We thank the reviewer for carefully reading our manuscript, and for her constructive comments. In the following we will respond to the various comments and point out any changes we intend to make to the paper based on them. Note that we have not provided exact manuscript corrections at this point, but we have provided the outline of planned changes. Line numbers and figure references in the reviewer's comments refer to the original manuscript. The reviewer's comments are in *black italics;* our responses are in blue.

Eddy life cycle experiments have been used as a framework for understanding eddy-mean flow interactions in the midlatitude atmosphere for decades, as highlighted by the references provided by the authors and a recent review (Maher et al. 2019). In this study, the authors show that the sensitivity of the final jet state to the initial jet state may partly be an artifact of the idealized nature of traditional eddy life cycle experiments. When a single wavenumber is forced, wave breaking is very sensitive to meridional shear: with low shear, waves break anticyclonically, shifting the jet poleward (LC1), while with higher shear, waves tend to break cyclonically, shifting the jet equatorward (LC2).

The authors consider variations on these single wave, or monochromatic, experiments by adding noise of varying levels to excite all wavenumbers. They show that even in the limit of very weak noise, the the final state of LC1 and LC2 lifecycles are quite similar due to secondary wave breaking that occurs after the initial anticyclonic or cyclonic breaking event. The net change is primarily to LC2 cycle, where the second breaking event is anticyclonic, shifting the jet back poleward. Thus the shear has a large impact on the initial wave breaking event, but less so on the final state.

I think these are interesting results which merit publication after the authors consider the following minor revisions. It is remarkable that we are still learning about lifecycle experiments after almost half a century!
Thank you for these encouraging summary remarks.

General comment
Throughout much of the paper I was concerned about how the results depend on the initial noise. This is to say, with a different realization of the noise, could the evolution ofthe lifecycle be materially different? This concern was partially addressed by results from 3 member ensembles (in the discussion surrounding Figure 7), but even here, it's not possible to gauge the variance across the ensemble. I take it that the lifecycles proceed more or less the same way as long as their is some noise in the relevant wave numbers (waves 1-10 or so); even if the most important wavenumber for the secondary breaking event (wave 4) was weakly forced by the noise, nonlinear transfer of energy would invigorate it. But it would be good to establish this early in the paper.
Thanks for pointing out. Indeed we will state clearer how our results depend on the noise realization we use. Based on sensitivity experiment with different realizations we found our results to be mostly independent of the initialized wave spectrum. Experiments in which the initial random noise perturbation only projected weakly on wavenumbers 2 and 4 showed the same qualitative behaviour as experiments which strong wavenumber 2 and 4 contributions in their initial noise. This further suggests the importance of the scale-selective non-linear interaction which accelerates the growth of waves 2 and 4 via energy transfer from the dominant wave 6.

To be constructive, would it be possible to show a few additional experiments (initiated with different noise) in Figure 2. (And possibly Figure 4, which shows the final jet states for the same integrations.) I hope that additional solid lines for the \eta=10^-3 experiments would not overly crowd the figure. If all the low noise experiments look exactly the same, the authors could just state this in the text and alleviate my concern from the start.

We plan to provide a figure on the evolution of the ensemble members illustrating the small intra-ensemble variability compared to the difference of the evolutions.

Another option would be an additional figure showing that the evolution of key quantities (momentum fluxes, EKE, etc.) follow very similar trajectories for different initializations of noise for all levels of \eta. (Perhaps the variation in noise matter more when \eta is large?) The key is to establish that the difference between lifecycles with different noise realizations is small compared to the difference between the experiments with noise and the monochromatic experiments.

We agree, this key question will be adressed in detail with a figure in the supplement as stated above.

Minor comments by wavenumber

12-3. I found this line to be a bit awkward. Consider "... for LC2 initialisations are found to become unstable eventually, with the onset of instability coming sooner for larger noise perturbations."

Line will be adapted.

28 "flavours, or paradigms, of"

Will be done.

Paragraph at 66: As the noise is the major contribution of the manuscript, it might be nice to explain the gist of it in the text. For instance, you could say that the perturbations are white in space, equally exciting all wavenumbers (on average). Perhaps this could be done at line 77 where the amplitude of the noise is introduced.

Will be done.

74 along the same lines, could you briefly characterize the meaning of parameter \hat{U_s} in the text, referring to the equation number in the appendix.

Will be done.

77 Appendix90. It was around here that I started worrying whether the realization of the initial noise
mattered to the lifecycle. If it does not, a sentence here could put the reader at ease. This could also be discussed in the figure caption.

Additional to a supplemental figure, the sentence in lines 82-83 will be slightly changed to make this clearer from start.

107 This is just a comment about style, but I find that footnotes almost over emphasize the point, as the reader breaks off the text to get to it. Consider just putting this material in the main text.

Thanks for that suggestion, we will put this footnote into the main text.

138. Could you describe this noise induced wave breaking as a secondary instability? The flow is presumably now stable to wave 6 perturbations, but not others?

Indeed, the growth of the noise could potentially be due to a secondary instability. However, there are several indicators that the increase in EKE for waves 4 and 2 for a second wave breaking is heavily fostered by wave 6 and only partially can be explained by linear theory. One indication for non-linear interactions between the different zonal wave numbers is the deviation in growth rate from the prediction of linear theory, which is already visible during the first days of the simulation. Furthermore, the EKE of wave 6 drops, when wave 4 and 2 reach substantial amplitudes (see e.g. Fig. 6b, day 22). This seems to point to an energy flux from 6 to 4 and 2. Nonlinear processes appear to alter the growth of the noise significantly in all non-monochromatic simulations.

142-145. This line seemed to come too early in the text. Please shift it back after Figure 5a is introduced and the result has been established.

Will be done.

167-7. Is this really similar to quasi-linear non-normal growth? That process is rather distinct from nonlinear wave interactions. Please provide more evidence to support this statement.

Thank you for pointing this out. We tried to express that the observed growth cannot be explained by single normal mode growth. We do not intend to focus on the distinction of normal vs. non-normal growth, but rather on the non-linear mechanisms still resulting in accelerated, quasi-linear growth. We will adapt the passage accordingly.

174. Is "upscale energy cascade" an appropriate way to describe this? Consider "upscale energy transfer" as the flow does not appear to be fully turbulent.

We agree, the substitution of "cascade" with "transfer" will be done.

213. What is "In general" meant to signify here. Is this is reference to the fact that different realizations of noise can lead to different behavior? Or does it refer to difference that occur with variations in the shear parameter \hat{U}_s or other qualities of the initial jet state.

We will include a clearer wording.

218. Same for "typically".

We will drop this word.

222 Could you clarify what is meant by "overall net-poleward jet shift periods." The tendency of anticyclonic breaking to shift the jet poleward should be reflected in the mean state.

“periods” will be removed. Then it says “overall net-poleward jet shifts”

232-4 The meaning of this sentence was a bit obscure to me. Do the authors mean that the remarkably sensitivity of monochromatic experiments to \hat{U}_s, which justified the LC1 vs LC2 paradigms, may not be justified with noise? That is, there aren’t really two kinds of wave cycles, but rather a continuum?

Indeed, describing whole simulations of a longer period including several wave breaking events in different directions with the LC1-LC2 dichotomy might not be justified. For single wave breaking events or phases however, we still deem the paradigms to be useful. In some experiments with only noise as initial perturbation, we even found spatial differences in wave breaking direction at certain times (LC1-like behaviour at some longitudes and LC2-like behaviour at others). We will try to make these aspects clearer.

236 and 263. Again, consider “upscale energy transfer”

Will be done.