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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Exploring the sensitivity of the large-scale atmosphere circulation to changes in surface temperature gradients using a Statistical-Dynamical Atmosphere Model" *by* Sonja Totz et al.

Anonymous Referee #1

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Review of "Exploring the sensitivity of the large-scale atmosphere circulation to changes in surface temperature gradients using a Statistical-Dynamical Atmosphere Model " Sonja Totz, Stefan Petri , Jascha Lehmann, Erik Peukert, Dim Coumou

Referee #1

The paper investigates the role of global mean, meridional and azonal temperature changes on large scale Northern Hemisphere atmospheric circulation (jet stream, storm track, planetary wave and Hadley Circulation) within properly designed simu-

lations performed with a statistical-dynamical atmosphere model (SDAM) Aeolus 1.0. The authors found that the strength of the Hadley cell, storm tracks and jet streams depends almost linearly on both the global mean temperature and the meridional temperature gradient whereas the zonal temperature gradient has little or no influence. The width of the Hadley cell behaves nonlinearly with respect to all three temperature components. In spite of the well structured manuscript (in terms of sections) the story flow is still poor and sometimes the main aim is unclear through the reading. Hence, I recommend a major revision. See major and specific comments below.

Major Comments:

After the first rejection in ESDD, the new manuscript addressed pretty well the points made by reviewers in the previous submission. Authors now described very precisely the model and the model set-up used for the study, highlighting the strong points of the Statistical-dynamical Atmospheric Model SDAM-Aeolus 1.0 with respect to conventional CGMs used to investigate Hadley Cell (HC) dynamics and its changes. This represents the novelty of this study.

However, it is very hard to find the main goal of the paper: in the beginning, disentangling the effect of changes in the meridional temperature gradient versus azonal temperature changes versus mean temperature changes on the boreal winter atmospheric circulation seems to be the main goal of the study. In the second part of the paper however, it seems that authors want to validate only the SDAM in order to assess its performance on the main characteristics of the atmospheric circulation under different forcing.

The paper needs for sure some editorial changes: the structure itself is not bad, but the story flow is very fragmented: the introduction seems a list of "this guy did this, this other guy did that" without really telling a story, being completely unbalanced and totally unfocused. The paper itself therefore resulted in a collection of results, but it is not really clear what authors are trying to prove.

Hence, I recommend a major revision.

The abstract is totally confusing. This sentence is completely out of context: “Under global warming the temperature gradients are expected to change: Enhanced warming is expected in the Arctic, largely near the surface, and at the equator at high altitudes, altering the meridional temperature gradient. Further, land-ocean contrasts will change due to enhanced land warming. Also there is a pronounced seasonality to these warming patterns.”

The abstract needs substantial review, in order to reflect the text and highlight the main findings. Moreover, also from the abstract it is not really clear what authors want to prove.

Furthermore, authors insist to use inappropriate metrics for Hadley Circulation and jet-stream, making results very hard to compare with the wide literature about it. This is another weakness of the study.

Minor aesthetic adjustments are needed to some figures (see comments listed below).

Specific Comments:

Title: “...using a Statistical-Dynamical Atmosphere Model” Consider rephrasing as “...using a Statistical-Dynamical Atmospheric Model”. Page 5 ln 20-21 “We change the temperature for each grid cell with respect to parameters for the three components in three steps. First, the parameter ðİŚďTphi ...”. I missed what is ðİŚďTphi . I understood only it is a parameter. I guess you used a varying ðİŚďTphi in order to have different sensitivities. Does ðİŚďTphi vary in a range? Can you specify it here and not at page 6 ln 4,8? Page 6 ln 24: “...To obtain the strength of the jet stream for this analysis, we use seasonally (DJF)”. It is not clear to me why you use specifically boreal winter season. Can you say something about it? Page 6 ln 25: “...for simplicity define the jet stream strength as the maximum of âŇľðİŚćâŇł between 10°N and 80°N at 9000 m height (corresponding to a pressure level of ca. 300 mbar). I like sim-

plicity, but not oversimplifications. If you change the meridional temperature gradient, this affects 1) the position of the subtropical jet-stream (phi, plev), 2) on the strength of the subtropical jet-stream. Are you really sure that your metric effectively captures the maximum and the latitude of the jet-stream under different simulated “climates”? You should be able to say something about the choice of this specific metric and find a climate-invariant metric for the jet-stream. Page 7 ln 1-4: “metrics for HC”. The way you describe the metrics is confusing. Are you computing the meridional mass stream-function? If yes, why don’t you say it, rather then saying “by computing the integrated southward mass flux in the lower troposphere between 1000 mb and 500 mb from the zonal mean meridional wind velocity”. As far as I understand this is equivalent to the classical stream-function by Oort and Yienger, 1996, Although you vertically integrated between 1000 hPa and 500 hPa. Be aware that 500 hPa is too shallow in order to get the full vertical extent of the HC, which actually it extends up to 200 hPa in deep tropics. I totally agree that there is confusion about metrics (I’ve read you reply to the reviewer from the previous submission) BUT, in order to get results comparable with literature and to avoid to introduce further confusion, you should take the vertical average of the stream-function between 400 hPa and 600 hPa, or 200 hPa and 700 hPa (usually it is a good practice to average first and stay above the boundary layer) and then calculate the width as the zero-crossing latitude. The integrated measure is not commonly used, therefore I do not recommend it. It can be potentially a wrong estimation of the HC strength. If you don’t want to follow the literature, then you have to prove that YOUR metrics are consistent with the metrics used from the whole community. There is no easy way out about it. You might want to refer also to the first paper on metrics came out from a CLIVAR project on metrics for tropical width: https://www.geosci-model-dev-discuss.net/gmd-2018-124/ The TropD software package: Standardized methods for calculating Tropical Width Diagnostics By Ori Adam , Kevin M. Grise , Paul Staten , Isla R. Simpson , Sean M. Davis, Nicholas A. Davis , Darryn W. Waugh , and Thomas Birner. So, concluding, USE THE STANDARD METRICS FOR THE HADLEY CELL.

Page 7 ln 20: “In particular, the maximum strength, defined as the minimum between the zero-crossings". This is really not clear to me. The strength is the max or the min value inside the NH or SH HC, respectively. Then the max strength is the max between NH and the SH poleward edges for the NH HC (if the NH HC is defined positive for clockwise overturning). This is also missing in the methods... Conventionally, the NH HC is positive, while the SH HC is negative. If you had written the equation for the stream-function this would have clarified explicitly. Please clarify.

Page 7 ln 20-22: "There exist bigger differences in the SH. This model bias might be related to the missing Antarctic ice sheet, upper-tropospheric ozone, the constant lapse rate assumption, or fundamental limitations of the equations." There is more than that. The cross-equatorial HC (e.g. the winter HC) is nearly inviscid limit. Therefore, its poleward extent and its strength are not dictated by eddy momentum flux (Schneider and Bordoni, 2008). At the same time, in the opposite hemisphere, the summer HC is dominated by eddy momentum flux divergence. Probably, the poor agreement in the SH is due to the statistical nature of the eddy representation in the SDAM. Therefore the use of the SDAM for HC analysis must consider only winter season. State it clearly. Page 11 ln24: "In this study, we observe a strengthening of storm track activity under increased global-mean temperature." The reference to the figure is missing. Provide it. Page 12 ln 28: "In our analysis the strengthening of the planetary waves depends on all temperature components. Larger meridional and zonal temperature asymmetries as well as global mean temperatures lead to stronger winds". The reference to the figure is missing. Provide it.

Figures:

Fig. 2 Caption: "Integrated northward mass flux in lower troposphere. ..." Please specify everywhere in the text that the winter you refer is the boreal winter. I have also some doubt about the magnitude. The conventional magnitude and unit for the atmospheric mass flux of the Northern Hemisphere psi_max_DJF is around 20 x 10ˆ10 Kg/s or 200 Sverdrup. Why do you have here Kg/ s m2 and such weak values? In order to compare values with previous study it is warmly suggested to change the unit

according to the literature by performing the standard meridional mass stream-function (Oort and Yienger, 1996).

Fig. 5 Panel a) has not the same size of the others. Furthermore, x-labels of panels b,c,d are incorrect: it is supposed to be the latitude, right? Then, label correctly please. Please also write the unit of <u> (should be m/s, right?). Additionally, in panel a) you have pressure levels on the y-axis: why do you have altitude in meters for panels b,c,d? Please be consistent throughout panels. The same for Fig. 7.

Suggested literature:

Adam, O., Grise, K. M., Staten, P., Simpson, I. R., Davis, S. M., Davis, N. A., Waugh, D. W., and Birner, T.: The TropD software package: Standardized methods for calculating Tropical Width Diagnostics, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-124, in review, 2018. Oort, A. H., & Yienger, J. J. (1996). Observed interannual variability in the Hadley circulation and its connection to ENSO.ÂăJournal of Climate,Âă9(11), 2751-2767. Schneider, T., & Bordoni, S. (2008). Eddy-mediated regime transitions in the seasonal cycle of a Hadley circulation and implications for monsoon dynamics.ÂăJournal of the Atmospheric Sciences,Âă65(3), 915-934.

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