

## ***Interactive comment on “Brief communication: CMIP6 does not suggest any circulation change over Greenland in summer by 2100” by Alison Delhasse et al.***

### **Anonymous Referee #2**

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This manuscript provides an update to analysis of CMIP5 data which will probably be of use. I'm sorry not to be able to provide a more positive review, but there are many ways that this could be improved. I'm not familiar with the journal though, so perhaps for a brief communication this is acceptable. However, several of the stated conclusions are unsupported by the analysis, so at present I would say that the manuscript is not suitable for publication with these in.

Unsupported statements:

- 'any circulation change' in the title. This clearly hasn't been demonstrated, as only one very simple circulation measure is used in the whole paper.

C1

- 'Significant melting events related to a highly positive GBI...' in abstract. The paper focuses on 21-year running means, so seasonal extremes have not been considered.

- 'a shift towards more favourable atmospheric dynamic precursors...' on p4. No analysis of this is given.

- lines 9,10 on p5: no separation between 'temperature variability' and 'circulation changes' has been attempted, and statements like this are very vague.

- line 30-32, p5: No analysis has been done on what 'drives' the GB2 'changes'.

Major points:

While simplicity is a good thing in general, some of the approaches here could be too simple. The 'blocking' indices are really just seasonal mean height diagnostics, so can't be said to reflect blocking necessarily. There are probably many other things mixed in to these indices, such as hemispherically symmetric annular mode variability. It's quite possible that the interannual variability of such a time series does represent blocking, but that trends could be dominated by a different process, perhaps a thermodynamic one. I would be tempted to remove the word 'blocking' from the paper. Similarly, the temperature indices are quite arbitrary - should be mass-weighted at least.

The NCEP1 reanalysis is very old now and should no longer be used on its own. Other obvious ones to add would be ERA-20C, JRA55.

The text in many places is not very scientific. The most striking example is the frequent use of the word 'changes'. Compared to what?

The paper is also not very objective. The authors clearly already have a view on this, that summer blocking is increasing over Greenland due to anthropogenic forcing. But this is certainly not shown in the evidence they have presented. A more objective approach would allow for the possibility that multidecadal variability is underestimated in the models.

C2

There is also a potential field significance issue here, in that the Greenland region has been pre-selected as a region where interesting 'trends' in height are apparent in the observations. If a 95% significance level is used, for example, 5% of Earth's surface might be expected to show a 'significant' trend in any analysis. Are the authors sure that this is not happening here?

Other questions:

Fig 1 is very interesting. Why are the GB1 and TA1 indices used for this, and how does it look for GB2 and TB2?

Fig 2 and the discussion could be clearer on the normalisation - this was done before the time filtering?

Why do the model projections show a decrease in TA2 over the 21st century. This seems very surprising given our expectations of polar amplification.

- Intro: why does increased inflow of moist air lead to increased absorption of solar radiation - this seems counter-intuitive.

- p2, line 12: Why a doubling here? The climate sensitivity is higher, but not by a factor of two...?

- 'significant increase of GB2' on p5 needs to be more rigorous. eg period?

- p6, line 2: doubling compared to what?

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-332>, 2020.