We thank both reviewers for their feedback and recommendations for improving the manuscript. We have adjusted the paper to take into account the responses from both reviewers, and a point-by-point explanation of those changes is presented below.

Reviewer 2

General Comments

The work entitled "Atmospheric Precipitable Water and its Correlation with Clear Sky Infrared Temperature Observations" investigates an indirect retrieval approach for Total Precipitable Water based on zenith sky temperature measurements using low-cost infrared thermometers. The approach used by the authors is interesting and scientifically accepted. I appreciate the general flow of the paper; the technique is described in detail and the topic is suitable for the scope of the journal.
I therefore recommend that the manuscript is accepted for publication after the following minor changes.

Specific Comments

A) Please further emphasize the novelty element of this work with respect to the previous related one, both at the end of the Introduction and in the Conclusions.

> Reviewer #1 had a similar suggestion, and we have addressed this issue in both introduction and conclusion

B) I am concerned about the TPW dataset, given the large distance between the measuring sites. Could you add any other source, closer to the site of interest?

> We have investigated other measurement sites (SUOMINET and AERONET, please see response to reviewer #1) that utilize a variety of different measurement techniques and have found that the NWS radiosondes are the most reliable sources of data available. We have added a discussion in the manuscript and a figure in the appendix to further address this issue.

C) Moreover, while I do not know if this is feasible and meaningful here, I reckon that separating your dataset into training/test subsets would be beneficial for this work, so that you could evaluate the fit on an independent dataset via the standard statistical analysis, hence improving the overall quality of this paper.

> We have implemented an 80/20 training and testing split into our analysis. Our new figure 4 includes the best-fit line generated by the training set and our analysis looks

into the results of our testing set to evaluate the fit. We have updated the appropriate sections to reflect these changes.

D) I wonder whether the –50 degrees instrument threshold has not been too strict a limit in this work and related measurements. Please add a few statements explaining why this has (not) been a limiting issue in your work.

The -50 degrees threshold is a limitation built into the sensor itself, not something we have control over. The paper has been revised to clarify this point. Fortunately, we have not seen a significant number of AMES measurements exceed the instrument threshold so this has not been a major concern.

E) Finally, I believe this is a paper on the retrieval of Integrated Water Vapor (IWV), since all measurements are in clear sky. If so, I would suggest rephrasing through the whole manuscript.

Per the recommendation of reviewer#1, we have updated the paper to use the term Precipitable Water Vapor (PWV) rather than Total Precipitable Water (TPW) or IWV.

Technical Corrections

Line 5: "We have analyzed this relationship: what relationship are we talking about? Please amend accordingly.

This refers to the relationship between zenith clear sky temperature and PWV. The paper has been revised to clarify this point.

Line 10/11: "but with parameters that are different than those obtained for the Gulf Coast". What are you referring to? Please add detail.

We were referring to the North American Gulf Coast (Texas), the location of the Mims et al measurements. We have updated the document to explicitly state this.

Line 36: I suggest replacing "Under clear skies that are the focus of our work" with "In clear sky (the focus of this work),"

Change implemented in revised manuscript.

Line 51: Please provide an adequate number of references for each method.

We have added citations to this section of the manuscript.

I suggest to completely remove the TE1610 sensor discussion from the paper, as I understand it has been no use for this work.

Discussion of the TE1610 sensor has been removed.

Line 174-175: I find this statement redundant, and overall, I am not expecting to see any type of results from the FLIR instruments, given that you decided not to include any. ***Again, in line 195,*** *I guess there's no need to mention it. I suggest keeping only the discussion about instruments/dataset effectively used in the end, as this would make the work neater and improve its flow. However, the discussion in the appendix is just fine, as it "proves" the reason why FLIR3 was not used.*

Discussion of the FLIR has been condensed to reflect the reviewer's comments.