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Interactive Comment

Interactive comment on “Comment on “Climate sensitivity in the Anthropocene” by Previdi et al. (2011)” *by* S. E. Schwartz

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In response to Schwartz 2012’s comments to Previdi et al (2011):

1) Schwartz 2012 contends that Previdi et al have made a mistake in interpreting their "no-feedback" Planck reference sensitivity by using an incorrect temperature (i.e., the emission temperature of the planet rather than the surface temperature). This is incorrect since it’s the emission at altitude which matters for planetary energy balance, and where most of Earth’s OLR originates from. One of Schwartz’s citations (Roe and Baker, 2007; see also Roe, 2009) correctly use 255 K, as this is the temperature near the level of some ’mean altitude’ of emission to space. Note also that the surface is not in radiative equilibrium, but the planet is. While Previdi et al (2011) could have been more clear in their derivation, it is mathematically correct and implies a linear

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relationship between emission level and surface.

In a strict sense, the no-feedback "reference system" can be anything, but is customary to be a uniform warming of the whole troposphere (with only the Planck response acting to bring the system to equilibrium). In evaluation in GCM's, the Planck feedback is not typically applied as a perfectly horizontally and vertically uniform warming (see Appendix A in Bony et al., 2006) and atmospheric absorption by gases play a role in realistic calculations (Roe, 2009) so slight differences in the number of 3.8 W m$^{-2}K^{-1}$ are expected.

2) Section 2 of Schwartz 2012 seems odd to me. It is unclear where Previdi et al ever explicitly define $\lambda\Delta T$ as the increased LW emission, or more importantly, use that in that context. It's clear that the shortwave spectrum can also change in a different climate, and that influences the radiative budget, but the main point in Previdi et al seems to be that the LW emission must necessarily change (along with temperature) to accommodate whatever change is necessarily for the planet to come into radiative equilibrium. The only way this wouldn't happen is if the absorbed solar radiation decreased enough to exactly match the forcing by greenhouse gases.

3) I largely agree with section 3. Regarding section 4, this section is a bit unclear (there's some sentence structure and extra wording issues on line 15-16). As a general comment that applies to both papers:

Generally TOA forcing matters more for surface temperature change than surface forcing (e.g., Pierrehumbert, 2010; Miller, 2011). In the well-coupled limit, the surface fluxes will adjust to minimize the temperature gradient between the surface and lower boundary layer, and in this case it is appropriate to think of the whole troposphere as one unit being dragged along with the TOA energy budget (and without requiring explicit detailed look at the surface energy budget). There are some possible exceptions, such as hypothetical moistening of the Saharan surface, which could provide substantial surface cooling even with increased CO2. It is also well known that anthropogenic

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land-use changes can play a role in altering the surface temperature or hydrological cycle independent of radiative changes.

It seemed that the Previdi et al (supplementary) only wanted to mention this minor point, and it does not really a play a role in their main paper. Moreover, no one really disagrees that changes in the global water cycle are important for climate change and sensitivity. I'm not quite sure if this was a necessary addition to the Previdi paper, but the Schwartz, 2012 criticism of it seems unjustified and irrelevant to the conclusions and framework of the paper.

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Interactive comment on Earth Syst. Dynam. Discuss., 3, 143, 2012.