

Dear Maarten,

We are very grateful for your careful reading of our manuscript and your insightful comments. Your comments are very valuable to improve the manuscript, in particular in terms of conceptual clarity and, hence, readability. Our general author comment (a separate document) provides an overview of modification that will be implemented in the revised version of our manuscript. In this author comment we will address more specifically your major and conceptual comments. A more detailed point-by-point response will be given when submitting the revised version of the manuscript. Your comments are repeated in black while our comments are colored in blue.

In my opinion this paper addresses the interesting and intriguing question whether Rossby wave envelopes exhibit higher predictability than the underlying waves. There are good arguments why this may be the case although there are equally good arguments for the opposite. The authors argue that the predictability of the wave envelope is no better than that of the underlying waves.

I found the paper interesting and well written, although a bit dense and not so easy to follow. I also have some reservations about some of the descriptions, but I would not consider those major issues, in the sense of requiring a major revision.

We believe that the revised version will be less dense and hopefully easier to follow. The revised version will consider the more common definition of RWPs based on the wind field (see our general comment) and will only later complement this view by an analysis of error-growth dynamics in the PV framework. The PV framework will thereby be introduced only later in the revised version, which should make the manuscript less dense and with a clearer structure in the beginning. In addition, the more common consideration of RWPs by their phase and envelope – as you have suggested – should further increase clarity. The explicit use of the Hilbert transform will simplify the notation, in particular with respect to the PV tendency equation for the envelope (and phase).

There is one more major point which I found hard to think through its implications, but which seems central to your argument: the normalisation of the errors with their individual saturation values. Perhaps it is worth emphasising again what you mean to do here. May I suggest you spend a bit more time at the start of your section 4, or perhaps in section 3.2 to flesh out the implications and interpretation of this methodological choice of normalisation?

We agree that this normalization is central. We had hoped to make the implications of the normalization clear enough with our discussion of Fig. 6 at the beginning of Sect. 4. We will further discuss the methodological choice in the revised version of section 3.2.

One way to define complete loss of predictability is when forecasts (in a statistical sense) are indistinguishable from random draws from climatology. Because this is how we compute the saturation level in our revised version (see general comment), we should be able to clarify why normalization with the saturation level is a suitable metric of predictability.

A further important aspect that we will stress in the revised version is that we perform *conditional verification*, i.e., we compute forecast errors only in situations when a specific feature – an RWP for example – is present in the analysis. Conditioning on large-amplitude anomalies usually implies larger errors, but at the same time a larger predictive signal. The saturation level needs to be adjusted for conditional verification to account for such differences.

If I understand this well then the normalisation allows us to see how the MSE grows as compared to what the maximum MSE could ever be for that individual component. [Yes, you understand correctly.](#) However, Fig 6a clearly shows that in terms of the PV error, the underlying wave still grows larger errors, presumably indicating the extent to which phase errors contribute to the underlying PV anomalies. In other words, it is still the case that the phase errors introduce a further mechanism of error growth, which is by definition not seen by the envelope. I think that what you actually show is that this phase error grows at the same rate as the amplitude error. If the phase error grew faster, then the underlying wave error growth would contain a faster initial timescale compared to the growth of the envelope error. [We are not sure that we fully understand this comment. Figure 6a does not show the normalized error. Yes, in absolute terms, the MSE is higher for the underlying wave pattern than for the envelope. We further agree that these larger errors are associated with phase errors and that our results indicate that phase errors do not grow faster than envelope errors. Our revised version will explicitly discuss the differences in error growth of the phase and of the envelope.](#)

I think this distinction between phase error and amplitude error can be easily extracted using Hilbert transforms. Perhaps the authors would find this of interest to pursue, but as reviewer I feel I am not in a position to insist on this. In my opinion that is a much clearer way to describe the main conclusion (if you agree with my interpretation): phase errors grow at the same rate as amplitude errors. (I do realize that you suggest that the Baumgart and Jankov papers, which I have not studied, show that the phase error grows quicker at the start; this may be contradicted by your results in this paper.)

[Thank you very much for this important comment. Albeit you did not insist, we have followed your suggestion. We believe that considering phase and envelope errors has much improved conceptual clarity in the revised version. Indeed, our result contradicts, at least for the synoptic scale and for medium to high wave amplitude, earlier ideas that initial error growth is dominated by phase rather than amplitude errors.](#)

Minor comments: [We will respond to these comments when we submit the revised version of the manuscript.](#)

L1: "... the long standing hypothesis ...": It seems that the Lee & Held paper is the only reference to this hypothesis. To call this a "long standing hypothesis" seems a rather grand description for a single somewhat vague idea published some 30 years ago, but has not had much specific follow up since. Perhaps just state the straightforward question: is the envelope of a Rossby wave better predictable than the Rossby wave itself?

L8 (and several other times in the ms): "the envelope is the wave": I think this is not at all an accurate description of what you actually found, and I strongly suggest you do not use this phrase, which just seems misleading to me. I think you found that the wave amplitude has the same predictability as its phase. I am sure you agree that in a literal sense it is not at all true that the envelope is the wave: a wave includes phase information, the envelope only contains the amplitude information. They are different things by definition.

L34-43: A nice description. I think it would be worth to follow this up with a description why it also could be argued that the envelope has the same predictability as the underlying wave. I disagree with the "larger scale" argument: the larger scale envelope would just contain fewer

wavenumbers (uncertainty relation from QM!): it is just an expression of what waves make up the packet, not whether the packet has some intrinsic larger scale with its own physics.

Fig 2: I would strongly advise to use the same colourbars for the winter and summer pictures; that way we can properly compare. It looks like there are few days with RWPs in winter, compared to summer (although it is not so easy to see with the different colourbars) which comes as a big surprise to me, given that winter is baroclinically much more active. Are there fewer but longer lived events in summer perhaps?

L123: I do not recognize the Hilbert transform in this equation; perhaps worth adding in the discussion and in the equation? I am also surprised by the notation for the Fourier components of q . Why not use something like $\hat{q}$?

L134: Are the differences between RWPs and maxima of E just due to the different basic states used to find anomalies? Perhaps make explicit in the paper?

L168: Perhaps worth noting that variances may be about twice as big, but the RMS difference is of course rather less.

L177: the neglect of N in the error growth. At first glance, this seems like a bad approximation for timescales beyond a week, also in the context of much recent interest in the contribution of diabatic effects on Rossby waves. Can you justify this better? The term N being smaller than the advection terms in a PV equation does not mean that this is also true for the anomaly equation (i.e., the advecting winds could be very well predicted, while the N might be rather uncertain).

L198: "residuum": I think that is German. Change to "residual"

L209: "ir-rotational" -> "irrotational"

L272: "orange" -> "brown"

L300: perhaps worth explicitly explaining at this point that you from now on will call the sum of BC and ADV "moist-baroclinic". I think it is a rather informal way of interpreting moist baroclinic processes how we normally understand them.

L305/6: I was confused why they would not add up to 1 in the figure. Explain more clearly what is used to normalise the curves.

L336/7: Sorry - I did not understand this sentence at all. Rephrase?

Section 4.3: I had (and still have) great difficulty in following this section. I think there is so much confusing methodology that the reader simply loses track. Please consider rewriting some of this and aim a bit more directly at the question rather than let the reader drown in the various percentiles inside and outside RWPs. Perhaps I am not clever enough to understand what is happening, but in general, I think you need to make things easy for the reader.

L405: I did not understand this conclusion at all: if you control for amplitude you find that predictability in and outside RWPs is the same, how can you conclude that RWPs "organize" high amplitude waves? Again, it probably hinges on the fact that I struggled to get much insight out of section 4.3.

Maarten Ambaum