RC1

I thank the authors for addressing the majority of my comments. There is now a better inclusion and discussion of NWD events in the study. The tone regarding the SSW definition is also improved.

I only have a few remaining minor/technical comments. Line numbers here are based on the "tracked changes" version of the manuscript.

Line 20: perhaps "lengthens the potential prediction horizon of the surface response" since changes in the predictive skill have not actually been evaluated here

See response to next comment.

Line 21: This last point regarding the SH is an interesting one, but it's not the focus of the study and is now only briefly brought up at the very end of the paper, so it seems out of place to be brought up as a main point in the abstract. I wonder if this list of PWD advantages in the abstract should more closely echo the bullet points in the Conclusions.

We now write: "… the wave activity flux definition captures with one criterion a variety of different event types, detects strong SSWs and strong final warming events, avoids weak SSWs that have little surface impact, and potentially lengthens the prediction horizon of the surface response."

Line 231: change to "ERA5 leads"

Done.

Line 253: change to "zero crossing to 80-90 days after the onset"

We now write: "As can be seen from the time of the u_{1060} zero crossing at 80-90 days after the onset of SSWs and PWDs, …"

Line 258: I don't see an "under-recovery" following NWDs, the line looks almost overlying the climatological line.

Looking closely one can see that the blue line is somewhat below the blue dashed line. We therefore write: "Afterwards, there is a slight "under-recovery" of the vortex and the FW date of NWDs is about normal."

Line 283: change "negative one" to "negative anomaly"

Done.

Line 284-287: I would delete this here since you talk about the ENSO relationship later on. It's somewhat counterintuitive to talk about the composite ENSO value in plots that are

supposed to be ENSO-neutral by construction. Plus, I'm not sure it's needed here (or the additional text about composite Niño 3.4 index values in the caption of Figure 4). If you do keep it, change "explaining" in line 287 to "explain".
It seems we did not explain this very well. The composites are for ENSO-neutral years only, but still the composite ENSO index is somewhat negative. We think it is important to note this here, but now write hopefully somewhat clearer: "Even though only events from neutral ENSO years are included in these composites, the composite Nino 3.4 index for NWDs during these neutral ENSO years is -0.24 K. This suggests that NWDs are somewhat favored by La Nina-like conditions, which explains the persistent positive SLP anomalies over the North Pacific that start long before onset."

Line 326: "also" is redundant to "In addition" here

We now removed "also".

Line 335: change to "March 2016". You should also mention that this was a final warming (the wind did reverse, but then stayed easterly so that's why it doesn't show up as a SSW).

Done.

Line 341: "warm vortex events" – here it's not clear what is meant by this new terminology. Are you referring to PWDs? SSWs? Both? From the dates provided it seems likely you mean PWDs.

We changed "warm" to "weak".

Figure 6 and discussion on lines 359-379: I realize the many samples here imply the small mean shifts in these distributions are still significant. However, I wonder if you could apply a statistical test to see whether these distributions are actually significantly different. For example a Kolmogorov-Smirnov distribution test. If anything to me Figure 6 actually suggests little difference in the distribution of 0-59 day SLP response after SSWs vs PWDs, even if the mean is slightly shifted. Without some statistical assessment I'm not sure this figure supports your argument of a more robust response for PWDs.

Figure 6 is not meant to support our argument of a more robust for PWDs. In discussing this Figure, we simply say: "Compared to SSWs, PWDs create on average a somewhat stronger mean response (2.0 hPa vs. 1.7 hPa), reduced response spread (3.5 vs. 3.6 hPa), and reduced chance of a negative $\overline{slp}^{PC,0-59}$ (29% vs. 32%)". Thus, we do not claim that the difference between the two distributions is statistically significant. However, from Figure 2b (top), and as also mentioned in the discussion of Figure 2b (section 3.2), one can see that for FZ=12.9 days the 95% confidence intervals of the surface response for SSWs and PWDs are well separated from each other. This indicates that PWDs lead on average to a significantly stronger surface response than SSWs.

Line 432: suggest changing "polar vortex events" to "PWDs", since there appears no deepening of the Aleutian low in the SSW panel (and SSWs are also polar vortex events)

Done.

Line 433: change to “except that the magnitude of the North Pacific SLP anomalies”
Done.

Figure 9 does not seem very necessary, given that you could convey the point of the figure in words, and Figure 5 shows something similar (but is more useful since it compares to SSWs as well).

We agree and remove Figure 9 and its discussion from the new manuscript.