We thank the referees for carefully reading our manuscript again, and for their additional constructive comments, to which we will respond in the following and point out any changes made based on them. Line numbers and figure references in the comments refer to the first version of the revised manuscript. The reviewer's comments are in black italics; our responses are in blue.

Response to Referee 2:

Philip Rupp and Thomas Birner

This is my second review of this manuscript. Thank you to the authors for their efforts in addressing my questions and concernts in the previous round of reviews. With the exception of a few small questions and some typos, I am satisfied by the present manuscipt and recommend publication. Line numbers below refer to the revised manuscript, not to the 'track-changes' version of the manuscipt, which do not agree; the latter seems to be a somewhat more recent version of the text and some of the typos identified below may have already been corrected.

l83: Can you be more explicit about how they disagree? This is not clear from the summaries given here.

The for us most relevant difference in results is that Smy and Scott (2009) observe a decrease in linear growth rate of the life-cycle with increasing polar vortex strength (in terms of a stratospheric PV anomaly) while Wittman et al. (2007) report an increase in growth rate for increasing lower-stratospheric shear (and correspondingly increasing stratospheric wind speeds). We, on the other hand, do not observe any significant dependence of the growth rate on the stratospheric state in our experiments. The results by Wittman et al. and Smy and Scott can potentially be explained by a modification of the tropospheric state and/or near-tropopause conditions (as pointed out in Sections 3.2 and 5 of our revised manuscript, and also in the discussion of Smy and Scott). To further clarify these connections we changes the corresponding part as follows:

"They [Smy and Scott (2009)] found a decrease in growth rates and general wave activity, and a corresponding reduction in magnitude of the induced surface geopotential anomaly of the final state, with increasing strength of the stratospheric PV perturbation. Note that Wittman et al. (2007) reported an increase in growth rate with increasing stratospheric shear (and hence increasing stratospheric wind speed) for low synoptic wave numbers. However, Smy and Scott (2009) also note that some of their results (e.g., regarding sensitivity of growth rates) might be explained by a change in tropospheric horizontal shear due to the non-local effects of the stratospheric PV anomaly and a corresponding fundamental change in the nature of the life […]."

Section 2: The authors' response to my previous comments suggest that their model is specified with an isobaric lower boundary; this is potentially of some quantitative relevance (e.g. Haynes and Shepherd 1989). If this is so it should be stated clearly in the model description.

Haynes, P. H. and T. G. Shepherd (1989) "The importance of surface pressure changes in the response of the atmosphere to zonally-symmetric thermal and mechanical forcing". Q. J. R. Meteorol. Soc. 115, pp. 1181-1208.

We thank the referee for pointing out this subtlety of our model setup, which could indeed lead to confusion. The model we use to conduct the life-cycle experiments uses an isobaric lower boundary condition. However, since the model is formulated entirely in pressure coordinates

it does not include any actual definition of Earth's surface, which is the main reason that we need to approximate the surface response as the response at our lower-most model (pressure-)level, e.g., in terms of a geopotential height anomaly computed by meridional integration of the geostrophic zonal wind equation. We added the following sentence to the model description (Section 2) to clarify the precise lower boundary condition used in our model and comment on the approximation of the surface response in our diagnostics:

"Further note that the pressure coordinate formulation of the model used here lacks an explicit Earth's surface. When considering the surface response (e.g., in Section 3.3) we analyse the lowest pressure layer, thereby effectively approximating the actual surface response, which would require a modified physically consistent lower boundary condition (e.g., Haynes and Shepherd, 1989."

l258, 281: "a simple way to quantify the eddy feedback processes" this claim is made in several places, but is not really expanded upon, and I am not sure exactly what the authors have in mind.

Our claim is simply that the presented framework of idealised baroclinic life-cycles in a dry dynamical model can be used to study the tropospheric eddy feedback to stratospheric conditions, and hence quantify the corresponding processes that are potentially involved in a downward coupling of troposphere and stratosphere. One simple example is the quantitative study of the model energetics. To avoid potential confusion we slightly re-phrased the corresponding statements as follows:

"This jet shift is analogous to the NAM-like signature that has been observed after SSW events, and its appearance as response to stratospheric conditions in the framework of a dry dynamical model further indicates the fundamental importance of tropospheric synoptic-scale eddy feedback in causing the observed negative NAM-signal, as has previously been shown by other studies (e.g., Domeisen et al., 2013; Hitchcock and Simpson, 2014), and allows for a way to quantify these eddy feedback processes (e.g., in terms of EKE and MKE evolution) in a simple and idealised setting."

and

"It further provides a simple model framework to quantify the eddy feedback processes (e.g., in terms of EKE and MKE response) potentially involved in creating the corresponding jet shift signal."

We further corrected the following typos pointed out by the referee:
l370: "stratospheric jet to substantially alter the" -> "stratospheric jet substantially altered the"
l373: This seems to be a note that was left in the text.
l448: extent
l487: life-cycle