper is very well written with detailed and in-depth analyses. The dataset is a significant addition to the current surface O₃ database over remote oceanic regions and is valuable for model evaluations. I recommend the paper to be published after the authors have addressed some minor comments that are detailed below.

Specific comments:

Page 1, L31: “less efficient dry deposition” than assumed in the model sounds very speculative. Dry deposition coefficient is generally considered very slow over the ocean, and it is unlikely that there is much room for a significant impact when adjusting the dry deposition coefficient.

Page 1, L33: “the observed O₃ level frequently decreased to ...”: could add “more” before “frequently”

Page 3, L1-5: I am not sure CO₂ observations are that relevant here.

Page 4, L27: please define “BC”.

Page 5, 1st paragraph: Can you briefly describe the chemical mechanism used in the TCR-2 framework? This will inform a later discussion of photochemical production.

Page 6, L10 & 11: Suggest naming the regions where these locations are.

Page 6, L22 & 23: “South of ...” & “In equatorial regions...” seem overlap in what the authors try to convey; South of 15°N implies the equatorial region as well, if it doesn't go beyond 15°S.

Page 7, L29: Could you elaborate a bit more on “overestimate of photochemical O₃ production”?

Page 9, L27, Is “less efficient production of O₃” related to the chemical mechanism

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used in the model?

Page 10, L20: Could you elaborate “Processes other than daytime photochemistry. . .”?

Page 11, L6: the significance can be established by Student’s t test.

Page 11, L17: “unlike large increments obtained at low and. . .” – what does “large increments” refer to?

Page 11, L29-30: what is the dry deposition coefficient over this region used in the model? How does it compare with literature values for a similar land surface type? It is unlikely that dry deposition plays a significant role here, especially over the ocean; see above.

Page 11, L31-32: Can you put a reference here?

Page 11, L33: what is “(AMAP 2015)”?

Page 12, P14: It looks like the comparison between observations and ACCMIP models depends on the frequency ranges, and it is too general to claim the comparisons are poorer for December. Maybe you could elaborate on the seasonal difference in model performance? Is there any systematic model bias that are season dependent?

Page 12, L16-17: “The large variations among the model results could be the result of different assumptions regarding the dry deposition velocity of O₃” – Do you have any reference to back this up? It surprises me that differences in dry deposition among the models over the ocean would result in such a large model spread. I’d rather think that differences amongst the models in the efficiency of transport of mid-latitude polluted air to the Arctic, coupled to maybe differences in mid-latitude ozone production, is likely the driving factor. The large model spread in the middle ranges in March might reflect the impact of large variations in transport.

Page14, L22: again, what is the dry deposition coefficient over the ocean used in here? See my previous comments regarding the unlikely impact from dry deposition

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over the ocean. Many of the ACCMIP models use “off-line” dry deposition schemes characterized by prescribed, fixed dry deposition velocities over open water and ice which are documented in the literature. Hence an assertion that variations in these assumed dry deposition velocities drive the differences in the simulation of O₃ needs to be backed up by a discussion of this literature and does not need to be the subject of speculation. Page14, L29: I would replace “later inter-comparisons” with “future intermodal comparisons”.

Technical comments:

Page 6, L5 & L9: may replace “namely” with “i.e.” Page 12, L5: Do you mean “studied region”? Figure 1: swap positions of (a) and (b), domain 3 is not clearly visible Figures 6 and 7: “CO” missing from the captions – should be “surface CO and O₃” Figure 13: add “mixing ratios” after “maximum”

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