I have copied the major comments of the reviewer in black, along with my responses in blue.

General Comments:

The paper presents an investigation into the Quasi Stationary Wave Strength and its dependence on surface forcing and internal dynamical factors. The investigation is performed using eight 100 year simulations from the CAM6 model. I believe the subject is of interest to the community and the set of simulations has been systematically/logically designed to probe the relationship between different surface forcings and Quasi Stationary Waves.

I found the paper quite difficult to read and spent a lot of time flicking back and forth between various sections to work out what was plotted in the various figures and why, so feel it would benefit from a careful revision to the text to make it more explicit in terms of analysis and interpretation and some restructuring to make the main scientific questions/hypotheses and methods clear. It is possible the paper is attempting to cover too much material so there are many multi-panel figures, which require a lot of explanation. Reducing the scope of the paper would in my view make it much clearer and stronger.

We thank the reviewer for taking the time to review our paper. Their comments and suggestions will significantly improve the manuscript. In response to this comment, we will remove the current Section 3.2 (on the role of zonal-mean zonal wind and meridional wind) to improve the readability and reduce the scope. We will also add more analysis to illustrate the distribution of quasi-stationary waves (QSWs) through temporal analysis, as suggested by the reviewer. Additionally, we will outline and summarize the hypotheses, analysis, and reasoning/conclusions as bullet points to better highlight the core message of the paper.

The main body of the paper is devoted to testing two hypothesis:

1 The spatial distribution of quasi-stationary waves is governed by that of stationary waves.

2 The spatial distribution of quasi-stationary waves is governed by that of dynamic factors such as Eady growth rate and local stationary wavenumber and transient wave strength. These hypotheses appear in the results section, but I think they should really appear in the introduction motivated by/linked to the discussion of literature. The methods section can then more clearly explain how the methods are designed to address the hypotheses

Thanks for your suggestion. We will fit the hypotheses in the introduction as suggested. In addition to these two main hypotheses on the distribution of QSWs, we also explore the impact of stationary forcings on the duration of QSWs, and plan to keep this analysis in the manuscript, as we believe it’s an important result for this work.

The partition between stationary, quasi-stationary and transient waves defined by the authors is as follows:

- Stationary waves: A day of year climatological mean.

- Quasi-stationary waves: 15 day low pass filtered anomalies the climatology

- Transient waves: 15 day high pass filtered anomalies to the climatology

All three of these are converted to wave-envelope amplitude (as in Zimin et al 2003) including only wavenumbers 4-15.

A major methodology employed in the paper is comparison of climatological mean spatial patterns of quasi-stationary wave amplitude to that of stationary waves and transient waves as well as to climatological mean spatial patterns of Eady growth rate and stationary wavenumber. So a number of the conclusions hang on the assumption that a similar climatological mean pattern implies a significant relationship between quantities/lack of a similar pattern implies no relationship. I think this method is employed in all figures except fig 1and fig 5 which makes use of time variation in the data. The robustness of this basic assumption requires some discussion in the text - it isn't clear from what is written why this is a useful method and what its possible limitations are. The authors could also consider including more results which more carefully examine the relationship between stationary wavenumber and Quasi-stationary wave strength by utilising information about time covariation of these two quantities - this to me would add weight to the argument that the two a closely linked.

Thank you for your important suggestion. We will add more discussion of this method and its limitations. We will add new temporal analyses, including lead-lag composites between QSW strength, stationary wavenumber Ks, and transient eddy strength, and composites of Ks during QSW and non-QSW days. Based on our current results, we find a clear lead-lag relationship between QSW strength and transient eddy strength, although the pattern varies across regions and experiments.

The main conclusion of the paper is that stationary wavenumber has a clear association with QSW strength suggesting a simple barotropic interpretation. This is an interesting conclusion, but the more clarity is needed in the explanations of results to justify this.

Thank you for your suggestion. We will revise the wording in the next version of the manuscript. Our argument is based on the fact that the stationary wavenumber Ks is derived from the dispersion relation for barotropic Rossby waves. Therefore, even if some nonlinearity is allowed in the process, the QSWs should remain (equivalent) barotropic throughout their lifecycle. We will include a more detailed explanation of our results and discussion of this point in the Discussion section to link the conclusions to the results more clearly.

Specific Comments:

- There is a long introduction which covers well the literature in the area. This ends with a short paragraph about the scientific question/hypothesis the authors' work seeks to address. The hypothesis in this final paragraph is too vague and doesn't really explain what the authors aims are and how these fit into the literature described prior to this.

Thanks for your suggestion. We will add more introduction about how these stationary forcings affect circulation, and then affect QSWs based on literature. We will also specify the hypotheses at the end of introduction more clearly as suggested in the next comment.

- Two hypotheses are given in section 3.3. The bulk of the paper seems to be aimed at testing these, so perhaps it would make sense to include these in the introduction and motivate them from the literature - why are these important and novel hypotheses to test. Then to include some discussion of the methods used to test these hypotheses in the methods section.

Thanks for your suggestion. We will state the hypotheses and design of analysis at the end of introduction.

- section 3.1 line 170 What does "frequency of all events is fixed" mean - explain this more clearly. Do you mean you are defining an amplitude threshold for an "event" such that the total number of events is the same between the different simulations and reanalysis?

Thanks for your suggestion. Indeed – in each dataset (reanalysis or simulation), we select the strongest 90 QSW events, corresponding to the 90 analyzed years, to ensure a fixed frequency of, on average, one QSW event per year. However, in response to other comments about reducing the panels in this figure, we will remove this analysis from the main paper, and keep it only in the Appendix. This analysis shows that our results on duration are not sensitive to how we select events in the different simulations.

- Main conclusions of Figure 1 seem to be that the model is biased towards shorter duration events, including stronger forcing increases the duration of events. You could consider whether these points could be demonstrated with a simpler figure with fewer panels. It would also be good to comment on whether these conclusions would be expected from what we know about climate models/other modelling studies or are specific to this particular model.

Thank you for your suggestion. We will move the fixed frequency results to the Appendix. We will also expand the discussion on the performance of the CTRL experiment, including its limitations—such as the shortcomings of using prescribed SSTs, and connect these to known biases of climate models.

- section 3.2 you state that amplitude of meridional wind is used as proxy for stationary wave amplitude. This implies you are calculating the zonal mean of magnitude of the time mean meridional wind, not the time-mean zonal-mean of the magnitude of the meridional wind which would include transient waves. However you don't state clearly in the text what is calculated.

Thanks for your suggestion. We've removed this section now.

- line 257: "As a first test, we calculate Spearman rank correlation coefficients". This appears to be the main test that is used in the paper to link QSWs to the other quantities.

Thank you for your comment. Our distinction was between the first test – correlation on the absolute fields from simulations, and the second test – correlation on the differences between experiments. We will now also include the temporal analysis, as suggested by the reviewer, and so will remove the language around 'first test'.

- Figure 3. Panels a and b are very clear, but it would be good to state the contour interval where they are described in the caption rather than at the end.

Thanks for your suggestion. We will include the contour interval in the revised manuscript.

- The effects of different types of surface on QSWs are investigated by examining differences in climatological means between different pairs of simulations, rather than just looking at anomalies from the control simulation. This makes the results quite difficult to follow, my question is whether this complication is necessary or anomalies from the control could be used instead or whether the anomalies are necessary at all - i.e. could you make the same points just by looking at the fields themselves and comparing? This might simplify the analysis. If it is necessary to look at these anomalies as in the current paper to make the points, then it would be good to give more explanation to help the reader understand what they are learning from the different pairings.

Thank you for your suggestion. As mentioned above, we compare differences in climatological means between different pairs of simulations because (1) each metric shows some degree of correlation with QSW strength when comparing climatological values directly, but the differences do not necessarily (see Table 1), and (2) we aim to understand whether different stationary forcings (e.g. topography vs diabatic heating) influence QSWs in distinct ways. We will add further discussion to clarify the reasoning behind this methodology, and on the potentially different impacts of diabatic heating and topography on QSWs.

- A significant portion of the paper addresses the link between stationary wavenumber and quasi-stationary wave amplitude. This to me was the more interesting part (particularly fig

5), however the method for calculating stationary wave number is missing some key details: the authors state it is calculated from zonal wind, but don't specify whether this is a single level a vertical integral. In figures 3c and 3d, I can not determine what the contours are showing. At first I assumed from the caption/title that these were the stationary wavenumber with only values 4 to 8 shown, however the contours range from 2 to 10 so is this just stationary wavenumber?, if so why have 4-8 in the title - please provide a clear explanation.

Thanks for your suggestion. We calculate the stationary wavenumber Ks at 200 hPa, the same as the level where QSWs strength is calculated. We will add it in the revised manuscript. Thanks for pointing out the incorrect title - we will correct it in a revised manuscript, and add details in the caption about the contours.

- Figure 5 is very interesting and appears to show a clear link between the Ks and the QSW amplitude. However it is not clear from the caption of text what is actually plotted here. The count refers to the number of days in DJF which is clear and one assumes this sums up to 90xlength of simulation in years. But how do the authors decide what the QSW strength and stationary wavenumber are for a given day - is this a spatial mean value?

Thank you for your suggestion. The caption is incorrect—it should refer to the count of grid points, not days. The values of both stationary wavenumber Ks and QSW strength shown are climatological values. However, as per your earlier suggestion, we will add more temporal analyses. In those, the count will be based on grid points per day, thereby incorporating time information.