# Replies to Reviewer #2

August 2020

We would like to thank the Reviewer for their positive outlook on our study and the constructive set of comments provided. We provide a detailed reply (in red) to the individual comments (in *italics*) below.

1. *Section 2.3 (discussion of dynamical systems metrics): This framework is not familiar to a large number of people. If you have the space I think it is worth it to expand this discussion and explanation in the manuscript, especially since one of the stated goals of this manuscript is for it to be a proof of concept for using this kind of analysis in atmospheric dynamics. I must confess that, from a reader's perspective, I don't know that I'd be particularly inclined to consult several additional references just to understand the basic framework. I think leaving, for example, the detailed derivation of d and θ out is fine, but I'd appreciate a little bit more background information, especially since at least some of the authors on this paper are likely comfortable with this framework and certainly capable of more in-depth explanations. A few areas that I think could benefit from an expanded discussion:*

   - *Freitas–Freitas–Todd theorem, and the physical meaning of d. I think that I followed the part where d represents a trajectory (and thus, both space and time). However, the link between d and predictability was not at all obvious to me. Since Messori et al. (2017) has discussed this, I urge the authors to reiterate some of this discussion in this paper to make this easier to follow for the readers.*
   - *Furthermore, I found physically interpreting d a bit challenging: is a smaller d related to a weaker jet? A jet with less meridional variability? Both? What does an "active degree of freedom" actually mean in this context?*
   - *Predictability of d vs predictability of θ. Lines 132–135 made me think that d and θ are related to different kinds of predictability–if this is the case, please elaborate on this (this was not obvious to me). Again, the authors reference Scher and Messori (2018), which makes me think that perhaps they could reprise some of these ideas in more detail in this manuscript.*

We understand the issue raised by the Reviewer, and have taken the following steps to make the paper more self-contained and instructive for the many readers who may not be familiar with our approach. First, we have revised Section 2.3 to address the Reviewer's queries. We have specifically added a brief description of the theorem underlying our approach, expanded the physical interpretation of "active degrees of freedom" and reformulated the discussion of the link to predictability. Concerning the latter, we did not mean to suggest that $d$ and $\theta$ correspond to different kinds of predictability, but rather that they provide two different viewpoints on a state's intrinsic predictability, which in turn is not the same as the empirical

(or practical) predictability one may infer from a numerical weather prediction model. We hope that the revised text clarifies this issue. We have further partially re-ordered the Section to improve the logical flow of our explanation. Additionally, we have added an appendix (new Appendix B) where we provide a detailed derivation of $d$ and $\theta$ for the more technically-minded readers. Finally, we now provide a brief discussion of the link between relative differences in $d$ and jet meridional variability vs. speed in Section 5.

2. *L149: "very weak latitudinal variability". In this case, "weak" to me kind of implies a weak jet, which one would sort of expect to be more variable? Maybe switch your wording to "low latitudinal variability"?*

We have rephrased this sentence as suggested.

3. *Line 215: Where do the low d, high $\theta$ jets fit in?(e.g. merged jets in Figure 3?)*

We would argue that in the barotropic zonal wind, which we use for the reanalysis data, there is no low $d$, high $\theta$ jet. Such combination may occur in isolated cases, but it does not correspond to a jet regime in the way we conceptualise it in the study. Indeed, the $d$–$\theta$ scatterplot for ERA-Interim barotropic zonal wind over the chosen region and season (Fig. 1 below) resembles reasonably closely the scatterplot of model data in Fig. 3b in the paper. To clarify this issue, we now specify that in this passage we are referring to Fig. 3b.

4. *Line 255: Change "one main" to "one might".*

Thank you for spotting this typo, which we have corrected.

5. *Section 5 (Discussion): I realize the point of this approach is to avoid using the conventional and complex physics-based analysis in place of this phase-space analysis,but I think that extending your results a bit farther to results gained from conventional physics-based studies would be helpful. If the long-term goal of this work is to encourage people to consider adopting this kind of analytical approach, giving them an example of how they can connect these phase-space results to more physically intuitive dynamics would be a useful guide. You touch on this a little but I think a slightly expanded discussion would strengthen this paper. For example, in section 2.3, you link both d and $\theta$ to predictability. In Line 255, you state that "one might expect a low d,high $\theta$ state to be more predictable than a high d, low $\theta$ state" – can you elaborate on this? Do the variability/persistence of the low d, low $\theta$ states match what you'd expect of a subtropical jet? And so on. I think that this paper touches on these ideas very briefly but I'd encourage the authors to draw more explicit and clear connections so as to really emphasize the utility of this approach.*

Following this comment and Comment #1 above, we have significantly expanded the discussion of how the dynamical systems metrics relate to predictability in Sect. 2.3, and have added a brief discussion of some recent results concerning the dynamical systems predictability and the skill of numerical forecasts in Sect. 5. In order to make the link with the jet regimes more explicit, which we understand is the main suggestion of the Reviewer, we have further added a paragraph providing some practical examples to the same section. There, we try to link the atmospheric dynamics underlying the different jet regimes – for

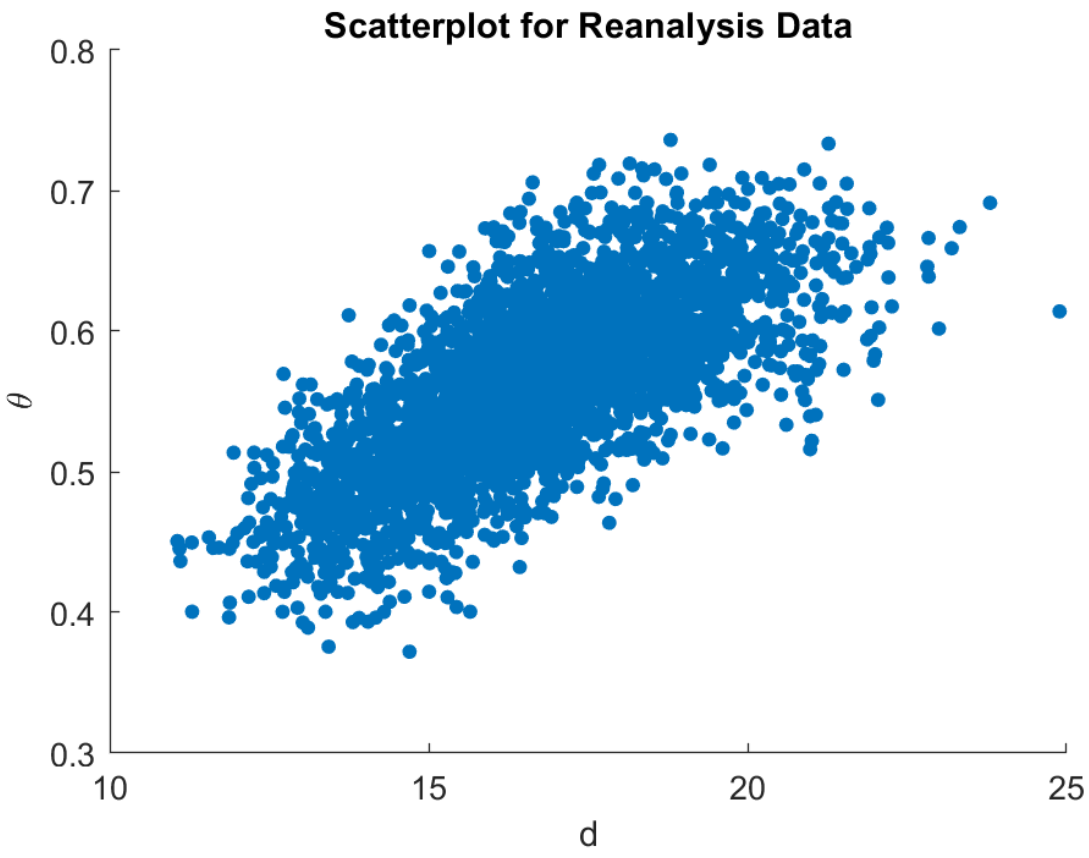


Figure 1: $d$–$\theta$ scatterplot of JJA barotropic zonal wind in ERA-Interim data

example their wave structure – with their dynamical systems characteristics. We specifically consider how energetic the waves are, in terms of eddy kinetic energy, how broad the wave spectrum is and how these characteristics reflect on the meridional variability and temporal evolution of the atmospheric flow. If the Reviewer has further specific suggestions in this respect we would be glad to include them in a successive revision.

6. *Figure 2a: Include the "x" symbol in the legend.*

We have updated the figure legend as suggested.

7. *Figure 3: I would suggest not using abbreviations in the legend. Furthermore, since persistence is related to $\theta^{-1}$, I would suggest reiterating in the caption that smaller $\theta$ corresponds to larger persistence. Alternatively, you could label the y-axes with "less predictability – more predictability" (similarly to what you did in Figure 4).*

We agree that the fact that persistence is the inverse of $\theta$ may cause some confusion, and have revised the figure caption accordingly. We have further modified the legend as suggested by the Reviewer.

8. *Figure 4: Please reproduce the legend from Figure 2, and include a colorbar. Again, I'd suggest reiterating in the caption that persistence is related to $\theta^{-1}$*

We have revised the caption and the figure as suggested by the Reviewer.

9. *Figure 5: Since the main point of Figure 5 is to point out a detail in Figures 3 and 4, I think it could be moved to the Appendix (if the authors are looking to streamline or shorten the manuscript).*

While we understand the Reviewer's viewpoint, we think that the figure makes for an interesting discussion point. We further believe that the revised manuscript is already reasonably compact, having only 7 figures in the main text, and have therefore opted to keep this figure in its original location.

10. *Figures 6/7: It might be helpful to combine these figures. Personally, I would find it helpful to look at the vertical and horizontal structures together. This is just a suggestion, and very much up to the authors' preferences.*

We have revised the figure as suggested by the Reviewer. Now all 6 panels are part of the new Figure 6.