Overview

Overall, this manuscript makes an interesting contribution to the physical climate's response to solar radiation management geoengineering. The analysis framework is centered on moist static energy transport, which has been a valuable way of understanding other climate perturbation (e.g., from carbon dioxide and other anthropogenic radiative forcing). Bringing this perspective to the geoengineering simulations is helpful to understand the ways in which the reduced solar constant is not perfectly offsetting the regional climate changes from increased carbon dioxide, even if the global-mean surface temperature is near zero. The conclusions concerning the critical role of the spatial pattern of the forcing (vs. the possibility that radiative feedbacks differ between solar and greenhouse gas forcing) is nice to broadcast clearly, as the abstract does.

I think the manuscript is suitable for publication, but there are some important caveats about the analysis that need to be added and the authors need to alter how they perturb the moist EBM to be more consistent with what we know about the radiative forcing of carbon dioxide (at least the global mean part).

Major Points

1) ITCZ shifts: recent literature

There are a few relevant new publications that the authors should engage with.

Seo et al. (2017) showed that the cross-equatorial energy flux does not always account for ITCZ shifts in GCM simulations of carbon dioxide warming. It's fine to continue to use this as a first cut toward understanding the simulations' behavior, but its limitations need to be acknowledged. It may be that the non-zero intercept in Fig. 4a comes about for a reason discussed there: the gross moist stability (energetic stratification) can also change.

Viale and Merlis (2017) analyzed simulations with solar forcing and carbon dioxide forcing and found the ITCZ shifted a different amount. They interpreted this result through the refined energy flux equator argument described by Bischoff and Schneider (2014). The result in Viale and Merlis (2017) is in line with the typical GCM results here: more sensitivity to carbon dioxide than solar forcing in ITCZ shifts, so a poleward shift in geoengineered climate. This comes about by a different spatial pattern in the radiative forcing (not differences in radiative feedbacks).

Last, the TRACMIP simulations (Voigt et al., 2016) have examples of radiatively forced warming where the ITCZ shift does not follow the energy flux equator. I've seen this shown in conference talks, though it may not have been published yet.

I think this literature needs to be invoked rather than ocean heat storage (p. 8 L30) for the breakdown in abrupt 4x simulations: the ocean heat storage is already taken into account through the surface fluxes. That cannot explain the why the cross equatorial energy fluxes in the atmosphere are uncorrelated to the ITCZ shift.

2) Moist EBM analysis

First, I appreciate that the methodological details are described in a greater detail than in previous papers that use this approach and the authors have made their codes available.

* Eqn. B1 has a mistake (see North 1975 and subsequent diffusive EBM papers). There is a missing $(1-x^2)$:

$$\frac{\partial}{\partial x}\left[(1-x^2)\frac{\partial MSE}{\partial x}\right]. \tag{1}$$

I hope this is a typesetting mistake and not a problem with the implementation of the EBM used for the analysis.

* The convention of $OLR = aT_s - b$ is confusing when other EBM literature has the opposite use of a and b (e.g., $OLR = a + bT_s$), but I understand the desire for continuity with Hwang and Frierson (2010).

* I think it's valuable to try to set the record straight concerning how Hwang and Frierson (2010) described the change they made to do the warming EBM simulations. I thought they made a terminology mistake, saying that they re-fit b "to capture the change in climate sensitivity", which would be a change in a. Changing just b is reasonable as you can think of that as a change in radiative forcing (e.g., Rose et al., 2014). This manuscript, however, suggests both a and b must be changed to simulate altered climates. I don't follow that, as Rose et al. (2014) and others have done moist EBM simulations that are just changing b and have changes in energy transport. The values of a don't seem to change too much, generally near 2 W/m^2/K as expected from the sum of temperature and water vapor feedbacks, so I'm confused.

* Again, I want to praise the authors for disclosing the methodological details. I have not seen the parameter values for a and b when using linear fits to GCMs before (Table 4). However, the results are troubling and perhaps earlier work would have attracted scrutiny if this had been disclosed. If we think of b as representing the carbon dioxide radiative forcing (change in OLR with unchanged surface temperature), the fits imply carbon dioxide radiative forcing that varies across models from 0 to 16 W/m^2 (just considering the abrupt 4x column vs. the control). This is a problem! It also affects the G1 simulations.

* The last important thing that should be noted is that the approach of prescribing TOA perturbations (e.g., from cloud radiative effect changes) in the moist EBM does not capture the fact that feedbacks are interacting with each other (Feldl et al., 2017) and with the changing energy transport (Rose et al., 2014; Merlis, 2014; Rose and Rayborn, 2016). There's still value in this way of diagnosing things (partially interactive), but it's not the full story and may help explain why EBM is changes less than the GCM.

3) Forcing estimates

The authors describe in conclusions that the radiative forcing for carbon dioxide is not uniform, but this seems important enough to come earlier when describing the EBM set up (p. 10 L1).

* The authors cite only Zelinka and Hartmann (2012) (p. 15) when discussing the extratropical response to carbon dioxide. Those authors neglected the structure of carbon dioxide, which was subsequently included in Huang and Zhang (2014). They also looked at the same CMIP5 abrupt 4x simulations, so it's important to confirm consistency.

* Other authors (e.g., Feldl and Bordoni, 2016) have taken the approach of diagnosing the forcing, including its spatial structure, from fixed SST simulations ("troposphere adjusted forcing"). Given my concern about the linear fit estimate of b values above, I think this is a superior way to drive the moist EBM. Another alternative would be to take previously published global-mean adjusted forcing estimates and use those—at least all the carbon dioxide forcing would be near 7 W/m^2.

Minor Points

p. 3 L19: "moisture fluxes" terminology is not ideal for the latent surface fluxes, given that meridional energy and moisture fluxes are discussed later.

Fig. 3: how close is the global-mean surface temperature change to zero in these simulations? I understand this is discussed in other papers using G1, but it would be helpful to at least state a representative number (e.g., within 0.2 K).

Fig. 3 caption: presumably these are zonal *and annual* mean *surface air* temperature changes? I didn't see this in the main text either. Likewise for saturation vapor pressure.

p. 8 L27, and elsewhere: a big excessive on the number of digits for r.

Fig. 5: plot 1-to-1 lines here

p. 16 L6: the ocean heat uptake is weighted toward subpolar oceans (vs. a uniform uptake which wouldn't affect extratropical atmospheric energy transport)

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