The authors have responded to almost all of my previous comments in a very satisfactory manner. There are two exceptions: details of the ΔSLP diagnostic (#3 below) and the effect of the bin size (#4 below) which in my opinion still require further consideration and revisions to the manuscript. Furthermore, when reading the revised manuscript, I noticed a few minor issues that lacked clarity (points 1, 2, 5, 6) and thus a few additional revisions are still warranted to improve both the clarity of the manuscript.

1. Line 60. This new paragraph in the introduction does not flow well with the rest of the text and it is not clear whether the purpose of this paragraph is the motivate / justify the choice of the two variables or explain what they are. I think this paragraph should be primarily motivation and this needs to be written more clearly. e.g "*The main motivation for selecting these two variables is...*"

2. Line 99. This sentence is confusing "*The static stability is not constant in the domain, but a 1d vertical profile is used instead*" as it includes reality (static stability varies in the horizontal and vertical) and what is done to compute to QG omega (static stability is assumed constant in the horizontal and only varies in the vertical). A few more words are needed in this sentence to make it clearer.

3. Figure 1. ΔSLP. The issue I previously raised (see reviewer 2, minor point 3) has not been resolved. It is still unclear when reading the manuscript exactly how this diagnostic is calculated, for example if the real time between time of genesis and time of minimum MSLP is 48 hours, are there four 12-hr values of deepening rate calculated (-48 to -36, -36 to -24, -24 to -12 and -12 to 0hr) or is a sliding window used or is only the maximum value used? Secondly, a sentence needs to be added to the manuscript explaining why there are positive values (weakening cyclones) in the distributions of figure 1 even when only the normalised times from -1 to 0 (the intensification phase) are considered.

4. Previously, I asked how the size of the bins and the differing number of points per bin may affect the results. Thank you for providing additional analysis on this matter. However, having seen Figure 6 in your replies, I do not think your conclusions (first bullet point in conclusions section and also text in section 3.2) as currently written are fully supported by your analysis - I think it is more complex than you state and there are a few subtle points that you should stress more clearly.

   Firstly, Figure 6 in your reply, bottom right panel (zoomed in part) shows that, for this limited part of the parameter space, that the strongest deepening rates occur for high EGR but low QG omega. This is not consistent with Figure 3a in the manuscript and needs explaining. Potentially the mean values in this part of the parameter space are not statistically different though and considering the distributions may clarify this point.

   Secondly, when Figure 6 in your reply is considering together with the 2D histogram shown in Figure 3a in the manuscript, I think the correct interpretation of these figures / analysis is that the while the strongest deepening rates occurs for high EGR and high Q, strong deepening rates can also occur for high EGR and <u>moderate</u> values of QG omega. This is somewhat written in the first bullet point of the conclusions but I feel it is a result which should be stressed more and better explained. Related to this, Figure 3a strongly suggests that EGR has a stronger influence on deepening rate than QG omega – this is already somewhat touched on (but rather indirectly and briefly) by the authors when discussing the asymmetry between the bottom right and top left corners. This subtle result is quite interesting and the manuscript would benefit if this was highlighted more clearly and physically explained. One hypothesis to consider is can the instability (high EGR) be effectively released as long as there is a reasonable amount of upper level forcing? (moderate to high Q)?

In summary, the authors should carefully revisit and revised section 3.2 and the first bullet point in the conclusions, potentially even splitting this conclusion into two.

5. Line 160. Suggest you revise “49 bins” to “49 linearly distributed bins”.

6. Line 276-277. This sentence could be clearer – currently it sounds rather alarming (that the results are almost meaningless). I think the authors intend to say that the mean evolution shown in Figure 4 is not representative of any one individual cyclone lifecycle. Please revise this.

7. Figure 4. While this is a nice addition to the manuscript, showing only the mean values clearly hides the large amount of variability as the values on the y-axes cover much smaller ranges than what is shown on the x- and y-axis of Figure 3. Is it possible to add some additional lines to this figure e.g the $25^{th}$ and $7^{th}$ percentile values?