Response to Reviewer Comment 1
(https://doi.org/10.5194/egusphere-2022-986-RC1)

Original review in ITALICS.
Author Response in bold.

The authors intend to investigate the influence of longwave radiation on evaporation. They claim that because longwave radiation is absorbed in the top 20 μm at the ocean surface this heat source/sink might cause significant deviations of the surface temperature from the underlying water. This effect changes the saturation water vapor pressure at the water surface. Therefore the concentration difference between the water surface and a reference height is influenced and with it the water vapor flux and evaporation rate. They claim that this potentially large effect is completely ignored in bulk formulas for evaporation.

The authors therefore want to study the effect of longwave radiation on evaporation in a wind tunnel specifically designed for this purpose. In the first paper (egusphere-2022-986) they focus on a thermodynamic characterization of the facility, and in a second one (egusphere-2022-986) on the radiative characterization.

The subject of the study is ill-defined and the described facility and instrumentation not really suitable for the intended purpose. Therefore the reviewer recommends rejection of the publication of the two manuscripts.

We thank the reviewer for the time taken to read the manuscript.

The review is clear-cut (e.g., ".. ill-defined ..", ".. not really suitable for the intended purpose ..", ".. rejection ..").

The very brief review does not include an assessment of any of the manuscript sections (2 Design and Operation, 3 Thermodynamic Evaluation, etc.).

Instead the review is based on an assertion that we are not aware of previous work. The (implied) implication is that the basic idea is fatally flawed.

The assertion/s are wrong.

Instead, we are conducting a series of very well-defined laboratory experiments as described below.

The claim that "mass transfer formulations for evaporation … not directly consider the langwave radiative fluxes" is simply not correct. The reviewer did not perform a systematic literature search, but quickly found two almost 30 years old papers, dealing with the subject: Zhang and McPhaden 1995 (https://doi.org/10.1175/1520-0442(1995)008<0589:TRBSST>2.0.CO;2) and Fairall et al. 1996 (https://doi.org/10.1029/95JC03190). The actual version 3.6 of the COARE algorithm *published on zenodo explicity includes longwave irradiation (named there IR flux): Bariteau et al., 2021 (https://doi.org/10.5281/zenodo.5110991)*

We are correct.

To see that, let us examine the first work cited by the reviewer (Zhang and McPhaden 1995). In that work the calculation of the sensible (their Eqn 1a) and latent (their Eqn 1b) heat fluxes are reproduced from their paper as follows:

$$F_h = C_p \rho C_h U (T_s - T) \qquad (1a)$$

$$F_q = L_v \rho C_e U [q^*(T_s) - q] \qquad (1b)$$

With reference to (their) Eqn 1b, this is the classical Dalton-type bulk formula for evaporation that we referred to as the mass transfer formulation in the manuscript. Neither the incoming or outgoing longwave radiation is explicit in the equation. (Note: The cited Fairall et al 1996 and associated COARE reference cited by the reviewer use exactly the same mass transfer formulation (see Eqn 17 in Fairall et al. 2003, J Climate 16: 571-591)). What the cited references do is calculate the evaporation using the mass transfer formulation (e.g., Eqn 1b above). They then combine the latent and sensible heat fluxes with the radiative fluxes to define the surface energy balance. We have done this ourselves on many occasions and this can be readily confirmed by a literature search. We note that this approach is described in standard texts (see references we cited on line 25 and line 40 of the manuscript) which present the same method as in the references cited by the reviewer.

However, we are going well beyond this long-used mass transfer formulation.

Instead, the fundamental basis for our new laboratory-based facility is described in lines 74-76 of the manuscript as follows:

> **"The unique feature is an augmented capability to independently vary the incoming longwave radiation at the water surface whilst holding the other variables fixed. The scientific rationale of this approach was to isolate the effect of a change in the incoming longwave radiation on both evaporation and surface temperature."**

We can explain this statement with reference to Eqn 1b (above). With L_v (the latent heat of vaporisation) and C_e (transfer coefficient) both more or less constant, one can envisage an experimental configuration where ρ, U, $q^*(T_s)$ and q in Eqn 1b were all held fixed. Eqn 1b would predict no change in the latent

heat flux if the incoming longwave radiation was independently varied because the incoming longwave radiation is not directly represented in Eqn 1b.

In our experiments we hold *ρ* (air density), *U* (wind), *q* (specific humidity of air) and *T (temperature of air)* constant. We then vary the incoming longwave radiation and measure how the surface temperature (T_s) and latent heat flux respond. Hence our work is not "*ill-defined*". Instead we are conducting a series of <u>well-defined laboratory experiments</u> to experimentally examine the fundamental basis of the mass transfer approach itself. To our knowledge our work represents the first-ever experiment to examine the fundamental basis of the mass transfer approach. For this reason it is critical to fully document the technique/s - hence our submission to the AMT journal which seemed ideal for this purpose.

The authors are obviously not familiar with the extended research work on the difference between the ocean surface temperature and the underlying bulk water ("cool skin"). Much of the pioneering work was done by Katsaros, see, e. g., Katsaros 1980 (https://doi.org/10.1007/BF00117914) or Katsaros 1990 (https://doi.org/10.1007/978-94-009-0627-3_9). A comprehensive account of the near-surface layer of the ocean is given in the monograph of Soloviev and Lukas 2014 (https://doi.org/10.1007/978-94-007-7621-0).

In response we confirm that we are aware of the extended body of work (especially in remote sensing) on differences between the bulk and skin temperature over the ocean (and over lakes). However, again we re-iterate, we are examining a more fundamental question: the validity of the mass transfer formulation itself.

There are still, of course, open question. Most of them are related to the mechanisms of the transport from the ocean surface down to the bulk water, especially the influence of wind waves. The wind tunnel built by the authors is not suitable to address these questions because of the tiny and shallow water basin. A large wind-wave facility, such as the LASIF at the University of Marseilles (France) would be required for such studies (https://www.osupytheas.fr/?-LASIF-Grande-Soufflerie-air-eau-de-Luminy-&lang=en) and instrumentation and methods to image the water surface temperatures and temperature profiles in the aqueous viscous boundary layer.

We agree with the reviewer that there is much science still to be done on many important topics.

We also agree with the reviewer that the facility we have built is not suitable to address the influence of wind waves on heat transfer into the interior of the ocean. The reason is that this is not the scientific question we are addressing.

The question we address: the validity of the mass transfer formulation for evaporation that has been in widespread use for the last 220 years, is, in our

opinion at least, a very important scientific topic. In that context, the experimental facility we have established represents the first-ever examination of the validity of the (220 year old) mass transfer formulation for evaporation.

Michael L. Roderick & Callum J. Shakespeare (on behalf of all authors), 15/3/2023

Postscript (editorial question):

We submitted two manuscripts to the AMT journal (titled Parts 1 and 2 in September 2022). This is the first review of that work we have received (in March 2023) and the review has recommended rejection of both manuscripts. However, only Part 1 has appeared as a preprint on the journal website. We seek guidance from the editor on the status of the Part 2 manuscript. We have previously been informed that Part 2 was suitable for the journal and that an editor has been assigned but we have not received a preprint and the Part 2 manuscript is not posted on the journal website.