

Interactive comment on “Arctic sea ice, Eurasia teleconnection pattern and summer surface ozone pollution in North China” by Zhicong Yin et al.

Anonymous Referee #2

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Review of “Arctic sea ice, Eurasia teleconnection pattern and summer surface ozone pollution in North China” by Yin et al.

Summary and General Comments:

This study attempts to link surface O₃ from a site in North China to May Arctic sea ice extent and the Eurasia Teleconnection Pattern (EUTP), which amounts to a total of three degrees of separation (May Sea Ice -> Eurasia Teleconnection Pattern -> Ozone Weather Index (OWI) -> MDA8 O₃ at the SDZ site). The main results of the paper seem a bit overstated because of this leap. The authors show some nice analyses linking each of these factors to one another, but the point of this paper is to show skill in predicting summer average MDA8 O₃ from EUTP and May Arctic sea ice. I am not convinced the authors were so successful in that regard. For example, on

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Figure 6b, the association with the Sea Ice Index and MDA8 is weak at best (see 2009, 2014, 2015, and 2017), and only ~11 years of JJA O₃ data from a single site are used to make these claims. The authors state in the abstract that May sea ice extent explains 60% of the interannual variability in summer surface O₃, but that is actually the relationship that they found between sea ice and the OWI (Lines 188-190), which has its own separate, imperfect, relationship with MDA8 O₃ levels at one specific site. In general, I found the statistical analysis and OWI formulation in the first half of the paper to be more enlightening than the second half. Perhaps a more careful discussion on EUTP and the sea ice effect will lead to a more convincing paper - Sections 4 and 5 are barely two pages of double-spaced text.

There is a wealth of information buried in the Supplemental Figures, which is often frustrating to a lot of readers. Figures S1 and S6 in particular receive a lot of attention in the text, and therefore should be included as regular figures. Figure S11 is of great importance to the result stated in the second last sentence of the abstract (though I have separate issues with it as I mention above). Several of the Supplemental Figures should be moved to the main paper.

What is the motivation behind analyzing NCEP/NCAR Reanalyses in addition to ERA-Interim? This adds unnecessary supplementary figures and text. You can simply state in the text that a sensitivity test was performed with NCEP/NCAR reanalysis, which yielded very similar results (this should be expected).

Recommendation:

In its current form, this paper is not yet suitable for publication in ACP. I am suggesting major revisions that include reorganization of Figures, and substantial edits to the text, mainly in the Introduction and Sections 4 and 5. I think that the paper discussion should remain focused on the meteorological relationships found linking the teleconnection patterns and surface O₃ (which are interesting and useful to quantify!), and less on claims of seasonal predictability that may not be warranted by the current study.

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Specific/Minor Comments:

Line 10: Surface ozone pollution is only increasing in certain parts of the world. Please be specific about China here.

Introduction: I find the discussion on “haze pollution” to be very confusing, especially because particulate pollution is not the focus in this paper. It would make the introduction much clearer to eliminate the use of “haze” and discuss only the O₃ pollution issues in China.

Lines 26-27: Surface O₃ is a major component of photochemical smog, so it is actually very frequently visible to humans when found in high amounts.

Lines 27-28: I disagree with this sentence. Surface O₃ pollution in China is now a heavily studied topic. Rewrite this sentence to mainly highlight the novel aspect of your research into this topic: “The impacts of climate variability on surface O₃ pollution in China have not been sufficiently studied.”

Lines 28-29: This sentence on European pollution controls is out of place. Find a local example of pollution controls in China to discuss or remove this sentence entirely.

Line 30: Please quote the current Chinese air quality standards for surface O₃.

Lines 35-40: These examples of large-scale atmospheric circulation patterns affecting surface O₃ are relevant, but need to be presented more clearly to allow the reader to understand why they are worth discussing.

Lines 40-47: Similar comment as the one above. There are several studies discussed in one sentence each, but the organization makes this cumbersome to read. In general, the Introduction would benefit from a careful rewrite.

Lines 47-52: This paragraph is a good example of how the rest of the Introduction should be written to motivate carrying out this study.

Line 79: The first figure introduced should not be a supplemental figure. Figure S1 is

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discussed extensively in the text, so it would be useful to just make this Figure 1.

Figure S1 Caption: Green “triangle”, not “triple”

Line 98: Are these statistics only for June to August? Please be clear.

Line 101: Clarify that you are still referring to the O₃ levels at SDZ.

Figure 1, panels g and h: Clarify in the figure caption which temperature is contoured and which is shaded in color. Does geopotential height at 500 or 250 hPa tell you more about surface O₃ variability than 200 hPa temperature?

Lines 105-106: Are the northerly winds associated with higher surface O₃ to the south of SDZ?

Line 108: Here and a couple of other locations, please fix the degree symbols.

Lines 108-112: Please rewrite these sentences to more simply state that clouds and precipitation are unfavorable for surface O₃ production, leading to the meteorological composite in Figure 1f.

Line 116-117: The temperature of the upper troposphere is much more dynamically-influenced than radiatively-influenced at synoptic timescales (i.e. through tropopause height variations). This sentence should be removed.

Figure S6: Much like Figure S1, this is a lot of text to dedicate to a figure that is buried in the Supplemental Information.

Line 136: Were there any sites that had a larger correlation between MDA8 and OWI than the SDZ site?

Lines 140-152 and Figure 3: I think analysis of OWI before 2006/2007 is not necessary, and introduces the uncertainty of a changing observing system (i.e. the satellite era beginning in 1979, ATOVS in 1998, etc.). These discussions are certainly outside of the scope of this paper, so I recommend not extending the OWI to periods when there

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are no O3 observations to support your analyses. Limit your discussion to 2007 to the present.

Figure 3: All of the other figures that show SDZ data begin in 2007. Why does this include 2006, and why is the line a different color green here? Why does the NCEP/NCAR data end in 2014? Please clarify.

Figures 4 and 5: I thought the OWI was constructed by using some of these same meteorological variables. What is the value in calculating correlations between these variables and OWI?

Lines 170-177: This discussion is essentially a repeat of Lines 110-126. What is new here?

Lines 185-186: What sea ice variable is used here? Is this sea ice extent (generally referred to as ocean areas with at least 15% ice coverage)?

Line 188: Was the MDA8 anomaly also detrended? Is the MDA8 anomaly an anomaly from the mean of all JJA 2007-2017 MDA8 values?

Lines 205-207: As I understand your analysis, this statement is not correct (same with the abstract). The May sea ice extent contributes to about 60% of the interannual variability of OWI, not surface O3 pollution. There is a separate relationship between OWI and MDA8 at SDZ to be considered. The May Arctic sea ice anomaly does not even have the same sign as the MDA8 anomaly about 30% of the time (Line 187).

Technical Corrections:

Line 15-16: Clarify by rewriting as "Increased solar radiation and high temperatures during the EUTP positive phase dramatically enhanced O3 production."

Lines 24-25: See comments on the use of "haze" in my Specific Comments. This sentence should be removed or at least rewritten.

Line 25: Peking University?

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Line 32: Rewrite this sentence "Surface O3 is a secondary pollutant."

Line 33: Change "Sedimentation" to "Surface deposition" and "attenuation" to "dispersion"

Line 50: Change "were" to "was"

Line 64: Do you mean the NCEP/NCAR Reanalysis? Please refer to these products as the NCEP/NCAR Reanalysis, instead of NOAA.

Line 82: "appeared to be delineated by the Yangtze River."

Line 84: Delete "approximately yearly."

Line 86: Replace "almost higher than" with "nearly." Also, what is the threshold of severe surface O3 pollution? Is this a definition set by the Chinese government? Are these O3 data publicly available?

Line 92: Rewrite this sentence, suggestion: "The distribution of correlation coefficients is similar to the MDA8 on Figure S1 panels a, c, e, and g."

Line 93: There is an extra period after China.

Line 94: Change "diurnal" to "daily"

Line 96 and in the references: Should be "Ministry of Environmental Protection"

Line 97: This information on surface O3 pollution definitions should be moved earlier in the paper.

Line 97: What is "nonsurface?" Please rename this to something like "non-polluted surface O3 levels"

Line 114: Delete "sufficient"

Lines 115-116: Rewrite: "In contrast, higher SAT enhances the photochemical reactions and resulted in higher surface O3 concentrations (Figure 1g)."

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Line 121: Is this referring to the entrainment of O₃ into the boundary layer? Please clarify and eliminate the word “downwash”

Line 126: Use NCEP/NCAR reanalysis.

Line 132: Delete “In contrast”

Figure S5: Either add a second panel with the ERA-Interim data for comparison or remove this figure. Consider also my comments about the inclusion of NCEP/NCAR Reanalysis in this paper in general.

Line 141: NCEP/NCAR reanalysis.

Figure S11 Caption: “1979 to 2017”, correct?

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