

Referee report for revised Zou et al. ACP paper

General comments

Thank you very much to the authors for addressing my comments. The issues I raised regarding statistical robustness have now largely been resolved, but I have a few minor comments remaining, which are listed below. Overall, I think that this paper is very interesting and would recommend it for publication, given the new mechanistic insight it provides into the important issue of haze pollution extremes in China.

Specific comments

Section 2.3: Thank-you for addressing my concerns about the autocorrelation. Your results presented in Tables R1 and R2, and Figures R5 and R6, convincingly show that the autocorrelation from month to month is minimal. Could you perhaps add in a sentence saying that the autocorrelation is minimal, to avoid confusion for other readers (perhaps after saying you have 90 samples on line 21 of page 6)?

Page 9, lines 35-40: With these new 95% confidence ranges from Tables S3 and S4, I am now convinced that the increase in frequency and intensity of ECP_PPI positive extremes is robust for the 30-year simulations (your Figure R3 was also useful for showing this, so thank-you for that). However, I think as well as stating that the increase in ECP_PPI extremes is statistically significant (which you have done on Page 10, lines 2-4), it should be more explicitly stated that the increase in MCA_Z500 extremes is not. This raises a bit of an issue, since the increase in ECP_PPI extremes is being attributed to the increase in MCA_Z500 extremes; if the latter isn't robust, then it's unclear whether the former is robust. However, given your Figures 4, 5, 6, and S7 in Section 3.3, there do appear to be robust effects of sea-ice loss on atmospheric circulation in the ECP region in the SENSr2 extreme members. This gives me more confidence that there is indeed a dynamical connection between SENSr2 sea-ice loss and increased ECP_PPI extremes. Perhaps this could be highlighted here?

Page 10, lines 1-2: I don't think there's enough evidence to make the following statement: 'The increase in the teleconnection pattern index MCA_Z500 is less significant than that in the regional air stagnation index ECP_PPI, suggesting a potential nonlinear relationship between large-scale circulation and regional stagnation'. In particular, as highlighted by the large 95% confidence range for MCA_Z500, there is large uncertainty in the response of MCA_Z500 extremes due to internal variability. As such, the apparent non-linear relationship between MCA_Z500 and ECP_PPI could equally be due to internal variability / chance. I therefore recommend this sentence is removed.

Section 3.3: Just a general comment - I think structuring this section around two questions has helped provide clarity, and that the addition of Figure 5c/f, Figure 6c/f/i/l, and Figure S7 has made it much more convincing that SENSr2 sea-ice loss does indeed have an effect on circulation extremes (and therefore pollution extremes) over the ECP region. This is relevant to my comment two paragraphs above.

Page 14, line 37: not sure what 'non-linear' is referring to here

Page 17, line 1: I'm confused about the phrase 'forced air stagnation response to internal variability'. How can there be a forced response to internal variability? The two are separate, although they can

resemble each other. I presume this is a typo and the response is to some forcing Callahan et al. (2019) prescribe.

Technical corrections

Page 2, line 34: I suggest just using 'correlation' rather than 'good correlation', as no correlation can be said to be 'good' or 'bad'. Also, I suggest splitting this sentence up as follows, as it is currently very long:

'However, a clear understanding of key dynamic processes linking complex meteorological changes to critical climate factors is still missing. This is necessary to establish a robust causal relationship between remote climate drivers and localized atmospheric responses, because a correlation does not necessarily imply causation.'

Page 14, line 20: it would help to clarify that this is for positive ECP_PPI extremes (i.e. not MCA_Z500 extremes)

Page 15, line 42: 'stratosphere' should be 'stratospheric'

Page 16, line 42: I would suggest reordering to 'Callahan et al. (2019) estimated a signal-to-noise ratio of less than one for the forced air stagnation response...'