

# Replies to reviews for: Large ensemble assessment of the Arctic stratospheric polar vortex

WCD, Ales Kuchar et al.

## 1 Reply to reviews

Dear Daniela Domeisen,

thank you very much for helping in the publishing process of this paper. We appreciate the helpful comments of the two  
5 anonymous reviewers and will revise the manuscript accordingly, such that we hope it can then be published in WCD. Please  
find our point-by-point answers to their comments below.

Best wishes

Ales, Roland, Maurice and Christoph

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## 2 Ref #1

The authors present a mostly statistical assessment of CMIP model performance in relation to the geometry of the perturbed polar vortex, plus the frequency of split or displacement SSWs. Their main tool of analysis are rank histograms created with models from the Multi-Model Large Ensemble Archive against ERA5 data.

15 I find the manuscript well written and the structure has a logical flow. I have only minor comments which I hope will help clarify some of the discussion.

Dear anonymous reviewer #1,  
thank you very much for your valuable comments. Please find our point-by-point answers (in blue) to your comments below.  
20 Furthermore, Figs. S10 and S11 will be merged and moved from the supplement as new Fig. 9 to the main text. The regression results therein changed slightly because we now include 50 instead of 30 ensemble members for MPI-ESM-LR (Olonscheck et al., 2023) and we rearrange the code to be fully compatible with Hall et al. (2021).

Detailed comments:

25

- Abstract: I would suggest expanding the second last paragraph to clarify what is meant. I did not understand this until after reading the manuscript. Then, I would suggest to remove the last sentence of the Abstract, or at least clarify: It is somewhat misleading as no attempt at improving the representation of SPV dynamics is made, and there is not enough discussion of what could be the cause for the observed biases.

30 To improve clarity of the abstract, we refine the second last paragraph. We also modif the last sentence to be more compatible with our manuscript. The part then will reads: *A good performance in representing the geometric-based diagnostics in rank histograms does not necessarily imply reliability and therefore a good performance in simulating displacements and splits. Assessing the model biases and their representation of SPV dynamics is needed to improve credibility of climate model projections, for example by giving stronger weightings to better performing models.*

35

- Introduction/Methods: It is argued that one has to use large ensembles to assess wintertime NH variability - which is fine. However, ERA5 is assumed the truth, even though it only represents one ensemble member (the one we call “reality”). I am not suggesting that’s a problem, but I would welcome a short discussion on why evaluating large ensembles to one single realisation still makes sense.

40 We acknowledge your concern regarding the use of ERA5 as the reference dataset, representing a single ensemble member. While ERA5 is widely accepted as a reliable benchmark, we understand your interest in the rationale behind evaluating large ensembles against a single realization.

In response, we emphasize that ERA5 is designated as a state-of-the-art benchmark regarding its constraint by observations and high horizontal and vertical resolution compared to other reanalyses (Hoffmann and Spang, 2022), not an 45 absolute truth. In the future work our results should be supported using multiple reanalysis datasets and analysing different time period to assess stationarity of model performance. However, our study aims to explore the period 1979–2014 covering years when as many observations as possible were assimilated in ERA5 including satellite observations (Hersbach et al., 2020). We recognize the importance of discussing why evaluating large ensembles against a single realization is meaningful, and we will incorporate a concise discussion on this aspect in our revised manuscript.

50 – It is not clear - neither from Methods nor Code availability - whether the actual code from Seviour et al. (2013) has been used to compute the aspect ratio and centroid lat/lon. Please clarify.

The actual code from Seviour et al. (2013) has been modified accordingly as seen in the updated version of Kuchar and Öhlert (2024). We will mention this explicitly in the revised manuscript.

55 – Section 3: It would be helpful to provide a sense of how large the biases are. If one is not accustomed to look at rank histograms, it is difficult to judge whether the the shown biases are small or large - that is, whether they are concerning or within acceptable limits. For instance, what does it mean if in Fig. 1c one of the bars peaks around the number 200

but the adjacent bars are below that value? The authors say on line 147 “such strong biases” but I don’t know if I should consider them strong for the work I would like to do with those models.

To support the interpretation of rank histograms, we provide bias, spread and the  $\chi^2$  statistics, which quantifies how close the rank histogram is to an ideal uniform distribution, for each model and variable above each rank histogram. We will add their description in new Fig. 3 and summarize models’ performance in new Tab. S1. Furthermore, we will state in the method section that these statistics should serve in relation to the other models instead of defining any threshold for a good or bad model. Consequently, we go through the manuscript and try to avoid bold statements such as “such strong biases”.

- I know it’s not the focus of this paper, but some interpretation of results would be great. For instance, models seem to have similar biases at all three levels in centroid latitude, but they have different biases in aspect ratio at different levels. Is there something we can induce or learn from that?

We will provide several discussion instances throughout the manuscript, particularly in Section 5. For instance, l220-225 describe a connection between lower stratospheric winds and SPV split frequency in the CMIP6 models was already noted by Hall et al. (2021) and Wu and Reichler (2020). This could explain larger biases at lower altitudes. However, this explanation is limited to particular models and a more detailed investigation of particular biases is needed. Furthermore, we state that using the rank histograms of aspect ratio at 50 and 100 hPa (see supplementary Figs. S3, S8), the models can be separated into two groups according to their behavior in relation to the results at 10 hPa. One group of models shows larger biases at lower altitudes (CanESM2, INM-CM5-0 and UKESM1-0-LL). In the other model ensembles the bias is weaker at lower altitudes (CanESM5, CESM2, INM-CM5-0, IPSL-CM6A-LR, MIROC6 and MPI-ESMs). We speculate that this is connected with tuning parameters of GW parameterizations as shown in Sigmond et al. (2023).

### 3 Ref #2

#### 80 GENERAL COMMENTS

The manuscript assess the representation of the geometry of the winter SPV in a few model ensembles, and compares the model ensembles’ SSW and SPV frequency to the observed. To pursue this analysis, geopotential height data over different stratospheric levels and at daily frequency is taken from the historical simulations of climate models participating in CMIP5 and CMIP6. Based on their results, the authors filter the models that are best at representing the SPV spatial distribution and SSW frequency, and conclude that a good calibration of the SPV shape does not necessarily correspond with an adequate representation of split and displacement SSWs. Outlook on the possible reasons underlying inter-model differences is also provided. Although the topic is of interest for the community and the results might be useful for the institutions developing the different models, I find that the present manuscript is of poor quality. This is because (i) the text is fragmented and difficult to follow, (ii) the language is often imprecise, (iii) the methods are not clearly presented, (iv) the many differences across models

90 and resolutions are difficult to grasp. The whole manuscript needs to be generally and carefully revised before being fit for publication in WCD. In the following, some suggestions that should help improve the afore-mentioned issues are included.

95 Dear anonymous reviewer #2,  
thank you very much for your valuable comments. Please find our point-by-point answers (in blue) to your comments below.  
Additionally we will go through the entire manuscript to improve presentation quality, make it easier to follow and clarify points that were not adequately described. Furthermore, Figs. S10 and S11 will be merged and moved from the supplement as new Fig. 9. The regression results therein will change slightly because we now include 50 instead of 30 ensemble members for MPI-ESM-LR (Olonscheck et al., 2023) and we rearrange the code to be fully compatible with Hall et al. (2021).

100 100 SPECIFIC COMMENTS

1. To improve the text please work on fluency and on the precision of the wording. Often adjacent sentences repeat the same concepts, or are fragmented, or badly connected. For example, see some of the points in the TECHNICAL CORRECTIONS.

105 We will implement the technical corrections and will work on fluency and on the precision of the wording in the whole manuscript.

2. The methods need to be expanded and/or clarified. Specifically, the meaning of the geometric diagnostics can be better explained, the methods behind the rank histogram (RH) and ROC/AUC calculations are not clear.

110 110 We will provide qualitative descriptions of the moment diagnostics and will add detailed descriptions in the supplement. The methods behind RH and ROC/AUC will be clarified too.

115 3. It would be useful to compare the list of ERA5 SSW split and displacement events determined with geometric methods (i.e., those identified here) to those identified with more traditional methods (lists of events and their types are available online or in literature). Additionally, please provide the magnitude of the mean SSW frequency for each model, together with the AUC.

120 Frequency of ERA5 SSW split and displacement events determined with geometric methods is within uncertainty of other methods (~6.94 events per decade, ~6.66 events per decade including displacement and splits events only, displacement/split ratio equals to 1.4). We provide the list of ERA5 SSW split and displacement events in Tab.R1 below to document this agreement.

We will also show and discuss the mean displacement and split frequency for each model in the y-axes of new Fig. 9 (formerly in the supplement) according to Hall et al. (2021). However, this metric also includes the zonal wind component as other more traditional methods, it is therefore not fully comparable with our AUC plots using only the geopotential height and no

**Table R1.** List of SSWs in ERA5

	onset dates	vortex type
0	1979-02-22	split
1	1980-02-29	displaced
2	1981-03-04	displaced
3	1981-12-04	unclassified
4	1984-02-23	displaced
5	1985-01-01	split
6	1987-01-23	displaced
7	1987-12-08	split
8	1988-03-14	split
9	1989-02-21	split
10	1998-12-15	displaced
11	1999-02-25	split
12	2000-03-20	displaced
13	2001-02-11	split
14	2001-12-30	displaced
15	2002-02-17	displaced
16	2003-01-18	split
17	2004-01-05	displaced
18	2006-01-21	displaced
19	2007-02-24	displaced
20	2008-02-22	displaced
21	2009-01-24	split
22	2010-02-09	displaced
23	2010-03-24	displaced
24	2013-01-06	split

constraints on event separation. Nevertheless, we will include AUC in Tab. R2 as suggested below.

125

4. The Discussion and Outlook section is dispersive, and should be organised more clearly, without mixing your results (discussion and limitations) with the outlooks. Probably, some paragraphs in the Results section would fit better in the Discussion section (e.g., see TECHNICAL CORRECTIONS). Finally, it would be useful to add a table summarising the performances/bases of each model, by geometric diagnosis and by type of SSW event.

130

We will go through the section and will improve its clarity, apply the TECHNICAL CORRECTIONS suggested below and also remove some parts. For several reasons, we refrain from doing a certain suggested restructuring, please see below. We will input the suggested table (see Tab. R2) summarising the models' performance in the supplement.

Table R2: Summary table including metrics: bias, spread and AOC. AOC related to displacement and split events is in the table associated with centroid latitude and aspect ratio, respectively.

		100 hPa			50 hPa			10 hPa		
		bias	spread	AUC	bias	spread	AUC	bias	spread	AUC
CanESM2	aspect ratio	590.99	213.27		210.74	54.06		148.61	7.10	0.55
	objective area	343.13	122.48		15.73	77.34		197.27	128.91	
	kurtosis	11.78	226.71		591.70	240.18		5525.35	3790.66	
	centroid latitude	418.52	79.54		336.71	53.66		781.14	47.83	0.66
	centroid longitude	118.84	66.86		3.13	141.43		224.01	13.95	
CanESM5	aspect ratio	184.78	48.57		266.95	59.42		772.06	140.74	0.45
	objective area	894.65	1.51		1520.33	23.75		425.06	11.87	
	kurtosis	118.64	411.00		16.80	353.75		4577.71	4368.73	
	centroid latitude	290.00	53.74		276.07	65.13		19.30	6.20	0.59
	centroid longitude	44.20	47.26		1.48	15.65		120.33	224.55	
CESM2	aspect ratio	104.88	10.34		271.81	32.92		576.90	127.07	0.59
	objective area	334.06	91.54		476.74	166.81		1587.79	344.51	
	kurtosis	6.82	217.62		61.09	378.66		2239.48	2203.39	
	centroid latitude	252.67	88.90		366.22	115.06		233.96	21.51	0.61
	centroid longitude	26.31	50.33		0.01	33.15		67.85	56.46	
CNRM-CM6-1	aspect ratio	6.74	0.72		14.67	0.03		108.77	0.28	0.64
	objective area	3.00	5.46		51.57	14.76		847.55	78.80	
	kurtosis	227.27	10.95		866.92	176.20		4018.98	2329.24	
	centroid latitude	0.06	2.24		22.69	0.35		15.46	8.79	0.71
	centroid longitude	0.39	4.15		15.80	2.72		0.90	2.94	
INM-CM5-0	aspect ratio	257.00	69.24		119.68	51.19		0.65	1.80	0.53
	objective area	26.65	71.31		71.70	80.82		165.95	436.67	
	kurtosis	52.26	221.07		114.18	133.80		3.01	25.45	
	centroid latitude	304.27	182.85		391.94	148.44		90.28	7.54	0.67

Continued on next page

Table R2: Summary table including metrics: bias, spread and AOC. AOC related to displacement and split events is in the table associated with centroid latitude and aspect ratio, respectively.

		100 hPa			50 hPa			10 hPa	
		bias	spread	AUC	bias	spread	AUC	bias	spread
	centroid longitude	55.00	53.04	0.03	75.01		0.52	7.36	
IPSL-CM6A-LR	aspect ratio	41.73	4.27	115.30	15.54		450.04	2.55	0.63
	objective area	507.80	75.51	137.37	71.86		570.49	730.88	
	kurtosis	1.26	94.65	10.65	132.02		45.86	69.42	
	centroid latitude	44.41	18.49	64.22	46.14		156.46	7.66	0.63
	centroid longitude	77.08	41.45	3.29	403.83		289.33	6.17	
MIROC6	aspect ratio	8.38	7.86	6.06	13.84		19.07	6.55	0.59
	objective area	841.65	420.28	900.00	790.83		624.89	1015.18	
	kurtosis	62.65	0.38	19.48	15.54		40.58	19.20	
	centroid latitude	0.16	19.33	97.24	43.67		332.99	19.25	0.62
	centroid longitude	101.61	9.90	26.33	199.91		209.12	4.14	
UKESM1-0-LL	aspect ratio	99.95	19.77	91.16	17.09		0.33	0.27	0.58
	objective area	0.15	127.42	161.29	91.84		67.10	14.83	
	kurtosis	54.18	107.00	17.97	51.80		421.63	99.53	
	centroid latitude	1.94	0.19	18.81	1.03		16.88	27.50	0.66
	centroid longitude	0.33	5.42	1.19	3.25		156.71	29.47	
MPI-ESM1-2-HR	aspect ratio	1.81	2.84	11.33	1.06		116.15	0.01	0.56
	objective area	535.27	72.35	484.98	196.42		1046.07	824.85	
	kurtosis	3.57	8.82	27.34	78.91		453.43	40.79	
	centroid latitude	151.91	10.35	89.40	7.42		123.33	0.91	0.59
	centroid longitude	16.50	9.09	1.26	13.32		4.53	80.94	
MPI-ESM1-2-LR	aspect ratio	3.77	0.56	11.93	0.55		242.95	7.78	0.65
	objective area	1030.16	372.84	776.32	568.10		1118.40	1266.02	
	kurtosis	1.91	13.73	0.04	71.31		324.23	102.14	
	centroid latitude	298.72	32.05	154.32	12.51		199.48	21.33	0.66
	centroid longitude	0.65	57.89	26.56	2.04		39.20	214.11	
GFDL-CM3	aspect ratio	271.25	116.71						

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Table R2: Summary table including metrics: bias, spread and AOC. AOC related to displacement and split events is in the table associated with centroid latitude and aspect ratio, respectively.

	100 hPa			50 hPa			10 hPa		
	bias	spread	AUC	bias	spread	AUC	bias	spread	AUC
objective area	6615.54	2364.37							
kurtosis		42.21	423.25						
centroid latitude		471.85	282.04						
centroid longitude		69.90	52.72						

135 5. How does the map of the mean gh10 climatological bias look like for each model? Is it relatable to the more specific rank histogram results? If so, such a figure could be included at the beginning of the Results section.

We show the geopotential height climatology at 10hPa (gh10) of all analyzed model ensembles and ERA5 for the period 1979–2014 in Fig. R1. It shows that there are models which apparently struggle in simulating gh10 comparable to ERA5 (CanESMs, CESM2). On the other hand, visual resemblance between ERA5 and UKESM1-0-LL corresponds to our indications as best-performing model. However, details about reliability of recent large-ensemble model simulations in representing the SPV and its spatial variability cannot be fully decomposed based on this figure. Thank you for the comment, we will include the figure to the manuscript in the Method section and use it to motivate the moment diagnostics for analysis of spatial variability.

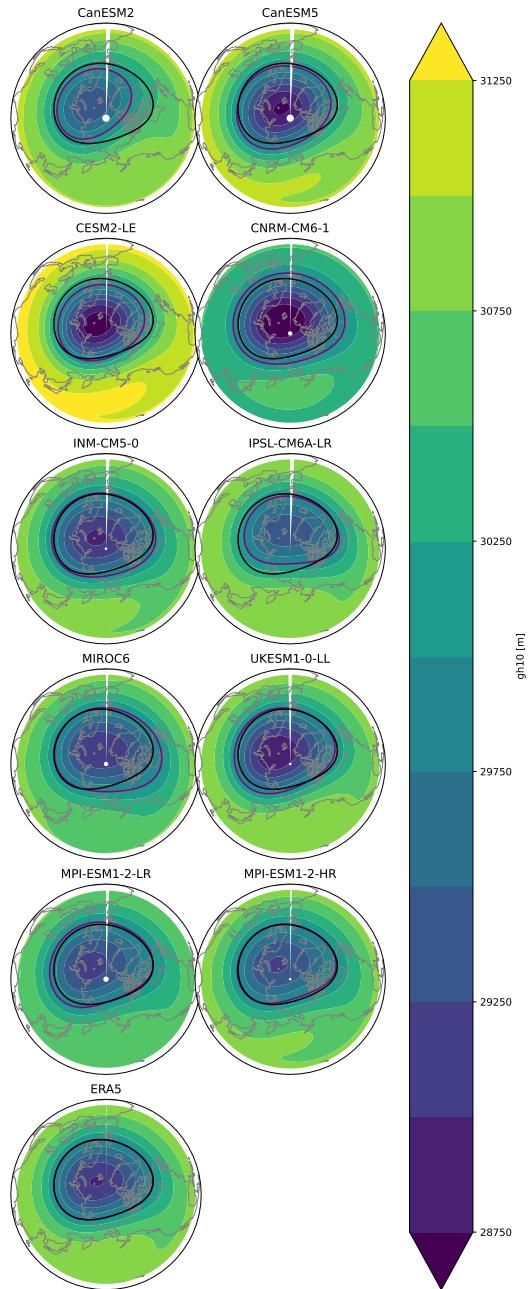
140 145 6. It would be useful to show an example of a ROC curve for one model, displaying both the split and displacement curve. This could go in the Methods or in the Results, and would be useful for explaining how the ROC curve is constructed.

Since we display all ROC curves in the supplement (see Figs. S1 and S2), we also explain the ROC curves in the supplement. As an example we include Fig. R2 to show ROC for displacements and splits. Fig. R2B demonstrates that CanESM5 cannot discriminate split events better than random guessing. We mention it in the Methods and in the Results sections.

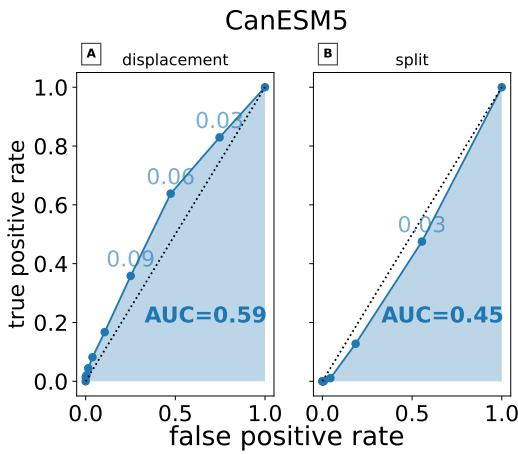
150 155 7. Since you mention the diagnostics of the vertical structure of the SPV in the abstract, it is advisable to show a couple of meaningful plots (or statistics values) for the other stratospheric levels in the main manuscript.

Thank you for the advise. We will include two new figures (R1, R2) and merge and move Figs. S10 and S11 from the supplement as new Fig. 9. Thus, we decided though to keep figures for 50 and 100 hPa in the supplement, since their discussion is overall limited, to keep the manuscript concise.

8. You should mention any previous use and results of geometric diagnostics in your Introduction section. Or did I miss this? On the other hand, SPV projections are not your main focus, so you should mention them briefly to motivate your work or more extensively only if directly related.



**Figure R1.** Geopotential height climatology at 10 hPa (gh10) of all analyzed model ensembles: CanESM2 (**A**), CanESM5 (**B**), CESM2 (**C**), CNRM-CM6-1 (**D**), INM-CM5-0 (**E**), IPSL-CM6A-LR (**F**), MIROC6 (**G**), UKESM1-0-LL (**H**), MPI-ESM1-2-HR (**I**) and MPI-ESM1-2-LR (**J**), and ERA5 for the period 1979–2014. The black and purple line represents contour of 30000 m in ERA5 and in a particular model, respectively.



**Figure R2.** Receiver operating characteristics (ROC) curves for displacements (A) and splits (B) in CanESM5. Bin values indicated along ROC are probabilities whether model simulates displacements and splits across its ensemble members, respectively. Area under the ROC curve is visualized and also specified in the figure. The dashed line represents random discrimination skill, i.e. AUC=0.5.

160 There are several references (Charlton and Polvani, 2007; Mitchell et al., 2013; Seviour et al., 2013, 2016; Hall et al., 2021) in the Introduction section and in Section 2.2 (Matthewman et al., 2009; Seviour et al., 2013). We also list a recent review on SSWs (Baldwin et al., 2021) where additional references can be found. In our opinion, the list is quite extensive. Furthermore, the discussion and abstract related to SPV projections will be adjusted to the following: *Assessing the model biases and their representation of SPV dynamics is needed to improve credibility of climate model projections, for example by giving stronger weightings to better performing models.*

165

#### TECHNICAL CORRECTIONS

General Please check the usage of the adjectives ‘low’/‘high’ (e.g. related to bias), and replace with ‘weak’/‘strong’ or ‘negative’/‘positive’ when ambiguous.

170 We will go through the manuscript and make appropriate changes.

Abstract Line 1 : « each hemisphere ».

Corrected.

175 Line 1 : The word « phenomenon » is not adequate for describing the SPV. Please change.  
Modified accordingly.

Lines 5-13 : Abstract would gain in clarity if the description was separated into geometric diagnosis and split-displacement diagnosis (e.g. line 6 shifted after the geometric diagnosis). The fluency of the text would improve.

180 Modified accordingly.

Introduction Line 25 : « relates ».

*relates* replaced with *relate*.

185 Lines 25-26 : The term « windiness » is ambiguous. I would suggest to replace the first two elements in the list with « the position of the jet and the precipitation patterns over Europe and the Mediterranean region ».

Modified accordingly.

Line 27 : « where the SPV is highly variable ».

190 We added *where*.

Line 31 : « from a final warming».

We skip the sentence since it is irrelevant.

195 Lines 32-33 : The sentence is difficult to read, could you please reformulate ?

We skip the sentence since it is irrelevant.

Lines 37-39 : The sentence is out of place in the present position. Please fit adequately in the discussion of literature.

We skip the sentence since it is discussed before.

200

Lines 57-58 : Do you mean that a limitation of the single-member analysis is that one can't distinguish between natural and inter-model variability ? This is not the meaning emerging from the sentence, please correct or clarify.

Modified accordingly.

205 Line 63 : « will be is », for consistency of the verb tense.

Corrected.

Lines 63-63 : The following sentence/content should be shifted in an adequate position, before the outline. « Due to the large SPV variability and the high SSW frequency in the NH, we limit our analysis to the NH SPV. »

210 We move the sentence accordingly.

Methods Lines 70-72 : You can remove the reasoning here, as it is already discussed in the Introduction.

Done.

215 Line 76 : Please change to « climate models used in our analysis are described in table 1 ».

**Modified accordingly.**

Line 78 : Inconsistency between the two sentences. Please modify.

**Modified accordingly.**

220

Line 80 : The first part of the line is accessory, please remove or include in compact form as part of the previous sentence.

**Done.**

225 Line 91-92 : The last sentence gives a qualitative description of the different moment diagnostics, but the link with the diagnostics (two sentences back) is not obvious – I suggest giving this information just following the list of diagnostics. Moreover, an extension of their description, here or at the beginning of each section in the results, would be welcome for any non-expert reader.

**We will provide qualitative descriptions of the moment diagnostics in the text and more detailed descriptions including their derivations in the supplement.**

230

235 Lines 95-99 : The description of rank histograms (RH) is confusing. Please describe precisely and clearly how the counts within each histogram are determined, and why the distribution should be uniform in a well-calibrated forecast. Is the sentence « The histogram counts of all bins greater or equal k are then increased by one » correct? From what I read from Sect. 2 in Hamill et al. 2001 (your ref.), only one of the bins is updated at each timestep, corresponding to the forecast ensemble bin where the observation falls.

**Thanks for your notice. We improved the description of RH in this paragraph accordingly.**

Line 119 : Mention where and how the perfect model range method is used within the present work.

240 **We will mark the description in the Section 2.4. The details of calculation can be found at our Github repository (Kuchar and Öhlert, 2024).**

Line 122-24 : It is not clear to me if your definition of displacements and splits corresponds to the description you give in these lines. Could you make it more explicit?

**Done.**

245

Line 126 : Insert ref. for ROC and AUC.

**See former 1125-126.**

250 Line 127 : Can you describe in this paragraph how the thresholds for drawing the ROC curve are computed ? And what you mean by true and false positive rates in a historical simulation (rather than in a forecast) ? An example of a ROC curve would also be welcome, including descriptive labels for x and y axes.

We described ROC in details in this paragraph and we also included Fig. R2 as an example.

255 Results Line 139 : It would be useful to specify again, within this sentence, that high values of the aspect ratio correspond to split events.

260 We will revise the introduction to the section to provide easier access to the topic for the reader. The part then reads: *The aspect ratio of the SPV is determined by the ratio of the major to the minor ellipse axis. Thus, it measures how stretched the SPV is providing high values for stretched and low values for more circular SPVs. Exceptionally high aspect ratio values are of particular interest for SPV dynamics as they are often associated with SPV disturbances, in particular indicating splitting events (Seviour et al., 2013).*

Line 143 : No need to repeat the aside « which is considered as ground truth for the analysis ».

Will be deleted as redundant.

265 Line 144 : « are biased simulating simulate ».

Modified to *are biased and simulate*.

Line 145 : « high positive ».

*positive* added.

270

Line 146 : How is the significance of the bias computed ?

We will omit the word *significant* and use *considerable* instead.

275 Lines 148-49 : You could merge the last two sentences of the paragraph, there is a partial overlap.

Done.

Line 152 : Does « low » mean ‘weak’ or ‘negative’ ?

*low* replaced by *weak*.

280 Line 153 : « model ensembles ».

Added.

Line 165 : Please specify pressure levels each time.

Specified.

285

Line 192 : « high » means positive ?

*high replaced with strong.*

Lines 200-01 : I don't understand the sentence starting by « However.. ». Please clarify or remove.

290

We delete this sentence as discussed in Section 5.

Lines 212-14 : Please specify large objective area or strong SPV, or describe what large SPV means. Check the same in the rest of the subsection.

295

We delete *small SPV* from l211. We also remove the following sentence: *This combination results in a strong underestimation of a large SPV occurrence.*

Lines 221-228 : These lines are better fitted for the Discussion section.

In Section 5 we discuss this aspect more generally in relation to other model parameters such as model resolution and GW parameterizations and their tuning parameters.

300

Line 240 : Specify that the assertion is valid for displacements. Or else present only the displacement results in the first introductory sentence.

Will be done by including 'For displacements,...' at the beginning of the sentence and additionally we add clarification of what can be seen in the 2 panels A and B (displacements and splits).

305

Lines 242-44 : I find the discussion quite vague. Could you clarify?

We will improve clarity and rearrange the whole section 4 according to new Fig. 9.

310

Sections 4.1 and 4.2 : The second part of each subsection is better fitted for the Discussion section, specially when the manuscript's results are compared with previous literature.

315

We refrain from moving these parts to the discussion section for several reasons. Firstly, we feel that a brief placement of the results (with literature) is fine for the results section, also to leave room in the discussion section for the more detailed discussions, and to not make the discussion section too lengthy. Secondly, we will include the scatter plots to the main text, to improve the results section, and that is connected with the mentioned parts, and would therefore lead to the discussion section having new results, which is not very pretty usually.

Lines 314-17 : These lines are rather vague and unrelated from the results and conclusions of the manuscript. Please remove if not otherwise justified.

We rearrange the paragraph to improve clarity and make the sentences relevant to previous statements.

320

Line 323 : It would be good to acknowledge that you are not analysing or describing GW parametrisation in this work.

We acknowledge that we do not demonstrate how the GW drag differences project to the intermodel differences in the SPV dynamics. For further discussion we refer to Hájková and Šácha (2023) and Sigmond et al. (2023). The part will read:

325 *Recently, Hájková and Šácha (2023) showed that the SPV climatologies in CMIP6 models are largely insensitive to high latitude wave drag, but also state that SSW simulation can be sensitive to small nuances in model dynamics. Dedicated analyses are needed to fully assess the impact of various wave drag mechanisms on SPV geometry and SSWs, including consideration of the non-linear feedbacks between wave drag and mean flow. For example, Sigmond et al. (2023) have attributed the difference in simulated SSW frequency between CanESM2 and CanESM5 to changes in settings of gravity wave tuning.*

330 Line 328 : Is this phrased correctly ? If so, it is difficult to understand what you are concluding.

The sentence will be rearranged to the following: *Therefore, these uncertainties in the models may be attributed to different gravity wave parameterizations (Sigmond and Shepherd, 2014).*

335 Lines 348-352 : This discussion is too general – it should connect more with the results of your work if you want to include it.

The discussion will be removed.

Conclusions Line 360 : What do you mean by « stability »?

We omit *stability* in the revised manuscript.

340

Line 370-71 : Can you tell why your results are different from previous literature ? Is it a different metrics?

345 Hall et al. (2021) use the preindustrial control simulation of each available model from CMIP6, which has a prescribed or calculated  $CO_2$  concentration, designed for the evaluation of unforced variability. They also use slightly different set of models. We rearrange our code for detections of SSWs to be fully compatible with Hall et al. (2021). Therefore, our results for displacement SSWs in new Fig. 9A are in agreement with Fig. 3 in Hall et al. (2021). In case of split SSWs, our results are more dispersed compared to Hall et al. (2021). Unlike their results, the reanalysis values lie within the 95% confidence interval of the OLS fit. As we can rule out that the size of ensemble members might not be sufficiently large in our study, we argue that the fit is not so robust for split SSWs because stretching tendency of polar vortex is accompanied with the centroid-latitude tendency to equatorward values (Mitchell et al., 2011).

350

Line 375 : At some point you had some contradictory results regarding resolution and SPV representation (see e.g. Line 199). Is it worth taking this into account in the present assertion?

We conclude that models with a finer vertical resolution generally simulate the SPV and SSWs better with reference to ERA5. However, the sentence regarding the MPI-ESM1-2 simulations will be modified in the following: *Both MPI-ESM1-2 simulations contains bias but dome-shaped RH in MPI-ESM1-LR indicates larger ensemble spread.*

Line 380 : « variability » instead of « behaviour ».

Thanks, will be done.

360 Line 380 : Replace « set the basis » with « can constitute/be a reference ».

Thanks, will be done.

Line 380 : Rephrase « with individual models changing individual model components ».

365 Will be rephrased to: ...our findings can be a reference for studies testing the sensitivity of individual models to particular model components or settings therein, such as...

Line 382 : « the SPV spatial variability ».

Thanks, will be done.

370 Line 383 : Please rephrase « for finding the adjustment screws to improve their performance ».

Will be rephrased to: "tuning and calibration to improve their performance"

Figures Figure 1-5 : Increase font size of model name and statistics above each plot. Mention the different models and the respective letter labels in the caption.

375 Thanks for your suggestion, will be done.

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