Thank you to the reviewer for their thoughtful comments that will make this a better paper.

The reviewer expresses concerns about the propagation of the boundary condition downstream over the short reach rather than results actually being predictions of the model. In comparing modeled vs. measured temperatures, this appears to be true in the first several nodes of the model however by node 5 or 6 the modeled temperatures clearly deviate from the boundary condition temperatures and share daily peaks closely with measured temperatures. In our revised manuscript, we add the boundary condition to Figure 4 and change it to show temperatures at a node further downstream less influenced by the boundary condition. We also add a statement about this limitation of the study. By and large, this paper evaluates modeled vs. measured temperatures with regards to thermal variability in the downstream portions of the modeled reach, far from the boundary condition where the model is simulating stream temperatures based on modeled physical processes.

Regarding the influence of the PCO on modeled temperatures downstream of its inlet, sensitivity analysis results done on the Parks Creek Overflow (PCO) lateral flow boundary condition described in the Boundary Condition Calibration section (10009 line 14-23) provide a basis for evaluating the model's predictions at nodes 10 and 11 downstream of its inlet.

Description of the channel geometry and how that is represented in the model is added to the revised manuscript. We also clarify and correct our statements regarding drivers of water temperature and correlations between measured and modeled temperatures. Specifically, we clarify that our observation is that the modeled water temperature correlates more closely with solar radiation whereas the measured water temperature correlates better with the air temperature. This is in line, as reviewer #1 pointed out, with the understanding that air and water temperature are both driven by solar radiation and therefore they exhibit similar characteristics, as observed. Yet, the model does not perfectly represent solar radiation. We will be careful to be clear that the correlations do not explain driving mechanisms but instead suggest a weakness in the model's ability to simulate those mechanisms with the heat balance.

Specific Comments:

p. 10002 Introduction - The comments regarding the use of DTS data as input data versus calibration data is confusing. (e.g., line 15, line 11 vs. 18-19). Many of the claims (e.g., line 27-28) are too broad and do not appear to have evidence backing them up.

This was pointed out by Reviewer #1 as well and in our revised manuscript we refocus our paper toward thermal variability of measured and modeled data. We take out claims about DTS data improving calibration and model accuracy and instead focus on and clarify our claim that DTS has value for 'post-processing' model results and understanding thermal variability and complexity of micro-habitats, specifically the role of the PCO on mainstem temperatures.

p. 10006 line 1 - If there was enough macrophyte growth to cover the cable, what were the consequences on mixing laterally in the channel? How did this influence the energy budget? Did this impede radiation from penetrating to the bed? How did this influence the parameter BEDALB calibration?

The macrophyte growth in most of the modeled reach does largely shield the bed from direct solar radiation. BEDALB was increased slightly to 0.3 from 0.25 which refines the model parameter from the original calibration. This is consistent with the role the macrophytes may play since increasing BEDALB has an inverse effect on heat exchange due to solar radiation in the RMS model code (Hauser 2002).

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      Qnsr = net solar radiation available for warming channel
bed   (kcal/m2/s)
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$$= (1 - A_b)(1 - \beta)\exp(-\eta(D - 0.6))Q_{ns}$$

Where A_b is bed albedo.

p.10008 Section 3.3 - Nothing really stated here about calibration. Methods for this are included in the results and discussion.

We have moved the description of the calibration process from the results and discussion section to the methods section per reviewer's comment.

p.10013 line 19-24 Based on one DTS cable placed in the middle of a channel doesn't really provide this either.

We qualify our statement with mention that the use of DTS in our case allows assessment of one-dimensional thermal variability and small micro-habitats. Different setups with the DTS could explore 2D characteristics of these micro-habitats, and we discuss this in our revised manuscript.

References:

Hauser, G.E. and G. A. Schohl (2002); "River Modeling System v4 – User Guide and Technical Reference"; Report No. WR28-1-590-164; TVA River System Operations and Environment; Norris, Tennessee; May.