We thank the reviewer for the thorough reading of the manuscript and the insightful comments, which clearly helped to improve the manuscript. We addressed all points in the revised version as described below. Reviewer comments are in normal font, answers in italics. A summary of the main changes in the revised version is given in the bullet point list here (with more details given in the replies to the specific comments below).

- The title of the manuscript has been revised to more accurately reflect its content, including the requested modifications.
- In response to Reviewer 2's suggestion, we refined the scale-based separation to distinguish between planetary, synoptic-scale, and mesoscale waves. The separation is now presented in sect. 2.2, and discussed with the new Fig. 3 in sect. 3.
- A decomposition into more physical atmospheric wave phenomena was requested by the reviewer 1. In response, the relation between wave scales and physical wave types is discussed at the end of sect. 2.2: planetary waves are associated with quasi-stationary Rossby waves, synoptic-scale waves represent a mixture between Rossby and gravity waves, and mesoscale waves are associated with gravity waves.
- As requested by Reviewer 3, we conducted additional analysis of upwelling trends to examine whether separating the ozone recovery period from the post-ozone recovery period improves the statistical significance of the trends.
- More information from reanalyses other than ERA5 was added to the sect. 3 and 4 of the manuscript as requested by Reviewer 2
- As requested by Reviewer 2 and 3 rewriting and restructuring of several text parts throughout the entire manuscript was done to improve clarity and readability eg. sect. 2.2, sect. 3 and sect. 4.
- The comparison of resampled ERA5 data with original fine resolution ERA5 data was expanded (see Sect. 5), as requested by reviewer 2.

General comment

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.

We thank the reviewer for the overall positive evaluation of the manuscript. We see the criticism that the text needs improvement. Therefore, we carefully worked on improving the entire manuscript text and worked on the wording as well as on the structure, in order to enhance the clarity of the presentation. Also, we separated Sections 3 and 4 into further subsections as suggested.

Detailed 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.

We thank the reviewer for the good suggestion to present the individual wave contributions more clearly to motivate the separation between wave numbers 3 and 4. We had analysed the individual wave contributions already before but had tried to condense the presented information. We see now that this likely causes more confusion and that it would be helpful to present more details. Hence, at the end of Sect. 2.2 we split atmospheric waves into planetary, synoptic, and mesoscale based on their scale. Upwelling and outflow from these wave types is presented in the new Fig. 3, a more detailed discussion of the wave types contribution to the circulation is now given in Sect. 3.

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).

We understand that the description of the method was not clear enough and that in particular the usage of different diagnostics was not motivated enough. We thank the reviewer for pointing to that. The DWCP is based on the TEM framework, and is used to relate residual circulation velocities to wave drag. On the one hand, the direct TEM approach is conceptually simpler and more frequently used in studies of the stratospheric circulation. Each estimate is subject to distinct uncertainties and limitations, and their comparison provides information on the robustness of our results. We tried to clarify the text throughout the paper, and inserted a few motivating sentences at the beginning of the Sect. 2.2.

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?

We thank the reviewer for pointing to that potential for misunderstanding. Of course, the maximum zonal wavenumber of 180 for our downsampled ERA5 data will be somewhat different from the effective resolution of ERA5. The original model data has a spectral resolution of TL639 (Hersbach et al., 2020). However, the effective resolution for atmospheric waves will be somewhat worse (e.g., Skamarock, 2004). In order to check whether our downsampled ERA5 data loses a significant portion of the waves resolved in ERA5, we have examined the effect of resolution change from fine resolution ERA5 data with 1200 points to coarser resolution with 360 points along 360 degrees of longitude. This sensitivity calculation has been performed for the full year 2010. We found that resampling led to a 20 % reduction in DWCP upwelling driven by waves 21–180 near the tropopause. However, the total upwelling in this region decreased by only 1.5 %. Additionally, we observed an approximately 1.3 % reduction in total outflow near the separation level. Hence we find that the effect of resampling is notable, yet acceptable for this study. The analysis of the effects of resolution changes on calculated upwelling and outflow is discussed in the discussion Sect. 5.

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?

Following advice from the reviewer the section 3 was split into three parts: 1) General structure of the stratospheric circulation 2) Separation of the deep and shallow branches by wave forcing based on climatological data, 3) Level of the separation between shallow and deep branches.

Indeed, only about 2/3 of the total TEM upwelling at 70hPa in Fig. 2 are reconstructed with the DWCP method. This number seems high, but is in good agreement with other studies based on other models. For instance Butchart et al (2011) found also that about 1/3 of the upwelling across 70hPa in climate models is related to parameterized waves (their Fig. 10).

Regarding Fig. 3, we acknowledge that the description of Fig. 3 was not sufficiently clear and thank the reviewer for pointing this out. To improve clarity and ensure a more logical flow, panels a and b have been swapped with panels b and c, and the corresponding figure description has been restructured accordingly.

Vertical resolution and the model's ability to resolve gravity waves are highly intertwined eg. Watanabe et al. (2015), where it was found that gravity wave momentum flux is decreasing with finer vertical resolution.

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.

Following the reviewer's suggestion, section 4 was split into two subsections: 1) Variability of the circulation, 2) Trends. Additionally, more results from reanalyses other than ERA5 were incorporated into Sections 3 and 4, and Table 2 was expanded to include corresponding data from these additional reanalyses.

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?

We thank the reviewer for this question. As noted in our response to Comment 4, an upwelling deficit of about 30 % at 70 hPa due to the absence of parameterized wave drag is not uncommon and is consistent with previous studies. The 'C'-shaped structure of the upwelling deficit can be interpreted as follows: the enhanced deficit at upper levels likely reflects the dominance of gravity waves, which account for nearly all wave forcing in the mesosphere. The increase of deficit in the lower stratosphere is likely related to the fact that gravity wave drag contributes significantly to the shallow branch of the Brewer–Dobson circulation eg. Eichinger et al. (2020); Diallo et al. (2019). This is briefly discussed now in Sect. 5.

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?

We thank the reviewer for the question. As noted in our response to comment 3, we conducted an additional comparison between the fine-resolution and coarsened ERA5 data and found that the impact of resampling on the analysis is limited. This conclusion is further supported by the minimal effect of resampling on the Eliassen-Palm flux divergence, as shown in Fig. 1. This new figure shows that the patterns in monthly averaged wave drag are not changing significantly between the fine and coarse resolution data, for the total wave drag as well as for the contributions from planetary and smaller-scale waves. The effects of resampling are discussed in more detail now in Sect. 5.

Technical comments

- Line 50: Do you mean: "stratospheric circulation is expected to accelerate" ?

  Yes, the text was corrected.

- 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?

  Information about vertical resolution of the reanalyses is now included in sect. 2.1.

- 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

  We thank the reviewer for the corrections, the text was corrected according to the technical comments.

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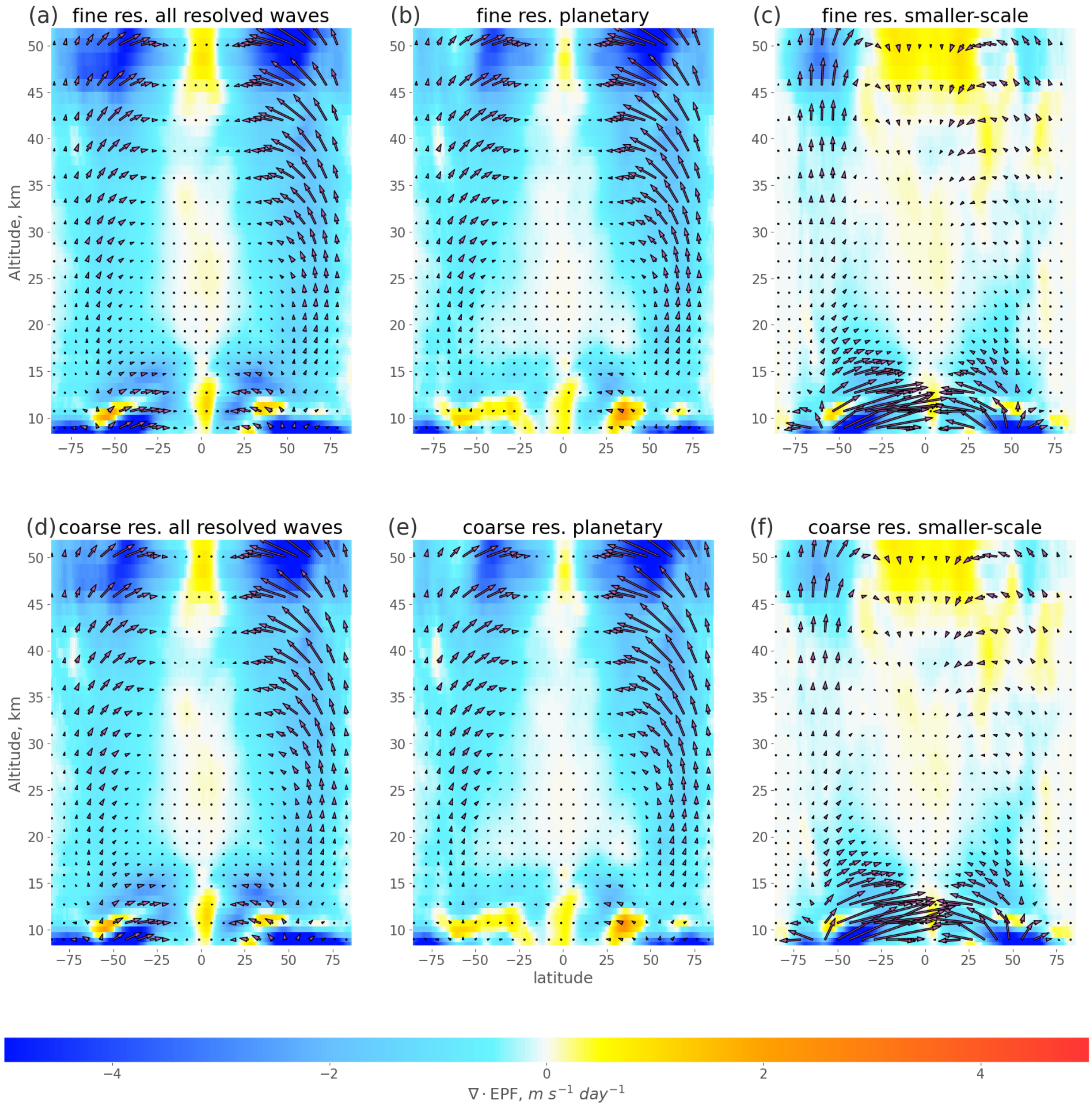

Figure 1: Eliassen–Palm flux divergence (color coded), together with Eliassen–Palm flux vectors from ERA5 reanalysis for the year 2010. The distributions are shown for the contribution from all resolved waves (left), from planetary waves with wavenumbers 1–3 (middle), and from smaller-scale waves (right).