Response to Reviewer Comment 2
(https://doi.org/10.5194/egusphere-2022-986-RC2)

Original review in ITALICS.
Author Response in bold.

A review of "Evaluation of a wind tunnel designed to investigate the response of evaporation to changes in the incoming longwave radiation at a water surface. I. Thermodynamic characteristics" by Michael L. Roderick, Chathuranga Jayarathne, Angus J. Rummery, and Callum J. Shakespeare.

In this manuscript, the authors describe an experimental setup for isolating the effect of longwave radiative flux on evaporation from a water surface. In short, the setup includes a wind tunnel containing a small water bath, with embedded sensors for measuring the humidity of the air and the temperature of the air and water. Additionally, incoming longwave radiative flux was measured via pyrgeometer and the water skin temperature was measured via microbolometer. The room containing the setup was described as being "temperature-controlled", though the cooling/ventilation system operated beyond the control of the authors, producing a noticeable oscillation in ambient temperature and humidity.

The topic as described by the authors is certainly of interest to the readership of AMT, and the manuscript was written with clear language. However, I have major concerns with some core elements of the laboratory setup. Furthermore, I found it difficult to assess the importance of this manuscript as an independent piece of research. The whole project is motivated by a desire to investigate the specific impact of longwave radiative flux on evaporation, but the details of the radiative component are left to the (as of yet unseen) part 2. I don't know that part 1 stands on its own as a meaningful contribution- is the result that bulk water temperature is sometimes close to the wet bulb temperature of the air above it? In any case, I believe that the work the authors have done can contribute the the body of knowledge, but I strongly recommend that they significantly revise this manuscript. Without seeing the second manuscript, I can only recommend that the revision to part 1 should include more details regarding the radiometric measurements (and results related to the total heat budget calculations). It may be that such a revision would combine the two parts- or that new radiative measurements need to be made with higher quality instrumentation, but it's impossible to say having only seen part 1 of the work.

We thank the reviewer for the careful and helpful review.

Both the second and third major comments made by the reviewer were good points that related directly to the radiative measurements that were the subject of a second (radiative) manuscript. We were able to adequately address the reviewers concerns using results taken directly from the second (radiative) manuscript.

The review comments raise a more general point: why not combine the manuscripts? For relevant background, we submitted two manuscripts to the AMT journal (titled Parts 1 and 2 in September 2022). However, only Part 1 (Thermodynamics, egusphere-2022-986) has appeared as a preprint on the journal website. For the Part 2 (Radiative, egusphere-2022-988) manuscript, an associate editor has been assigned but we have not received a preprint and it has not yet been sent for any form of review as far as we are aware. We note that the associate editors for the two articles are different. We had anticipated that both articles would be handled by a single associate editor and sent simultaneously to the same reviewer/s. This would have been helpful in this case since the 2 of 3 major concerns by the reviewer related to content in the second manuscript.

In response we will independently contact the editorial staff to discuss options to combine both manuscripts into a single manuscript.

Major Concerns

The room that was described as "temperature-controlled" showed a regular oscillation of ambient air temperature and specific humidity (approximately 1000 second period). This has a meaningful impact on what should be a sensitive measurement. To this point, the oscillation in incoming longwave radiative flux appears to track (in both shape and phase) the oscillation in humidity- not temperature. Does this mean that radiative flux sensed by the pyrgeometer is representative of more volumetric (path) absorption/emission in conditions of elevated humidity? In any case- unless this periodic behavior can be leveraged as an asset in the heat flux balance calculations (i.e., the longwave radiative flux is the only oscillating flux, allowing the authors to parse its effect on the evaporation), I believe it will be a major liability in the calculations.

Close examination shows that the oscillation in longwave ($R_{i,F2}$ in Fig. 5e) due to the oscillation in the laboratory temperature control closely tracks the laboratory air temperature (T_L in Fig. 5a, and equivalent to Black body at T_L in Fig. 5e) and not the specific humidity. Hence the variations in specific humidity (e.g. q_L in Fig. 5c) have no measurable impact on the longwave radiation.

The variations in specific humidity do follow the variations in laboratory air temperature but we know that the specific humidity has little impact on the radiative flux. We know that because at any given instant of time, the water vapour is more or less at the same temperature as the bulk air and the walls of the room (= T_L). Some of the (near blackbody) radiative flux emitted by the walls at T_L would be absorbed by water vapour (also at T_L). The effective optical path length will change with the specific humidity as noted by the reviewer and the absorption will increase. But the absorption and re-emission by the water

vapour occurs at the same temperature as the air and the walls of the room and this does not materially alter the flux. There is an in-depth explanation of the physics in the second radiative manuscript. With that, we note that the temperature-controlled room behaves as a black body, all be it, with an oscillation induced by the heating system.

We handled the oscillation by taking the steady state period to be integer multiple of the oscillation period (as described in lines 286-288). As a consequence the standard deviations we reported (Figs 7a, 7c, 8d) are overestimates. This is discussed in detail in the manuscript (lines 433-450). We can extend that discussion here by using results from the second radiative paper which show an overall uncertainty of around 2 to 3 W m^{-2} in estimating the incoming and outgoing longwave fluxes at the water surface. Hence any residual effect of the oscillatory behaviour on the evaporation or longwave forcing is small relative the imposed longwave forcing at the water surface of 50 W m^{-2}.

Since the thin film-covered window only occupies a small segment of the hemisphere above the pool, how will the authors account for the difference between radiative flux emanating from the walls of the room and the flux emanating from the inside of the wind tunnel? Would a view factor (or some sort of solid angle accommodation) be helpful in accounting for this difference? Regardless, I'm not certain that a hemispheric pyrgeometer is the ideal sensor for this type of indoor, spatially heterogeneous measurement.

This is a perceptive comment. In response that is exactly what we did. We show below Fig. 10 from the second (radiative) paper.

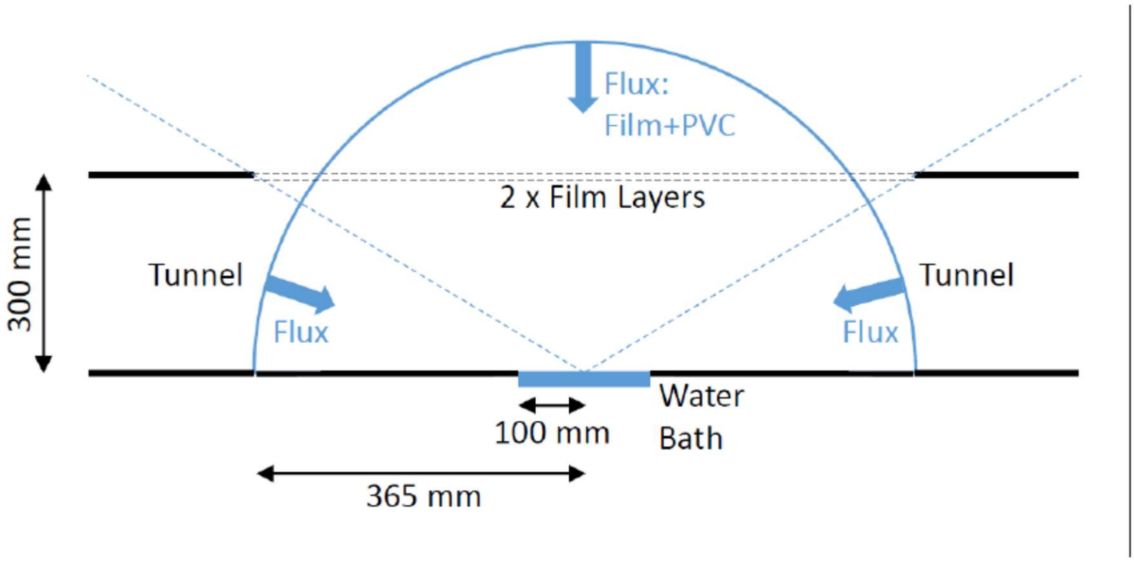

Figure 10: Schematic drawing showing separate contributions to the incoming longwave radiation at the water surface. The diagram is a cross section along the centreline of tunnel showing the hemispherical geometry used to estimate the incoming longwave radiation at the water surface arriving from the tunnel, film and PVC frame.

To model the incoming longwave flux at the base of the tunnel we developed a theoretical model (see Fig 10 above, taken from the second radiative paper) that separately accounted for the longwave fluxes from the inside of the tunnel and from the room. The theoretical model was then evaluated using hemispherical measurements made using the radiometer located at the same position as the water bath. The theoretical model was found to predict the incoming longwave at the water surface with an RMSE of 2.2 W m^{-2} (see Fig. 11b below, also taken from the radiative manuscript).

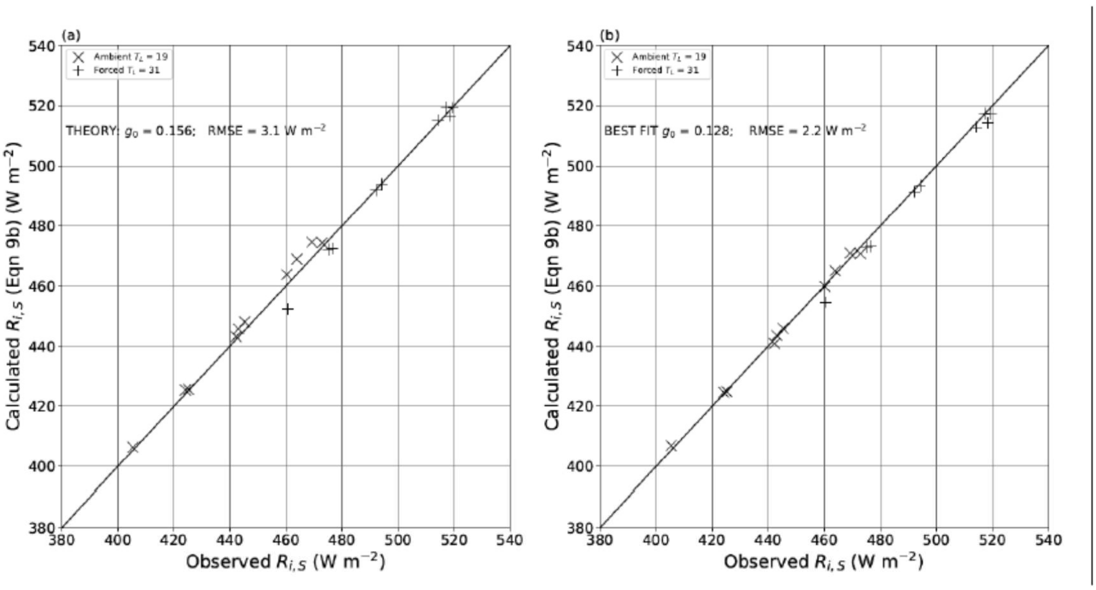

Figure 11: Comparison of theoretical and observed incoming longwave radiation at the water surface. (a) Uses g0 = 0.156 as per theory (Linear regression: y = 0.9855 x + 7.3, R2 = 0.991, RMSE = 3.1 W m-2, n=20). (b) Tuned to locate the value of g0 (=0.128) with the best fit (Linear regression: y = 0.9772 x + 9.7, R2 = 0.997, RMSE = 2.2 W m-2, n=20). Full lines are 1:1.

I mentioned the need for more information about the radiometry. It strikes me that measurements of the skin temperature are missing here. But even if they were included- can the authors make a case that the FLIR E50 is up to the challenge of providing high-quality radiometric measurements? It appears to be a handheld system that is optimized for qualitative evaluation of heat sources in industrial/construction use cases. If the authors are able to provide the results of blackbody calibration to establish the instrument's accuracy, stability, and low noise, that might put these fears to rest. But non research-grade microbolometers are notorious for being poor in those categories and are exceptionally prone to drift (which might be the worst type of error one could have when making the "steady state dis-equilibrium" measurements described here). A cooled single-point infrared radiation thermometer is usually regarded as the superior instrument for these sensitive measurements.

The FLIR E-50 model has a nominal temperature resolution of 0.05 degC. To test the accuracy, precision and overall repeatability, we constructed a black body (a copper plate covered with black paint of emissivity 1) and by varying the temperature of the plate we could compare the flux measured with the FLIR E-50 with that measured by the Kipp and Zonen CNR1 net radiometer under a variety of conditions. We found the FLIR E-50 measurements to be repeatable and the recorded flux was the same (within error tolerances) as that of the research grade Kipp and Zonen radiometer.

As noted by the reviewer, the surface temperature measurements were missing here because they are included in the second radiative manuscript.

Minor Comments

- *For most parameters, variability is represented via standard deviation (or 95% confidence intervals). However, several quantities (ambient air temperature, ambient air humidity, incoming longwave radiative flux) oscillate with the room's cooling system. Perhaps the authors could report the amplitude of oscillation for these quantities?*

Good point. For the variables mentioned (air temperature (T_L) and specific humidity (q_L) in the laboratory, incoming longwave flux at the top of the film ($R_{i,F2}$)) we have reported the standard deviation (σ) as the reviewer noted. The oscillation was the main source of (temporal) variation (see lines 287-288). With that, we note that the standard formula (for a sine wave, $Amplitude = \sqrt{2}\,\sigma$) can be used to estimate the amplitude.

- *The variation in enthalpy (Figure 9) is computed via temperature difference between the beginning and end of steady-state period. What could be learned from performing multiple short-window linear least-squares regressions during the steady state period, thereby obtaining a LHF estimate for each subwindow?*

Good point. We tried this during our initial experiments and found the "noise" became too great over short time intervals in the LE (what the reviewer called LHF, latent heat flux) and also in the enthalpy change in the water bath (what we called *G*). Hence we were obliged to calculate the steady state values of the fluxes using longer periods that ranged from 850 (i.e., ~ 14 minutes) to 3300 (i.e., 55 minutes) seconds as described on line 442 of manuscript.

- *The Kipp & Zonen radiometer is said to be located 'in the laboratory (but outside the tunnel)' during evaporation experiments. Could this be pointed out in Figure 2?*

During routine evaporation experiments the Kipp and Zonen radiometer was located inside the cardboard box sitting on the top of the wind tunnel (to the left of the number 3 in Fig. 2 as shown below). This was done to avoid thermal interference. For example, if the radiometer was sitting on top of the tunnel in the open, it would respond to the body heat of staff members as they walked past. Shielding the radiometer by placing it inside the cardboard box proved a simple solution that removed thermal interference.

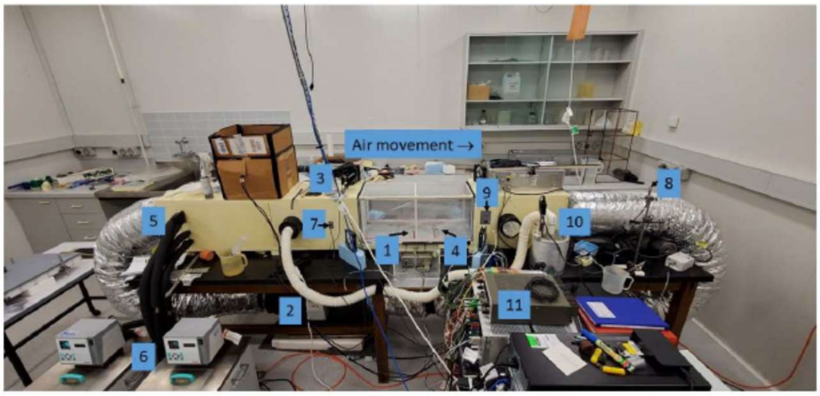

Figure 2: Photograph of the wind tunnel in the temperature controlled room of the Geophysical Fluid Dynamics Laboratory. Key numbers as follows: [1] Water bath and digital balance (AND Corporation: Model GX-6100); [2] Variable speed fan; [3] Thermal camera (FLIR: Model E50); [4] Camera Spot (used for thermal camera calibration); [5] Radiator (for air temperature control); [6] Constant temperature water bath (Julabo: Model PP50); [7] Humidity/Temperature sensor (for measuring tunnel air, VAISALA: Model HMP140); [8] Humidity/Temperature sensor (for measuring laboratory air, VAISALA: Model HMP140); [9] Temperature sensor (thermistor for measuring tunnel air, Thermometrics NTC: Model FP07DA103N); [10] Vapour source (humidifier for humidity control of tunnel air); [11] Digital controller.

- *It is a bit taxing to jump between Figure 3 and the body of the manuscript to find the definitions for the state variables. I recommend adding descriptive labels on the figure or more content to the caption.*

We can do that is a revised version of the manuscript.

- *For Figure 3, I think that a hashmark or dot pattern along the solid regions of the tunnel setup would aid the reader in interpreting the setup. Furthermore, arrows or streamlines in the tunnel portion would be helpful.*

We can do that is a revised version of the manuscript.

- *Figure 11 is effective, but the extra information presented in Figure 12 is difficult to digest. Perhaps the authors could establish the concept in Figure 11 (as already done), then replacing Figure 12 with scatter plots that relate Tw, TB (or better, Tskin), and the inferred psychrometer constants.*

This is a problem caused by having two manuscript parts that describe the (i) thermodynamics and (ii) radiation separately.

We have actually done what the reviewer suggested in the second radiative manuscript. We could not do that in the thermodynamic manuscript because the measurement of the surface "skin" temperature had not yet been described. We show Fig. 13 from the second radiative at manuscript below: with T_B (x-axis) the mean bulk water temperature, and T_S (y-axis) the measured surface temperature.

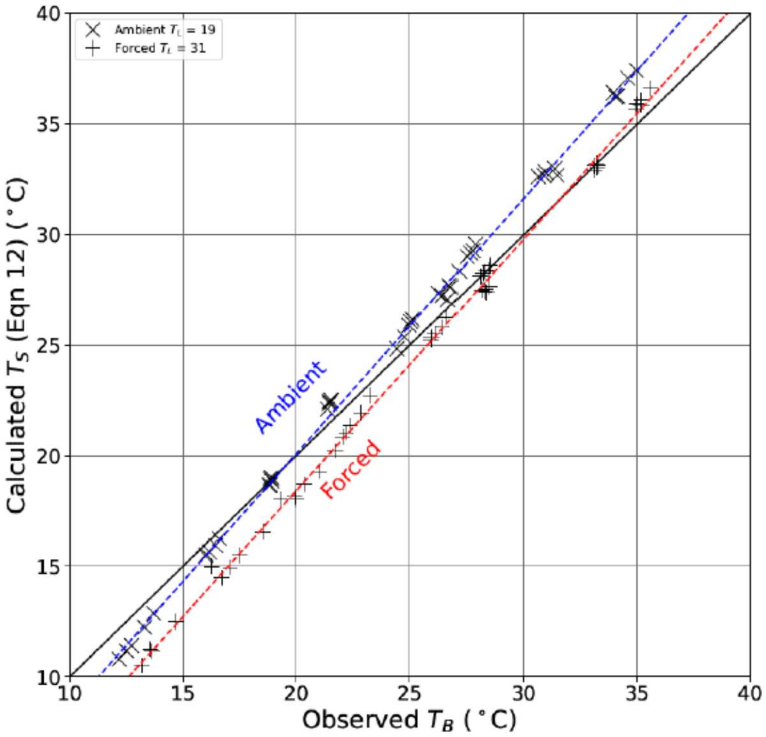

Figure 13: Comparison of observed bulk water temperature (T_B) with calculated surface temperature of the evaporating water bath (T_S) during all evaporation experiments (n = 90). Full line is 1:1. Linear regressions for ambient (blue dashed line, y = 1.155 x – 2.97, R^2 = 0.998, RMSE = 1.3°C, n = 45) and forced (red dashed line, y = 1.139 x – 4.32, R^2 = 0.998, RMSE = 1.3°C, n = 45) conditions also shown.

Michael L. Roderick & Callum J. Shakespeare (on behalf of all authors), 24/3/2023