Interactive comment on "Exploring the role of hydrological pathways in modulating North Atlantic Oscillation (NAO) teleconnection periodicities from UK rainfall to streamflow" *by* William Rust et al.

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We would like to thank Anonymous Referee #2 for their detailed review comments. We found them to be insightful, and, through our responses to them set out below, we believe that they have resulted in a much-improved paper.

General Comments

General Comment #1: "I do struggle to fully grasp the significance of the work, however. The wavelet analysis reveals intriguing patterns but these periodicities are difficult to

interpret. I find this paper like many similar wavelet/frequency papers in that a lot is down to the interpretation of the plots and assumptions made on driving processes - there is a lot of faith here seems to be on 'NAO like' signals which are taken and run with but to me are far from clear in the plots, and wrt previous work. There is no actual statistical link with the NAO (cf. FrancoVilloria, etc) which should be made clear from the outset (possibly even title?)."

Response to General Comment #1: We agree that there needs to be improved narrative around the reasons we are suggesting the detected 7-year cycle is the result of a wide-spread control such as the NAO (i.e. wide spatial domain that matches the known control of the NAO; common 7-year cycle found in both rainfall and streamflow). However, we also understand that we have not made any statistical comparison to NAO in this paper and therefore we are at risk of overinterpreting these results. As such we will add cautionary statements to the paper to highlight this and briefly discuss other possible causes for a wide-spread 7-year cycle. Furthermore, we would remove reference to the NAO in the title of the paper to avoid this bias and adjust the title to "Exploring the role of hydrological pathways in modulating multi-annual ocean-atmosphere teleconnection periodicities from UK rainfall to streamflow"

General Comment #2: As far as I understand the NAO literature it is somewhat debatable as to whether such patterns are physically meaningful. They do crop up in some papers but I have not seen so much that is convincing as to plausible physical interpretations (this can be said about a lot of work on cyclicities in hydroclimatic series in general). Most studies indicate that if NAO cyclicity exists it is weak, sensitive to choice of NAO index, season (DJF, DJFM) and tends to come and go over time as noted in the original Hurrell paper (these nonstationaririties being another barrier to practical applications; see also Franco-Villoria on nonstationarity of NAO-flow linkages). This should be elaborated on. See also important statistical work of Mills (2004). There is some discussion of some of this literature in the current paper (a lack of references in the reference makes it hard to find some of the key papers cited in relation to NAO period-

icity) but I find some claims quite tenuous as to what the cited literature reveals wrt the findings of Rust et al. The discussion is very wide ranging, but at times highly speculative and overreaches beyond the analysis made here, and as noted below sometimes reflects misinterpretation of existing papers, or reference to work that is not directly relevant.

Response to General Comment #2: We agree that the nonstationary of the interpreted period relationship between the NAO and UK streamflow requires further discussion in the narrative of this paper, and that discussion is also required as to the question of whether the periodicities in the NAO are physically meaningful. We will supplement the current discussion to address this and to add levity to our assertions that the 7-year cycle may be the result of the NAO, and any resultant implications for practical application.

General Comment #3: Finally, I am not sure of the practical significance of such findings. The authors suggest this reveals a 'degree of forecasting' and 'critical process understanding' but I am not sure how water managers can really use such information beyond a general raised awareness. And for that, there are already operational physically based seasonal to decadal prediction systems (the Met Office DePreSys system being one) which already give an indication of potential NAO states (and moreover, more importantly, the large-scale variables that drive the NAO, like ENSO, QBO etc which are much more predictable). How would findings really be used in reality? Perhaps the authors could comment. That said, there are wider applications of these findings in explaining general time series evolution (trends and variability) a huge area of interest in the literature.

Response to General Comment 3: This has also been highlighted by Anonymous Referee #1. We agree that there is a need to soften these statements about 'critical process understanding', and indeed 'degree of forecasting' to take account of the high-level scale of the study and that we are documenting general tendencies, rather than specific physical processes. We will soften these claims throughout the paper. Fur-

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thermore, we will improve the narrative around the practical implications; that our identification of an NAO-like influence on streamflow (particularly summer low flows given BFI relationship) provides a practical purpose for water managers to use the NAO forecasting systems. Rather than focusing on NAO prediction, but demonstrating where an understanding of the NAO can be helpful in forecasting hydrological change. âĂČ Specific Comments

Specific Comment #1: L18 – 'degree of forecasting' – I see this word is being used in a general sense but I don't think this is really what the paper is offering up, rather some general tendencies of multiyear river flow behaviour in time series, itself possibly useful but not really 'forecasting', although perhaps for general preparedness. I think any findings from studies like this are more useful in providing context for studies of long-term trends and variability (helping shed light on widely reported 'flood poor', 'drought rich' periods and so on) rather than providing any information on preparedness. The authors could comment on this potential application too.

Response to Specific Comment #1: We agree with this comment that 'degree of forecasting' infers a quantitative prediction of future river flow variability. We will adjust the text to discuss the benefits of these cycles in improving understanding of flow behaviour in general and focus on preparedness for extremes, rather than referring to forecasting specifically.

Specific Comment #2: L42 – L44 (around) maybe also worth clarifying early on that the NAO is primarily a driver of wintertime variation, noted later but should be in the intro.

Response to Specific Comment #2: Agreed – we will update the text in the introduction accordingly.

Specific Comment #3: L48 – [and into discussion]. I wanted to look into past research on claims of NAO cyclicities but found it a bit lacking in these papers – Tabari doesn't really look at multiannial cycles; Su found cycles but did not appear to look at the NAO. Neves relevant and useful. Kuss and Gardak not in reference list; Meinke looked at

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ENSO; no reference in list for Tremblay; no reference in list for Olsen, but found it and appears to be a different beast on Paleo timescales so not sure of its relevance here. It's difficult to examine the wider evidence based with these missed references. Some that are cited appear to be not entirely directly comparable – I'd recommend a careful re-reading and re-positioning of this work with the literature here (and in the discussion]. See also missed references in intro and other useful international papers e.g. Labat (2010) on various possible mechanisms of cyclicity globally.

Response to Specific Comment #3: We agree that this comment that this work needs to be placed more carefully within the context of existing research; specifically, the presence of NAO cycles and their teleconnections. This follows on from Comment #2, and comments made by Anonymous Referee #1. We will adjust the introduction literature review and the discussion to make clear the fact that existing research is unclear as to the presence of cycles in the NAO and their impact on North Atlantic hydrology / meteorology. We will also ensure that the reference list is updated and complete.

Specific Comment #4: L67, L68 – there are many more studies that look at NAO influences on UK streamflow in winter and more generally and these shed important light on regional patterns in some detail, and also catchment characteristics – would be worth referring to these. See e.g. Laize et al. 2012, Svensson et al. 2015 and references in both. Laize & Hannah really looked at this propagation question in some detail (for interannual rather than periodic behaviours but still relevant).

Response to Specific Comment #4: Thank you for highlighting these studies, we will review these and review our literature discussion accordingly.

Specific Comment #5: L136 – L139. At 705 catchments, this will inevitably be a very mixed set of catchments subject to all sorts of human influences. Ideally hydroclimate studies linking large-scale drivers to hydrological response should use relatively undisturbed catchments - human disturbances can alter the chain of propagation from signal

to streamflow response. I agree however that there are few such catchments (see e.g. Harrigan et al. 2018) which would limit the range of BFI/GRTs, so using the wider set is reasonable. While I expect that the outcomes from the broad-scale national picture is unaffected, there will likely be significant heterogeneity in the catchment results. This warrants a comment.

Response to Specific Comment #5: We agree that the influence of heterogeneity of catchment properties and the role of confounding influences in general needs a wider comment in the paper. We will highlight that over the period of analysis there have been both changes in regulatory and water resource management practices and that the latter will not have been applied in a consistent manner over all the catchments. Given this we might expect anthropogenic effects to add noise to the observations, but that there is no reason to expect that they should impart a systematic signal or bias to the data and so systematically effect the observations or results. We will add text to the discussion to this end, in addition to highlighting the potential influence of anthropogenic modulation of climate teleconnections.

Specific Comment #6: L163. This categorisation into even BFI classes will lead to very uneven numbers in the groups, given how skewed (and slightly bimodal) the BFI distribution is. Being that there is nothing meaningful about these arbitrary thresholds why not try and make them cover the distribution better? There are very few catchments at BFI <0.25, and not so many >0.75, with the great majority in the middle two classes. (I do not have this to hand but there are NRFA BFI distributions available somewhere in the literature I am sure and it would be easy to check). How would a more even classification effect the later results?

Response to Specific Comment #6: We agree that the distribution of sites in each category of BFI is not normal and therefore may lead to a skew in the results. This is however, mitigated by the Mann Whitney U significance testing in the final relationship testing which we draw our conclusions. We have previously tested equally sized bins and this did not change which relationships were signification, their direction or the final

outcomes of the analysis. The current bins were chosen to allow us to test a spread of BFI values in their relationship with signal propagation (i.e. understanding the influence of increasing BFI from low baseflow proportions); which we would lose by generating equally-sized bins. However, we appreciate that the rationale for this was not discussed in the paper. We will highlight the assumptions implicit in the categorisation we have chosen, and improve the narrative around our reasons for choosing this categorisation.

Specific Comment #7: L184. The point here is a valid one that BFI is simply an empirical property from the baseflow, but this could be generalised – rather than referring to throughflow, it can reflect any terrestrial storage in soils, lakes (lake and reservoir catchments also have high BFI) and so on.

Response to Specific Comment #7: Agreed, we will adjust the text here to generalise the conceptual model of BFI as a representation of hydrological pathways.

Specific Comment #8: L187 – I do not know how GRT is distributed but mirroring my BFI Comment above can the authors comment on this? It looks like it is very uneven from Fig 7 with many more in the highest class. Fig2 caption – might need to explain why this is log GRT as you have not introduced that yet, only referred to the categories.

Response to Specific Comment #8: We agree that more explanation is required for the specific groupings for GRT. Similar to BFI the groupings of GRT have been specifically chosen to investigate a range of GRT that are relevant to the propagation to these signals according to existing literature (such as Townley 1995). We will add text to the Methodology section to highlight this, and that our chosen categories are not equally populated.

Specific Comment #9: L204 – just a comment really for future work. This study does not consider the transition seasons, which is fine. But if the focus is really on propagation from winter NAO met signals I would have thought looking at spring would be really interesting – it may help better explain the propagation from winter.

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Response to Specific Comment #9: Noted and we thank the Referee for their insight regarding this. For the purposes of this paper, we want to focus on understanding a general trend between baseflow contribution and NAO-like signal presence. As such we chose to test between winter and summer flow to obtain the greatest difference between baseflow contributions (given the perennial nature and temperate climate of all the catchments within the study).

Specific Comment #10: L250. (and 2.3.4 generally). I'm not sure I fully follow the logic here so it needs some clarification – at the moment it sounds like this process is doing some heavy lifting. I failed to follow the process leading up to L250 that indicates "this produced a wavelet power for each dataset that is considered NAO like'. Why would this be considered NAO like a priori? This sounds particularly important given the 'NAO like' signal is then used to producing the residual series that is so important thereafter for capturing the 'measure of modulation of signal strength'.

Response to Specific Comment #10: We agree that further detail is needed to explain the rationale for selecting the representative 7-year periodicity from the dataset and indeed the a priori 'NAO-like' description. The intention is to take an a priori understanding that any wide-spread multi-annual periodicity found in both rainfall and streamflow around the 7-year range is likely the result of the NAO, given the NAO's wide spread control and assertions of previous research that the NAO exhibits a weak ~7-9 year periodicity (although we agree that we need to soften these claims and add cautionary text to explain that existing research is undecided as to whether the cycles in the NAO are meaningful). We agree that this narrative is not clear and that further text is needed to clarify that no statistical relationship between these cycles and the NAO is drawn. This text will be added to the Methodology and to the Discussion.

Specific Comment #11: L258. Following the above, it also appears to be a big assumption to produce this residual series for the summer based on winter rainfall. While in very high BFI catchments a very long lag time may be expected a priori, this is not necessarily the case in many (most?) cases (See my earlier comment about the

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spring season being omitted). I'm just concerned that two comments (while partly no doubt reflecting my lack of understanding of the process) are reflective of some major assumptions being made in this study which are (at face value) in danger of baking in some of the conclusions, somewhat – hopefully a clearer exposition will help allay these fears.

Response to Specific Comment #11: Our rationale for following this method is that (due to the perennial nature of all UK catchments in this study) even in catchments with lower BFI, variability in winter rainfall (in the dominant period of groundwater recharge) will influence summer flows that are generally baseflow-dominated. Where catchment have very little baseflow contribution, summer flows and their periodic behaviour are minimal and so our residuals simply show a lack of periodic behaviour in these instances. We will add text to the methodology section to clarify this aspect.

Specific Comment #12: L271 - Given this pairwise testing between groups, is my earlier comment about the irregular distribution of BFI a potential issue (would this look different with different more representative groups?)

Response to Specific Comment #12: We agree that the distribution of sites in each category of BFI is not normal and therefore may lead to a skew in the results. Our pairwise significance testing for these groups is the Mann Whiney U test which is non-parametric and is appropriate for non-normally distributed data. As such this issue is mitigated by the significance testing. Additionally, these bins were chosen to allow us to test a spread of BFI values in their relationship with signal propagation.

Specific Comment #13: L285 I am no expert on wavelets. But when I look at Figs 3 and 4 I wouldn't say a c.7 year cycle leaps out at me – rather, higher powers at a range of years <c.8 years. Especially for winter rainfall. That is, I see there are peaks in significance but are you really that confident in there being a (even approx.) 7 year cycle in these? IN fact I don't really see the 'two discrete bands of periodicity' (l279). I guess this is all down to interpretation but this causes some modest concern if this is

the basis of the identification of 'NAO like' signals – please elaborate on this. It's also really difficult to see the variability in the wider cloud of catchments shaded grey, but it looks like there is a range, especially for rainfall – some comment of this would be useful.

Response to Specific Comment #13: The purpose of Figure 3 and 4 is to show that the 7-year periodicity is apparent, especially in monthly and winter streamflow, and we agree that at present these do not do that sufficiently. We would suggest the individual catchment lines in these figures are made clearer to better show the range of periodicity strengths. Furthermore, we will alter the text in the results and the discussion to better highlight the purpose of these figures (i.e. that we expect to see a band of increased strength and increased significance around the 7-8 year band in monthly and winter rainfall, if there was a periodic teleconnection between the NAO and rainfall, that propagates to streamflow), and that this aligns with periods found in previous research.

Specific Comment #14: L290. 'Wavelet p values indicate the detected wavelet powers are not the result of external forcing'. Is this strictly true, I thought this just indicates it is not AR1 generated – I assume it does not rule out that it is internal variability, which could be driven by all sorts of long-term persistence processes (see the extensive work on the Hurst phenomenon and many papers of Demetrios Koutsoyiannis), as opposed to external forcing. (corollary to this, re: my point in the general intro about physical significance, when I backtrack into the literature on NAO periodicities, back to the Hurrell papers cited, it seems far from clear as to being settled whether NAO periodicities, as they are, are externally forced or internal variability).

Response to Specific Comment #14: We agree that there is a need for clarity of internal vs external variability in the NAO. In this instance, we need to make clear that even if the NAO is only internally variably, this can produce a behaviour in rainfall and streamflow that is externally forced (and therefore what we are testing the significance of). We also agree that additional text is required to highlight that this AR1 test does not specifically mean it is the result of the NAO, but simply not entirely the result of internal noise of the

rainfall / streamflow data. Text to clarify this will be added to the Methodology section and again highlighted in the Discussion section.

Specific Comment #15: L294 – should this say 'river flow records' instead of ground-water?

Response to Specific Comment #15: Yes, this should be 'river flow records'. This will be corrected.

Specific Comment #16: L340 onwards and Fig 6 – note whether previous question of BFI distribution has any impact on these findings?

Response to Specific Comment #16: We agree that we need to highlight that the data it not normally distributed, however our choice of significance testing is suitable for non-normally distributed data therefore this will have minimal impact on the overall conclusions from this figure. We will, however, add text to highlight the non-normal distribution of the bins.

Specific Comment #17: L416 – I would not have said this strong conclusion on the difference between the periodicities between winter ('present') and summer ('absent') rainfall really emerges from Figs 2 and 3 as noted earlier. I may be missing something but this seems quite an open interpretation of those data. Important as the seasonal differences are majored on.

Response to Specific Comment #17: We agree that the wording here could be improved and would propose changing to "Additionally, the stronger signal presence in winter compared to summer rainfall apparent in Fig 4 generally agrees with existing research showing that NAO's control over European rainfall is primarily expressed in winter months (Trigo et al., 2004; West et al., 2019)."

Specific Comment #18: L420 – L425. Following on from this, this discussion hinges on there being multiple periodicities at different peaks between the seasons, but my reading of those plots makes it hard to really pick out any of these as 'peaks'.

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Response to Specific Comment #18: We agree that this paragraph doesn't progress the narrative of the overall paper. We would remove this paragraph and add to the previous paragraph stating that we will be focusing on the 7-year periodicity.

Specific Comment #19: L425 – I also think this 16 – 32 periodicity is very difficult to see, let alone link to the EA pattern – and I could find no work on this in the Rust et al. 2019 paper cited? An important general point though is that while the NAO is the leading mode of variability there is a whole laundry list of (interacting) influences (Scandinavia pattern, AO, etc) as well as the lower frequency SST drivers (AMO, ENSO) that are not considered here given focus on NAO-like, even though other work suggests they could also manifest themselves on similar timescales (see e.g. Labat, Villoria). Worth comment in intro & discussion?

Response to Specific Comment #19: We agree that the discussion around 16-32 periodicity is difficult to see in the figures provided, and that ultimately the discussion of the 16-32 periodicity is unnecessary to the overall narrative, meaning this paragraph feels unnecessary. However, we also agree that there needs to be a more robust discussion as to the confounding influence of multiple other climatic oscillations that have been shown to influence the NAO's control on UK weather, which further adds complications to the teleconnection. We would remove this paragraph and add text to the introduction that prefaces this work by highlighting the known confounding influences in climatic teleconnections and that to soften the links directly to the NAO again highlight the uncertainty in understanding these teleconnections.

Specific Comment #20: L453. Haarsma not in the reference list, But on looking at this paper I don't see this SE England outcome on any of their results maps? Please clarify. In general, I find this whole gulf stream section really speculative. I looked (admittedly quickly) but could not really find much in these papers to support this e.g. concurrent but lagged correlations in Wavelet rather than periodicities; little specific mention of GS in Hurrel and Deser. A lot is made of the GS as a mechanism for the key NAO-like behaviour central to this study, so this reference to other work is important and could

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be checked and strengthened.

Response to Specific Comments #20: We would propose strengthening the literature review of this section and highlight that understanding the atmospheric – oceanic pathways is not the purpose of this paper, and additionally highlighting the need for further work to be undertaken.

Specific Comment #21: L576 – L582. Related to my points in the intro, good to see potential applications but this is quite a long way off from what is discovered in this paper so some of these claims could be moderated.

Response to Specific Comment #21: Agreed, we will seek to moderate these claims and the slimier claims made elsewhere in the paper to take account of the high-level nature of the study, and highlight the need for further work to investigate these processes in more detail before this is used directly for screening purposes

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