Review of "Energy transport, polar amplification, and ITCZ shifts in the GeoMIP G1 ensemble" by R. Russotto and T. Ackerman (ACPD, 2017)

General Comments

In this study the authors investigate the impacts of solar geoengineering and global warming on atmospheric meridional energy fluxes by using results from an intermodel ensemble (GeoMIP) and by employing a moist energy balance model (EBM). The EBM is combined with GeoMIP output in order to attribute energy transport changes to different climate forcing agents and climate feedbacks. The study seeks to answer two separate questions related to meridional energy transport: 1. how does solar geo impact the poleward energy transport at high latitudes, and 2. how does solar geo impact the cross-equatorial energy flux. In essence, the first question asks whether solar geo would exacerbate or counteract high-latitude warming while the second question asks which aspects of GCMs contribute most to uncertainty in changes to tropical precipitation. The authors show that solar geo would counteract the poleward energy transport enhancement under global warming, which they rather elegantly attribute to changes in moisture transport. This result helps to explain the observed mid-latitude drying signal in previous solar geo studies (e.g. Tilmes et al., 2013) and is a very useful inference. It also helps to explain why solar geo effectively (though not completely) offsets high-latitude warming. In an attribution study with an EBM, the authors attribute the largest source of uncertainty in poleward energy transport changes to cloud feedbacks, in agreement with previous studies looking at energy transport under global warming. They also show that cloud feedbacks contribute most to uncertainties in cross equatorial energy transfer (and hence tropical rainfall migration) under solar geo.

The paper is a very useful contribution to the solar geoengineering literature and helps to shed light on important results of previous studies such as residual high-latitude warming. The background information (section 1) is comprehensive and the methodology is sound. The use of an EBM is appropriate, although I think the disparities between the results of the EBM and GCMs (e.g. Fig. 5) should be elucidated more carefully in the text. My primary concern with the paper is the lack of consideration for oceanic energy transport changes – in particular for cross-equatorial energy fluxes where oceanic energy transport is important. The manuscript would benefit from looking at oceanic energy transport changes explicitly (a methodology for calculating meridional oceanic energy transport is provided by Hawcroft et al, 2016). This may elucidate why the ITCZ shifts in abrupt4xco2 are not correlated with atmospheric cross-equatorial energy transport changes. If the authors are unable or unwilling to investigate oceanic energy fluxes, then I would suggest altering the title of the manscript to "*Atmospheric* energy transport, polar amplification, and ITCZ shifts in the GeoMIP G1 ensemble" to better reflect the paper's contribution. Once these changes (and various minor changes listed below) are made, I'd be happy to recommend publication.

Specific Comments

P1 L5:	First instance of CO_2 – define as carbon dioxide
P1 L8:	Mention explicitly that it is the *radiative forcing* from enhanced GHGs that is being offset by solar reduction in G1
P1 L8:	Sentence beginning "In G1,..." - consider starting with "We show that ..." to distinguish your results from prior results
P1 L19:	Consider replacing "compensated for" with "counteracted". Also add a suitable reference at the end of this sentence
P1 L23:	Sentence beginning "since reflecting sunlight would affect ..." is ambiguous. Explain why solar geoengineering leaves residual warming at high latitudes – at least give the primary theories – e.g. more sunlight in the tropics on average – with a suitable reference (e.g. Kravitz et al., 2013)

P2 L12: Replace “subtropical” with “tropical” - Haywood et al identified Sahelian drought as a concern of hemispheric geoengineering and did not look at the subtropics

P2 L14: Add a suitable reference to the last sentence of this paragraph – which study explicitly identifies an ITCZ shift following a symmetric SAI application?

P2 L15: You say “ITCZ shifts are closely related to the meridional transport of energy by the atmosphere”. This is irrefutable, although you should add an appropriate reference, but only tells half the story. Add more discussion about the relative importance of ocean heat transport here, and its ability to control ITCZ position. The following references may be useful: Green and Marshall (2017), Hawcroft et al (2016), Haywood et al (2016), Hwang et al (2017), Marshall et al (2014)

P2 L30: The aim of G1 is not to “keep global mean temperature at approximately preindustrial levels” as you say – be more explicit about the simulation design

P4 L5: Define $\nabla \cdot F_L$ at first use (i.e. latent energy transport)

Figs 1 and 2: Consider also changing the y-units to 'poleward energy transport' rather then 'northward energy transport' to assist comparisons. Lastly, I'd suggest putting 'Latitude (N)' as the x-title rather than 'Latitude'

P7 L2: Add a suitable reference for the DSE response to high-latitude warming

Eqn 3: This equation is valid for temperature in units in Kelvin, whereas your plots give temperature in units of oC – pick one for consistency and use throughout the manuscript – I'd personally go with the SI unit K

P8 L14: Sentence “This leaves the differing spatial patterns of forcings as the only possible explanation” should have a caveat that meridional ocean heat transfer is negligible at high-latitudes (e.g. Fig. 6 in Hawcroft et al 2016)

P8 L30: I recommend that the authors explicitly look at changes to meridional ocean heat transfer in the G1 and abrupt4XCO2 simulations using the methodology of Hawcroft et al (2016). Whilst the caveat about ocean heat transport in P8 L30 is appreciated, explicity looking at changes to ocean heat content / transport would significantly improve the manuscript whilst not altering the primary results

Fig. 4: Consider adding the correlation coefficients to the the respective figures

P9 L2: Reference the Haywood et al (2013) study at the end of “shifts toward the warmed hemisphere”

P9 L2: Change “It implies” to “The ITCZ shifts in Fig 4b imply”

P10 L10: Change “40 ºN” to “40 ºN/S”

P10 L11: Add a space between piControl and (Figure 5c)

P10 L12: Yes there are strong correlations, but that doesn't mean the EBM is doing a good job! For instance, for abrupt4XCO2 the EBM predicts a negative poleward energy transport anomaly at 40 ºN for 6 out of the 8 models where the GCMs give a positive anomaly. This issue should be discussed and not glossed over.

Fig. 5: A minor suggestion - put “40 ºS/N” into the plot titles for b) and c)

Fig. 6: In the caption and the titles note that it is the *northward* energy transport that you are plotting

P13 L5: Again – I urge you to explicitly assess oceanic heat uptake in these simulations – you say that the results of the EBM imply that oceanic heat uptake differs between the GCMs – it would not be difficult to assess this hypothesis and would add value to your results

P13 L7: Sentence beginning “The impact of the solar forcing term ...” is very wordy and does not read well – rephrase

P13 L25: “has” → “have”

P13 L27: Sentence beginning “Models with a greater negative change...” - include an example

P13 L30: I would also use Fig. 6 in Hawcroft et al (2016) to argue your point that poleward energy transport changes in are likely not due to oceanic heat transport changes – i.e. more background meridional heat transport in the atmosphere than the ocean

P14 L8: You should add caveats that the EBM does not get the sign of the energy transport right in the Northern Hemisphere (Fig. 5c). I would seriously consider removing Fig. 8A and associated analysis due to this issue as it indicates the EBM is missing the point and any analysis is compromised

P16 L18: Following “poleward atmospheric energy transport decreases” refer to Figs 1a,d

References

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