Reviewer #1 (Comments to Author):

I insist my opinion that this MS is scientifically wrong and cannot be published. Here is my response to the author's reply.

1. From opinion on the S-B relationship, I can interpret that the author is not really understanding radiation. No matter climate physics or climate model, the S-B relation is intrinsic in the radiation budget. The linear relation of Pyle et al., 2016 is only valid when the change of temperature is very small to its own value, say ~1% meaning 3 K change for a 300 K climatology. For the error range in this paper, if 30 K error appears in the change, it is already 10% of the climatology, for which lamda is never a constant. For Plank feedback, the strongest negative climate feedback, the calculation should be based on power 4 of temperature, and lamda is propositional to power 3 of temperature.

2. Again, the error in climatology cannot be directly counted for change. It should be as the similar percentage in magnitude. The dimension of +-4 W m-2 year-1 is not right, but should be +-4 W m-2, since it is calculated from 20-yr means but not 20-yr trend. The author lacks basic knowledge on averaging. Thus, the error in climatological LWCF is not the error in its trend, which should be at similar percentage of calculated LWCF trend as the percentage of the error in climatological LWCF. The wrong conclusion of the author comes form that he use +-4 W m-2 as error in trends, because he artificially gives it a year-1 in unit.

3. The source of this fault is that the author is always confusing about change and climatology. He does not know the definition of cloud forcing, as in the following link:

## https://en.wikipedia.org/wiki/Cloud\_forcing

Note that this term here is for climatology, not for change, as represented by cloud feedback. The author insist that his term "forcing" only represents radiative change during global warming against my reminder about the confusion of his usage, just because he did not correctly understand the term used in literature.

4. About model tuning. As I know, tuning in climate models is not to deal with errors in such a big magnitude, which is intolerable. All literatures the author cited is to deal with error in magnitude of ~10%, but not as huge as one that can be cumulated as ~100 times. No one can tune a nonsense model to be close to observation, not even with data assimilation. The evidence to reject my point is invalid here.

5. Again, the major problem of this paper is that the author misused a stable model error of ~10% as an error in trend, due to his artificially adding a unit of year-1. His understanding of error propagation and accumulation starts from this wrong foundation, and goes nowhere.