RC2: 'Comment on egusphere-2025-321', Anonymous Referee #2, 19 Mar 2025 reply

The manuscript by Shirinov et al. can contribute to advancing the modelling of wave propagation over seagrass beds in field conditions by including the influence of seagrass flexibility and seasonality in a widely employed numerical framework. All the relevant aspects of the work have been described on the manuscript, including field data of seagrass biomechanical properties and model validation. However, there are quite a few issues that should be tackled by authors with a thorough revision. My major concerns are:

- The equations employed to describe seagrass bending in response to the flow are valid for unidirectional flows. If this is not a typo, the simulations should be repeated implementing the right set of equations developed for oscillatory flows.
- To assess the impact of flexibility, I would expect that a comparison between the case of rigid seagrass and that of flexible seagrass is performed. The present comparison between non-vegetated bed and beds with flexible seagrass is inherently influenced by the presence of the seagrass regardless of its mechanical properties. How can the influence of flexibility be isolated with the results presented?
- The wave attenuation predicted by the model is compared across the four Sites of Community Importance and three different substrates. However, the discussion does not critically assess why the impact of flexible seagrass is different across sites/substrates based on the phenotypic traits of the plants available from the sampling campaign. I suggest the authors to work on this aspect to strengthen the manuscript by linking better the outcomes of their model with the (expected) underlying physical mechanisms.
- Conclusions are rather weak and should be re-written.

More specific comments, and comments related to the presentation quality, are included in the attached document.

Reply

Citation: https://doi.org/10.5194/egusphere-2025-321-RC2

Dear Referee,

We sincerely appreciate your time and thoughtful evaluation of our work on modelling the impacts of wave-vegetation interactions on wave attenuation. We have carefully considered your comments and integrated several key aspects into the manuscript, both in the main text and supplementary material (Appendix A and B) and the title.

Shortly on the three general comments highlighted above:

1. The equations referenced and employed in this study were formulated for oscillatory flows, utilizing wave orbital velocities. However, we acknowledge that the manuscript was unclear in this regard and lacked sufficient elaboration, which has now been addressed in methodology [lines 120-127] and Discussion sections [line 364]
2. The validation of the flexible seagrass term was conducted in 'Model validation' section using a case study of (Infantes et al. 2012). In that section, model outputs were compared across three scenarios: a non-vegetated bed, rigid seagrass, and flexible seagrass. Seasonality was not incorporated in this validation exercise due to the short duration of the experiment, during which no significant variation in leaf length was expected. Having validated the improvements and given that numerous prior studies have demonstrated that neglecting flexibility leads to significant over-dissipation, an additional validation was deemed unnecessary for the Civitavecchia case study.
3. The Results section has been expanded and refined to better link the predicted wave attenuation across Sites of Community Importance and different substrates to the underlying physics, while also providing further elaboration on unexpected results [lines 338-345].
4. The conclusion section was re-written to convey main outcomes of the work alongside limitations and future developments in a concise manner.

This review enhanced our understanding of the study's implications and strengthened the findings through a deeper analysis of the results.

Below, each comment was addressed separately in a concise manner, with the provision of the original manuscript's **Line** number and **Text** upon which the concerns were raised, the **Referee**'s comment**,** and **Authors** comment. Proposed changes and additions are highlighted in **blue** in the updated manuscript.

Title
Text: *Modeling wave-vegetation interactions: the impact of seagrass flexibility and seasonal variability*
Referee: *Please revise this part of the title to reflect what the work is about, i.e. wave attenuation. The present form is very generic and may generate wrong expectation for the paper.*
Authors: Thanks to the reviewer, we revised the title as: *"Modelling vegetation-induced wave attenuation: the impact of seagrass seasonal variability and biomechanical flexibility"*

Line 6
Text: *We advance wave-vegetation modelling by integrating …*
Referee: *Unclear what is meant with this. Please, be more precise. I do not think this is the most appropriate verb to be employed here. Please, revise*
Authors: The phrase was indeed uncertain. Rephrased to put more clarity on the scope of work:

'We refine the seagrass parameterization in a spectral wave model by incorporating an enhanced representation into the bottom dissipation source term, explicitly accounting for the effects of plant flexibility, seasonal growth patterns, and phenotypic traits, all informed by site-specific observations.' [line **5**]

Line 12
Text: *These findings offer valuable insights into the role of seagrasses as nature-based solution, facilitating more effective coastal management strategies and guiding restoration efforts in vulnerable marine ecosystems.*
Referee: *This sentence is a bit far-fetched. Please, stick to the main findings of this work and its most direct impacts (e.g. flex and seasonality should be included in numerical modelling to provide more accurate predictions).*
Authors: Thank you pointing at this. We have refined the sentence as advised: *'These results highlight the necessity of resolving seasonal cycles and biomechanical flexibility of aquatic vegetation.'* [line **10**]

Line 15
Text: *patches*
Referee: *meadows or canopy would be more appropriate*
Authors: Using '*meadows'* instead:

'It is widely acknowledged that vegetation meadows help mitigate wave activity.' [line **14**]

Line 16
Text: *fields*
Referee: *"bed" is more commonly employed when referring to seagrass*
Authors: Using '*bed'* instead: *'seagrass beds'*

Line 18
Text: *work*
Referee: *should be more precise and say what performs this work on the stems; namely, the waves*
Authors: We start the sentence with "*Wave-vegetation interactions*" implying the action of waves on vegetation. Following up with '*dissipation of wave energy through mechanical work on stems*' clarifies on the source of mechanical work.

Line 18
Text: *plant*
Referee: *if this statement is general and aims to cover all vegetation types, this term should be replaced because seaweeds are not plants (at least not all of them)*
Authors: In that case, we can simply leave '*stems'*, as self-sufficient in the context.

Line 19
Text: *This phenomenon*
Referee: *While this mechanism has been considered as the only source of wave damping, a very recent work has demonstrated that plant canopies can induce wave damping also through the generation of turbulence at the canopy*

top (at least in the presence of a current). I think it is worth mentioning. Ref: *https://www.pnas.org/doi/10.1073/pnas.2414150122*

Authors: Thank you for providing this reference**.** We acknowledge that turbulence generated at the canopy top can contribute to wave damping in the presence of currents. Hence, we address this in the Conclusion section as follows:

'Another important consideration is that relying solely on a wave model, without incorporating circulation dynamics of the region discussed in Section 4.2, overlooks the current-induced turbulence that develops above the seagrass canopy, as investigated by (Vettori, Giordana, and Manes 2025). (Beth Schaefer and Nepf 2022) showed that currents can either amplify or diminish vegetation-induced wave damping depending on wave conditions, and the inclusion of this interaction would represent a valuable enhancement to the present study.' [line: **408**]

Line 20

Text: *wavelength, thereby reducing wave steepness. As a result, the presence of vegetation contributes to a localized reduction in sea surface elevation behind the patch (Beudin et al. (2017))*

Referee: *This two sentences are unclear to me. Can you elaborate and/or be more precise?*

Authors: Rephrased related sentences to put more clarity:

'*The process known as wave damping, as described by Dalrymple et al. (1984), effectively reduces wave height and increases wavelength over the seagrass canopy, both contributing to a decrease in wave steepness. Consequently, the reduction in wave height leads to a localized drop in sea surface elevation in the lee of the vegetation patch (Beudin et al. 2017).* [line: **18**]

Further we provide details on this mechanism. The impact on wavelength can indeed seem counterintuitive, but these effects of vegetation on wave characteristics in terms of an increase in mean wavelength and decrease in wave height were described in the experiment by (Beudin et al. 2017), section 3.3. To support the statement, below is an example from our study (Figure 1b) that shows the drop in a wave steepness profile (annual average) along the transect line (Figure 1a) over the vegetated area. On the right axis of the Figure 1b we see that for VF (flexible vegetation) experiment the mean wavelength increased over the canopy, whereas the opposite is true for the NV (no vegetation) experiment, where we see smooth decrease in wavelength toward the shore. The figure was added to the **Appendix B1** along with a short description and a mentioning in the discussion to clarify on this comment. [line: **336**]

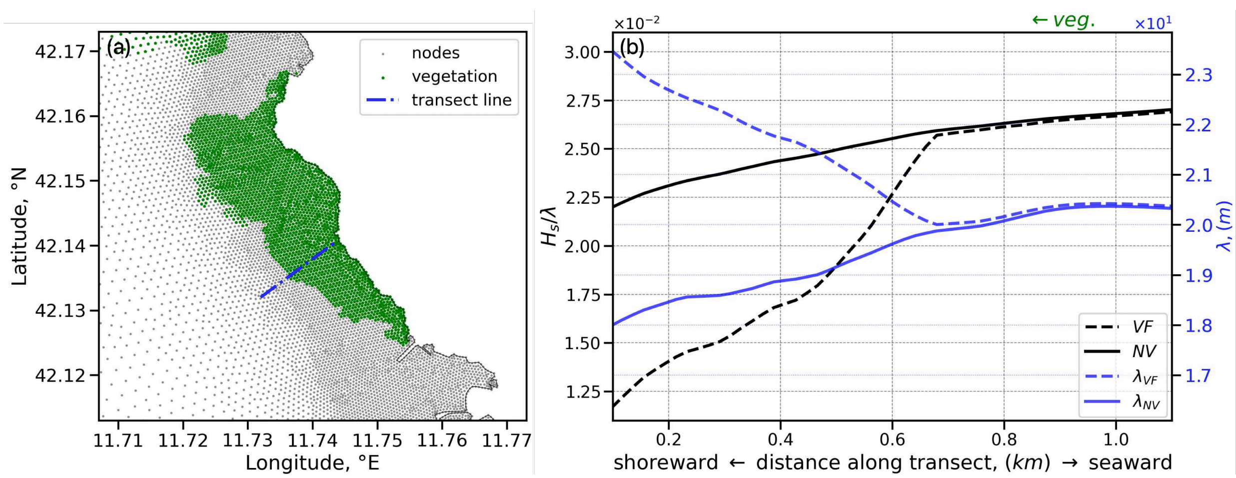

Figure 1. The impact of vegetation on wave characteristics. a) Transect line at SCI 2 site, near P05 sampling station; b) Wave steepness and mean wavelength profiles (annual means) along the transect line, where the shore end is the origin of the x-axis.

Line 37

Text*: (Luhar and Nepf 2011) sought to develop a physics-based model to predict wave decay in a submerged meadow, accounting for the adaptive responses of flexible plants to wave orbital velocity.*

Referee: *This work deals with unidirectional flows. Recent relevant works that aimed to develop wave decay model for seagrass beds are Lei and Nepf (2019), cited at the end of the manuscript, and Vettori et al. (2024).*

Ref: https://www.sciencedirect.com/science/article/pii/S0378383924000206

Authors: True. The older version of their work was cited, focusing on unidirectional flow, although a few lines later the version of 2016 was cited correctly. Rephrased using the original work incorporating wave orbital excursion:

'*(Luhar and Nepf 2016) sought to develop a physics-based model to predict wave decay in a submerged meadow, accounting for the adaptive responses of flexible plants to wave orbital excursion.'*. [line: **36**]
The mentioned papers of *Lei and Nepf (2019)* and *Vettori et al. (2024)* are based on the version of 2016.

Line 45
Text: *covers*
Referee: *mimics/models?*
Authors: Rephrased onto *'canopy mimics'*

Line 45
Text: *in shallow systems where they occupy a large fraction of the water column*
Referee: *This is incorrect. In the last years more and more lab experiments have been performed with high submergence ratios. Please see the works mentioned in the previous comments, e.g. Lei & Nepf (2019) and Vettori et al. (2024).*
Authors: Thank you for pointing out inconsistency, we have revised this section to more accurately reflect the cited studies:

'So far, wave attenuation by seagrass canopies has been primarily measured during experiments in flumes using canopy mimics (Sánchez-González et al. (2011); Stratigaki et al. 2011; Manca et al. 2012; Lei and Nepf 2019; Vettori et al. 2024). Limited field measurements have been conducted in meadows due to challenges in deploying and maintaining instruments and platforms in underwater environments that can withstand intense weather events (Fonseca and Cahalan (1992); Bradley and Houser (2009); Infantes et al. (2012)).' [line: **43**]

Line 45
Text: *Fonseca and Cahalan (1992), Koch and Beer (1996), Mork (1996), Chen et al. (2007), Bradley and Houser (2009)*
Referee: *Some/most of these works do not describe flume experments with seagrass canopy mimics. Please, revise.*
Authors: Previous line response holds here as well.

Line 49
Text: *(Infantes et al. (2012))*
Referee: *An article authored by Contii Neto et al. (2025) that describes long-term field experiments has recently been published, I think it is worth citing.*
Ref: https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2024JC020938
Authors: This is indeed an interesting work that comes to similar conclusions and fits the context of the paragraph in the manuscript. Added to the manuscript:

'*A recent study by Contti Neto et al. 2025, using extensive high-resolution flow measurements, concluded that accounting for flow-induced deflection of seagrass blades, which alters effective canopy height, significantly improves wave dissipation predictions.*' [line: **48**]

Line 50
Text: *observational system design*
Referee: *what do you mean with this? Please re-word*
Authors: Rephrased to put more clarity:

"*In this study, we combine numerical simulations with observational data to emphasize the need for continuous monitoring and the effective integration of empirical measurements into numerical models*." [line: **51**]

Line 51
Text: *cohesive*
Referee: *it doesn't seem to be the most appropriate term. please, revise*
Authors: Addressed in previous comment (Line 50).

Line 57
Text: *PO*
Referee: *It is standard to refer to a species using the 1st letter of the genus followed by the spcies. Please, follow such standards.*
Authors: We thank the reviewer for this suggestion. Using *P. oceanica* instead in the text body and in figures.

Line 71
Text: *funded by the Port System Authority of the Northern-Central Tyrrhenian Sea,*
Referee: *funding should be reported in the acknowledgement section, not in the main text. I recommend to delete.*
Authors: Removed from the text. Instead, the citation is added, and the funding is mentioned in the acknowledgement section.

Line 76
Text: *Such effort aids in identifying optimal coastal restoration solutions, supported by the numerical model, and enhances coastal resilience against extreme wave events.*
Referee: *It is unclear how the project aims to do this. Please be more precise or delete.*
Authors: Rephrased and condensed related sentences to put more clarity and simplify the text:

'*In this context, RENOVATE project (Marcelli et al. 2023), adopts an ecosystem-based approach to compensate for and mitigate anthropogenic impacts in the Sites of Community Importance (SCI) near Civitavecchia port. Using a model-based approach, it focuses on restoring ecosystem services provided by marine benthic habitats, such as P. oceanica and coralligenous substrates to enhance coastal resilience against extreme wave events.*' [line: **72**]

Line 79
Text: *This research utilizes a numerical modeling framework to capture the flexibility and seasonal dynamics of PO.*
Referee: *the numerical modelling is used to capture the effects of flexibility and seasonality on waves. Please, revise the text accordingly.*
Authors: While the original text hints at exploring vegetation flexibility and seasonality, the goals of the research, as the Referee has noted, is to study the effects of these mechanisms on waves and improve the accuracy in the coastal zone. Rephrased onto: *'This research employs a numerical modelling framework to quantify the effects of flexibility and seasonal dynamics of P. oceanica on wave attenuation, with the aim of enhancing the accuracy of coastal zone simulations.'* [line **82**]

Line 80-84
Text: *To the best of our knowledge, this marks the first study of its kind to validate the numerical model's response using high-resolution local vegetation data, serving as a foundational step toward developing a Coastal Digital Twin in support of NBS, enabling future exploration of coastal management strategies to adapt to and mitigate the impacts of climate change.*
Referee: *Relatedly, Vettori and Marjoribanks (2021) reported how seasonality strongly influences the drag force exerted on seagrass (Z. marina in that case) at a plant scale. Even though it doesn't involve wave decay directly, the authors may find it useful.*
Ref: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020WR027747
Authors: The referenced article is very useful, as it addresses the temporal variation effects. Hence, incorporating the findings and sharpening the text as follows:

'*We attempt to validate a numerical model's response using high-resolution local vegetation data, addressing the need to incorporate temporal variability in seagrass biomechanics. As shown by Vettori et al. (2021), seasonal changes and nutrient availability over the year influence blade size, rigidity, and buoyancy, affecting seagrass interactions with hydrodynamic forces. Integrating this information into wave model could enhance the accuracy of simulations in wave-dominated environments, although such data are frequently limited or lacking.'* [line **86**]

Line 93
Text: *Modeling framework*
Referee: *In the present form, a reader needs to go at the end of the article to search for the meaning of all symbols employed in this section. This is very unpractical and should be amended. Please, define all relevant parameters as soon as they appear in an equation.*
Authors: We acknowledge the concern regarding the placement of symbol definitions, and we place the list of symbols after the introduction section, with the following comment: *'The symbols and their corresponding units of measurement used in this study are provided in Table 1.'* [line **92**]

Line 100
Text: *WW3 has been used worldwide from global to coastal applications in both standalone and coupled versions*
Referee: *If that is the case, I suggest to provide a few examples as references.*

Authors: Adding a couple references: '*WW3 has been used worldwide from global (Mentaschi et al. 2020; (Sharmar et al. 2021) to regional (Salvatore Causio et al. 2021; 2024) to coastal applications in both standalone, as in the present work, and coupled modes (Clementi et al. (2017), (Causio et al. 2024))'* [line **106**]

Line 100

Text: *augmenting*

Referee: *Isn't it simpler to state that the two source terms are linearly summed?*

Authors: In the sentence, we are addressing the augmentation of the particular source term, and by *'augmenting'* we suggest an intentional modification that enriches the model's representation, referring to the fact that the bottom dissipation is being 'modified' by adding the vegetation impact. Additionally, the exact implementation through simple arithmetical summation is then given further in Eq. 2.

Line 113

Text: *Some of these formulations utilize a drag coefficient that depends on hydrodynamic conditions, such as the Reynolds number or the Keulegan-Carpenter number, as recently described by Abdolali et al. (2022). Another approach is based on plant motion*

Referee: *These statements are not precise. The Reynolds and KC numbers are not hydrodynamic conditions. Moreover, the approach employed herein accounts for plant deformation/bending rather than motion. I recommend the authors to check this type of statements as they can be misleading.*

Authors: Thank you for pointing this out. We refined these statements and simplified to avoid misleading:

'Several alternative formulations have been proposed to address the overestimation of wave damping, some of which incorporate the Reynolds number or Keulegan-Carpenter number, as recently described by (Abdolali et al. 2022), while others are based on plant bending.' [line **119**]

Line 116-119

Text: *Luhar and Nepf (2011)*

Referee: *As written in a previous comment this reference is not appropriate. Eq. 4 for is not appropriate as it was developed and validated for unidirectional flows. The authors should repeat the relevant analysis using the correct eqs as described in Luhar and Nepf (2016).*

Authors: It is indeed a very accurate and reasonable comment, and we would like to elaborate. The modifications made in *(Luhar and Nepf 2016)* were considered in this work through the use of the wave orbital velocities (as per Eq. 4 – U_b) instead of a unidirectional current as in the original publication of *(Luhar and Nepf 2011),* similar to the proposed by the Referee article in Line 37 (Vettori et al. 2024). Although, the formulation of the effective length concept was used that adheres to the rigid formulation with the length depending on the Cauchy number (Ca) and the buoyancy parameter (B). As highlighted by *(Luhar and Nepf 2016)*, the definition of the effective length (l_e) can be complicated and vary from species to species, and 'given the likely variation in vegetation stiffness and buoyancy, the calibrated drag coefficient for one species will not hold for other species', hence the effective length concept proposed by *(Luhar and Nepf 2011)* was suggested in *(Luhar and Nepf 2016)*. The intention of the current work is to approximate the flexibility effect at a reasonable complexity and generalize for different species, although this does not take part of the discussion in the manuscript as more studies are required. Another difference can be seen in the presence of C_D (=1.95), and 0.5 in the definition of Cauchy number, which almost equate to unity in the formulation, thus not having any significant impact at the scale of our domain. Although, *(Luhar and Nepf 2016)* had also hypothesized that the flat plate C_D can be used for flexible blades in oscillatory flows, '$C_D = max(10KC^{-1/3}; 1.95)$', thus it's preserved for further adaptation of C_D based on Keulegan–Carpenter number.

In summary, we used the formulation of the effective length as per suggestions made in *(Luhar and Nepf 2016)*, with the adaptation of the orbital velocities (mentioned in the preprint – line 115) for the wave induced oscillatory flows. Having demonstrated significant improvement in a well-measured test case of (Infantes et al. 2012), along with wave attenuation magnitudes comparable to those in the referenced studies, we consider the model implementation sufficiently accurate.

The modifications to the preprint were made to clarify this approach: '*Our study adopts the latter, specifically by implementing the formulation proposed by Luhar and Nepf 2011 and refined in Luhar and Nepf 2016. In this approach, the flexibility effect is incorporated into the source term computation by replacing the vegetation length, l_v, with an effective length, l_e (Eq. 4). Although the original work considers a unidirectional flow field, we formulate the Cauchy number, Ca, as a function of the wave bottom orbital velocity, U_b, to account for the wave-induced oscillatory flows. Similar modifications were proposed by (M. Luhar and Nepf 2016) using the oscillatory velocity scale as a function of the wave orbital excursion, and later adapted by (Vettori et al. 2024).*' [line **121**]

Line 123
Text: *2.1 Idealized test case*
Referee: *I would suggest re-naming this into: Model validation or something of the sort.*
Authors: It would be more reasonable indeed, as the test case represents an actual field study replicated in a numerical simulation to validate the implementation of the flexibility term. Adapting to: *"Model validation"*

Line 124
Text: *in 3 and 4*
Referee: *unclear what is meant here. Please, revise*
Authors: The preceding *'Eqs.'* was missing: *'To validate the bottom vegetation effects described in Eqs. 2.3 and 2.4, we developed an idealized test case based on the study by Infantes et al. (2012).'* [line **134**]

Line 167
Text: *The realistic study case*
Referee: *I suggest this header to be renamed. Based on the present header, a reader would assume that the previous case, used for validation, was not realistic even though it was based on field observation.*
Authors: Since both case studies refer to real fields and the numerical simulations are conducted using field measurements, similarly to previous comment (Line 123), we rename the section onto: ***'**The Civitavecchia case study'*. Changes throughout the preprint are made accordingly.

Line 173. Figure 4.
Text: Figure axes lack labels
Referee: *It may be obvious, but the axes should feature labels to clarify what the numbers stand for*
Authors: True. The labels to the axes were added.

Line 180
Text: *brown*
Referee: *Isn't it red?*
Authors: We thank the reviewer for noticing this. We substituted "brown" with "red" throughout the study, as well as aligning the colours used in figures to be consistent.

Line 185
Text: *The lack of specific management plans for SCIs, such as ecofriendly buoys to prevent anchoring of recreational boats on PO meadows, anti-trawling barriers to deter fishing boats from using nets within SCIs, and an early warning system for dredging and accidental hydrocarbon spills, renders PO vulnerable to various anthropogenic stressors present in the study area. Urban and industrial discharges from aquaculture and power plants, the presence of an oil platform, trawling activities, and harbor activities connected to Civitavecchia port have significantly impacted the health of the meadows.*
Referee: *I think this part would be more suited in the introduction when the authors describe the project and the area.*
Authors: Thanks for the suggestion. This section has been moved to the introduction, providing a more complete response to Line 76 raised by Referee. [line 76]

Line 211
Text: *elastic module*
Referee: *modulus. And the value of such modulus needs to be expressed with some units of measurements*
Authors: Thank you for pointing out at this. Corrected where applicable onto: *'elastic modulus'* along with the unit of measurement.

Line 215
Text: *types 5*
Referee: *what is meant here?*
Authors: The preceding 'Figure' was missing: *'…the three substrate types in Figure 5 exhibit a trend…'*

Line 227

Text: *While the growth factor is inherently site-specific and influenced by physical variables such as wave action, turbidity, temperature, and proximity to river mouths requiring localized data collection, the proposed formulation offers a significant advantage: it eliminates reliance on abiotic parameters like temperature, light, and nutrients. These parameters often face challenges such as limited temporal coverage (e.g., cloud cover disrupting high-resolution satellite observations) or insufficient spatial resolution in coastal zones (e.g., the 4 km grid used by Copernicus Marine Services regional models).*

Referee: *I find this text confusing. What is the message you are trying to convey? Maybe worth removing.*
For what concerns the growth curves:
1) why do the different curves reach a minimum at different times? And are these differences biologically reasonable/realistic considering that the area under study is quite small?
2) eqs for these curves should be reported somewhere

Authors: Indeed, the text is missing the connection to the rest. It meant to justify the preference of our methodology over others, hence we slightly modify it to put more clarity as follows:

'The growth factor is inherently site-specific, influenced by physical and environmental variables such as wave action, turbidity, temperature, proximity to river mouths, and nutrient availability, requiring localized data collection. Unlike biogeochemical models, the proposed formulation, based on in-situ observations, does not rely on abiotic parameters, which are often limited by poor temporal coverage (e.g., cloud-obstructed satellite data) or insufficient spatial resolution in coastal zones (e.g., the 4km grid used by Copernicus Marine Services regional models).' [line **241**]

1) The growth curves are derived by fitting a fifth-degree polynomial to canopy height data collected on-site. Notably, data for the winter months (December–February) were not acquired due to unfavourable weather conditions, which made scuba diving infeasible. This gap in data introduces a degree of uncertainty in the estimated curves during this period. The differences in the minimum canopy height across substrates may stem from this uncertainty, as well as from biological variability in growth and senescence cycles. We further elaborate in the manuscript on it as follows:

   'According to the growth patterns, maximum leaf development in P. oceanica occurs toward the end of the summer season. This is followed by the onset of intense autumnal storms, which induce the detachment of senescent leaves, leading to a marked decline in canopy height during winter. At this stage, only juvenile shoots persist, characterized by their minimum annual leaf length. Notably, the timing of minimum canopy height varies with substrate type, a pattern that may reflect both biological variability and senescence cycles, as well as observational uncertainty during winter months, when adverse weather conditions hindered data collection due to the infeasibility of scuba diving. Specifically, in the study area, P. oceanica growing on sandy and matte substrates exhibits faster growth and greater leaf elongation during summer months. On a sandy substrate, which facilitates root penetration (Di Maida et al. 2013), meadows demonstrate greater resilience by postponing the onset of senescence and maintaining a higher minimum canopy height during winter. In contrast, rocky substrate imposes mechanical limitations on root penetration, restricting P. oceanica to establishing within crevices (Hemminga and Duarte 2000), reflecting a greater demand for anchorage and the reduced nutrient availability (Giovannetti et al. 2008). Given that sediment-based nutrient uptake through the roots is a primary pathway for this species (Touchette and Burkholder 2000), these constraints likely contribute to diminished growth performance in winter months. Similarly, canopy height is lower for P. oceanica growing on degraded matte, as the reduced shoot density offers limited protection against intense storms, which tend to uproot nearly all leaves, leaving only those a few millimeters long. Thus, the values shown in Table 4 for initial leaf length reflect initial conditions of P. oceanica meadows in October and are subject to change over the simulation according to Figure 5 for VFS experiment.' [line **225-240**]

2) The coefficients used for the fitted equations are appended in *Appendix A2* and referenced in the caption of Figure 5.

Line 269

Text: *Model validation*

Referee: *I find this section a bit misleading. The validation of the model is done regardlessly of the vegetation as the wave buoy is located offshore with respect to the seagrass meadows/beds. I think this point should be made clear. As it stands, the text (and caption of fig.7) gives the wrong impression that the validation relates to how vegetation was modelled*

Authors: Although the text does not suggest the validation of neither seasonality nor flexibility implementations using buoy data, it is indeed not stated explicitly so. Hence, the text was refined to put more clarity:

"We assess the wave model's performance using offshore buoy data described in Section 3.2. However, since the buoy is located outside the vegetated area and beyond its influence, the validation does not account for the effects of seasonal variability or flexibility in the model implementation, but on the quality of the numerical simulation offshore. Nevertheless, the SWH timeseries in Figure 7a at the wave buoy location (see Figure4a) provides valuable insight into high-amplitude waves over the simulated period, with wave heights reaching 4 meters, peaking in March." [line **283**]

Line 285

Text: *This variability in bias is likely associated with the absence of coupling effects with currents.*

Referee: *this statement is very generic. Please, elaborate providing supporting evidence.*

Authors: The statement was supported with the addition of the studies of the region as follows:

'*The variability in bias is likely associated with the absence of coupling effects with currents in the region. Studies identify the Bonifacio Strait as a principal driving force of cyclonic and anticyclonic gyres (Astraldi and Gasparini 1994), forming the Bonifacio cyclone, where the winter convection is particularly strong reaching the northeastern coasts (Iacono et al. 2021). From autumn to early spring, the northward principal stream entering the Tyrrhenian Sea from the Sardinia Channel and Sicily Strait follows along the eastern coast and splits into an outflowing stream via the Corsica Channel and a cyclonic southward flow entrained by the Bonifacio gyre (Vetrano et al. 2010). These circulation patterns may influence wave dynamics, potentially affecting the model's performance, though the extent of this impact remains unclear and requires further investigation.*' [line 302]

Line 285. Table 4.

Text: *UBR*

Referee: *this has not been defined anywhere in the text up to this point. All acronyms should be defined the first time they are employed.*

Authors: Thanks for noticing this. The acronyms were defined at first appearance.

Line 288

Text: *We compare the three different experiments, described in Table 2, to estimate the significance and contribution of vegetation and seasonality in wave attenuation.*

Referee: *I suggest rewording. This sentence is a bit hard to read and understand.*

Authors: Rephrased to put more clarity: *'We compare the three experiments described in Table 2 to evaluate the contributions of flexible vegetation and seasonal effects to wave attenuation.'* [line **310**]

Line 294

Text: *The effect of seasonality accounts for 10% of variation for both SWH and UBR (VFS – NV).*

Referee: *I do not see how comparing VFS and NV allows assessing the effect of seasonality. VFS-NV should provide info on the cumulative effects of flexibility and seasonality. Can the authors elaborate or revise?*

Authors: We thank the reviewer for noticing this, the '(VFS – NV)' in the text is a mistake, although, a line above we reference: '…*the performances of both VFS and VF experiments*…', similarly with the followed-up text. Hence, the 10% variation refers to (VFS-VF) rows in Table 4. Corrected accordingly in the text onto: *'(VFS-VF)'*

Line 298

Text: *However, this comparison does not capture the impact of specific phenotypic traits, their distinct growth patterns, or wave-energy-dependent dissipation.*

Referee: *The analysis shown in the following subsections doesn't address these aspects. Please, revise the sentence to reflect the actual content.*

Authors: This is true, the text does not align with the followed-up analysis. Rephrasing to be more precise:

'However, this comparison does not fully capture the influence of P. oceanica meadows across different substrates on wave dissipation and their temporal patterns.' [line **322**]

Line 303

Text: *phenotypic traits of PO*

Referee: *It is not clear to me what the authors mean with this. The analysis distinguishes across habitats/substrates rather than seagrass phenotypic trait. Please, elaborate or revise.*

Authors: Phenotypic traits refer to plant characteristics, and we observed that *P. oceanica* exhibits different phenotypic traits depending on the substrate on which it grows. In this study, we distinguished three groups of phenotypic traits, they mainly differ in shoot density and leaf length and named each group according to the substrate that influenced their development. In the introduction we clarify the use of traits as: *'Wave-vegetation interactions result in the dissipation of wave energy through mechanical work on submerged vegetation, determined by vegetation traits such as shoot density, canopy height, stiffness, and bending, as well as wave characteristics.'*. [line **16**] Throughout the paper, we link these traits to the substrates on which the seagrass grows, providing detailed characteristics in Table 3. To bring the reader's attention back to this, we note in the conclusion: *'The study considered seasonal variation of plant characteristics, and three different groups of P. oceanica identified according to different phenotypic traits, induced by the substrate on which they have grown. It is important to emphasize that the vegetation-induced wave damping effects analyzed here were based solely on these varying traits and not on the dissipation properties of the different substrates.'*. [line **429**] However, to avoid the confusion we rephrased the text as: *'across P. oceanica over different substrates*.'.

Line 305

Text: *wave energy.*

Referee: *Eq. 3 contains all the parameters influencing wave damping due to vegetation. It is more correct to refer to hydrodynamic conditions in general or report all other relevant hydraulic variables.*

Authors: We thank the reviewer for this comment. The sentence has been removed as it was abrupt and lacked substantive relevance to the discussion.

Line 324

Text: *deviation bars*

Referee: *It is not clear what deviation bars are? Do the authors refer to the overall trend throughout the year shown in Fig. 10?*

Authors: Yes, this section is referring to Fig. 10 only until explicitly stated otherwise, as 'Figure 5' in the sentence of concern. We rephrased it to put more clarity: *"Overall, the seasonal variation patterns conform to the growth curves in Figure 5."* [line **352**]

Line 325

Text: *Seasonal*

Referee: *also flexibility enters into play here. the header should reflect this.*

Authors: True, both seasonality and flexibility effects are considered in VFS experiment as described in Table 2. Although, the 'Seasonal' in this context refers to the seasons of the year in general and not to the implementation of the model. This section aims to study the wave attenuation patterns of the site given the implementations, similar to Figure 6 in Section 4.1. We refine the first sentence to clarify that both effects are considered: *'Figure 11 illustrates the node-wise Hs attenuation capacity of vegetated areas along the Civitavecchia coast by quantifying the wave height reduction attributable to flexible canopies and their seasonal growth patterns.'*. [line **355**] The Section header has been adjusted dropping off 'VFS vs. NV' to: *'Seasonal wave attenuation maps'*, as the quantification of wave damping capacity implicitly reflects the comparison between VFS and NV configurations. The term 'VFS vs. NV' is retained in the figure headers for clarity.

Line 334

Text: *Discussion*

Referee: *The authors should discuss the seagrass performance (in terms of wave attenuation) across the substrates in the light of the phenotypic traits reported in Table 3 and Figure 5. Since the plant density and leaf length are maximum in the sandy substrate, I wonder why the effect of seagrass (for both VFS and VF) is not at its highest for that substrate. Can the authors elaborate?*

Authors: We appreciate the Referee for raising these valid concerns. We would like to answer in two parts: 1) general comment on Discussion section structure and changes; 2) the performance of the meadows on sandy substrate.

1) In this section we summarise the seagrass performance both across the SCIs, e.g.: "*The maximum wave damping reached 16% - 18% on average in September over SCI 1and SCI 2, and 10% - 12% over SCI 3 and SCI 4."* [line **386**], and substrates: *"Similarly, we observed consistent results in the analysis of wave damping across various substrate types, with peak SWH reductions of 24%, 22%, and 9% for P. oceanica traits over rock, sand, and degraded matte, respectively."* [line **389**]. We focus on overall performance, limitations and uncertainties, whereas the results

are discussed in more details in preceding Results section. However, we removed superfluous statements and added references to back our conclusions, also in accordance with the Referee's earlier comments in this review.

2) This is indeed a good point raised by the Referee that was overlooked in the analysis. Hence, below we provide more information.

We do not demonstrate the results of VF simulation in terms of each substrate performance for the Civitavecchia case study (as in Figure 9 for VFS), since the aim of this section is to reflect on the seasonal patterns, and the impact of including this temporal variable, the results of which we can infer from Figure 10 (VFS – VF) as the seasonal impact related to different traits, although in terms of relative variations between the corresponding substrates, and not the comparison among different ones. However, for VFS experiment we show the performance in terms of these substrates in Figure 9. It indicates the higher wave damping capacity seen in terms of both SWH and UBR for *P. oceanica* over sandy substrate during months of Nov-Jan, when they exceed the leaf length of the corresponding traits over rocks as seen in Figure 5. Contrary, for the months of Feb-Sep the wave dissipation is more pronounced for *P. oceanica* over rocks despite the shorter leaves than those over sandy substrate. As seen in Figure 4, the location of *P. oceanica* over sand is farther from the shore compared to rocks, and since the dissipation is a function of the leaf length to the water depth ratio, this spatial arrangement influences dissipation efficiency, and thus, Figure 9 does not linearly correspond to Figure 5 growth curves.

We expand Section 4.3.2 to add this observation:

"Notably, the trends associated with different substrate types do not follow the expected seasonal patterns in Figure 5, suggesting that P. oceanica meadows on rocky substrates exhibit greater wave dissipation capacity than those on sandy substrates during summer, despite the observed variations in leaf length and shoot density. This discrepancy can be attributed to the spatial distribution of the meadows, as illustrated in Figure 4a, where P. oceanica over sand is located farther from the shore compared to those on rocks. Given that wave dissipation (Eq. 3) is a function of the ratio of leaf length to water depth, this spatial arrangement influences dissipation efficiency, as also demonstrated in great detail by the laboratory experiments of (Anderson and Smith 2014).". [line **338**]

Line 341. Figure 10.

Text: *Figure 10. Timeseries of mean monthly SWH and UBR percentage difference between VFS and VF simulations across substrate types, averaged over the vegetation points per substrate in the mask (bars) and over all substrates (orange solid line).*

Referee: *can the authors elaborate on how the average values across all substrates were computed? The values reported in Fig.10 do not seem to match averages across the 3 substrates, they are always too low in magnitude. At the same time, from Fig. 4a rocky substrate seems to be the most common in the study area but the averages reported in fig. 10 are always much lower (in magnitude) than those for rocky substrate. Why is that?*

Authors: We appreciate the Referee bringing this up, as we have found an error in post-processing while applying the mask for vegetated areas. We will address the Referee's comments reflecting on each concern separately:

1) *Can the authors elaborate on how the average values across all substrates were computed?*

The relative mean monthly percentage differences in wave heights (*H*) were computed as $(H_{VFS} – H_{VF})/H_{VF} \cdot 100$, [%] (*added the formulation to the figure caption in the manuscript*), where H_{VFS} and H_{VF} represent monthly averaged wave heights for the respective experiments. Only corresponding nodes per substrate type (present in both VFS and VF simulations) were included in the calculations, whereas all the vegetated nodes were considered for the 'all substrates' curve.

2) *The values reported in Fig.10 do not seem to match averages across the 3 substrates, they are always too low in magnitude*

It should be noted that the number of nodes varies between substrate types (as seen in Figure 4a), meaning their contributions are weighted differently when computing relative differences. Consequently, the mean of the individual substrate differences does not align with the 'All substrates' curve. The 'All substrates' curve ensures that the relative difference reflects the overall vegetated area rather than averaging precomputed substrate-specific differences, which would be redundant and less informative and accurate, as the contributions from different substrates vary. The values for 'All substrates' curve correspond to those listed in Table 4 (SWH (%) VFS-VF row). We modify Figure 10 caption to explicitly state the fact of weighted average: *'Timeseries of mean monthly SWH and UBR percentage difference between VFS and VF simulations across substrate types ($(H_{VFS} – H_{VF})/H_{VF} \cdot 100$). Bars indicate the average differences over vegetated points for each substrate, while the solid orange line represents the weighted average across all vegetated areas in the domain. PO stands for P. oceanica'*

3) *At the same time, from Fig. 4a rocky substrate seems to be the most common in the study area but the averages reported in fig. 10 are always much lower (in magnitude) than those for rocky substrate. Why is that?*

Thank you for pointing this out! We have detected the use of incorrect mask which accounted for SCI nodes (that also includes non-vegetated areas as seen in Fig. 4) instead of substrates, therefore yielding this discrepancy. Hence, we have regenerated the figure using a mask that covers only vegetated areas over all substrates. Similarly, we re-computed the results for Table 4. The manuscript was therefore corrected throughout to reflect on new values of the results. [Abstract, Section 4.3, 4.3.2, Discussion].

Line 352-361

Text: *From a spatial point of view, in terms of SCI sites, the model showed a wave damping of approximately 10% during peak waves in March for SCI 2 on average. As both SCI 1 and SCI 2 are predominantly composed of rocky substrates and are exposed to direct waves from the southwest, they experience a greater impact and, consequently, more significant wave reduction compared to SCI 3 and SCI 4. The maximum wave damping reached 16% - 18% on average in September over 1st and 2nd sites, and 10% - 12% over 3rd and 4th sites. The seasonal effect did not show a linear correlation with the monthly average wave reduction across SCIs when compared to non-vegetated simulations. This aligns with the vegetation model, where wave dissipation is closely related to both wave energy levels and leaf length. Similarly, we observed consistent results in the analysis of wave damping across various substrate types, with peak SWH reductions of 24%, 22%, and 9% for PO traits over rock, sand, and degraded matte, respectively. It's noteworthy that SCI 4, characterized by a higher concentration of degraded matte, exhibited a lower wave attenuation capacity of maximum 10%.*

Referee: *Assessing the differences across SCIs is a bit risky, in my opinion, because within an SCI different substrates are present. Since the seagrass biomechanical traits were assessed across substrates, I suggest the authors to stick with the same classification when discussing the results.*

Authors: We completely agree that there are many factors to consider while addressing SCIs, including those related to vegetation, such as shoot density, leaf length, seasonal growth patterns, spatial distribution, depth range, and coverage per site, as well as site-specific wave characteristics, as discussed in Section 4.1. However, this section is addressing the RENOVATE project that considers SCIs, as mentioned in the introduction: *'In this context, RENOVATE project (Marcelli et al. (2023)), adopts an ecosystem-based approach to compensate for and mitigate anthropogenic impacts in the Sites of Community Importance (SCI) near Civitavecchia port in the northeastern Tyrrhenian Sea.'*, to provide overall insights across these sites facilitating more effective coastal management and guide restoration efforts as per project goals.

Line 362

Text: *This research provides a comprehensive analysis of the impact of submerged vegetation on wave propagation in the nearshore zone, aimed at evaluating the role of seagrass as a Nature-Based Solution ((Pillai et al. (2022)); Jacob et al. (2023); Unguendoli et al. (2023)). Additionally, it seeks to enhance the management of Sites of Community Importance (SCIs) by providing insights into wave dynamics to identify optimal areas for restoration activities (Rifai et al. (2023); Pansini et al. (2022); Chen et al. (2024)), thereby maximizing the ecological benefits of such interventions.*

Referee: *these statements are too generic. This work focuses on a specific aspect of this problem. Please, be more precise.*

Authors: We thank the reviewer for this comment. Indeed, this sentence does not carry relevant information as per the analysis of the observed results in the Discussion section. Since overall conclusions are addressed in the Conclusion section it was removed.

Line 366-370

Text: *Among marine phanerogams, PO has a high capacity for wave attenuation as it forms extensive and dense meadows in coastal areas, with leaves that frequently exceed one meter in length Koftis et al. (2013). Due to changing wave energies, PO meadows bend and straighten, causing varying shear stresses depending on wave orbital velocities. This dynamic interaction reduces shear stresses and leads to a lower wave damping effect, which was accurately replicated with the flexible vegetation model (Luhar and Nepf (2011)).*

Referee: *this part of text is unrelated to the previous lines in the paragraph. Consider deleting them or move to a dedicated paragraph.*

Authors: True, this part was restructured along the previous corrections and moved to the Conclusion section, where it is fit in summarizing the achievements of the implementation:

‘*Among marine phanerogams, P. oceanica has a high capacity for wave attenuation as it forms extensive and dense meadows in coastal areas, with leaves that frequently exceed one meter in length Koftis et al. (2013). Due to changing wave energies, P. oceanica meadows bend and straighten, causing varying shear stresses depending on wave orbital velocities. This dynamic interaction reduces shear stresses and leads to a lower wave damping effect, which was accurately replicated in this work with the flexible vegetation model adapted from Luhar and Nepf (2011) and Luhar and Nepf (2016).’* [line **420**]

Line 378-382

Text: *In this context, the application of innovative monitoring techniques, including the use of autonomous vehicles (e.g., Unmanned Surface Vehicles - USVs) equipped with acoustic instrumentation capable of measuring the height and coverage of marine vegetation, would be highly beneficial. These techniques can increase the spatial and temporal coverage of data, provide information on plant characteristics up to a few meters from the shore, and detect seagrass canopy coverage and height changes following significant extreme events (Piazzolla et al. (2024)).*

Referee: *It is unclear why these techniques are so needed if your modelling approach performs well already. Please, revise this text to improve the link between different sentences.*

Authors: We thank the reviewer for this comment. We reworded this text to improve its clarity:

“*The measurements used to reproduce the behavior of P. oceanica were obtained through point-based scuba diving observations, which are inherently non-synoptic and spatially heterogeneous. High-resolution spatial and temporal studies typically require extensive in situ data collection, which is both costly and time-consuming. Therefore, the adoption of innovative monitoring techniques, such as autonomous vehicles (e.g., Unmanned Surface Vehicles – USVs) equipped with acoustic sensors, could significantly enhance both data coverage and synopticity. These systems are capable of measuring seagrass height and coverage in shallow coastal areas that are inaccessible to traditional hydrographic platforms, while also detecting canopy variations induced by extreme events (Piazzolla et al. 2024).’* [line: **400**]

Line 385

Text: *Furthermore, the sole use of a wave model without considering hydrodynamics means that relying only on near-bed orbital velocities does not account for the shear stresses induced on the vegetation canopy by currents.*

Referee: *another generic statement here. Hydrodynamics includes wave mechanics. Please, revise this sentence. For what concerns the effect of currents, recent works by Schaefer and Nepf (2022) and Vettori et al. (2025) can provide some insights.*

Ref: https://aslopubs.onlinelibrary.wiley.com/doi/full/10.1002/lno.12102

Authors: We thank the reviewer for this comment. The sentence was rephrased with the link to the currents patterns of the region mentioned in the previous comment:

‘Another important consideration is that relying solely on a wave model, without incorporating circulation dynamics of the region discussed in Section 4.2, overlooks the current-induced turbulence that develops above the seagrass canopy, as investigated by (Vettori, Giordana, and Manes 2025). (Beth Schaefer and Nepf 2022) showed that currents can either amplify or diminish vegetation-induced wave damping depending on wave conditions, and the inclusion of this interaction would represent a valuable enhancement to the present study.’ [line: **408**]

Line 389.

Text: Conclusion

Referee: *The conclusions should be rewritten completely. At the moment they consist of sentences copied and pasted from other parts of the manuscript and statements about the project funding the work and seagrass restoration — how does this work provide guidance for seagrass restoration? Conclusions shouls focus on the main findings of the work and their potential impact.*

Authors: This was greatly overlooked, and we sincerely appreciate the Referee for pointing at this issue. Upon reviewing the conclusion, we recognize that it was not sufficiently focused. We have revised and condensed the section to emphasize the main findings of the study, their implications and future work. Additionally, we have removed extraneous information related to the funding and broader project context, as per the Referee's suggestion.

Line 420. Appendix

Text: *A1*

Referee: *I am not an expert of numerical modelling, but the text reported here is very dense to me and I wonder if it is indeed possible to replicate the simulations with the information provided… Is the model described in more detail in a previous paper from some of the authors (i.e. Pillai et al., 2022)?*

Authors: The simulations were conducted with different implementations of the flexibility term and using different bottom friction formulations. However, due to the uncertainties in the results, these findings were not included or discussed in the manuscript. To ensure accuracy and reliability, we implemented the formulation that produced the most reasonable and validated results, as benchmarked against the test case of (Infantes et al. 2012), which was further used in a more heavy and computationally demanding simulation of Civitavecchia case study. In light of the Referee's provided references (Lei and Nepf 2019; Vettori et al. 2024) we have addressed concerns regarding misleading citations and adjusted the manuscript accordingly. As previously discussed, our approach aligns well with these studies. Given this alignment and the validations already conducted, we believe that re-executing the simulations is not necessary.

We would like to refer to the WW3 manual cited in the manuscript (*User Manual and System Documentation of WAVEWATCH III* (version v6.07) 2019) for details on the brief information provided on used physics and techniques in Appendix A1, as being the original source of information.

Line 435. Appendix. Table A1
Text: *UBR*
Referee: *this is an acronym rather than a symbol. Please, define acronyms in the text the first time they appear*
Authors: Thanks for noticing this. The acronyms were defined in the text.

Line 435. Appendix. Table A1
Text: *g Acceleration due to gravity*
Referee: *This way of referring to gravitational acceleration is quite peculiar. I suggest to stick with more traditional terminology.*
Authors: Agreeably 'gravitational acceleration' is more widely used, although 'acceleration due to gravity' is a rather traditional old terminology, where the use of both could be seen in the references provided by the Referee (Lei and Nepf 2019; Vettori et al. 2024). The list of symbols is adjusted to 'gravitational acceleration'

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