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Interactive comment on "Future Meridional Wind Trends Through the Lens of Subseasonal Teleconnections" *by* Dor Sandler and Nili Harnik

Anonymous Referee #1

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GENERAL COMMENTS:

The authors present a nice study about the representation and future trends of CTP in CMIP5 models and observations, which is a very important topic to understand circulation patterns in a future climate under global warming. In general the plots are clear and well chosen to support their results. However, it was not always easy to follow their conclusions and some further explanation or proof for some of their statements seems necessary. I would therefore recommend major revision to tackle this before publication.

- the authors put a lot of emphasis in the conclusion on the necessity to better understand the connection between mean flow and associated teleconnection patterns. But in their results the authors mainly discuss wave patterns. Maybe it could be helpful to

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put some more focus into the mean flow for the discussion of their results. E.g. the authors successfully did overlay the MMM V trend and the regional EOFs, but Fig. 11 only shows one contour of the mean flow (20m/s). Therefore the reader is not able to interpret the anomalies, because there is no relation to the spatial variance of the mean flow. Further the authors could focus a little bit more on the role of the mean flow in their discussion. This could maybe also include some large scale global flow anomalies associated with NAO, PNA, ENSO, etc. The anomalous patterns in the jet strength in their Fig. 11 seem to show similarity to such known global pattern indices. Further, there are already some studies looking at the connection between large scale flow patterns and the resulting wave response (or how they are associated, not suggesting a cause-effect relationship). I think this was also done in the reference they cite (Souders et al, 2014), so maybe could be worth including in their discussion (how do their findings of wave packet anomalies relate to the finding here)?

- Authors are analysing wave pattern with non-zonal waveguide. Wouldn't it then not make more sense to use the wind perpendicular to the climatology (as representation of the waveguide)? Wolf and Wirth (2017, Diagnosing the horizontal Propagation of Rossby Wave Packets along the Midlatitude Waveguide) have shown that this does have an important impact on identifying the correct path of propagating waves. Here the authors do focus on stationary or quasi-stationary waves, but as soon as the waveguide has a meridional component, the more physical quantity to measure the wave is the wind perpendicular to its waveguide. What is the authors point on this, do they assume they would see an even clearer signal in their results or do they expect that this does not have a relevant impact on their results because of the large scale of the wave patterns they are investigating?

SPECIFIC COMMENTS

- Introduction ——————————————-

- p.1, line 21: Why do the authors only highlight "shifts" in the climatological mean flow?

The strength of the mean flow is important as well, isn't it?

- p.2, line 37-40: Could the authors please give some more explanation on this for the reader who is not familar with the details of Branstator 2002? As this point is crucial for the paper, it seems that some more explanation on this is well invested. How crucial is the exact way of calculating the EOFs and how should they be calculated? I assume they are calculated for the whole hemisphere, either northern or southern hemisphere. The EOFs are further calculated for DJF using subseasonal anomalies - does this mean the deviation of the meridional wind from the mean of the specific season or the deviation from all seasons taken together? As there is still a shift of the jet during the season, would using the meridional wind as deviation from a running mean or low pass filtered signal change the resulting EOF signals or are they a very robust signal? Further, the first two EOFs do show a very similar but shifted wavenumber 5 pattern with more or less opposite sign? Otherwise this combination given in Equ. (1) would make no sense as it tries to capture the phase of a wave that can have nonzero phase propagation, right? How many variance is explained by these EOFs and are they well separated from the next order EOFs? -> I think this was explained later in more detail, so maybe refering to this here or including a short descriptions to make the reader more familiar with what this method does capture/what the associated pattern represent.

- p.2, lines 48-50 Maybe this description of RWPs must be reformulated? RWPs are not necessarily restricted to 1 or 2 wavelengths. Further they are not necessarily consisting of pairs of troughs and ridges; if one has identified a RWP consisting of a trough and a ridge which further leads to downstream development than there will appear either a trough or a ridge, but not necessarily a pair of both (or by decay of individual anomalies on upstream side).

- p.2, line 51: How do the authors conclude what the value of the phase speed (near-zero) of the RWPs is that contribute to the CTP?

- p.3, lines 63-64: What is the statmenet here? The CTP is already a current wintertime feature, so what do the authors mean by "CTP will be easily excitable by future greenhouse gas forcing"? The statement is that the CTP pattern will become stronger? Also following lines, the authors mention the "CTP-like trend". This means a strengthening of the current CTP pattern or what does the term "CTP-like" mean here? A CTP-like trend could also be a weakening of this pattern or not?

- paragraph lines 67-73: this paragraph describes to what the authors previously refered as linear RW theory (p.2, line 35), correct? The increase of jet strength in winter does lead to a shift in zonal wavenumber toward smaller values. What do the authors mean here when they say that this is not necessarily the case in boreal summer? The strength of the jet in boreal summer does not have an impact on the stationary wavenumbers? r is the focus here on the excitation/amplification of wave responses than rather a shift of wavenumbers (what exactly are the authors refering to by using "the response")?

- p.3, lines 77-78: why is it surprising that seasonal and subseasonal variance of V behave differently? If there is a shift in the power spectra of meridional wind towards lower wavenumbers this could probably also mean a shift towards lower phase speeds (?). If there is an increase in more quasi-stationary wave patterns the contribution of the faster propagating signal could decrease (what one would probably expect if one sees more stationary wave patterns or do I confuse sth here?).

- Data and Methods ——————————————

- p.4, lines 99-100: Doesn't this subtraction of the seasonal mean automatically cause some anomalies by the shift of the jet position. What would happen if one would remove something like a running mean of 90 days, would this have a huge impact on the result? -> I think I mentioned this further above already in a bit more detail.

- Fig. 1, colorbar?

- p.5, paragraph lines 139-148: Again, the climatological background field is evolving over the period of the winter. What is the reasoning to subtract the full DJF mean instead of the climatology for the individual months. Wouldn't that be more physical with the reasoning given in this paragraph (to get rid of the evolving planetary scale for the anomalies)? Did the author estimate the impact of using monthly climatologies instead of seasonal climatologies?

- p.5, line 148: why are the data with low projection scores excluded and how were these 70% chosen. Do results depend on the percentage?

- p.5, lines 149-50: Could the authors explain why the focus should be on models with a specific phasing, which do represent a higher score? What would happen if specific models would produce a persistent pattern of an overall slowly evolving pattern. Let's say often a very strong Ridge over the Rockies with downstream amplified wave pattern, as given by EOF1 in Fig.2. If this pattern would break down towards the mid/end of the season with a more zonal flow, this more zonal flow would appear as opposite sign of the EOF1 (ridge less strong than for climatology, downstream meridional tilt of the jet less strong), wouldn't it? This process would represent the recurrent build up and destruction of a specific wave pattern. If such a case would happen it would not be identified in the average score (it would average to zero), right? Could the author comment on this, why they think something like this is not expected to occur or if it would occur, why it would be fine if the score would still identify it as close to origin (not specific relevant wave pattern case)? Or differently formulated, why is the strong focus exclusively on a prefered phase?

- p.6, line 157 (CTP events): So, usually if the authors talk about CTP they have in mind the patterns derived from the EOF of monthly fields. How does such a CTP relate to the CTP events mentioned here in this paragraph. Do the CTP events using daily data (with a high index) mainly represent two RWPs which are located in a way that they have at the same time the correct phasing relative to the "monthly" CTP pattern? Or are those events only restricted to specific regions (the 144 lon mentioned earlier) not for the whole hemisphere. An eastward propagating (phase propagation) RWP will then be captured several times moving through this region (after moving one wavelength). This means, if I understand it correctly, two RWPs at lag 0 in the composite, could actually be the same RWP at lag 0 and then something like 5 days later after shifting a whole wavelength further eastward if it consists of more than one wavelength (because in the time in between it will not fullfill the phase criteria given in line 161). Is that correct? What would this mean for interpreting these lag-composites?

- p.6, line 161: This phi_d and phi_m refer to the phase as used for the phase in Equ. (1) which was given by gamma, right? So to be clearer and reduce confusion, it could help to be consistent and name them the same way (gamma_d and gamma_m).

- p.6, line 163: What is the future climatology? is this a similar 30 year average (which period?) as for the historical climatology (1900-1930)?

- Results ———————————————————

p.6, lines 175-178: How do the percentages compare to the findings of B02, weren't they significantly higher and if so, how can the differences be explained. For the separation of the EOFs it does not really matter if the first two are clearly separated, if they are both used or combined in the index of Equ. (1). However, doesn't the close connection to the third EOF mean that it is difficult to analyse them seperately? How does this change for a longer dataset (NCEP-I), are they well seperated from each other or to the third EOF? What is the consequence for this (as the authors mention specifically this test)?

- p.6, line 185 (Fig.S1 - Fig.3): the multimodel mean has higher absolute values than the individual models. It seems like the Fig. S1 only show the first two contours, ommitting the following ones.

- p.7, line 195: I have some difficulty to interpret those number of 0.78 and 0.5 - what is the score range? 1 is as mentioned a perfect copy. What is the lowest score, 0 or -1? And the lowest score is then the exact opposite of the signal? If that is the case and looking at Fig. S1, this suggests -1 is the lowest skill score, otherwise 0.5 would somehow be like a wavenumber 5 signal with random phasing compared to observations (which obviously is not the case according to Fig. 3). Probably it would be helpful if the authors could give the reader some idea what this score does tell (apart from the upper boundary).

- p.7, line 1999-200: The quasi-stationary wavenumber 5 pattern is per se not a response of GHG forcing as it is already present in the historical data. Is this comment about the trend and differences of this pattern to the historical pattern? Maybe the authors could clarify this.

- p.7, line 204 (Fig. 4): It could be helpful for the reader if the authors would include here the underlying past climatology. Does the MMM response really shows a convincing wavenumber 5 signal? There seems to be a low wavenumber signal over eastern asia/western pacific and a higher wavenumber response over North America/Atlantic. A Fourier analysis would probably not show a single peak at wavenumber 5. The authors further say that although this wavenumber 5 response shows up in the MMM (Fig. 4a), the individual models trends represent only low scores if projected onto the EOFs. This seems to suggest that the MMM response has a high score, but is this really the case? Looking at the response wave signal path from North America southward into the Atantic, this does not seem to have a counterpart in the EOFs from Fig. 2. Does the MMM shows a much higher score? Further, I like that the authors included the supplementary Fig. S1 showing all models. But I would have been even more interested in the same kind of Figure, but based on future data as there is a very good agreement between the historical EOFs between models and observations, but there seems to be some stronger discrepancies in the wave response for future projections. So it is not really obvious that the future projections go for a CTP pattern or rather increase in the evolution of separated wave responses as can be seen to some extent already in Fig. S1 for some models in the historical data (EOF2 showing a seperate NA wave pattern?).

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- p.7, line 207: what exactly is the point that should be tested further? Is it the low projection score?

- p.7, lines 213-214: Why does the division into different regions allow to inspect CTP-like responses? Could it not be that doing the calculations for different non-global regions highlights regional wave patterns that are not circumglobal? Fig. 4b shows that the variance of most models cannot be explained captured by the MMM global EOFs, but mainly by EOF 4 (is that correct interpretation?). Wouldn't it be interesting to show this EOF 4? Is it a non global wave pattern of a higher wavenumber? If so, wouldn't that mean that for the future trend the CTP pattern is less relevant and the projections tend to prefer another global wave response? Why isn't that the interpretation of Fig. 4aa and b.

- p.7, line 217 (Fig. S2): What are the values for shading and contours? Shouldn't be the shading in Fig. S2 be identical to the contours in Fig. 3? But blue values increase in Fig. S2 while at the same time red values decrease in spatial extent. Does this mean that the colour contouring is asymmetric between negative nad positive values in Fig. S2?

- p.7, lines 217-219: This is not shown anywhere or is it? The reader hasn't seen the regional EOFs for the obersvational data. So maybe indicating this by sth like "(not shown)".

- p.7/8, lines 220-224: Any conclusion or interpretation the authors could provide the readers? Comparing the regional to the global EOFs in Fig. S2 I'm surprised this makes so much of a difference. But the wave pattern for the EOF1 in NA (Fig. S2a) seems to show a wave pattern coming from the south and pointing to the south, suggesting that this wave pattern does not feature a CTP pattern. Could the authors provide some explanation and interpretation to their results. It seems here that the authors now just use the regional EOFs because they better represent the trend of the models. But why is that, is there some possible explanations behind these signals or is this just a useful thing to do to capture the trend, because going to smaller scales usually should allow one at some scale to capture all globaltrends if added together.

- p.8, lines 234-235: I really like the visualization of the data in this phase space diagram of Fig. 5. However, what is the measure of uniformly spread? Starting to look at Fig. 5a this seems a bit strange, as the negative phase of EOF2 does not seem to occur very often for weak EOF1 values, in one of the eight parts there are only 4 blue dots, whereas in others there are more then 20. So it is not really uniformly. Could the authors quantify this to some extent with sth like X % in a 90 degree angle range? That shouldn't be too difficult and this would allow them to make their point more convincingly of how much more the RCP8.5 data is concentrated into a specific region of the phase space diagram. Probably not necessary, as now I realize this is what Fig. 6 does. Further to this, Fig. 5 does only show one model, right? This should be mentioned by the authors (it is described as all models in the text), so I totally missed this at the beginning.

- p.8, line 238: Is it obvious that models shows a preference for either NA or AS regions. Is it necessarily "either" for being able to explain why they not project strongly on the global pattern? Isn't it possible that one model prefer boths, some periods with increased wave patterns over the AS which ressemble the given EOFs there, and some periods with increased wave patterns in the NA region with some arbitrary signal everywhere else. The authors seem to exclude this possibility. What is the reasoning for their conclusion?

- p.8, line 244 (Fig. 6): How is the circle calculated? Not clear to me where the assymetry of the circle (right vs left and top vs bottom of the phase space) comes from. This is the standard deviation in physcial space of the individual model monthly data also projected onto the EOFs and then shown in this phase space? If it is the standard deviation of the values in this phase space (as it does sound like in the text), shouldn't it have an equal distant from the origin - or does the circle only seems asymmetrical because of the dashed lines which do not represent an adequate measure of distance?

Maybe use the same order (NA on top of AS) as in Fig. 5. I made this assumption switching from Fig. 5 to Fig. 6, which did confuse me for a moment.

- p.8, lines 246-247: I'm still confused with everything related to this sigma-measure. Aren't most of the rectangles of RCP8.5 inside the circle? The filled ones represent the same models (the group) in Fig. 6a and 6b, right? But this would mean that most models don't get above 1 sigma for any region or do I misunderstand the plots? Further, isn't the standard deviation calculated from the RCP8.5, so wouldn't we expect most models to be inside of the circle by definition (for simplicity assuming a normal distribution)?

- p.8, lines 247-249: Maybe the authors could spend some more time better explain this. I was expecting them to refer to the red rectangles (the mean projection of the RCP8.5 models), but I cannot identify those numbers? First of all, I assume the authors refer to rectangles outside of the circle, but they mention only the sign of the EOF (which would include all of the red rectangles). Further I can identify the eight rectangles outside of the circle in the NA region but not for the AS region (is that because they are on top of each other?).

- p.8, line 251: Again, do the authors really mean non-zero or outside of the circle, because their statement is only true for those outside of the circle, isn't it? Following line as well, this is about the rectangles outside of the circle. The 7 cases are then the 6 inside the quadrant and the one with nearly 0 EOF1 very close to it, right? But how can these numbers be associated with the previous statement of 8 cases with negative (positive) EOF1 (EOF2) for the AS, (representing the second quadrant)?

- p.10, line 288: How is it filtered? Difficult to follow in detail, if it is just mentioned that the data is filtered without specifying how.

- p.10, lines 290-293: Can the authors say anything about the persistence of the CTP events?

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- p.10, lines 296-297: Is this really the case? The regions are very different with a shift of partly 60 degrees and huge overlaps. The NA region here captures main parts of both, the Pacific and Atlantic region in Souders et al. (2014). But I assume it depends for what this comparison is used and there is no need for a good agreement between the chosen regions. The authors directly refer to Souder's result of RWP formation, in which case most of the RWP formed in the Pacific region will result in amplified RWPs over the NA region given here, whereas the formation of RWPs in Souder's Atlantic region will contribute to the RWP in the AS region given here - although a direct comparison is not simple because Pacific RWPs could also go all the way towards the AS region whereas RWPs formed in the Atlantic can also decay in the Atlantic.

- p.10, lines 297-298: Difficult to compare as Souder's climatology also captures faster propagating RWPs with wavenumbers around 10. So I agree with the authors that Souder's climatology should give a high upper boundary. The waves the authors are interested in (low wavenumbers around 5) should be more comparable with the quasi-stationary waves investigated in Wolf et al. (2018, Quasi-stationary waves and their impact on European weather and extreme events). Their Fig. 9 should probably give a lower boundary for the wave occurrences with about 0.6 per season in ERA Interim for very persistent wave signals (minimum 10 day lifetime). So the findings here seem to be well in this range of different climatologies.

- p.10, line 306: Maybe not using the term "wave activity", which would be strictly speaking a measure of wave strength/amplitude, but here the authors rather mean frequency.

- p.10, lines 305-309: The authors are measuring also the persistence of the the signal (as this is part of the definition of a CTP event). Would be nice if they could make a statement about the persistence of those events. This should also be sth of interest in terms of climate extreme, because such persistent signal do often lead to extreme events. So knowing about the trend in the persistence could give some hints about the evolution of possible extremes.

- p.10, lines 312-313: Why is the RWP at its peak of the life cycle at lag 0? Strictly speaking, one cannot tell this with the applied measure which can only tell its projection peak onto the CTP. But even this probably does not occur at lag 0, or does it? Lag zero is defined as the first of at least 3 consecutive days when the projection value excees the given threshold. But then, would it not be more likely that the peak occurs around day 1 or slightly afterwards?

- p.10, lines 315-316: where does this conclusion come from? What characteristics specifically and how where they affirmed to be realistic? Where does the conlusion for near stationarity comes from? Not clear where the conclusion and description of RWP comes from in this paragraph. Could the author give some more details and explanations for this paragraph?

- p.11, lines 320-325: How do the author conclude that the wave source is located over Southeast Asia (IPSL case) when there is no statistical significant signal over southeast asia and no clear wave propagation. Further lag 2 and 5 seem to indicate eastward moving phases, contradicting the assumption of a stationary or near stationary signal. This raises again he question if there are not also RWPs with nonzero phase speed. If that would be true, this would mean that individual RWPs could be accounted for more than once (if they include more than one wavelength) which would further be problematic for the identification of the source of this signal or its overall pattern. Further, how many cases are included in this composite? Shouldn't it be something like 1-3 events per season (mentioned last page) for this 93 year period? It is therefore somehow surprising that there is such a strong distortion of the wave signal at positive lags. Do the authors have an idea where this is coming from? Concerning the amplification of the signal, is that not a result of the composite for which the RWPs are forced to have equal phasing around day zero, but no such constraints exists for the days before and afterwards, why one would expect the signal to smooth out and decrease in strength.

- p.11, lines 340-342: Could the authors explain this more thoroughly how the MM trend (Fig. 4a) can be understood as result of the NA and SA related wave patterns? They add up together to explain the trend given in Fig. 4a? Maybe the authors give some further explanation for the reader.

- p.11, lines 344-350: What are the implications or the main conclusions here? The main take-away message is that there is a strong jet anomaly upstream of the wave signals? Do the authors have some ideas or hypothesis why the jet should be modified in this way for being able to be associated with a strong wave signal downstream? Nice findings. Do the authors have the information about the connection of their patterns to large scale pattern indices as Fig. 11b looks like a positive NAO (correct?)? Which would be very much in agreement to the findings of Wolf et al (2018, Quasi-stationary waves and their impact on European weather and extreme events), where they showthat a strong increase in QSW activity along the subtropical jet and the Mediterranean region (EOF2 and 4 in their Fig 11) can be associated with a positive NAO phase. So is the SA group associated with models showing more frequent positive NAO phases? Similar conclusion could be concluded from the PNA, which in its negative phase leads to increased activity of quasi-stationary waves over NA (EOF1 and 3 in Fig 11, wolf et al 2018).

- p.11/12, lines 351-355: This is rather a question, referring again to a point mentioned earlier: are the authors sure about the compositing of RWP events (is there no double counting for lagged RWPs)? Because those double counting with time lags could obscure the temporal relation between tropical forcing and the associated RWP.

- Discussion and Conclusion ———————————

- p.12, lines 369-370: But this was not working for the projection onto the global CTP, was it? This result was associated with the separation of the signal into different regions. So why can the results be associated with the evolution of the overall CTP? It is not obvious why the results for the analysis, restricted to specific regions, should explain the behaviour of a circumglobal teleconnection pattern. Could the authors maybe make this clearer?

- p.13, lines 418-420: This link between mean flow (waveguide) and the resulting wave pattern seems indeed crucial. The authors show the spatial relationship which seems to show increased/shifted jet stength upstream of the onsetting wave pattern. Do the authors have some interpretation or insight into the dynamical link for this connection, or is this just a result of increased jet strength (shifted away from) in the locations were are climatologically seen less activity of occurring wave patterns (for those regional wave groups)?

Interactive comment on Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2020-6, 2020.