Short Comment 3: The differences between Flatau et al. (1992) and Davies-Jones (2008).

From Reviewer #1, major comment 6:

> "The paper implies in a very misleading calculation (lower panels of Fig. 1) that there are significant problems with the way saturation vapor pressure is calculated in GCMs, when the errors are tiny and well-documented in the existing literature."

From Reviewer #1, minor comment 44:

> "Figure 1. The variable "q" should I guess be "Q", the specific humidity? The plot for q is un- useful and indeed misleading because it implies a very large error in q which is not true. What is actually happening is that you the computation is being done at fixed total pressure p, but as e approaches p this becomes impossible and implies a vanishing (and then negative) dry air pressure. This is not sensible. If the calculation is done at fixed dry air pressure (more sensible since this is what would actually happen with a fixed mass of dry air and g), the curve for q will look similar to that for e. I recommend deleting the figure entirely and dropping all claims or innuendo in the paper about the inaccuracy of saturation algorithms—you are beating a dead horse, these small errors are already documented in the literature, and there is no way that errors of no more than 2% that don't begin to appear until temperatures are 30C higher than any on Earth today are of any significance."

From Reviewer #2, major comment #1:

> "The author emphasize the implementation of more accurate moist thermodynamic calculations. However, based on the results shown here I am not yet convinced that this is particularly relevant. At least Fig.1 suggest that there is hardly any difference in the typical range of tropospheric temperatures. Please quantify the effect for the application here."

In the original Figure 1, the variable 'q_s' is saturated specific humidity, Q_s. We agree with the reviewers that the differences in saturated vapor pressure algorithms are less than 2% different at temperatures below 60°C. Our first intent of Figure 1 was to confirm that our incorporation of QSat_2 for saturated vapor pressure calculations in the model has no appreciable effect at these temperatures. However, our other purpose of Figure 1 was to show that the advantages of adding wet bulb temperature and equivalent potential temperature warrant using QSat_2. We agree with the reviewers that this figure failed to do so.

We agree with Reviewer #1, that at higher temperatures, that both the QSat_2 and QSatMod modules have responses that are not sensible, and we will incorporate the reviewer's language regarding this into our manuscript. At higher temperatures, water vapor is a considerable mass fraction of the atmosphere, and general circulation models do not represent this mass fraction (Ramirez et al., 2014). We believe this is important, because extreme temperatures >>50°C exist in future climate states (Sherwood and Huber, 2010, Figure 1e). Additionally, the theoretical differences between QSat_2 and QSatMod are not shown in our cited literature. However, one of the main objectives of our study is demonstrating improvements in the moist thermodynamics of CLM4.5, and our new Figure 1 (see short comment 2) demonstrates this. Thus, we will remove Figure 1, as suggested by the reviewers.

Ramirez, R. M., Kopparapu, R. K., Lindner, V., and Kasting, J. F. Can Increased Atmospheric CO 2Levels Trigger a Runaway Greenhouse? Astrobiology, 14(8), 714-731, 2014.

Sherwood, S. C., & Huber, M. An adaptability limit to climate change due to heat stress. Proceedings of the National Academy of Sciences, 107(21), 9552-9555, 2010.