Response to the comments of reviewer #1 on the manuscript, entitled 'Estimations of climate sensitivity based on top-of-atmosphere radiation imbalance' by Lin et al. submitted to ACP

First of all, the authors thank the reviewer's positive comments. The text below with *italic* fonts represents the original reviewer's comments, while those with normal fonts are the point-by-point response from the authors.

I. Overall Comments

The manuscript tries to estimate climate sensitivity, which is specifically expressed by "heat transport coefficient for deep ocean" and "system memory". The method seems sound and the development seems correct. It may be potentially important on this subject after some important details are clarified. In addition, some expressions need to be improved so the readers can more easily understand the methodology and appropriately appraise the results.

R: Thank the reviewer's encouragement. In current revision, the authors have clarified all details requested by the reviewer in the specific comments. Also, the expressions that may cause confusions for readers have been improved by adding extra explanations and clear definitions. These improvements can be found in the response to the specific comments below.

II. Specific Comments

1. P. 24734, l. 2. Since you have used "time constant", clarify "climate response time scale": is it just time constant, or you have different meaning here? If it is the latter give a clear scientific definition.

R: The reviewer is correct. It is the time constant. We have changed it to 'The time constant of the climate system …'.

2. P. 24735, l. 22. Through the text, F is TOA net radiative flux so it is better to define F more specifically when you first introduce it here: either TOA net flux or radaitive forcing for the whole atmosphere-earth system, etc.

R: There may be some confusion here. As the original statement said, the F is defined as the radiative forcing, not the net radiation. The global temperature change, on the other hand, is depended on both the forcing and the feedback, as shown in Equation 1. The combined radiation of the forcing and feedback would be the net radiation. In order to reduce the confusion, we add an explanation here to clearly differentiate the forcing with the net. The text reads: 'A combination of the forcing and feedbacks represents the net radiation of the climate system that decides the change of global mean climate.' .

3. P. 24736. (1) l. 3. Give the reference for "any small temperature perturbation would cause at least -3.3 W/m^2/K...". (2) l. 11. Please quantify "short-time scale", which is important in

understanding your development. (3) l. 20. Give the reference for -6 W/m^2/K. (4) l. 21. Scientifically defining "fast radiative heat release processes".

R: There are multiple questions here. We answer them separately.

(1) The 3.3 $W/m^2/K$ parameter of the TOA thermal emission (E) change rate can be easily estimated from the outgoing Earth's blackbody radiation. Since the mean global TOA emission $E = \varepsilon\sigma T_s^4 = 239$ W/m^2 (where ε and T_s are the effective planetary longwave emissivity and surface temperature, respectively), the change rate of E in T_s would be $\Delta E/\Delta T_s \approx 4\ \varepsilon\sigma T_s^3 = 4\ \varepsilon\sigma T_s^4/T_s = 4\ E/T_s$. Taking global mean surface temperature 288K, $\Delta E/\Delta T_s$ results in 3.3 $W/m^2/K$. Anyway, to keep the paper in reasonable length, we are not planning to explain the details of deriving this value and only have added a reference (Schwartz, 2007) in current revision as suggested by the reviewer. Note that Schwartz used $\Delta T_s/\Delta E$ that equals to $(\Delta E/\Delta T_s)^{-1}$ or 0.3 $K/(W/m^2)$ in his discussions.

(2) We have defined 'short-time scale' as the time scale shorter than decade. This revision has added a phrase 'shorter than decade' here.

(3) We have put a reference (Spencer and Braswell, 2009) in this version.

(4) Since we do not know exactly what those fast radiative heat release processes are, we have changed the phrase to 'other negative feedbacks'. The new phrase more actually reflects observational results.

4. P. 24738, ll. 5-7. You assume that O is proportional to F, but Eq. (3) is not precisely so. Please more clearly define O. This is also important in your method development.

R: As the response for the second specific comment, F is NOT the net radiation. From the manuscript, we know that O is proportional to the net radiation as the reviewer indicated here. Thus, the equation is correct. Because we have clarified F and net radiation before (also see the response to the comment 2), we do not change anything here.

5. P. 24739, ll. 1-10. Your development here implies that Cp' = constant. It needs supporting materials.

R: Yes, we implied that Cp' is a constant in individual calculations. Since this Cp' value is basically only dependent on the mixed layer depth and deep ocean heat transport coefficient, we have added: 'This Cp' value is assumed to be a constant once both the mixed layer depth and ocean heat transport coefficient are specified in our simulations.' These added materials clarify the issues discussed here.