

Weather Clim. Dynam. Discuss., referee comment RC2
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Comment on wcd-2021-5

Anonymous Referee #2

Referee comment on "Linking air stagnation in Europe with the synoptic- to large-scale atmospheric circulation" by Jacob W. Maddison et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-5-RC2>, 2021

GENERAL COMMENTS:

Maddison et al. examine the relationship between stagnation, defined by the Air Stagnation Index, and various dynamical features related to synoptic-scale circulation over Europe. By building a model using a MLR approach and stepwise procedure, they are able to explain a majority of the variance of stagnation frequency across different regions in Europe. They found regional heterogeneity and seasonality in the features (e.g., STJ, RWB) linked to stagnation.

I thought that the paper was well-written, and I appreciate that they tested other stagnation indices (Lines 411ff), as this has been a question of mine for quite some time. I do, however, think that the focus on stagnation is problematic and doesn't really give information about pollution events. As discussed in my comments below, I believe this paper needs a direct assessment of the link between air pollutants and the large-scale dynamical features. If these comments are heeded, I believe the paper has the potential to be relevant for forecasting pollution events with short lead times.

SPECIFIC COMMENTS:

- The stagnation index is problematic as it doesn't correlate well with actual pollution events [Huang et al., 2018; Kerr & Waugh, 2018; Wang et al., 2019; Garrido-Perez et al., 2019] and it's boolean, based on fairly arbitrary thresholds. The authors mention some of the known issues with the stagnation index in the Conclusions (Lines 461-462), but this comes across as a brief afterthought. In light of these issues, some of the findings contained in the study are not that groundbreaking or useful, especially for actually understanding the dynamical drivers of *pollution* events.

For example when you describe stagnation frequency and where stagnant conditions are most likely to occur (e.g., Lines 30ff, Figure 1a, Lines 211-212), I'm not surprised that stagnation frequency is lower in mid-latitude Europe (near the latitude where the jet is

located and where there are mid-latitude storms) versus in the southern part of your domain, where conditions might be more dry and where upper-level winds are not as great as in the mid-latitudes. Moreover, you also make statements like such as "air stagnation in Europe is shown to be strongly influenced by the large-scale circulation" (Line 432). Since stagnation, in essence, is just a measure of the local winds and precipitation, it doesn't seem at all surprising that local weather conditions are influenced by the large-scale circulation.

I believe that a more relevant study and one that would be impactful and important for the community would be to directly look at the relationship between pollutants and the dynamical features rather than the relationship between stagnation and the dynamical features, which may or may not have any direct relevant for surface-level pollution events. I think you could more or less repeat your methods and approach using the dynamical indices but subbing in perhaps PM and/or O3 from CAMS/ERA5. For example, you mention "stagnation is around 30% more likely to occur when there is a block or ridge present" (Lines 243-244). I would like to see a similar statement but for O3 or PM (e.g., "Extreme PM is X% more likely to occur when there is a block or ridge present"). As previously mentioned, I think this would be of wide interest to the air quality/atmospheric chemistry community.

I noticed the other reviewer pointed out a similar comment, and hopefully by addressing this comment you could kill two birds with one stone....or, more accurately, you could shut up two reviewers with one additional analysis :)

References: Wang et al. <https://journals.ametsoc.org/view/journals/bams/99/1/bams-d-16-0301.1.xml>

- Equations 1-3. You mention that variables such as $BI_{\{region\}}$ and $RWBI_{\{region\}}$ are monthly time series of blocked days. What do the distributions of these (and other) variables look like? Do they have many values = 0 and a few other values? I'm wondering if it's appropriate to use linear regression to derive the coefficients in the case that the distributions are highly skewed. Plotting some examples would easily lend insight to this question.

TECHNICAL CORRECTIONS:

- Figure 2a,b,e - It might be helpful to include latitude labels on these maps so readers can match them with the latitudes shown in c, d, f, and g. Additionally, can you clarify the quantities being shown in the shading in these subplots? For example, you mention in the figure caption that Figure 2b shows the "ridge index (shading)" but in the text you mention "an ~20% increase in the climatological frequency of subtropical ridging over the region" (Lines 178-179). So shouldn't the quantities in shading be referred to as a departure from the index? Or am I misinterpreting?

- Figure 2a,b,e - On a related note to my above comment, the changes in climatological frequency are generally at most +/- 20% but often quite a bit less than this value. Would, for example, a 5 or 10% increase in frequency even be statistically significant? Can the authors denote statistical significance here (and elsewhere, when applicable)?

- Figure 3c - I understand that the RI departure plot is based on an index that considers three separate zonal sectors. However, it looks a little strange to me to have a negative box over part of Southern Europe and then a sharp cutoff to weak positive values. I realize that this affect is an artifact of the index, but what does it imply about its suitability?

- Figure 6 - I think readers might benefit from having the region acronyms (e.g., SE, SW, CEU, etc.) labeled on the maps in case they forgot where these regions were.