

ACPD Review

Arctic sea ice, Eurasia teleconnection pattern and summer surface ozone pollution in North China

Summary:

This paper uses a combination of observation and reanalysis data to investigate the possible impact of large-scale meteorological conditions on surface air quality (specifically ozone) in North China. Arctic sea ice concentrations in the spring are identified as a driver of the Eurasian teleconnection pattern which during the negative phase leads to meteorological conditions which are favorable to the photochemical production of ozone over North China.

While I find this may be a novel result, I find this study lacks substance that demonstrates to me that the authors fully understand how they have come to these conclusions. I recommend the manuscript undergoes major revisions to include more details.

Major Comments:

The paper is very short compared to the number of figures included (6 Figures plus 12 in the supplement). It reads to me like a "Letters" type of paper (i.e., Geophysical Research Letters) where one has a new time-sensitive idea or maybe a Nature or Science paper, where the article itself is short but the detailed description of data/methods/etc are in a supplemental at the end. However, this manuscript is short and lacks the detailed description of the data and methods and discussing the results in the greater context of current literature as I would expect from an ACP article. I found the Introduction section haphazard without a clear focus. Take the time to clearly outline and describe each idea. It jumps from ozone in China (Line 27) to European clean air laws (Line 28; could talk about US Clean Air act too) back to China (Line 30), to finally talking about how ozone is formed (Line 32), to how the NAO impacts European ozone (Line 34) to North American ozone and the jet stream position (Line 35-36), back to Asia (Line 36) and briefly mentions the Eurasia teleconnection pattern (line 41). Each one of these ideas could be and should be expanded on.

The Wang and He (2015) EU calculation was adapted from the EU pattern of Wallace and Gutzler (1981). Does the reason they adapted the equation apply for this project? Which calculation was used in the citations in the Introduction (line 42) versus in the Methods section (Line 73)? A full description in the introduction regarding the EU original teleconnection pattern and the characteristics of its positive and negative phases are not described or illustrated and this would be beneficial for the Section 4 and 5. Perhaps a useful reference would be Wang, N. & Zhang, Y. *Clim Dyn* (2015) 44: 1017. <https://doi.org/10.1007/s00382-014-2171-z>

The first portion of the Results section refer to figures in the supplemental and are actually referred to before Figure 1 (Lines 79-100). Notes to the authors from ACP states "The supplement **shall contain only complementary information** but no scientific interpretations or

findings/messages that would go beyond the contents of the manuscript.” Consider including Figure S1 at least in the main body of the text. Figures S6, S7 and S8 are also referenced in the results sections as more than complementary analysis to the main findings and should be considered for the main text.

Of the figures in the main text, some improvements should be made in order for the reader to follow along with the results in Sections 4 and 5. In Figure 1g,h, which temperature is color and which is contoured? It is hard to see the Wind arrows in some of the plots (Figure 1a,b and Figure 4), consider rescaling or decluttering? Figures 4 and 6c are difficult to read. In the figure captions with the contours, it is not stated what are the contour intervals. In some of the supplemental figures the contours are labelled. Either label or define (e.g., is dashed for negative in Figure 4a?). Some of the figures are too small or the shading is too dark (saturated) or the presence of wind arrows makes it difficult to see the dotted significant areas (Figs 4, 5, 6c,d). As for the supplemental, I do not understand the blue lines in Figure S2, and the labelling of the x-axis (does it start June 2006 or June 2007, the tick makes no sense with the figure caption). I also do not know how to read figure S3 (maybe a table would be better?). It looks to me like the histogram has been cut off and values well above 90 should be shown.

Throughout the manuscript, both ERA-Interim and NCEP/NCAR reanalysis data (referred to as NOAA data in the paper, but more commonly referred to as NCEP/NCAR reanalysis) are used, with ERA-Interim being used as the main result and NCEP/NCAR reanalysis shown in the supplemental. In a similar vein, it is said that Shangdianzi station (SDZ) is one of three regional background stations in China (Line 56); Is it possible to use the other two stations to test the OWI methods?

In Section 3, the boxed regions that are used for the OWI calculations are shown in Figure 1. Make a better connection between these boxed regions in the text to the respective figure and also state that SDZ is located within these boxes. This is why you are doing the correlation coefficient between the SDZ ozone concentrations to the meteorological fields within these boxes, right? The boxes look to fit the maximum correlation for the shaded composite fields and therefore are different sizes. Can the authors discuss more as to the methods which lead to these boxed regions?

At the start of Section 4, the authors state “After 1979, the quality of the reanalysis data was improved to support studies of climate variability and change.” This isn’t quite true, the quality of reanalysis data improved for the period in the datasets after the assimilation of satellite data, which was made possible starting in 1979. The NCEP/NCAR reanalysis covers the period prior to the satellite era, therefore studies of climate variability and change must take into consideration the introduction of satellite data as well as subsequent changes in the observation system (introduction of new satellites and when satellites are no longer in operation). This needs to be properly addressed in the paper.

In Section 5, only the month of May sea ice is discussed. Did the authors investigated other lag periods? Is there literature that describes the interaction between sea ice concentrations and large-scale atmospheric circulation that can be referenced in this manuscript?

Minor Comments:

Line 26: smog is visible to humans and ozone is a key ingredient to smog.

Line 27-28: Can you provide any references which have looked at ozone pollution in China linked to climate variability

Line 28: 'benefitted' with two t's is the British spelling.

Line 28: Provide a reference and further details on the European 'rigorous air protection act' and what you mean by 'maintained good air quality' and in the same sentence 'ozone levels are increasing'.

Line 30: is the ambient air quality standard set by China or the World Health Organization? Please define and reference.

Line 31: Can you define North China, or indicate it on a map? It is confusing as on Line 93 it is written 'in the north of China, especially in North China'.

Line 32: What do you mean by discharge?

Line 33: Describe the suitable weather conditions here, versus later in Lines 38-39 describing in detail the unfavorable weather conditions for ozone formation.

Line 41: Is it more common in the literature to refer to this teleconnection pattern simply as EU? In some of the figures EU is used. Commit to either using EU throughout or EUTP throughout the manuscript and figures.

Line 45: Can you list any more recent studies?

Line 45: Why talk about eastern China when the paper is interested in North China. Are there references that look at future ozone in North China?

Line 47: Which "previous studies" are you referring to?

Line 54-58: Is this ozone data publicly available?

Line 58: What is special about the MDA8 calculation of the Technical Regulation on Ambient Air Quality Index that it required referencing it?

Line 62: What is the native resolution of ERA-Interim? Did you download the data to this resolution or regrid it? Did you download it originally at the 6-hour resolution and then created daily and monthly datasets?

Line 69-70: As stated above, this reanalysis is more commonly referred to as NCEP/NCAR reanalysis.

Line 73: Correct Wang et al to Wang and He.

Line 80: Is it possible to label on a map these three regions?

Line 82: What is meant by "which appeared to be bordered by the Yangtze River".

Line 82: 'rather high' is subjective. Change to be more qualitative.

Line 86: Who's threshold?

Line 89: State that SDZ is labelled on Figure S1a (though really should be in S1b logically since right hand panel compares to SDZ).

Line 92: There are a few instances where the degree symbol is not superscript in the manuscript.

Line 94: How can there be a diurnal difference in a maximum daily average? This sentence makes no sense to me.

Line 97: Can you switch the order, introducing NOP before MOP.

Line 99: The mean number of MOP days is not explicitly shown in Figure S3.

Line 100: significant at what level? If this is from a figure in the supplement, consider moving that figure out of the supplement.

Line 103: use SAT instead of spelling it out and introduce why cooler temperature in the high troposphere (T_{200}) is favorable for surface ozone pollution (mentioned later in Line 117).

Line 121: what is downwash

Line 126: Why is Figure S5 included in the supplement

Line 130: Any time MDA8 is used, is the reader expecting it to be the SDZ MDA8 unless otherwise stated? Make that clear earlier on in the text.

Line 137: Reference Figure S7b-d. 'in North China (Figure S7b-d)'

Line 143-144: remind the reader that SDZ data began in 2006. This sentence is not clear to me.

Line 145: What do you mean by a staged minimum?

Line 148: Significant to what test and level?

Line 150: provide a reference for the sentence ending '...due to the steady economic development in China'