

Weather Clim. Dynam. Discuss., referee comment RC2
<https://doi.org/10.5194/wcd-2021-16-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on wcd-2021-16

Isabell Krisch (Referee)

Referee comment on "Dynamical and surface impacts of the January 2021 sudden stratospheric warming in novel Aeolus wind observations, MLS and ERA5" by Corwin J. Wright et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-16-RC2>, 2021

The provided manuscript of Wright et al. on "Dynamical and Surface Impacts of the January 2021 Sudden Stratospheric Warming in Novel Aeolus Wind Observations, MLS and ERA5" is a very extensive analysis which is overall well written. It contains a very detailed overview over the most recent SSW including all necessary details on the formation and impacts. In contrast to many other studies, Wright et al. focus on the use of measurement data and mostly only use model (reanalysis) data to complement and support the conclusions. The newly available wind data from the Aeolus satellite provide new possibilities for the analysis of SSWs which is nicely demonstrated by this paper.

However, there are three major points which I would propose to improve before publication:

- The two different objectives of this paper make it sometimes difficult to read. The authors should therefore revise the manuscript and think about possible ways of restructuring to make it easier to read.
- The use of measurement data where possible is a large asset of this paper. However, this is not fully consistent throughout the paper. At many locations I would recommend to use MLS data (GPH and/or geostrophic wind) instead of ERA5 reanalysis data. More details on this are given below.
- The calculation of the Aeolus projected zonal and meridional winds (which is in detail described in the appendix) should be revisited. More details can be found in the comments to the appendix.

More detailed comments are given below.

A paper on SSWs should probably include a reference to the most recent review on this topic, Baldwin et al. 2020 (e.g. to be added in line 35).

L1-4: Major SSWs lead to a reversal of the wind. For SSWs in general (minor + major) the given definition is correct. The rise happens over just a few days, but has to remain reversed for a longer period. Maybe revise this sentence.

L5-7: Sentence itself is correct, but might be misleading as multiple other missions measuring temperature also provide wind products (even though not direct). This point is nicely explained in the introduction, but should also become clear in the abstract.

L9-10: As you notice correctly in the introduction, already in Jan 2019 a major SSW was observed by Aeolus. Additionally, in Sep 2019 a minor SSW was observed over Antarctica. The data from Jan 2019 is reliable, but not yet available for scientific analysis due to large systematic biases. These biases need to be removed by reprocessing before the data can be used for scientific analysis. The reprocessing is planned for end-2021.

L17-18: Does this paper really show 3)?

L69: Maybe include Stoffelen et al 2020 here.

L73/74: Are there no newer publications on ALADIN? 1989 is 30 years before launch!

L76: vertical resolution is ~0.5-2km (0.5km is rare nowadays)

L77: The hot pixel correction is described in Weiler et al 2020

L78: Rennie and Isaksen 2020 could be updated to Michael Rennie, Lars Isaksen, Fabian Weiler, Jos de Kloe, Thomas Kanitz, Oliver Reitebuch: The impact of Aeolus wind retrievals in ECMWF global weather forecasts, QJRMS, 2021 (if this becomes available in time; the revised version was submitted recently and publication is expected soon).

L90-94: As Aeolus data is one of the major components of this paper, I would propose to explain the vertical sampling of Aeolus a bit more in detail. First, the vertical sampling changes in height (maybe even include typical altitude profiles), then in location (the range bin settings are terrain following and change e.g. at 60°N), and last but not least

also in time (however, this should not be the case throughout your study period). Especially the change at 60°N is worthwhile mentioning as this impacts all your estimates (means). Thus, I recommend plotting two sampling profiles at >60°N and <60°N to explain this change.

L69-94: The following information is missing here and should be added for clarity: Aeolus horizontal resolution, which data (baseline) was used, and which quality filtering has been applied.

L102: Here you give precision, whereas for Aeolus you state the systematic bias (accuracy). As your analysis uses mean values, the precision of MLS is probably not representative for your data and the accuracy should be given instead.

L130: Is MLS assimilated in ERA5/OPAI?

L145-151: This approach is a good idea!

Figure 1 and others: Are you sure the differences at 22km are not due to the Aeolus sampling (only data available >60°N -> data plotted is for 62.5°N, not for 60°N). You should verify that the averaged latitude is consistent for the different datasets.

Figure 1: Interesting, that just before the SSW onset (day 0), Aeolus shows weaker winds than ERA5 at 22km whereas stronger winds at 15km.

L162: many previous studies (even though they are by Manney et al.)

L170/171: The paper is long enough as is, thus, I understand the authors here. Still for such a new dataset as Aeolus closer investigations of the differences at geographical scale could probably raise the confidence in the projected meridional winds.

L173/174: Please see comment above.

L173-177: From my point of view, this paragraph is not really necessary, wrong (reliability of Aeolus data) and could be removed completely.

Figure 2: To be consistent with Figure 1: Why not use similar or even the same altitude levels? Especially, why not show 10hPa (32km) as this altitude is used for the main definition of SSWs?

L187/188: Maybe citation of a 2017 paper not correct for 2 events out of which one is from 2019?

L214: mission-to-date day-of-year median: Is this for the time period 2004-2021?

L220: Also validation campaigns do not show a strong altitude dependence of the bias and reasonable values down to 2km (e.g. Witschas et al 2020, Lux et al 2020)

Figure 4: Again, I am not sure if an average around 60°N is recommendable for Aeolus data above ~17km as this might actually be the average at 62.5km. This sampling bias should be investigated and discussed in the paper. For validation of Aeolus winds, it would be good to include MLS winds in this plot as well.

Figure 3 & 4: I would somehow propose to combine these two Figures or put them close too each other as otherwise the reader is constantly going back and forth.

Figure 5: Before only measurement data (MLS & Aeolus) was used. Why introducing here ERA5 if MLS GPH data is available? What is the additional information or do MLS and ERA5 differ significantly?

L323/324: Citing from Baldwin et al. 2020 (link provided above): "About a third of the observed 36 major SSWs in the 1958–2012 period can be unambiguously classified across all methods as splits and another third as displacements (Gerber et al., 2021). The rest of the events are more ambiguous across methods, perhaps because in some cases, the polar vortex both displaces and splits within a period of several days (Rao et al., 2019)." Thus, the 2021 event just seems to fall in the third category.

L330-332: I am not sure, I fully understand this sentence. Could you maybe rephrase it?

Figure 6: For analysis of SSW, I would include 10hPa. This should be possible by using MLS meridional winds. Also, for lower altitudes, it might be valuable to compare data using only MLS (u, v, T), MLS T + Aeolus (u, v), MLS (T, v) + Aeolus u, and reanalysis and hybrid as plotted. For a consistent use of the combination of Aeolus winds with an external v (ERA5 or MLS), the external v should already be used when converting HLOS to u.

Maybe also think about using some kind of "apriori" u and v values for the conversion of HLOS to v and u, respectively. For this topic, please also see my comments to the appendix.

L363: Are the mentioned sampling pattern issues by any chance related to the use of different numbers of ascending and descending orbits? For details see my comments to the appendix.

L374: If this is a real difference should be easy to check by using an apriori u from either MLS or ERA5 when deriving v from HLOS. See comments to appendix.

L377: "note the different vertical scales" can be mentioned here, but definitely belongs to the figure caption.

L425-427: When averaging over Aeolus data you should definitely pay attention that ascending and descending orbits contribute evenly as then the errors due to u (or v) better cancel out. See comments to the appendix.

L440: Why again using ERA5 instead of MLS?

L464/465: Is there already "typical" Aeolus winter data?

Figure 9: Again, why not use MLS? Does MLS give significant different results or is the data too coarse?

Sections 7.1 and 7.2 are somewhat repetitive. Maybe they could be joined and all Figures could be discussed simultaneously?

Line 532: units not consistent between text and figure.

Line 539-540: Can MLS winds provide this missing information?

L 563: 85°N

L639: Referring back to my citation of Baldwin et al 2020: 1/3 of all SSWs are not easily classifiable. Thus, better remove "unlike many others".

L644: For clarity add 60°N^* .

L670: "timing" double

L670: Is there a way to assess if this is really a timing effect or an offset effect?

Appendix:

General comment: This appendix is very interesting and important to interpret the Aeolus data quality. However, I would propose to include the different orbit geometry for ascending and descending orbits as described in the following. Regarding the formulas, I am not 100% sure what the authors have done in equations (A2) & (A3). To my understanding, converting HLOS to u_{HLOS} (and neglecting the v_{HLOS} contribution) should lead to $\text{HLOS}/\sin\theta$ (not $\text{HLOS} * \sin\theta$, as given in A2). Same for v_{HLOS} in A3, btw. If I now follow these corrected equations, I get to a zonal wind error ($u_{\text{HLOS}} - u$) of $v/\tan\theta$ (or $-v/\tan\theta$ depending on the definition of θ). This has opposite sign (for same/similar v) for ascending and descending orbits. Thus, averaging u_{HLOS} over the same amount of ascending and descending orbits should remove this error almost completely, but not if one considers only ascending or only descending orbits.

In a similar way, one should be able to construct a more or less reliable meridional wind by first averaging over HLOS for both ascending and descending orbits ($u * \sin\theta$ part cancels out) and then dividing by $\cos\theta$.

Additionally, two minor comments to this appendix:

L704: Why do you use January 2020 data even though your paper mainly treats January 2021 data?

Figure A2: Which binning is used here?

References:

Baldwin, M. P., Ayarzagüena, B., Birner, T., Butchart, N., Butler, A. H., Charlton-Perez, A. J., et al. (2021). Sudden stratospheric warmings. *Reviews of Geophysics*, 59, e2020RG000708. <https://doi.org/10.1029/2020RG000708>

Stoffelen, A., Benedetti, A., Borde, R., Dabas, A., Flamant, P., Forsythe, M., Hardesty, M., Isaksen, L., Källén, E., Körnich, H., Lee, T., Reitebuch, O., Rennie, M., Riishøjgaard, L., Schyberg, H., Straume, A. G., & Vaughan, M. (2020). Wind Profile Satellite Observation Requirements and Capabilities, *Bulletin of the American Meteorological Society*, 101(11), E2005-E2021. Retrieved May 4, 2021, from <https://journals.ametsoc.org/view/journals/bams/101/11/bamsD180202.xml>

Weiler, F., Kanitz, T., Wernham, D., Rennie, M., Huber, D., Schillinger, M., Saint-Pe, O., Bell, R., Parrinello, T., and Reitebuch, O.: Characterization of dark current signal measurements of the ACCDs used on-board the Aeolus satellite, *Atmos. Meas. Tech. Discuss.* [preprint], <https://doi.org/10.5194/amt-2020-458>, in review, 2020.

Witschas, B., Lemmerz, C., Geiß, A., Lux, O., Marksteiner, U., Rahm, S., Reitebuch, O., and Weiler, F.: First validation of Aeolus wind observations by airborne Doppler wind lidar measurements, *Atmos. Meas. Tech.*, 13, 2381–2396, <https://doi.org/10.5194/amt-13-2381-2020>, 2020.

Lux, O., Lemmerz, C., Weiler, F., Marksteiner, U., Witschas, B., Rahm, S., Geiß, A., and Reitebuch, O.: Intercomparison of wind observations from the European Space Agency's Aeolus satellite mission and the ALADIN Airborne Demonstrator, *Atmos. Meas. Tech.*, 13, 2075–2097, <https://doi.org/10.5194/amt-13-2075-2020>, 2020.