

Response to Anonymous Referee #1

We thank the reviewer for their useful comments

The paper is short, well written and scientifically sound.

The main concern I have is to clarify what the actual contribution of the paper is. The paper should be better positioned wrt state of the art. In particular the intro should make clear what were the findings from Screen and Williamson (2017) and Sandersen et al. (2017), how the present study differs from those, and how the present methodology brings something different from / completes these studies.

If a clear added value can be defended, then the paper can be published nearly as is. I would add that, if the contribution is an independent evaluation of the likelihood of an ice-free Arctic under 1.5, 2 and 2.5°C targets, using an alternative method (ensemble vs multi-model vs emulator), I'm quite supportive for the paper to be published, even if the final result duplicates previous findings. Independent, repeated tests are in my view as important as original studies.

In practice, this would probably mean moving material from the end of the paper to the end of the introduction, and complete what is only being suggested at the moment by being more explicit.

Material moved forward to introduction as suggested

More specific comments below.

- The advantages / specificities of the SRM method should be clearer and the reason why it has been chosen as well.

The use of SRM is arguably a plausible mechanism to attain the 1.5C target. It is also a simple mechanism, compared with Sanderson et al (2017), as it requires no new emissions scenario (which would be inconstant with the RCPs). This is an idealised temperature sensitivity study and not suggesting how SRM might be employed. An alternative approach might be to fix CO2 in RCP4.5 when temperatures reach 1.5, 2.0 and 2.5C, but this would leave residual effects from secondary greenhouse gases, aerosols and feedbacks (comparing the 2 methodologies might be an interesting study) SRM is a tried and tested methodology for HadGEM2-ES and the other models of the GeoMIP. We add:

“Here we take a different approach with the intention of assessing if the outcome of three different approaches, including the above, can provide a robust answer to the probability of a seasonally ice-free Arctic at 1.5 and 2.0°C above pre-industrial. Our methodology is to construct an ensemble of simulations of the CMIP5 model HadGEM2-ES using solar radiation management (SRM) to restrict the global temperature rise. We employ SRM because of its simplicity in requiring a change in just one component to the model, hence maintaining traceability. It is also a plausible scenario, in addition to mitigation, to the 1.5°C target (Sugiyama et al., 2017). This work expands on that of Jones et al (2018) where the SRM methodology is established for HadGEM2-ES. “

- I find the methodology not fully clear. In particular the story of the time dependence of SO₂ emissions. Could you illustrate or better describe how SO₂ emission depends on time? Is this constant then stabilised? Is it ramped up? Is it non-linear?

The explanation has now been expanded. The injected SO₂ volume is time varying to offset the time-varying difference in temperature between RCP4.5 and say the target temperature (say 2.0C). In practice the process (SO₂ loading and climate feedbacks) is non-linear with temperature, but for small Delta T this does not matter (as now shown in Figure 3).

- It is well known that the rate of Arctic sea ice decrease depends on mean state, in particular ice volume. Do you expect a model with less volume and the same experimental setup to give higher probability of sea ice volume loss at 1.5°C ?

A brief comment to this effect, and reference to Bitz (2008), is now included in the conclusions.

- p. 1 l. 23 I would say that there is a net increase in winter growth because ice is thinning (Bitz and Roe, 2004), but I'm not sure which effects dominates. You should come up with more references or more arguments (for instance a mass balance study in CMIP-X).

A comment added to the conclusions refers to the need for such a mass budget analysis.

- p.1 l. 22. "With global, and regional, warming" sounds weird to my ears.

Removed 'and regional'

- p. 1 l. 28 "increased" instead of "increase"

This has been corrected

- p. 1 l. 29. I think the increase in extreme weather due to reduced sea ice is quite challenged, in particular the quite convincing study of Blackport and Kushner J. Clim 2016.

This is still debated e.g Smith et al. (2017) <https://doi.org/10.1175/JCLI-D-16-0564.1> and Blackport and Kushner (2017). I think it is still reasonable to say 'may cause'

- p. 2 l. 7. Replace "this is because" by "we make this choice" or reference others to clarify whether you propose this or whether this is standard practice.

This has been corrected as suggested

- p. 2 l. 20. Explain why you use this method.

Have added to the justification at the end of the introduction. Please see response at the start of this response

- One inconsistency is how °C is spelt. Sometimes without the °, sometimes with space, sometimes not. Make it consistent.

Changed to be consistent throughout