Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2021-337-RC1, 2021

Comment on acp-2021-337

Anonymous Referee #1

Referee comment on "A weather regime characterisation of winter biomass aerosol transport from southern Africa" by Marco Gaetani et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-337-RC1, 2021

General Comments

The authors present a classification of the circulation over southern Africa and the South Atlantic into six weather regimes (WRs). They link these WRs with the transport of biomass burning aerosol (BBA) via the corresponding wind field and with the aerosol optical depth (AOD). Furthermore, they investigate whether there are significant links between the observed AOD at surface stations and the WRs.

This study appears to be the first linking WRs to aerosol transport in this region. It provides valuable insight in the circulation over the South Atlantic and southern Africa and shows significant relations between AOD at surface stations and some of the WRs. When WR classifications have been performed in the Northern Hemisphere, e.g. the Euro-Atlantic sector, these regimes are quite persistent and describe variability on timescales of 5-15 days, i.e. for a large part beyond synoptic timescales, while most WRs studied here describe precisely that synoptic variability in the region. Therefore, the regimes in this region are more a useful descriptive tool for the circulation than persistent features in phase space. Nevertheless, these WRs prove to be well suited for studying BBA transport in this region and could have a lot of potential for studying other circulation processes affecting southern Africa. The authors get this across quite well, although I think the potential of the WR can be emphasized more.

Specific Comments

- Can you comment on the robustness of the WRs? The time series used is relatively short, being only 15 years of ASO data, for the classification of WRs. It would be good to know the WRs are robust with respect to the reanalysis products used by computing the WRs for e.g. ERA-5 which has a longer available time period.

- Are the WRs you obtain dependent on the selected domain? Most of the variability, especially in WR1, 3, 4 and 5, is south of 20S. Do the WRs change if you e.g. only take the domain up to 10S, or extend the domain beyond 40S? A similar question on the number of PCs, have you looked into any dependence of the WRs on this?
- You comment on the limited coverage of the CAMS reanalysis not being sufficient for defining a climatological seasonal cycle (p4, 100-102). As you only consider three months of data, would it make sense to simply use a fixed climatology, i.e. average over all months, with respect to which to compute the anomalies? And would this affect the WRs?
- In your study you use 6 WRs, and in the supplement you also show results for 7 WRs. Why did you choose to work with 6 WRs in the main manuscript instead of 7? If it's because the results simply are better, could you state that more clearly in the main manuscript. In the end that's the aim of using the WRs. From my point of view, selecting 6 only because it's the lowest significant number of WRs would already be a valid argument. Also, I'm quite surprised 2 WR yields such a poor classifiability index (supplement), can you comment on this?
- Could you expand on the computation of BBA emission and transport. For the transport, are you discussing anomalies with respect to the low frequency component (season-wise) or absolute values? Similarly, for AOD, are you plotting anomalies? It would be good to clarify throughout.
- The WRs come in two types, with 2 and 6 describing the oscillation of the South Atlantic pressure field and the other four representing propagating disturbances along the midlatitude mean flow. The latter four are not very persistent, and represent a travelling wavetrain (1-5-3-4). How do these two sets of WRs relate and could you also study the BBA transport using a travelling wave perspective?
- You look into the sub-seasonal variability of the WR regime frequencies (sec 3.1, p7). I am unsure whether you have sufficient data to state that these differences are robust, as noise will affect these results. Specifically, if you have insufficient data to define a seasonal cycle for it is affected by interannual variability, do you have enough to do something similar for the WR frequencies? I.e. on average you have 15x30/6 = 75 days in each WR during one month, to which changes of 5-10 days in occurrence can have quite an impact.
- Can you expand the discussion of the link between your WR and the SAM, which you briefly mention (top of p8)? Why do you think WR6 relates better to the SAM than WR2? As WR2 overall occurs more often, it would be interesting to know any discrepancies. Can you include the results that are not shown in the supplement?
- Would there be any effect of the duration of the WRs onto the CAMS AOD, and that of the surface stations? I can imagine that if a regime lasts longer the effect on AOD is stronger as well, more so for WR2 and 6. Also, is it possible that there is an effect of past WRs (i.e. delayed) onto the measured AOD, possibly linking to the discrepancies between surface station AOD values and those of CAMS?
- Three (maybe four) stations are located close together (GO, HB, HE, and BO(?)). Differences in AOD distributions (Fig 6) for each of the WR thus could be primarily caused by local effects, e.g. for WR3, can you comment on this? Similarly for the WR frequency anomalies in Fig 7. Also, would it make sense to pool these stations together to get more robust statistical results (I am not familiar with the data itself, so do not know whether this would be sensible)?
- More generally, station data is limited. Can you comment on how many days are in each of the WR for the different stations? This would help the reader get an idea of the robustness of the results in Fig 6 and 7.
- How robust are your results on interannual variability, having only ~90 days with associated regimes for each year? In my experience interannual variability in regime frequencies of reanalysis products can be very large, and does not necessarily allow for clearly showing a signal through the noise. Could you use a longer reanalysis to look into links with e.g. ENSO to make these links more robust? I am not surprised WR 1,3,4,5 do not show any significant links with SST, as these are short-lived regimes.

Have you thought about the persistence effect of the regimes and whether that could be affected (I appreciate there's too little data for this)?

- Most plots are not very clear and take a long time looking at to understand what they show. Could you add labels to all axes and colorbars where they are not there, increase the fontsize of ticks, labels and captions, and remove any redundant information. The z700 patterns of the regimes are not clear at the moment (Fig 4), maybe increasing the line thickness would help, or otherwise I think it would be good to add them in a separate figure, as they are important to get across well. For Fig 6 and 7 it would be good to clearly see which results are significant and which are not, e.g. change the colour in Fig 6 and lines around the relevant cells in Fig 7. Moreover, there is some discrepancy between the p-values given in Fig 6 and Tab 3. I think it is sufficient to show these values only in the table if you indicate the significant ones in another way.

Technical Corrections

- Could you rearrange the WRs such that their order links better to the regimes themselves? For example, change WR2, 6 to WR1 and 2, and WR1, 5, 3, 4 to WR3, 4, 5, 6 (in order of transitions), respectively. It would also be good to line up the 7 WRs in the supplement with the six in the manuscript.
- Please check the figure numbers as they are not everywhere correct, e.g. p9, 268, Fig. 6a/6b should be Fig 7a/7b and p11, 318, Fig. 10 should be Fig. 11. I might have missed some others.
- Throughout the manuscript: South Atlantic -> the South Atlantic

p2, 27	by -> of
p2, 35-39	Partly repeating what is mentioned earlier in the abstract, I would recommend rewriting or weaving it into the earlier part of the abstract
p3, 44	Remove "on"
p3, 47	originated -> originating
p3, 48	Insert "and is" between "(Fig. 1a)" and "a prominent"

p3, 52	definition -> term
p3, 59	shed light -> shed light on
p3, 63	the Antarctica -> Antarctica
p4, 76-78	This sentence is nearly the same as the one above
p5, 106	You already mention AOD here as an abbreviation but only define it in line 114, also it's already mentioned in the caption of Fig 1, so might be good to clarify it there as well
p5, 124	with -> of
p5, 131	as linear -> to be linear
p6, 139-140	before to perform the linear regression -> before the linear regression is performed
p6, 148	to mask -> mask
p7, 182	southward the midlatitude westerly flow -> the midlatitude westerly flow southward

p7, 182-187	Do you have any references supporting this, or is it solely based on Fig 2a?
p7, 189	filed -> field
p7, 192	the WR2 -> WR2
p7, 203	Can you change the order of the preferred transitions in the brackets, so they are in order of transitioning?
p8, 215	coherently -> coherent
p8, 216	results statistically -> results are statistically
p9, 249	Can you repeat the null hypothesis here?
p12, 357	WR then clustering -> WR clustering