Reply to reviews of Cook et al.

Review # 1

Comment #1: *The authors suggest that rainfall in the southern part of the Amazon is decreasing while in fact has been increasing (Marengo 2004-TAC, Satyamurty et al 2009-TAC).*

Response: We did not suggest that rainfall is currently declining in the SAB, but only that it is projected to decline in the future.

Whether the recent trend is consistent with model projections for the future is a complicated question. Because of natural variability, there is no reason to think that short-term trend should be the same as long-term projection. Of course, model projections could be wrong. Such issues are beyond the scope of this work which analyzes model projections and the underlying mechanisms.

Comment #2: *The authors also suggest that various models show the die back of the Amazon by middle 220500s, when in fact only one or two global models do so. Mahli et al (2009) made a review of the knowledge of the die back, and they show that considering the projections from the IPCC AR4 global models, what could happen in the future may be more of a change in the vegetation type, perhaps a seasonal forest, rather than savanna type vegetation as shown by the HadCM3 model. Some of the findings in MAhli et al (2009) are similar to this paper by Cook et al (2010), I would like to see something new from the later as compared to the former.*

Response: We find that the core of the Amazon will largely be intact and there is no dieback there. We do find risk of vegetation degradation in the southern Amazon and central Brazil (SAB), for which we actually avoided using the term 'dieback'. While HadCM3 model is an end member, most other models also show drier dry season and loss of vegetation (Fig. 9c).

While Malhi (2009) used a discretized rainfall-vegetation classification to determine change in vegetation types, our model vegetation distribution is continuous in fractional cover so they are not directly comparable. Although we can not say if LAI decreases in our model (Fig. 9b) match exactly Malhi (2009) for E. Amazon, the change of 0.5-1 in LAI does not contradict their finding of a possible transition to seasonal forest.

Thank you for pointing out Malhi et al (2009, PNAS): We were not aware of this work! Our work was conducted independently over the last 4 years. To the extent that Malhi et al. showed precipitation change in the IPCC models, their results are consistent with our analysis. Major new aspects of our work include: (1) Our focus is on the southern Amazon and central Brazil (SAB), while Malhi (2009) focuses on eastern Amazon (EA), while we both find western Amazon wetter, thus the two work largely complement each other; (2) Our work went into much more depth by emphasizing the mechanisms of change such as linking it to a global scale subtropical drying, as well as identifying the sub-regional differences; (3) We delineate the dry vs. wet season nonlinearity in the hydro-ecological response, which is generally not well studied; (4) The models used by Malhi used the emissions scenario A2, while ours are for A1B. We hope the revised manuscript has better highlighted these findings while discussing Malhi (2009).

Comment #3: *Southern Amazon is a region where predictability is relatively low, and uncertainties are high, due to land surface processes not well represented by the models.It is true that on this region fires occur every year, because the relatively dry season of it (not dry season, since rainfall still falls, but we can considered a month with pp<100 mm as a "dry" month). It is also true that soil moisture (low) is a factor that contributes to the propagation of fire, but more important is the lower atmospheric humidity, strong winds and large incoming solar radiations, that are not considered on the analyses of this paper.*

Response: We agree with you that humidity and wind are key factors in the ignition of individual fires. However, here we discuss 'fire risk', i.e., the probability of fire over long-term climate change, not synoptic weather events that drive individual fires. On such long timescales, the probability is largely determined by soil moisture (which is also closely related to humidity on longer timescales).

Comment #4: *Human influences (deforestation) are not considered in the IPCC AR4 model ruins, so that last statement of the abstract may sound more as speculation than a fact.*

Response: Good point, we have added the qualifier "should deforestation and land use change continue" to the text in order to emphasize that it is speculation.

Comment #5: *What does this statement means: 3. Higher maintenance cost and possibly reduced growth at higher temperature. Maintenance cost of what?, reduced growth of what?*

Response: To be more clear, we changed the text to read "Higher maintenance cost (autotrophic respiration) and possibly reduced photosynthesis at higher temperature".

Comment #6: *I would like to see a discussion of the decrease of the dry season rainfall, which is relatively small in its absolute magnitude. Is the drying corresponds to a lengthening of the dry season by 11 days, as the authors suggested?. DO the analyses included daily data?, I do not see that on the paper.*

Response: Thank you for pointing this out. We used monthly data, so that the 11 days is interpolated. We believe that the direction is meaningful, though the error may be large because of the time resolution so that we can't say its 11 days but not 10 or 12 days. We now added some caveats in the text.