Report to the Editor on manuscript revision

Manuscript ID: egusphere-2026-2024

Title: *cSINGV v1.0: Development and Evaluation of a 1.5 km Coupled Atmosphere–Ocean–Wave Modelling System for the Western Maritime Continent*

Manuscript Type: Development and Technical Paper

Dear Editor,

We thank you and both referees for their constructive assessment of our manuscript. We have revised the manuscript in response to the referees' comments and suggestions. This report identifies the revisions made in response to each comment and distinguishes manuscript changes from additional evidence provided in our responses. Please note that the line references are approximate ranges from the supplied tracked-change manuscript.

Response to Referee 1

General comment

Reviewer comment:

The manuscript describes the UK Met Office regional coupled modelling suite configured for the Singapore region and highlights the performance for the first half of 2019. Two-way coupling improved precipitation and the diurnal cycle over the oceans compared to an atmosphere-only simulation. The focus lies in understanding air–sea interactions and their impact on regional weather, particularly on momentum transfer and large-scale precipitation. The structure of the manuscript could see a few improvements, mainly for readability, clarity, and focus. One major concern is the role of both tuning and evaluation of ocean-only and wave-only components, specifically the wave part for Betamax with a value of 1.75 (Appendix A). It contradicts the primary objective, "the development of a fully coupled system enabling two-way interaction" (line 15–16). The parameter is significantly higher than the Met Office value of 1.39 and is equivalent to that from an ERA5-driven wave hindcast. The Miles–Janssen-type wind input is known to significantly increase wave feedback to the atmosphere through the Charnock coefficient in two-way coupled simulations and reduce surface wind speed. In addition, the manuscript could benefit from a clearer description of the coupled system and its components, to support the results of fully coupled and atmosphere-only simulations presented in Sect. 4. If the coupled system is valid, then surface variables and diagnostics will improve and agree well with observations. On the ocean side, similar concerns as for the waves. "Sensitivity experiments were conducted to optimise configuration choices, including light penetration schemes and bulk forcing formulations, resulting in improved SST representation […]" on lines 156–157 sideline any form of coupling feedback entirely. The total water-side stress is equivalent to the air-side stress only if the wave field is in equilibrium, i.e. the sum of wind input, dissipation and nonlinear interaction is zero. Most of the time, the wave field is not in equilibrium, which means that the air-side stress differs from the water-side stress. Atmosphere–wave–ocean coupler should take care of the flux exchange in the wave boundary layer, i.e. similar to Fig. 8 in Breivik et al. (2015).

General comment of study scope and component development

Comment summary. Clarify the purpose of standalone component tuning and evaluation in a paper presenting a fully coupled system.

Response and manuscript changes. We have revised the objective to distinguish component development, implementation of the coupled system, and initial coupled-system evaluation. The Introduction now states that comprehensive optimisation of all coupling parameters and feedback processes is outside the scope of this study. Section 4 explains that standalone ocean and wave assessments provide baseline checks, helping distinguish component biases from coupling effects. The fully coupled marine evaluation is now presented separately in Sect. 4.3.3.

Location. Introduction, approximately lines 99–107; Sect. 4 opening, lines 310–314; Sect. 4.3.3, lines 496–515.

General comment on wave tuning and atmospheric feedback

Comment summary. Explain the selection of betamax = 1.75 and its possible influence on roughness and surface winds.

Response and manuscript changes. Section 2.4 and Appendix A now clarify that the wave parameters were selected during standalone WW3 development using winds from cSINGV_A and were then held fixed in the coupled experiment. Appendix A summarises the betamax and shallow-water sensitivity results. The added diagnostics examine SST, wind speed, Charnock parameter and latent heat flux differences, while the near-surface evaluation reports comparable wind-speed RMSE in the two configurations. The limitations explicitly acknowledge that betamax has not been demonstrated to be optimal for the coupled system; these comparisons do not isolate its individual effect.

Location. Sect. 2.4, lines 201–205; Sect. 4.3.2, lines 478–486; Sect. 4.5, lines 592–596 and Fig. S17; Sect. 5, lines 669–671; Appendix A, lines 718–723.

General comment on ocean sensitivity experiments

Comment summary. Clarify whether preliminary ocean tuning was used to adjust the coupled response.

Response and manuscript changes. Section 2.3 now explains that the standalone NEMO experiments established a stable and physically credible regional baseline. The selected configuration improved SST representation in deep-ocean and shallow-shelf regions and was subsequently held fixed in the coupled simulation. This separates component configuration from interpretation of the coupled response.

Location. Sect. 2.3, lines 173–178;

General comment on momentum transfer

Comment summary. Clarify the stress pathways and avoid implying that NEMO receives simultaneous atmospheric and wave-derived stress.

Response and manuscript changes. The revised coupling description states that the atmosphere supplies 10 m wind components to WW3 and WW3 supplies NEMO with the stress transmitted to the ocean after accounting for the evolving wave field. A new Table 1 lists the exchanged variables and directions for cSINGV. Figure 2 has been replaced. Our referee response

(https://doi.org/10.5194/egusphere-2026-2024-AC2) additionally explains that direct atmospheric stress is used only in the atmosphere–ocean configuration without an interactive wave component.

Location. Sect. 2.5, lines 223–229; revised Fig. 2; Table 1 near line 240.

Specific comment 1

Reviewer comment:
Line 174: "wind field and current interpolation". Interpolation is performed within OASIS-MCT using remap weights. This setting will break the wave model because it does not know the future time step to establish linear regression.

Response and manuscript changes. Section 2.4 now clarifies this. Wind and current interpolation are not enabled for the coupled model (use WNT0 and CRT0). The spatial remapping between component grids using precomputed OASIS3-MCT weights.

Location. Sect. 2.4, lines 195–199; Table S2.

Specific comment 2

Reviewer comment:
Line 185: "The atmosphere, ocean, and wave components […] exchange variables at an hourly coupling frequency". What is the reason for hourly intervals? Convective-scale processes could benefit from a higher coupling frequency.

Response and manuscript changes. We now describe hourly coupling as a practical compromise among computational cost, exchange overhead and representation of evolving air–sea interactions. The text refers to previous Maritime Continent work and explicitly states that the interval is a design choice rather than a demonstrated optimum. Shorter intervals are identified as potentially useful for rapidly evolving convection and high-frequency interactions, and coupling-frequency sensitivity is included in future work.

Location. Sect. 2.5, lines 215–219; Sect. 5, lines 670–671; Sect. 6, line 694.

Specific comment 3

Reviewer comment:
Line 191: "the coupling architecture and exchange variables" appears to double up on stresses received by the ocean. NEMO is subject to stress from both the atmosphere and the waves. It is unclear what wave fields are supposed to mean.

Response and manuscript changes. The revised text and Table 1 now clarify the WW3-to-NEMO fields. The stress pathway is explained in Sect. 2.5 in detail.

Location. Sect. 2.5, lines 226–229; Fig. 2 and Table 1.

Specific comment 4

Reviewer comment:
Section 4.1. What is the purpose of an ocean-only evaluation? The title of the manuscript is about the development of a fully coupled modelling system.

Response and manuscript changes. The opening of Sect. 4 now explains that the standalone evaluations establish whether the ocean and wave components reproduce observed SST and significant

wave height adequately before their fields are used interactively. It also explains why component biases could otherwise be mistaken for coupling errors or benefits. The new coupled evaluation tests whether marine fidelity is maintained after coupling.

Location. Sect. 4, lines 308–314; Sect. 4.3.3, lines 496–515.

Specific comment 5

Reviewer comment:

Section 4.2. There appears to be a threshold in the Indian Ocean that the wave model cannot represent swell waves greater than 2 metres. It appears to be a feature coming from ERA5.

Response and manuscript changes. Our Referee 1 response (https://doi.org/10.5194/egusphere-2026-2024-AC2) provides a six-month time series of regional maximum significant wave height showing values above 2 m, with maxima approaching 4 m in June (response Fig. R2). This evidence rules out an intrinsic 2 m ceiling in total significant wave height. It does not establish the fidelity of remotely generated swell, because only combined significant wave height was archived and partitioned swell and wind-sea diagnostics are unavailable. In the supplied manuscript, Sect. 6 identifies remotely generated swell as a topic for future sensitivity work.

Location. Referee 1 response, specific comment 5 and Fig. R2; manuscript Sect. 6, lines 694.

Specific comment 6

Reviewer comment:

Lines 94–95: "Through evaluation against observational and reanalysis datasets, …". This sentence reads like a summary or highlight and does not belong in the objective statement.

Line 192: "The development of this fully coupled system represents the main technical contribution […]", in the wave model configuration, is the main objective of the paper and should appear at this point.

Response and manuscript changes. The revised Introduction defines development of the kilometre-scale coupled system as the primary technical contribution and provides the initial evaluation objective. The former results-style sentence beginning "Through evaluation against observational and reanalysis datasets" is deleted from the objective paragraph. The technical-contribution statement is removed from its former position in the coupling-framework description and added to the introduction.

Location. Introduction, lines 100-104; Introduction lines 108–109 and Sect. 2.5, lines 224–226, where the former text are marked for deletion.

Specific comment 7

Reviewer comment:

Lines 124–126 are riddled with acronyms. Why list all the applications for data assimilation, ensemble prediction, climate, and coupled configuration here? It is distracting for the reader and has no relevance to the manuscript's objective.

Response and manuscript changes. The paragraph listing the data-assimilation, ensemble, urban and climate applications has been deleted. Section 2.2 retains the atmospheric and land-surface details relevant to the present configuration.

Location. Sect. 2.2, lines 137–140, marked for deletion.

Specific comment 8

Reviewer comment:
The paragraph on lines 315–321 appears better positioned in the Introduction and for the justification of the objective.

Response and manuscript changes. The material explaining the importance of WMC precipitation and the mixed impacts reported in earlier coupling studies is now in the Introduction.

Location. Introduction, lines 93–98; Sect. 4.3.1, lines 390–394, showing deletion of the former motivation paragraph.

Specific comment 9

Reviewer comment:
Line 325–326: "Differences [in daily accumulated precipitation] between cSINGV and cSINGV_A are relatively small in this representation, and no clear systematic impact of coupling is evident at this scale." It is quite difficult to read the diagram and tell how different the simulations are from the observations. With a biased distribution, one could explain the drop in fractional skill score in Fig. 6.

Response and manuscript changes. Section 4.3.1 now describes Fig. 5 as a qualitative representation of temporal and longitudinal rainfall evolution. A daily precipitation-bias diagnostic is cited as Fig. S6. The domain- and period-mean wet bias is +0.96 mm day^{-1} in cSINGV and +0.55 mm day^{-1} in cSINGV_A, a difference of +0.41 mm day^{-1}. The text acknowledges that the coupled configuration has a larger overall mean wet bias despite locations and periods with reduced bias.

Location. Sect. 4.3.1, lines 399–405; Fig. S6.

Specific comment 10

Reviewer comment:
The diurnal cycle of precipitation in Fig. 7 lacks units and has inconsistent scales. There appears to be significantly higher precipitation over land at noon, regardless of coupling, which is surprising, since about three-quarters of the domain is ocean and one-quarter land. Is this due to the land surface configuration RAL2-T?

Response and manuscript changes. The replacement Fig. 7 adds precipitation units (mm h^{-1}). The discussion links the land-wet/ocean-dry pattern to previous SINGV-RCM findings. The experiments do not isolate the role of RAL2-T or its land-surface component. The replacement figure retains different vertical-axis ranges for land and sea because the values differ substantially. Using a common scale would obscure the variability and make the figure less informative. This approach is consistent with that used in previous published studies (e.g., Prasanna et al., 2025, their Fig. 7).

Location. Sect. 4.3.1, lines 445–447.

Specific comment 11

Reviewer comment:
Figure 12 shows the mean differences in latent heat flux and their decomposition into components: wind-speed contribution and moisture-gradient contribution. The coupled simulation indicates an increase in the upward flux of about 10–15 W/m^2. The decomposition into the two components highlights regional differences: wind speed confined to the Indian Ocean and moisture in the South

China Sea. It would be interesting to the audience whether reduced fluxes arise mainly from weaker surface winds or weaker sea surface temperatures. For example, additional information showing differences in surface winds, Charnock coefficient, and sea surface temperature would help clarify. From a wave-field perspective, on the other hand, it looks like wind-sea contribution in the South China Sea versus swell contribution in the Indian Ocean. Clarification on this would greatly support the authors' claim for improved air–sea interaction in the region.

Comment summary. Relate latent heat flux changes to winds, SST and Charnock parameter, and avoid unsupported wind-sea or swell attribution.

Response and manuscript changes. Section 4.5 now compares January–June mean differences in latent heat flux, SST, 10 m wind speed and Charnock parameter, citing Fig. S17. It describes widespread enhanced latent heat flux broadly coinciding with warmer SST, with smaller and more spatially heterogeneous wind changes. The interpretation is qualified: spatial correspondence alone cannot isolate the individual contributions. Our response explains that significant wave height alone cannot distinguish wind sea and swell, so the flux differences are not attributed to partitioned wave regimes.

Location. Sect. 4.5, lines 592–598; Fig. S17.

Response to Referee 2

Reviewer comment 1

The uncoupled run uses SST prescribed and updated daily from OSTIA, while the coupled run has hourly two-way SST feedback. Since much of the paper's argument rests on diurnal-scale processes (nocturnal offshore propagation, diurnal LHF), how much of the coupling benefit could simply reflect the difference between daily-updated and sub-daily-evolving SST, independent of two-way coupling per se? Authors can add an explicit paragraph in the Discussion or Limitations acknowledging this confound and, using only existing model output, show the actual sub-daily SST variability produced by cSINGV relative to the daily-fixed OSTIA field, so readers can judge the plausible magnitude of this effect?

Comment summary. Acknowledge the SST-frequency confound and quantify the subdaily variability in cSINGV.

Response and manuscript changes. Section 4.3 now explicitly contrasts daily-updated OSTIA SST in cSINGV_A with hourly evolving SST in cSINGV. The mean daily SST range, calculated from the 24 hourly maxima and minima and averaged over January–June 2019, is reported as typically 0.5–0.75 °C over the open ocean and above 1 °C in some shallow and coastal waters. Figure S5 is cited. The manuscript states that the differences cannot be attributed solely to two-way coupling. The conclusions repeat this limitation and identify an atmosphere-only run forced with hourly cSINGV SST as a possible isolating experiment.

Location. Sect. 4.3, lines 373–381; Fig. S5; Sect. 5, lines 660–663.

Reviewer comment 2

cSINGV_A uses a fixed Charnock parameter of 0.011, while cSINGV derives surface roughness from the WW3 wave state. Authors can diagnose, from the existing coupled output, the effective or implied Charnock parameter or roughness length produced by the wave-dependent formulation over the domain

and compare its range with the fixed value of 0.011? This would allow readers to assess whether the two configurations differ sufficiently in roughness to explain part of the flux and wind differences. This caveat should also be stated explicitly in Sect. 4.3 rather than left implicit.

Comment summary. Compare the wave-dependent Charnock parameter with the fixed value of 0.011 and explain its possible contribution to flux differences.

Response and manuscript changes. Section 4.3 now explicitly states that cSINGV_A uses a fixed Charnock parameter of 0.011, whereas cSINGV receives an evolving value from WW3. It acknowledges that the resulting roughness differences can affect winds and turbulent fluxes. Section 4.5 cites Fig. S17 for the Charnock differences alongside SST, wind and latent heat flux differences. The conclusions identify roughness as another factor that cannot be isolated from the present configuration comparison.

Location. Sect. 4.3, lines 381–384; Sect. 4.5, lines 592–598 and Fig. S17; Sect. 5, lines 663–665.

Reviewer comment 3

Figures 6, 9, and 10 report differences between cSINGV and cSINGV_A qualitatively. Using the existing daily time series already computed for these figures, could the authors apply a simple paired significance test, such as a sign test or day-by-day bootstrap of the RMSE or FSS differences, to determine whether the coupled advantage is statistically distinguishable from zero, particularly for the near-surface variables described as showing "limited sensitivity"?

Response and manuscript changes. Section 3.3 adds paired daily-score analyses: mean ΔFSS with a 95% confidence interval, and a sign test for daily wind-speed and air-temperature RMSE differences. Section 4.3.1 reports ΔFSS of approximately 0.020–0.026 for rainfall ≥0.1 mm day^{-1}, with intervals above zero, and smaller, less conclusive differences of approximately 0.002–0.007 for ≥10 mm day^{-1}. Section 4.3.2 reports wind-speed mean RMSE of 1.96 versus 1.95 m s^{-1} and temperature mean RMSE of 1.82 versus 1.81 K for cSINGV and cSINGV_A, respectively, with frequent changes in the sign of daily differences. Figures S8, S11 and S12 are cited.

Location. Sect. 3.3, lines 296–300; Sect. 4.3.1, lines 423–428; Sect. 4.3.2, lines 482–486; Figs. S8, S11 and S12.

Reviewer comment 4

All results derive from single deterministic six-month runs. Since generating an ensemble is not feasible given the computational cost of 1.5 km coupled simulations, the authors should add a clear limitations statement noting that the squall case-study differences (Sect. 4.4) and the regime-dependence conclusion are based on individual realizations of convection. Some of the apparent coupling impact could therefore reflect internal atmospheric variability rather than a systematic coupling effect. Tempering the causal language in this regard would strengthen the scientific credibility of the manuscript.

Response and manuscript changes. The squall interpretation is now revised. Section 4.4 states that the inferred regime dependence is a hypothesis requiring a larger event sample or ensembles. Section 4.5 describes a plausible contribution of enhanced moisture flux to sustained convection and recognises the need for broader testing. Section 5 explicitly states that single deterministic simulations do not account for internal atmospheric variability and that the event-scale differences are indicative rather than conclusive. Section 6 reports ongoing analysis of a larger squall sample.

Location. Sect. 4.4, lines 577–579; Sect. 4.5, lines 613–614; Sect. 5, lines 654–659; Sect. 6, lines 692–693.

Reviewer comment 5

Sect. 3.1 indicates that a wave-only configuration (cSINGV_W) was run for the full January–June 2019 period, yet Sect. 4.2 validates Hs only for January, chosen for Typhoon Pabuk. Since these data already exist, could the authors extend the Fig. 4-type statistics (RMSE, r, SI, KS) to the full six months to confirm that the tuned betamax = 1.75 configuration remains appropriate outside the strong-wind period?

Response and manuscript changes. Section 4.2 now reports the full six-month evaluation and cites Fig. S4 and Table S3. Monthly RMSE ranges from 0.179 to 0.287 m, correlation from 0.910 to 0.966, SI from 0.150 to 0.270 and bias from −0.018 to −0.195 m. These results extend the assessment beyond the Pabuk strong-wind month. The detailed response includes monthly KS statistics as well as the other metrics.

Location. Sect. 4.2, lines 355–360; Fig. S4 and Table S3.

Reviewer comment 6

Sects. 4.1–4.2 evaluate cSINGV_O and cSINGV_W as standalone components. Since the fully coupled cSINGV run already exists and presumably outputs SST and Hs, could the authors add the equivalent RMSE and bias comparisons against OSTIA, CMEMS, and altimetry from the fully coupled run itself? This will clarify whether two-way coupling degrades or maintains ocean and wave fidelity relative to the standalone components.

Response and manuscript changes. A new Sect. 4.3.3 evaluates coupled SST against OSTIA, with CMEMS as a comparator, and coupled significant wave height against altimetry. Figure 11 shows daily spatially averaged SST and daily spatial RMSE; Fig. S13 is cited for monthly SST bias maps. Mean daily SST RMSE is 0.45 °C for cSINGV and 0.48 °C for CMEMS. Figure 12 and Table S4 document coupled wave statistics: RMSE 0.175–0.283 m, correlation 0.913–0.969, SI 0.152–0.267 and bias −0.025 to −0.194 m.

Location. New Sect. 4.3.3, lines 496–520; Figs. 11 and 12; Fig. S13; Tables S4.

Reviewer comment 7

Both configurations tend to produce more moderate and intense rainfall compared to the observations. Using the existing GPM comparison performed for Fig. 5, could the authors quantify, for example through a simple bias or intensity histogram, whether this wet bias differs systematically in magnitude between cSINGV and cSINGV_A? This would clarify whether coupling exacerbates or mitigates the rainfall bias.

Response and manuscript changes. The revised precipitation discussion reports both mean biases and their difference: +0.96 mm day^{-1} for cSINGV, +0.55 mm day^{-1} for cSINGV_A, and +0.41 mm day^{-1} for coupled minus atmosphere-only. It explicitly acknowledges an exacerbation of the overall mean wet bias, with spatial and temporal heterogeneity. The longitude–time bias diagnostic is cited as Fig. S6.

Location. Sect. 4.3.1, lines 399–405; Fig. S6.

Reviewer comment 8

The squall case studies rely on four events selected from the existing MSS squall database using a threshold of >50 mm h^{-1}, of which only two show a clear coupling signal. Could the authors report, how many total qualifying events occurred in the study domain and period? This would clarify whether the four selected cases are representative of the broader population or constitute a favourable subsample.

Response and manuscript changes. Section 4.4 now reports five qualifying observed events during January–June 2019: 4 April, 27 April, 8 May, 19 May and 3 June. Only the two April events were reproduced sufficiently well by at least one configuration for process-based comparison; neither simulation reproduced the other three. The September 2019 and November 2020 cases came from separate simulations outside the continuous six-month experiment. The conclusions explicitly caution that these individual cases should not be interpreted as representative of all Sumatra squalls.

Location. Sect. 4.4, lines 531–535; Sect. 5, lines 658–659.

Reviewer comment 9

Since Sect. 6 frames cSINGV as a step toward an operational system, could the authors report the computational cost, such as core-hours and wall-clock time per forecast day, already logged for the completed coupled and atmosphere-only runs? This is a straightforward reporting that would substantially strengthen the paper's relevance to operational centres.

Response and manuscript changes. The computational-cost discussion has been added to Sect. 5 and cites Table S5. It reports approximately twice the allocated core-hours for the fully coupled system. The additional resources allow the atmosphere, ocean, and wave components to run concurrently, keeping the wall-clock time close to that of the atmosphere-only simulation, with an increase of only approximately 5%.

Location. Sect. 5, lines 672–677; Table S5.

Minor points

Reviewer comment 1

Figure 3 caption enumerates panels (a), (b), and (c), but the panels are unlabelled in the figure. Please add the corresponding panel labels.

Response and manuscript changes. The replacement Figure 3 now includes panel labels (a), (b) and (c), matching the full-domain, Indian Ocean and South China Sea descriptions in the caption.

Location. Revised Fig. 3 in Sect. 4.1; line 325.

Reviewer comment 2

Please clarify whether the FSS in Figure 6 is computed within the red evaluation box shown in Figure 1 or over the full domain

Response and manuscript changes. Section 3.3 explicitly states that FSS is calculated within the red-box evaluation domain in Fig. 1. The revised Fig. 6 caption repeats this information.

Location. Sect. 3.3, lines 295; Fig. 6 caption, lines 429–430.

Reviewer comment 3

A one-line in-text summary of Tables S2–S4, including the NEMO version changes and the betamax/shallow-water sensitivity results, would aid readability.

Response and manuscript changes. The revised Sect. 2.3 identifies the NEMO upgrade from version 3.6 to 4.0.4 and explains the baseline purpose of the ocean sensitivity experiments, citing Table S1. Appendix A summarises the wave sensitivity results: betamax = 1.75 gives the preferred balance of metrics and Experiment 10 is selected for the shallow-water configuration, citing Tables S6 and S7. Table S8 lists the final wave-growth and swell-dissipation parameters. These are the revised table numbers rather than the numbers used in the original comment. The specific NEMO version changes remain delegated to Table S1 rather than being individually summarised in the main text.

Location. Sect. 2.3, lines 173–178; Appendix A, lines 711–723 and Tables S8.

We appreciate the referees' comments, which have strengthened the description, evaluation and interpretation of cSINGV. The revised manuscript presents the additional diagnostics alongside explicit limitations on isolating coupling mechanisms and generalising from individual events.

On behalf of the authors,
Rajesh Kumar
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