Review of egusphere-2024-4088: “A dynamical separation of deep and shallow branches in the stratospheric circulation” by Baikhadzhaev et al.

The study by Baikhadzhaev et al. aims to investigate the stratospheric residual circulation and its wave forcing, focusing on separating the circulation branches. More specifically, they focus on defining the deep and the shallow circulation branch of the residual circulation based on the wave driving and also want to assess changes over decades. These questions are addressed by utilizing reanalysis data, with a focus on ERA5 data along with diagnostics for the residual circulation. They use the TEM framework along with the EP flux divergence to analyze the residual circulation and the wave drag which affects the circulation. They also use the downward control principle to determine the effect of wave dissipation to the residual circulation. A separation between large-scale waves and smaller scale waves is applied to study their contribution to the driving of the lower and the upper part of the stratospheric circulation. Defining upwelling and outflow across the so called turn around latitudes they study the variability and possible trends in the stratospheric circulation as well as they define the level of separation between the two branches. This separation level is found on average at 43 hPa but shows strong seasonal variability, influenced by wave activity and drag. Possible trends in the separation level are found to be not statistically significant, but the level seems to be relatively consistent across different reanalysis data sets. The separation in a deep and shallow branch is proposed based on the wave driving with the deep branch associated with the large scale waves and the shallow branch more related with the smaller scale waves, while the contributions of gravity waves may be underestimated due to the lack of resolution.

The authors aim to improve our understanding of the transport in the stratosphere. This is an important topic since the transport determines the distribution of trace species in the stratosphere which in turn affects the chemistry and radiation in the stratosphere and ultimately can affect processes in the troposphere. Since there is still a lot of uncertainties in how the circulation might change with climate change, further analyses are required in this field and this study can be a valuable contribution and it definitely fits well into Atmospheric Chemistry and Physics. The written language of the manuscript is acceptable. Sometimes the line of thought is hard to follow. The figures are of good quality and support the statements of the text. Section 3 and 4 would profit from some subsections which would separate the various topics better from each other and would increase the readability. In total, I see several points which should be addressed before I would recommend a final publication which from my point of view sum up to major revision. I will lay out my comments in detail below.

Comments

1. One major aspect of the paper is the separation of waves into large scale and smaller scales waves. However, I did not really understand why the authors limit themselves to only two categories separated at between wavenumbers 3 and 4. In particular, I found discussions could have been sharpened when a further separation would be used, either by at least separating the synoptic scale from the meso scale. One could go as far and do really a wave number separation to better assess the contributions of the individual wave numbers (at least for the planetary and synoptic scale) to the wave driving of the stratosphere (like in Fig. A1). In particular, the discussions in section 4 would profit substantially from a further

splitting.

2. Although the theoretical background is provided I am still wondering why the authors decided to include the TEM and the downward control principle in their analysis? This could be presented in more detail. In particular, what is the benefit of using both approaches? Is there a complementary aspect or is it just to see whether both diagnostics will result in the same answer? I also found it often difficult to distinguish whether the authors discuss the TEM or the DWCP results throughout the text. Maybe it would be good to separate the analysis more clearly and bring them together in the discussion (see also point 4).

3. I have some issues with the explanation why waves with wave number 180 are considered in this study (lines 131-134). It is stated that a wave can be resolved by two data points. That is against any viable source which I know related to resolution in atmospheric modeling. To resolve an atmospheric phenomenon in a numerical model usually several grid points are required, the number varies between 4 and 8 (one source related to data from ECMWF would be: https://www.ecmwf.int/sites/default/files/elibrary/2013/17358-effective-spectral-resolution-ecmwf-atmospheric-forecast-models.pdf). Also since the reanalysis data in this study has been coarse grained, I wonder how this translates into the resolvable wavenumber. So I wonder how the definition of resolution used in this study fits to the effective resolution commonly used in atmospheric modeling?

4. Section 3 contains several topics which are discussed. It starts with the residual circulation and the EP flux divergence, goes over the upwelling and outflow at the TAL and adds an analysis based on statistical and mechanistic methods about the vertical contribution of the waves driving the circulation. And finally a discussion using all reanalysis data comes along with Fig. 4. I would highly recommend to split this section into more subsections with meaningful headings to provide more structure and increase the readability. I also think that this will definitely help to present the results in a more obvious way.

   In Fig. 2 around line 194 the difference between the TEM and DWCP derived vertical velocity is discussed and is mainly attributed to the parameterized gravity waves. Maybe I got something wrong here, but the differences in question are about 50 % (0.1 m/s vs 0.15 m/s) which is in my opinion quite large to be attributed to the wave drag from parameterized GW. Can you provide more evidence here that the effect of the parameterized gravity waves is most responsible for the difference?

   The discussion around Fig. 3 starting from line 208 is hard to follow but I think is quite essential since the authors use their findings in Fig 3 to determine the separation level. Maybe it would help if is first of all better motivated why a statistical and a mechanistic approach is used here and if they are discussed first clearly separated from each other in individual paragraphs.

   It is also stated in this section 3 that the separation level for ERA5 is mostly higher than for all other reanalysis. This is attributed to the increased potential of ERA5 to resolve gravity waves. Since ERA5 also has the finest vertical resolution in the stratosphere, can you rule out that this result is not simply an effect of the finer grid spacing?

5. In section 4 two topics are discussed (the upwelling and the trends) and I would also suggest to separate them here more visually by introducing subsections.

   The upwelling is also mainly discussed for ERA5 while the trends are immediately discussed for all 4 reanalysis data sets. To my eyes this looks a bit like an inconsistency and the authors do not really explain themselves why they sometimes use all reanalysis data sets and why they sometimes only focus only on ERA5 within a section. Generally, I would appreciate it if all results would be discussed for all reanalysis data sets in the manuscript consistently.

6. Based on my comment 4 about the impact of parameterized GW drag, in section 5, line 404ff the C-like shape of the upwelling deficit is again related to the parameterized GWs. Can the authors support their claim here with additional reasoning or data?

7. I find the comparison shown in Figure A5 quite interesting, in particular the effect of 1h to 6h. The 6h data seems to include spurious effects in w*, which are more prominent in the daily but which are even seen in the monthly mean data. Do the authors know the source of these patterns? Can they have a lasting impact on the analysis?

Technical comments

- Line 50: Do you mean: “stratospheric circulation is expected to accelerate” ?
- Section 2.1: The differences in horizontal resolutions are addressed but not in the vertical. Can this be included? In particular, is the data used on model or pressure levels? What are the differences in vertical grid spacing between the various reanalysis data sets in the stratosphere?
- Line 150: (e.g. (Abalos et al., 2015)) → (e.g., Abalos et al., 2015)
- Line 163: condition(Vallis, 2006) → condition (Vallis, 2006)
- Line 189: vbarstar
- Line 193: DWCP Eq. 5a → DWCP (see Eq. 5a)
- Line 210: negtive → negative
- Line 283: banches → branches
- Fig A2a: TEM upwelling betwen TAL → TEM upwelling between TAL