nature portfolio

Peer Review File

A Sentinel Watching Over Inter-Tidal Seagrass Phenology Across Western Europe and North Africa.

Corresponding Author: Dr Bede Davies

Due to technical limitations, attachments originally included by the reviewers as part of their assessment appear at the end of this file.

Version 0:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Davies,

Your manuscript titled "A Sentinel Watching Over Intertidal Seagrass Phenology." has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When revising, please take into account three major points that were raised by the reviewers. Please address these specifically in your response letter, otherwise we may not send the manuscript back to the reviewers: 1) The methods need to be detailed further and improved. Any published study should be able to be replicated, and presently this is not the case. 2) The overall structure of the manuscript needs to be refined and the data presentation needs to be improved. 3) repetitive language around the importance of the study needs to be removed.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

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EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements (Download the link to your computer as a PDF.)

For your information, you can find some guidance regarding format requirements summarized on the following checklist: (https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This paper presents a new application of an extensively approach from terrestrial vegetation dynamics to a set of 12 seagrass sites on the north-east Atlantic coasts and demonstrates new insights into spatial and temporal dynamics of inter-tidal seagrass communities. The use of continental to global scale, high spatial resolution satellite image archives to assess the spatial and temporal dynamics of terrestrial vegetation traits (structure and physiological parameters) is a well established field. The application in inter-tidal seagrass beyond plot scales is novel.

Although the application of satellite-time series to assess vegetation phenology is not new, the application to seagrass environments is. This paper represents and initial step to develop, test and scale-up this type of approach.

In its current form the paper needs significant extra work to its overall structure, content, narrative and key findings to present a coherent, justified and clearly explained set of text and graphics. The current paper is quite difficult to follow to determine exactly what was done and why in terms of the Sentinel2 processing, and how it was linked to the drone and field measurements at each of the 12 sites. The findings are beyond the scope of the data sets (12 sites) and analyses used. The justification for why to develop this type of capability, to map and monitor seagrass communities and understand their spatial and temporal dynamics needs significant extra work to move beyond general scientific knowledge to influencing modelling and management. These limitations are compounded by the confusing inter-changeable use through the entire text of what is actually measured and mapped in terms of seagrass cover (which is a % per unit area) and seagrass extent. It should be possible for readers with sufficient technical and scientific background to understand how to replicate the work from the paper – in its current form this is not possible. The table of detailed comments below sets out changes required to address these points.

The authors and the seagrass scientific and management communities would benefit significantly from this work being presented first in a discipline specific journal that focuses on detailed technical and applications based projects, such as Remote Sensing of Environment. This would allow the authors to explain in full detail the methods used and to critically assess the output and its implications for understanding seagrass spatial and temporal dynamics at that location. Using this format would allow the expansion and refinement required to explain this method in full and demonstrate its strengths and weaknesses. Once that is complete it can provide the basis for examining changes over multiple sites and ideally the entire coastline of this study area, or even other sites around the globe. This follow on work and its interpretation would be suitable for Nature Communications. This second stage could also build collaborations with others like Prof. Nick Murray's group that have extensive experience in global scale inter-tidal remote sensing monitoring. Specific comments are in the table below and attached file.

Reviewer #2 (Remarks to the Author):

This MS provides an enormous and hugely promising dataset on the extent of intertidal seagrass beds across Europe. It not only attempts to map the seagrass beds, but also aims to examine temporal trends in that seagrass that are described as phonological trends. The scale of assessment conducted here is unprecedented in Europe, so I can see the reason for submission to a major journal and feel, in principle, this is the right decision.
As the authors explain, seagrass beds are commonly covered by macroalgae. A quick search of the WFD status of many of the locations in question raises the fact that many are in zones of elevated nutrients, mostly nitrate that will lead to high

levels of Ulva macroalgae. The authors do provide a means of trying to separate the algae from the seagrass, but this is not substantiated and explained very well, and the authors do not provide any level of validation for this approach in their study. Within each 10m square of seagrass, there is a strong possibility that there exists algal cover. It may simply be the case that none of your sites have significant algal problems, therefore the data is representative, but you don't provide any evidence for this.
The analysis seems to be a big stretch to say that training data was only done in Bourgneuf Bay, Ria de Aveiro and Cádiz Bay. I find this extremely spatially (and therefore environmentally) biased. Is this really appropriate for assessment of sites much further North? So you had no additional validation data for the other sites?
I wonder whether there is more data available for the sites, or local experts who can guide the analysis and help validate it, or perhaps alternative imagery that can validate it?
For me the findings around phenology are not really that unique and novel, the peaks change spatially with respect to local environmental forcing, what would make them unique is to provide the environmental data with them to show this reasons for the changes.
Overall I consider it a novel study but that the problems raised need to be dealt with first.

Reviewer #3 (Remarks to the Author):

Review for COMM EARTH & ENVT
Paper: Davies et al., A sentinel watching over intertidal seagrss phenology

The journal mentions a series of criteria and also presents what are listed as major questions in its guidance to reviewers, and end with some more classical review queries. In My view there is partial overlap between criteria and questions, but I answered them all.

Based on my assessment below, I suggest a major revision before a final decision

A criteria
1. are the conclusions novel?

The authors conclude that their finding of variable seasonality in areal extent of intertidal European seagrasses is novel. This is only novel if one ignores the relation between seasonally varying areal of a seagrass patch and shoot density or biomass at the heart of the patch. This relation, however, cannot be ignored (e.g. Suykerbuyk et al. 2016 Ecosystems 19, 296-310), and the variable seasonality in density or biomass with latitude has been published on extensively (Duarte, 1989, Mar Ecol Progr Ser 51, 269-276; Phillippart et al, 1995, Neth J Sea Res 33, 205-218; Vermaat & Verhagen 1996 Aquat Bot 52, 259-281; the authors cite De Los Santos, but could also have cited Brun or Perez-LLorens form the same group in addition to the papers I cite above). I therefore think that the statement on L69-70 that 'the population structure of the dominant habitat-forming species .. is still lacking for most intertidal seagrasses' cannot be maintained – a lot is published, and not only on Zostera marina or Z. noltei, but also on Cymodocea nodosa, which may not be that common intertidally.

Spatial extent of a seagras patch in principle is not the same as standing biomass at a point in the patch, but these are closely related, certainly if we take the growth form into account of the predominant species in the European intertidal seagrass beds studied here, Zostera noltei, beyond a sufficient light availability branching of the rhizome network increases the local density and expands the bed at its edges. The latter is picked up by the Sentinel imagery as soon as a threshold density is passed.

2. does the paper present strong evidence for these conclusions?

Well, the seasoanlity in the extent and its variation is apparent from the main body of the paper, the data and fitted curves presented in figure 2.

3. are the data presented and analysed in a state-of-the art way, including a coverage of uncertainty?

Yes and no. Yes, because the curve fitting appears to be state of the art, but no because obvious driving factors could easily have been included in an analysis of this observed change in seasonality. Tidal amplitude is known to vary greatly, and may be positive for net growth in muddy estuaries in the North, but negative where the risk of desiccation and high summer temperatures is more obvious in the South. I recall Leuschner & Rees having published something on desiccation in the German Wadden Sea but cannot retrace it. At least Suykerbuyk has a paper in PeerJ 2018 with elaborate experiments on precisely this subject. So replacing latitude by net light availability as well as temperature would have led to a more insightful analysis. And such data or relevant proxies should be easily available to the authors. This is themore peprtinent given one of the statements of the authors in the conclusions section where they write that their analysis has shown 'how important local information is'.

4. is this manuscript important to scientists in a particular field, and an advancement in the understanding?

The authors claim in the conclusions that their work 'could pave the way for automated real-time intertidal seagrass monitoring', which 'would support more effective use of effort and resources for seagrass resoration and management'. I would like to remove some of the rhetoric embellishments in these and similar claims, but I do indeed think that this is a main

achievement of the work. Having mapped intertidal Zostera noltei myself in situ I can only confirm that this is hard work. Using Sentinel 2 imagery rather than having to fly drones locally no doubt is also saving labor and thus cost. So I think this convincing assessment of seagrass extent at so many sites with one and the same method is useful – more as habitat mapping for coastal zone management, than for the advancement of intertidal seagrass ecology maybe.

B. Major questions
1. what are the major claims of the paper
that they present a cost-effective and sound mapping method for the extent of intertidal seagrass beds. And I think they can rightly claim this.

2. are these novel? If not, who else did it before? – overlap with C1
See my answer to criterium 1 above. Not so novel. Zoffoli published much on the method, but the comprehensive assessment of so many sites with the same method is good.

3. does the paper influence the thinking in the field – overlap with C4

I do not think this influences our thinking. Satellite imagery has been used before for the estimation of seagrass bed extent. The work presented is just very thorough and consistently done for most of the European Atlantic shores.

4. are the claims convincing? What further experiments would be needed?

Not necessarily experiments, but a multivariate analysis of the potentially explanatory facotrs for this NS gradient in seasonality can improve the paper and should not be a major effort.

5. Are possibly needed further necessary experiments feasible?

Further statistical analysis is necessary and feasible, yes, in my view.

6. Are the claims approriately discussed in the light of the literature?

The justification is lengthy and unnecessarily so. I wish they had abstained from the rhetoric trick that so little is known about these intertidal seagrass beds, that they are in peril or that they 'directly provide ecosystem services' (summary paragraph L28). The paper does not need to repeatedly claim how important these seagrass beds are – one sentence in the intro referring to one of these key papers like Waycott (2009) or Orth (2020) – they cite both) should suffice to then come to the point. I also think the authors ignore or forget the major literature on seasonal seagrass biomass and density variation – I argue above that spatial extent and standing biomass are related.

7. Is the paper sufficiently promising to recommend re-submission?

In my view the data set is. But I would recommend a rigorous removal of partly repetitive and certainly rather wordy stretches of text (L210-228 for example can be missed without any pain, L191-208 is not based on the data presented and can easily be missed). And I would recommend inclusion of a multivariate analysis of potentially explanatory factors (light availability, desiccation,..) for this shift in seasonality, plus a brief justifcation of some testable hypotheses (one or two) that would conclude my introduction.

C. And then a list of more classical review questions
1. Is the paper written in a clear fashion?
The paper is wordy.

2. can it be shortened?
Yes. I could easily remove whole paragraphs without loosing critical informarion – see comment under Q7

3. no overselling?
To my taste there is very much overselling in the way the paper is written. Martin Attrill is one of the co-authors. He is a highly experienced publisher and respected scientist (https://www.plymouth.ac.uk/staff/martin-attrill). It would have been easy for him to adjust the style rigorously, which would improve this paper substantially.

4. fair treatment of previous literature?
If my proposed relation between extent and standing biomass is accepted, a considerable literature is ignored.

5. Sufficient method for the work to be repeatable?

I am not so sure. The authors mention for example on L 82 that green algae created problems (Ulva, Enteromorpha), and then argue that machine learning can overcome this. In the methods, however, they do not show how they tackled this. They classify pixels into 9 classes including one for Ulvophyceae. However, they do not show how they learned their AI machine to remove these potentially confounding macroalgae that may also have filled pixels. This is an issue because the reflectance wave bands are so similar and mats of these taxa can have very high cover inside and outside intertidal seagrass beds. Also the text explaining the validation procedure is rather brief and the reference (6) cited for the geo-located photoquadrats is incomplete and thus not traceable.

6. Are the statistics sound?

I trust the manipulation of the remote sensing spectra to arrive at these NDVI and then vegetation cover since the key reference, the Zoffoli paper is accessible and in my view a solid piece of work. I also trust the GAM fitting leading to figure 2.

However, I do not think the rate of change plots in this panel Figure 2 add much – these are only derived form the observations and we can see when the slope is positive or negative in each of the upper panels. I would however ensure a more spaceous layout of each of the individual plots apply consistent vertical axes and few major ticks as well as no or few grid lines. I like the pattern suggested in figure 3a,much more than the far less clear one of figure 3b. Fig 3a would benefit from an explanation in simpler language of what is presented.

Then, as I said before, the paper would benefit from including a multivariate analysis of potentially driving foctors for this change in timing of seasonality, rather than just a limited speculation in L 146 and beyond. Such an analysis would give empirical support to what now is just a conceptual diagram in figure 5.
Figure 4 in my view does not add that much to the many images presented already in the Zoffoli paper.
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Author Rebuttal letter:

Reviewer 1

Reviewer 1 Comment 1: More specific title: A Sentinel Watching Over Intertidal Seagrass Phenology." in western Europe and north Africa
Response: We have updated the title to include location.
Reviewer 1 Comment 2: Line 25: Change âseagrass meadowsâ to "seagrass cover and composition" (see https://goosocean.org/what-we-do/framework/essential-ocean-variables/)
Response: We agree. This has been amended.
Reviewer 1 Comment 3: Entire Manuscript: Change: subtidal to sub-tidal and intertidal to inter-tidal
Response: We have made this change throughout.
Reviewer 1 Comment 4: Line 30-31: Define "large" in km^2
Response: This was more a general comment, we have added 10s to 100s of km^2 but many intertidal areas may be smaller than 10 km^2 but will still be resource intensive to monitor for other practical reasons. The line now reads: âThe process of monitoring large Inter-tidal areas that can be 10s to 100s km^2, is, however, resource intensive and unfeasible in many regions.â
Reviewer 1 Comment 5: Line 33: Note this is "moderate spatial resolution and high-temporal resolution" - as "high" spatial resolution is < 5m
Response: Compared to moderate resolution satellite missions such as MODIS (250 m) or Sentinel-3 (300 m), Sentinel-2 (10 m) is high spatial resolution, so we prefer to keep âhigh resolution (10 m)â to avoid the risk of confusion with coarser satellite missions.
Reviewer 1 Comment 6: Line 34: Delete âcost effectiveâ - there is no evidence presented this is cost effective, plus you are using $billions of resources that others have invested in the satellites and processing environments and field data etc.

Response: Cost-effective has been remove as this has not been proven in the current work. However, the original meaning was more that it is cost-effective for the end user. Like you state, billions of public and private money has gone into these tools but a large portion of that has been specifically to make them freely and easily accessible for users.
Reviewer 1 Comment 7: Line 35 Define continental scale
Response: We have added more detail to this line, saying the continental scale is the Atlantic European Coastline.
Reviewer 1 Comment 8: Line 64-66: Yes, but based on your reasoning in preceding sentences, the main impact here is on limiting our ability to have fundamental baseline and monitoring information to develop and implement on-ground/water management activities through legislation to sustain these critical systems. Then you can get to goods and services and however you want to value them for the purposes of carbon accounting and trading, which is/are purely economic â not about direct action to conserve.
Response: We agree. This sentence has been altered to reflect that seagrass coverage is required for any of these types of assessment. The sentence now reads: âHowever, there are large uncertainties around regional and global estimates of seagrass coverage, which are the baseline requirements for assessments of the ecological goods and services that they provideâ
Reviewer 1 Comment 9: line 71: Replace âcost effectiveâ with "accurate, robust, repeatable and cost-effectiveâ
Response: We agree. This has been added.
Reviewer 1 Comment 10: Line 72-76: Suggest this is revised to present an evidence based and pragmatic justification â no tone that uses the unfortunate cliche of "traditional field survey is more costly and time consuming and remote sensing is not..." The references provided here do not support your statements, nor do you have evidence nor does the paper assess cost effectiveness. Focus on what the paper does and the unique reasons why remote sensing with supporting field data can provide continental to global scale mapping and monitoring at detailed (< 10m) spatial scales. e.g. we now have access to regularly collected (daily to 5-7 days) high spatial resolution satellite image archives, allowing cloud-free, specific tidal stage data to be collected and processed globally to map and monitoring specific environments/vegetation communities - see the examples at which cover intertidal, coral reefs, sand others https://www.globalecologylab.org/datasets-and-software
Response: We have removed the first sentence, which highlighted the disadvantages of traditional field survey methods (with an incorrect reference as highlighted by the reviewer) as it is not the focus of this work.
Reviewer 1 Comment 11: Line 86-87: Delete these lines - S2 bands are suitable for terrestrial and aquatic application and an advance on Landsat bands.
There are no operational satellite hyperspectral imaging systems, nor will there be at the scale of Sentinel 2, Landsat or the Planet archive
Response: We have removed discussion of S2 here to more emphasise the original point of the uses of Machine Learning.
Reviewer 1 Comment 12: Line 106-107: Extend sentence to include - and enable development and implementation of management to conserve these environments.
Response: We agree. This has been added.
Reviewer 1 Comment 13: Line 108-109: This could be expanded to capture more of the detail and significance of this work, e.g. Our hypothesis is that there are significant seasonal variations in inter-tidal seagrass extents and cover and that the timing and magnitude of these differ across continental scales. Understanding these variations is essential for development and implementation of seagrass conservation and management legislation.
Response: We agree and have added the recommendation.
Reviewer 1 Comment 14: Line 115-116: Delete as this is out of scope
Response: We agree. This has been removed.
Reviewer 1 Comment 15: Line 118-119: To be understood and assessed by readers there needs to be statement about the validation data used - e.g. source, number of sites and how it was matched spatially and temporally with the Sentinel2 mapping results.
Response: We agree, without reading the methods the reader would not have this information. Information about the validation has been added, as well as a brief summary of seagrass classification and seagrass extent assessment, in order to ease the reading.
Reviewer 1 Comment 16: Figure 3: Change the symbols and link to caption to clearly show which is minimum and maximum points, as at present it is very hard to tell the difference and interpret graphs a) and b) .
OR
for 3a) present each site as bar, with + or - std error on each end. This shows the latitudinal difference more clearly.
Response: We agree. We have edited shapes of points to be different for maxima and minima in Figure 3 a.
Reviewer 1 Comment 17: Line 155-189: This section is very rough and appears as a first draft in terms of its lack of structure, overall narrative and key points.
Suggest the authors read more widely on the range of global, high spatial resolution

mapping and monitoring programs in place using Sentinel 2 and Planet data sets, as well as considering NDVI limitations, then re-write this in a form that has a clear main message and easy to follow structure.
Response: We thank the reviewer for highlighting this. The section has been altered to have a more clear narrative by splitting into sections.
Reviewer 1 Comment 18: Line 156-158: It is not clear if this is actually over the entire continental scale, or it is just from 12 sites over a continental extent?
Response: We thank the reviewer for highlighting this. This has been clarified. âThis work constitutes the first assessment of inter-tidal seagrass phenology, spanning 12 sites across a continental scale, the North-East Atlantic (over 23ï° of latitude), utilising 7 years of Sentinel-2 multispectral imagery (~800 images).â
Reviewer 1 Comment 19: Line 160-163: This does not reflect the current state of EO capabilities for monitoring coastal and inter-tidal areas at high spatial resolution (< 5m pixels)m, from multiple daily collections, with suitable spectral bands (e.g. S2, Planet Super Dove) and high radiometric resolution that are consistent calibrated over time - AND all available from global archives , located next to geospatial processing and compute capabilities (GEE, AWS, Planetary Computer, SentinelHub etc) .
Response: We thank the reviewer for this comment. However, from our current experience this comment still stands, although we have changed the wording to read: âthere are many technical and practical considerations inherent to EO, such as spatial, temporal and spectral resolution, data access, processing and computing capability (Veettil et al., 2020), where assessment objective will dictate the choice of the sensor and the processing pipeline, by prioritising certain elements over others.â.

Firstly, Planet Super Dove is the only global scale satellite imagery with high spatial resolution, adequate spectral resolution, high revisit time and consistent calibration. However, it is only available through a registration process that can take up to 6 weeks. It is also only available to users âlocated in the territory of ESA Member States (including Canada) and in the European Commission Member Statesâ. Conversely, Sentinel-2 imagery is freely available after creating an online account within minutes, but does have lower spatial resolution and revisit frequency.

Secondly, computing capabilities are advancing, and we have added details to this extent in the discussion thanks to your recommendation. However, there are still barriers for some on these platforms. AWS requires credit card details even when opting for free usage. GEE is free to use and easily accessible from multiple frameworks (python, javascript code editor etc.), and the authors are utilising this framework extensively in work that you even suggest further up in your general comments. Although, integration with neural networking capabilities and data downloading issues/delays have slightly detracted from the level of processing power available when totally cloud based.

Reviewer 1 Comment 20: Line 164: Note that NDVI is not a suitable index to use where you are attempting to estimate vegetation cover on background substrates that are dark in colour and/or wet it has a highly non-linear response to cover at low (< 30% cover) and high (> 80% cover) which have been shown consistently across range of vegetation communities globally. As other indices, e.g. SAVI , EVI d not remove these biases there has been a shift to direct estimates of cover or fractions (e.g. see the approaches used by kelp mapping and monitoring communities) using satellite data set as indices are not suitable.
Response: We thank the reviewer for this comment. As stated, there are a few biases inherent with using NDVI to estimate vegetation cover. Firstly low values are troublesome, but as mentioned in the methods and in discussion of limitations of EO, all SPC values below 20% were removed from analysis. We also highlight the saturation effect found at high levels of NDVI. However, the NDVI required to reach 100% SPC for inter-tidal seagrass is relatively low compared to other vegetation types ((100+22.18)/172.06=0.710; calculation using Zoffoli et al. 2020). Thus, it is below the value where NDVI begins to show non-linear behaviour, such as for other vegetation types (~0.75). This has been added to the discussion.
Reviewer 1 Comment 21: Line 190-208: Suggest delete this section as it does not add any new knowledge and does not take in the work already done on building close to global scale inter-tidal, seagrass and coral reef monitoring systems.
Response: We agree. This has been removed.
Reviewer 1 Comment 22: Line 209-228: Similar comment to last paragraph, the purpose of this paragraph, in terms of how it builds on the results presented and their potential application in seagrass restoration is not clear -if results obtained were directly linked to restoration project planning and monitoring requirements this would help.
Response: We thank the reviewer for this comment. We have more emphasised how the

analyses relate to changing climates, specifically the new analysis assess temperature, and how this relates to future restoration projects.
Reviewer 1 Comment 23: Line 230-233: Change sentences to keep in scope of sites usedâ This research has provided the first full phenology assessment of intertidal seagrass using 12 sites across a latitudinal gradient at continental scale using satellite remote sensing. Contrary to established assumptions, we find that there are substantial differences in timings, magnitudes and rates of change of seagrass extents at 12 sites across 23â of latitude along the North-East Atlantic coastline.
Response: We agree. This has been edited as suggested.
Reviewer 1 Comment 24: Line 236-241: Requires more refinement based on more thorough assessment and synthesis of published seagrass and intertidal mapping and monitoring.
Response: We thank the reviewer, refinement of this section has been added per some of the other comments.
Reviewer 1 Comment 25: Methods: This needs an introductory section with detailed flowchart explaining in text and graphically the sequence of operations used to collect the S2 data, drone data, and photo-quadrats and to then process each data set to produce the output seagrass extent and cover maps. The individual sections contain a lot of detail, but it is not clear how they fit together.
This introduction/overview should also include details of how the satellite/drone/validation data are matched in terms of location and time, and pixel/sample size to generate validations statistics.
The methods should have sufficient detail and logical structure that they could be replicated by another group with similar skills - this essential information is lacking.
Response: We thank the reviewer for this comment, the methods have been extensively rewritten with the addition of a workflow (textual and graphical) to describe the process of creating the Neural Network Model.
Reviewer 2:

Reviewer 2 Comment 1: As the authors explain, seagrass beds are commonly covered by macroalgae. A quick search of the WFD status of many of the locations in question raises the fact that many are in zones of elevated nutrients, mostly nitrate that will lead to high levels of Ulva macroalgae. The authors do provide a means of trying to separate the algae from the seagrass, but this is not substantiated and explained very well, and the authors do not provide any level of validation for this approach in their study. Within each 10m square of seagrass, there is a strong possibility that there exists algal cover. It may simply be the case that none of your sites have significant algal problems, therefore the data is representative, but you donât provide any evidence for this.

Response: We thank the reviewer for highlighting this issue. We detail more clearly the method by which model classification was validated to avoid misclassification in the presence of green algae.

Reviewer 2 Comment 2: The analysis seems to be a big stretch to say that training data was only done in Bourgneuf Bay, Ria de Aveiro and CÃ¡diz Bay. I find this extremely spatially (and therefore environmentally) biased. Is this really appropriate for assessment of sites much further North? So you had no additional validation data for the other sites? I wonder whether there is more data available for the sites, or local experts who can guide the analysis and help validate it, or perhaps alternative imagery that can validate it?

Response: We thank the reviewer for highlighting this potential confusion. We feel this has been addressed by our improvement and clarification of methods. Firstly, training data of the classification model ranged from Veys Bay down to Ria de Aveiro Coastal Lagoon, covering almost 10 degrees of latitude (Supplementary Figure 1). Seagrass training data specifically came from 4 different French estuaries and Ria de Aveiro Coastal Lagoon. The validation data came from a different group of locations, from Cadiz bay to Tamar estuary. The validation score of around 82% (across ~12,000 Sentinel 2 pixels) shows that the model is apt at predicting seagrass outside of the sites and latitudinal ranges it was trained on.

Reviewer 2 Comment 3: For me the findings around phenology are not really that unique and novel, the peaks change spatially with respect to local environmental forcing, what would make them unique is to provide the environmental data with them to show this reasons for the changes.

Response: We thank the reviewer for highlighting this. We have added a new analysis assessing two drivers of seagrass extent, temperature and direct solar radiation. The novel element for this work is that the phenology of intertidal seagrass is different across this large latitudinal gradient, which while it is a logical assumption (given other phenology work) has not been shown and has even been overlooked for inter-tidal seagrasses.

Reviewer 3

Reviewer 3 Comment 1: The authors conclude that their finding of variable seasonality in areal extent of intertidal European seagrasses is novel. This is only novel if one ignores the relation between seasonally varying areal of a seagrass patch and shoot density or biomass at the heart of the patch. This relation, however, cannot be ignored (e.g. Suykerbuyk et al. 2016 Ecosystems 19, 296-310), and the variable seasonality in density or biomass with latitude has been published on extensively (Duarte, 1989, Mar Ecol Progr Ser 51, 269-276; Phillippart et al, 1995, Neth J Sea Res 33, 205-218; Vermaat & Verhagen 1996 Aquat Bot 52, 259-281; the authors cite De Los Santos, but could also have cited Brun or Perez-LLorens form the same group in addition to the papers I cite above). I therefore think that the statement on L69-70 that âthe population structure of the dominant habitat-forming species .. is still lacking for most intertidal seagrassesâ cannot be maintained â a lot is published, and not only on Zostera marina or Z. noltei, but also on Cymodocea nodosa, which may not be that common intertidally. Spatial extent of a seagras patch in principle is not the same as standing biomass at a point in the patch, but these are closely related, certainly if we take the growth form into account of the predominant species in the European intertidal seagrass beds studied here, Zostera noltei, beyond a sufficient light availability branching of the rhizome network increases the local density and expands the bed at its edges. The latter is picked up by the Sentinel imagery as soon as a threshold density is passed.

Response: We thank the reviewer for his suggestions of references we have now included in the manuscript. We now mention these data, obtained from in situ measurements, in the introduction and the discussion. We modified the statement L69-70, which now reads, âRegular mapping is still lacking for most intertidal seagrassesâ.
Besides the assessment of seagrass phenology, an important result of our work is also to provide, for the first time, a consistent assessment of inter-tidal seagrass distribution and extent over 12 sites from North Africa to northern Europe. Providing up-to-date data on seagrass distribution is one of the most important challenge for seagrass conservation (Unsworth et al., 2018, Global Challenges for seagrass conservation).

Reviewer 3 Comment 2: 3. are the data presented and analysed in a state-of-the art way, including a coverage of uncertainty?

Yes and no. Yes, because the curve fitting appears to be state of the art, but no because obvious driving factors could easily have been included in an analysis of this observed change in seasonality. Tidal amplitude is known to vary greatly, and may be positive for net growth in muddy estuaries in the North, but negative where the risk of desiccation and high summer temperatures is more obvious in the South. I recall Leuschner & Rees having published something on desiccation in the German Wadden Sea but cannot retrace it. At least Suykerbuyk has a paper in PeerJ 2018 with elaborate experiments on precisely this subject. So replacing latitude by net light availability as well as temperature would have led to a more insightful analysis. And such data or relevant proxies should be easily available to the authors. This is themore peprtinent given one of the statements of the authors in the conclusions section where they write that their analysis has shown âhow important local information isâ.

Response: We thank the reviewer for this comment and have added the mentioned analysis. It is definitely not an exhaustive analysis but does help the discussion of the phenology itself.

Reviewer 3 Comment 3: 6. Are the claims approriately discussed in the light of the literature?

The justification is lengthy and unnecessarily so. I wish they had abstained from the rhetoric trick that so little is known about these intertidal seagrass beds, that they are in peril or that they âdirectly provide ecosystem servicesâ (summary paragraph L28). The paper does not need to repeatedly claim how important these seagrass beds are â one sentence in the intro referring to one of these key papers like Waycott (2009) or Orth (2020) â they cite both) should suffice to then come to the point. I also think the authors ignore or forget the major literature on seasonal seagrass biomass and density variation â I argue above that spatial extent and standing biomass are related.

Response: We thank the reviewer for this comment and have consolidated the justification.

As above, we have avoided discussion of biomass/shoot density, as remote sensing at the scales discussed is poorly understood and relationships between these different metrics of intertidal seagrass meadow composition are unknown. As mentioned in our answer to the first comment, we have now added previous references on seasonal seagrass variations.

Reviewer 3 Comment 4: 5. Sufficient method for the work to be repeatable?

I am not so sure. The authors mention for example on L 82 that green algae created problems (Ulva, Enteromorpha), and then argue that machine learning can overcome this. In the methods, however, they do not show how they tackled this. They classify pixels into 9 classes including one for Ulvophyceae. However, they do not show how they learned their AI machine to remove these potentially confounding macroalgae that may also have filled pixels. This is an issue because the reflectance wave bands are so similar and mats of these taxa can have very high cover inside and outside intertidal seagrass beds. Also the text explaining the validation procedure is rather brief and the reference (6) cited for the geo-located photoquadrats is incomplete and thus not traceable.

Response: We thank the reviewer for highlighting this. The methods have been extensively improved to show full details of all steps done, with clearer descriptions of data collection, model building, and model validation.

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Davies,

Your manuscript titled "A Sentinel Watching Over Inter-Tidal Seagrass Phenology Across Western Europe and North Africa." has now been seen by 2 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication. Specifically, we ask you to:

1.Compellingly demonstrate the potential of Sentinel-2 satellite data to study spatial and temporal dynamics of inter-tidal seagrass communities and their phenology across western Europe and north Africa.

2. Make a robust argument regarding the potential of discriminating Ulva-like greens and angiosperm seagrasses.

3. Compellingly demonstrate the impact of light and temperature on the extent of seagrass beds.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Christopher Cornwall, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-6154-4082

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have addressed the majority of my requested changes in full.

Where the authors have not addressed the requested changes, the explanations provided are adequate.

I appreciate the thorough and constructive approach taken in responding to my comments.

The paper is in a form that can be published.

Reviewer #3 (Remarks to the Author):

see the attached document

Author Rebuttal letter:

Reviewer 1

Reviewer 1 Comment 1: The authors have addressed the majority of my requested changes in full.

Where the authors have not addressed the requested changes, the explanations provided are adequate.

I appreciate the thorough and constructive approach taken in responding to my comments.

The paper is in a form that can be published.

Response: thank you very much! We thank the reviewer for all previous comments, which helped us to improve the revised manuscript.
Reviewer 3
Reviewer 3 Comment 1: Style of the written text. Both myself (reviewer 3) and reviewer 1 were quite critical about how the paper is structured and written. I cite from rev 1 â.. needs significant extra work to its structure, content, narrative and key findings..â. I am afraid this issue has not been resolved sufficiently yet. The authors do not respond to this in their rebuttal and appeared to have only ârepaired a bit here and thereâ, which is their good right, but it has not convinced me of the maturity of this manuscript. In my first review I argued for a shorter and more factual introduction and asked for hypotheses â since the authors have been publishing more on this data set (e.g. Davies et al, 2023 RSE 290, 113554, same first and several other authors), their work should have allowed the formulation of such hypotheses. This may feel artificial when the work has been started out as mainly exploratory, but it simply helps in a clear structure and it allows the removal of generic bits of text improving the readability.
I am glad that the authors have now presented a hypothesis in the into, with the following formulation âOur hypothesis is that there are significant seasonal variations in inter-tidal seagrass extents and cover, and that the timing and magnitude of these differ across continental scalesâ. I would propose that the authors can be far more specific â hence make the hypothesis testable or even rejectable. Now, when read alone, this hypothesis is largely confirmed by textbooks in seagrass ecology (Hemminga & Duarte; Larkum et al) and would not need any assessment of seasonality in spatial extent with hyperspectral sensor data.

Response: First of all, we would like to thank the reviewer for all the fundamental and detailed comments, this is really appreciated.
We agree that our objectives could be more specific and reformulated the last paragraph of the

introduction with two explicit hypotheses: 1) there is a latitudinal trend in inter-tidal seagrass biomass seasonality (building on the discussion from Duarte 1989 and Ito et al., 2021); and 2) the broad-scale latitudinal pattern is driven by climate-related parameters such as air temperature and solar irradiance, as previously demonstrated for sub-tidal seagrass (Clausen et al. 2014) and terrestrial grass (Zhang et al. 2004). In this last paragraph, we also removed two phrases to improve the readability as suggested. We also changed the structure of the first part of the introduction, removing unnecessary sentences and reformulating others (answering comments 7 and 8) to make it shorter and more factual as suggested.

Moreover, all reviewers desired clarification of the methods and methodology used, which has now been extensively changed.

To avoid any misunderstanding, we also would like to highlight that the dataset and analysis carried out here are not the same as that in Davies et al. (2023). The analysis performed by Davies et al. (2023) was based on the analysis of a library of hyperspectral in situ reflectance measurements (see also our response to your comment #2), whereas the present study is based on multispectral remote sensing data of the Sentinel-2 satellite mission.

Reviewer 3 Comment 2: The distinction between Ulva-like greens and angiosperm seagrasses. Together with reviewer 2, I had difficulty in understanding how the spectra of these two groups with similar chlorofyll a and b were separated. The authors respond in their rebuttal that they âdetail more clearly..â It may be my limited understanding of the matter, but I could not find how they separated exactly these two classes in the current paper. Fortunately they refer to the Davies et al paper (2023, RSE), which has this question as one if its subjects across a range of sensor platforms. Davies et al (figure 5) show that only a handheld ASD spectrophotometer and the PRIMS satellite with a hyperspectral sensor on board actually do a really good job, the other sensors tested are substantially worse to really poor. Unfortunately the Sentinel 2 (with 10 m resolution) also did not differentiate very well and the paper then also carefully and rightfully concludes that âour work demonstrates the potential of discriminating intertidal seagrasses from Ulvophyceaeâ. However, the current paper uses Sentinel 2 with 10 m resolution. I think this issue should be clarified in a simple and straightforward text.

Response: We thank the reviewer for highlighting this confusion between methods, which indeed originates from a misunderstanding of Davies et al. 2023. In order to avoid any risk of confusion, this issue has been clarified in the revised manuscript.

As the reviewer correctly states, in Davies et al. 2023, a library of hyperspectral in situ reflectance data was analysed to assess different spectral resolutions at distinguishing between taxonomic classes of intertidal habitats/classes. The primary visual based analyses, shown by figures 4 and 5 in that work, found it difficult to separate these classes unless at hyperspectral resolution (such as with a handheld ASD spectroradiometer or using the PRISMA satellite mission). However, reading further on in the paper (e.g. figure 6) the authors use a random forest analysis to show that, even at the low spectral resolution of Pleiades (i.e. with only 4 spectral bands in the blue, green, red, and near-infrared), classes could be distinguished with an accuracy of around 80%; the discrimination accuracy increased up to 95% with the 8 spectral bands of Sentinel-2 20 m. In the present work, we used the full spectral potential of Sentinel-2: its 10 spectral bands, which have a native spatial resolution of 10, 20, or 60 m but were spatially resampled at 10 m (this is probably here that the confusion arises). In particular, Davies et al. 2023 showed class specific accuracies of over 95% for this spectral resolution between Ulva and Seagrass (see in their figure 9, middle right hand panel for Sentinel-2 20 m). The ability of Sentinel-2 to distinguish between green algae and seagrass was explained as follow in Davies et al. 2023: in the visible range, âspectral differences have been observed between two classes having a similar pigment composition, the Magnoliopsida and the Ulvophyceae (Table 3). This indicates that the pigment concentrations and relative proportions, which can vary inside the main vegetation groups (Bargain et al., 2013; Beach et al., 1997), contribute to the spectral discrimination between taxonomic classes sharing the same pigment compositionâ. Moreover, âNIR wavelengths have long been recognised as relevant for the spectral discrimination of terrestrial plant diversity (Schmidt and Skidmore, 2003). At these wavelengths, spectral signatures are mainly a function of light scattering determined by the internal structure of leaves for angiosperms or thallus for macroalgae (Guyot, 1990). Fyfe (2003) showed that seagrass species could be separated using NIR wavelengths, with a significant change in the slopes between 700 and 900 nm.â

Reviewer 3 Comment 3: The inclusion of light and temperature as potential drivers is a clear improvement. But this could also have provided the basis for hypotheses to structure the paper. For example, itis pretty counter-intuitive that light has a negative effect on seagrass bed extent (Fig 3.). Is this due to the analytical approach chosen where light and temperature are GAM-regressed as independents explaining the extent (E, L 144 suppl material explaining methods), but without their interaction. This joint analysis could cause a stronger apparent effect of

temperature, possibly. Also, the authors do not present any statistics of goodness of fit other than the confidence intervals, I would be interested in an overall RMSE or r2 and p level of the GAM model for this whole dataset in the caption of figure 3. Then, could it be that the light dose range for Z. noltei between a minimum (for survival) and a maximum (saturating growth rates) is surpassed by most sites studied here? Intuitively I would find it unlikely. (b) And there is one caveat: the light units the authors have downloaded are not PAR, and of course they are not the net available PAR over the tidal cycle (in mol /m2/d). Now the time window the authors use is about two weeks and this is close to half a tidal cycle, so periods of darkness under turbid water during daytime high tide will likely be averaged out and the light in units used here will be useful anyway. But if they had Secchi disk measurements â or would have done a separation of the spectral profiles during high and low tide, their new figure 3 may have benefitted. A further, easily quantifiable driver, tidal amplitude, could have helped in approximating light availability, but may also be important in itself as it governs tidal flow rates and hence erosivity. (c) the authors have downloaded temperature data, but are these water temperatures or air temperatures? Intertidal water temperature is far more variable than subtidal water temperature and possibly than air temperature, but the authors should at least specify which temperature they include.

Response: We thank the reviewer for appreciating the added analysis. Following your suggestion, the inclusion of light and temperature was also used to improve the introduction (see answer to comment 1).
The comment on the counter-intuitive nature of the negative effect of light was actually alluded to by the reviewer in the previous rounds, while discussing the effects of temperature and tidal amplitude: ââ¦negative where the risk of desiccation and high summer temperatures is more obvious in the Southâ. Thus, we concur that increase light radiation will lead to higher chance of desiccation in the southern sites.
Regarding the comment on the statistics, we argue that as a General Additive Mixed effect Model carried out in a Bayesian framework, RMSE, r2 and p values are not the most suitable practices. For this type of model the generally accepted methods for checking model function is values of rhat (for MCMC functioning) and posterior predictive checks (for model parameterisation), which were carried out with all rhat values being 1 and posterior predictive checks showing graphically the data suited the model well. However, as the reviewer comments on the effect plot (fig 3), the overlap of 89% confidence intervals can be used in a similar way to see large differences between locations in the effects.
This is the norm for interpreting these types of models, with any effects overlapping the 0 line being considered as ânon-significantâ (to use frequentist terminology). We also feel, thanks to one of the reviewers other comments, that by converting the effects to be in km2 units has made this apparent effect less defined due to the smaller size of many of the southern seagrass beds. We have also added the light and temperature ranges to be clearer.

(b) We agree that the units of the irradiance data (i.e. the Direct Normal Radiation) could be expressed in W/m2 rather than in J/m2/h and it has now been changed in the manuscript. We agree that tidal amplitude could be a key driver of seagrass extent, but our analysis was designed and aimed at being descriptive of seagrass phenology and not an assessment of all potential drivers, this is hopefully coming soon elsewhere. Furthermore, tidal amplitude will be relatively consistent across a location over the years of analysis, therefore the location factor in our model will be already providing this information in a factorial manner. (c) we thank the reviewer for highlighting this and direct the reviewer to line 116-117 in the methods where the Environmental Driver Data are introduced: âMonthly Air Temperature and Direct Normal Radiation from 2017 to 2023 were downloaded â¦â. Moreover, we now explicitly refer to air temperature at the end of the introduction.

Reviewer 3 Comment 4: Title: I would rather use âNorth East Atlanticâ, than âWestern Europe and North Africaâ, just like you do in most of the text.

Response: We appreciate this comment from the reviewer. However, this was a request from another reviewer previously and we feel more clearly states the geographical extent of the sites assessed.
Reviewer 3 Comment 5: References in text and supplementary material: quite some capitals and italics are missing.

Response: We thank the reviewer for highlighting this. Alterations to errors in the references have been made according to the citation method of Nature Comms Earth and Environment.

Reviewer 3 Comment 6: The notion in the summary paragraph and main text that intertidal seagrass beds also provide services to terrestrial ecosystems (L30 revision) is odd to me. First, ecosystems provide services to humans (see e.g. IPBES, or a vast proportion of the ecosystem services literature), and second, what services to land do you think of? Marba et al have provided evidence that Caribbean seagrass beds reduced damage and erosion after a

hurricane passed by, but that is again, a service to mankind. If we have to think of Z. noltei as a major food source to for example brent geese, then which part of the food web is then terrestrial? I would simply remove this part of the introduction, as I already argued for in the first round. Please be precise when using ecosystem services as a concept, or as precise as possible, and just sticking to the functions of these plants in the coastal ecosystem(s) should suffice.

Response: We thank the reviewer for highlighting this. This has been removed as suggested.

Reviewer 3 Comment 7: A.o. on L89 the authors argue that the community has been biased to the period of peak growth (literally: âa focus of the community on the morphometrics or density of seagrass during these so-called periods of âpeakâ growth or âpeakâ extentâ). I do not see why the limitations of other work has to be used so extensively to justify your work. Also, your justification derives from for example the papers by Soissons et al â and their work did not have the aim to unravel the spatial variation across seasons, but rather the seasonal variation in mechanical strength and carbon storage. I would be so much more pleased without the rhetorics that âothers have not done something so obviousâ so that now you can do it. I challenge you to rephrase your work in a positive way.

Response: We thank the reviewer for this comment and have reformulated this phrase in a more positive way, avoiding the stigmatisation of previous work

Reviewer 3 Comment 8: Somewhat similar to my previous point I respond to the argument in your intro that this type of remote sensing of extent is needed for restoration. I really wonder whether it truly is. Your argumentation runs as follows âTo be successful at scale, these efforts, especially restoration, require accurate and detailed information on spatiotemporal variability in the population structure of dominant habitat-forming species. Regular mapping is still lacking for most inter-tidal seagrasses.â. I largely agree but would counterargue that such remote sensing results only produce the coarse outlines of the extent of these seagrass beds, whilst ignoring the low density ones, whereas understanding the interplay of light requirements, clonal branching and (low) seedling establishment success versus losses due to the abrasive intertidal flow environment as well as herbivory would be far more helpful to understand what you call population structure.

Response: We agree with the reviewer that this methodology produces outlines of seagrass bed extent and definitely agree that elements such as light requirements, clonal branching and seedling establishment will be important. However, we do not argue against those elements: we just highlight, as other researchers have (see for example the âchallenge 2â in Unsworth et al., 2019), that a gap in current knowledge is the extents of intertidal seagrass beds and how those extents change over time. Our work is trying to fill this fundamental data gap, with the ambition that our results can be used to the scale up other more local results to the meadow or continental scale. We agree that the expression â population structureâ was not adapted and have reformulated this phrase in the introduction, adding also that knowledge of phenology could be useful for restoration.

Reviewer 3 Comment 9: Figure 5 in my view is still redundant. Figure 4: really illustrative of the difference in seasonality and the complexity of the local coastal geomorphology, but I wonder whether your areal estimate with three decimals is justified by the method. I would have chosen 2.1 km2 rather than 2.087. Figure 4: for some of the colourblinds among us (including me) the colours are very difficult to distinguish. I understand you chose a warm-cold gradient from S to N and that helps a lot since it aligns with the vertical axis. Can you, in addition, give the ranges in temperature and light that exist across the range studied? And explain what -1 0 +1 as population level estimate actually means in km2? It will make the units more understandable for me.

Response: We thank the reviewer for these comments on figures. We think that figure 5 is useful to visualise a theoretical scenario discussed in the text and may aid some readers to better understand the point. We agree that the precision of the km2 in figure 4 is not necessary, we have decrease significant figures to 3 rather than 4 (S2 scale of each pixel). The next point we assume refers to figure 3 not figure 4. We have, where possible, tried to negate the need for colour to interpret the graphs (or used well known colourblind friendly palettes), but as the reviewer points out this figure could be hard to distinguish. As such we have added site labels alongside their latitudes (meaning the graph is interpretable without colour). Temperature and Light ranges have been added within figure caption, and population level-effects have been converted to change in km2 per unit light/temperature.

Version 2:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Davies,

Your manuscript titled "A Sentinel Watching Over Inter-Tidal Seagrass Phenology Across Western Europe and North Africa." has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

Dr Alireza Bahadori
Associate Editor
Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #3 (Remarks to the Author):

please see my uploaded document. In my view the authors have met my major objections and only minor issues remain that can be delt with by the editor (fig 5 yes or no) or editorial office (linguistics,mainly).
Jan Vermaat

Author Rebuttal letter:

Many comments herein have been deferred to the editor so will be left as such.
Reviewer 3

Comment 3: (a) MINOR but RELEVANT: I do not think I argued for the use of W/m2 or KW, but rather for mol or E/m2/d, as it is the photons that enter into the photosystem rather than the energy they contain. One could argue that this is hairsplitting, but a plant physiologist would gretly prefer the flux unit of mol quanta /m2 /unit time. (b) MINOR BUT also RELEVANT. I understand the justification that the factor location will cover some of the effect of tidal amplitude, but it is less quantitative than the amplitude itself, and it may explain why some intertidal beds that share the same tidal amplitude share the same seasonal variation in extent. I have not done the analysis but I would guess the explanatory strength of a model incorporating tidal amplitude rather than just location would be stronger. â (a) We agree with the reviewer, they did not argue for KW, they in fact just argued against the previous units with no alternative suggestion. The difference between the two units come down to whether one considers light to be a wave of a particle. When considering light as a particle mol quanta / m2/unit time is used. However, this unit does not take into consideration the wavelengths of the light that make up the unit. As this analysis and manuscript are driven by spectral responses from satellites, wavelengths are highly relevant. Therefore, we have maintained the unit that is consistent with the theme of the research. (b) We appreciate that the reviewer agrees with are argument, and we also agree with the reviewer that providing further information on tidal amplitude and the spatial/temporal elements of how this changes and how that effects intertidal seagrass growth would indeed be interesting. But again as before, this is planned in future work and not the aim of the current research.

Comment 4: on the title. I leave this to the editor. A matter of taste what is more elegant. The currently proposed title certainly is more lengthy.

Comment 5: If the authors have followed the instructions of the journal I wonder how the journal looks at the spelling of geographical names or journals. New Zealand would not be spelled with lower case, would it? Refs 37-40 are examples as are ref 35 vs 36 for the odd use of âet alâ. Just do a careful manual/visual check of all references please.- We will be checking the references alongside the editor in the proofs stages of submission.

Comment 6-8: OK!

Comment 9: I maintain my point. Fig 5 is redundant and just occupies precious journal space. But this of course is to the discretion of the editor.

Small issues in the text
Title: drop the stop at the end please- We agree.

L 58 but several places in both âsummaryâ and âmainâ: the use of the â;â is very odd to my feeling of how English is properly written. But I am not a native speaker and leave this to the editors of the journal. â This manuscript has been written and lead by a native English speaker (from England) but we do appreciate that editorial style may dictate how semi colons and colons are used for readership. Further to this point we have maintained to English spellings of words consistently throughout the manuscript.

L63 how these may change = how this may change - Done

L80 confusions = confusionL87 rewrite like: â.. monitoring of the temporal variation in extent and distributionâ simpler and easier to read. - Done
L101: please remove âfor the first timeâ You do not need such claims and what if someone falsifies it? Just rely on the content of your work rather than on such rhetorics.-This is important

to the story so has been maintained.

L105: S2 I understand, but I do not think you explained that S2 = Sentinel 2. Jus introduce this abbreviation somewhere in the text.-Done

L109: âa myriad ofâ = several. By now you get my point on style I guess.-This has been kept.

L127: drop âhighlyâ-this has been kept.

Caption fig 3: lower case on Seagrass in first line. And seriously reconsider the unit KW/m2. Lowercase seagrass now included, and see above comment for units.

Fig 4. The âaâ and âbâ have a too small font size. The percent cover scale for seagrasses need sto mentioned only once. The same is true for the words latitude and longitude along the axes.- We agree this has been changed throughout.

L209: âother assessmentsâ: such as? Add a reference. And Ref 45 is NOT on Rio de Aveiro but on New Zealand. â This is a confusion where the âother assessmentâ is ref 45 from new zealand that we compare to Aveiro, the ref has been moved forward in the sentence to reflect this.

L226: âboth showed differing effectsâ = âshowed contrasting effectsâ.- Done.

L227: drop âvitallyâ- not changed for style.

L243 and 244: timings , magnitudes = use singular, timing, magnitude - Done.

L249: drop âstrongâ. â not changed for style.

NGS-2024-0081_T__review_Phinn

Title: "A Sentinel Watching Over Intertidal Seagrass Phenology."

Reviewer: Stuart Phinn
The University of Queensland

General Comments:

This paper presents a new application of an extensively approach from terrestrial vegetation dynamics to a set of 12 seagrass sites on the north-east Atlantic coasts and demonstrates new insights into spatial and temporal dynamics of inter-tidal seagrass communities. The use of continental to global scale, high spatial resolution satellite image archives to assess the spatial and temporal dynamics of terrestrial vegetation traits (structure and physiological parameters) is a well established field. The application in inter-tidal seagrass beyond plot scales is novel.

Although the application of satellite-time series to assess vegetation phenology is not new, the application to seagrass environments is. This paper represents and initial step to develop, test and scale-up this type of approach.

In its current form the paper needs significant extra work to its overall structure, content, narrative and key findings to present a coherent, justified and clearly explained set of text and graphics. The current paper is quite difficult to follow to determine exactly what was done and why in terms of the Sentinel2 processing, and how it was linked to the drone and field measurements at each of the 12 sites. The findings are beyond the scope of the data sets (12 sites) and analyses used. The justification for why to develop this type of capability, to map and monitor seagrass communities and understand their spatial and temporal dynamics needs significant extra work to move beyond general scientific knowledge to influencing modelling and management. These limitations are compounded by the confusing inter-changeable use through the entire text of what is actually measured and mapped in terms of seagrass cover (which is a % per unit area) and seagrass extent. It should be possible for readers with sufficient technical and scientific background to understand how to replicate the work from the paper – in its current form this is not possible. The table of detailed comments below sets out changes required to address these points.

The authors and the seagrass scientific and management communities would benefit significantly from this work being presented first in a discipline specific journal that focuses on detailed technical and applications based projects, such as Remote Sensing of Environment. This would allow the authors to explain in full detail the methods used and to critically assess the output and its implications for understanding seagrass spatial and temporal dynamics at that location. Using this format would allow the expansion and refinement required to explain this method in full and demonstrate its strengths and weaknesses. Once that is complete it can provide the basis for examining changes over multiple sites and ideally the entire coastline of this study area, or even other sites around the globe. This follow on work and its interpretation would be suitable for Nature

Communications. This second stage could also build collaborations with others like Prof. Nick Murray's group that have extensive experience in global scale inter-tidal remote sensing monitoring. Specific comments are in the table below and attached file.

Specific Comments:

Line number(s)	Comment
1	More specific title A Sentinel Watching Over Intertidal Seagrass Phenology." in western Europe and north Africa
25	Change "seagrass meadows" to "seagrass cover and composition" (see https://goosocean.org/what-we-do/framework/essential-ocean-variables/
Entire manuscript	Change: subtidal to sub-tidal intertidal to inter-tidal
30-31	Define "large" in km^2
33	Note this is "moderate spatial resolution and high-temporal resolution" - as "high" spatial resolution is < 5m
34	Delete "cost effective" - there is no evidence presented this is cost effective, plus you are using $billions of resources that others have invested in the satellites and processing environments and field data etc.
35	Define continental scale
64-66	Yes, but based on your reasoning in preceding sentences, the main impact here is on limiting our ability to have fundamental baseline and monitoring information to develop and implement on-ground/water management activities through legislation to sustain these critical systems. Then you can get to goods and services and however you want to value them for the purposes of carbon accounting and trading, which is/are purely economic - not about direct action to conserve.
71	Replace "cost effective" with "accurate, robust, repeatable and cost-effective"
72-76	Suggest this is revised to present an evidence based and pragmatic justification - not one that uses the unfortunate cliche of "traditional field survey is more costly and time consuming and remote sensing is not..." The references provided here do not support your statements, nor do you have evidence nor does the paper assess cost effectiveness. Focus on what the paper does and the unique reasons why remote sensing with supporting field data can provide continental to global scale mapping and monitoring at detailed (< 10m) spatial scales. e.g. we now have access to regularly collected (daily to 5-7 days) high spatial resolution satellite image archives , allowing cloud-free, specific tidal stage data to be collected and processed globally to map and monitoring specific environments/vegetation communities - see the examples at which cover intertidal, coral reefs, sand others https://www.globalecologylab.org/datasets-and-software
86-87	Delete these lines - S2 bands are suitable for terrestrial and aquatic application and an advance on Landsat bands. There are no operational satellite hyperspectral imaging systems, nor will there be at the scale of Sentinel 2, Landsat or the Planet archive
106-107	Extend sentence to include - and enable development and implementation of management to conserve these environments.

108-109	This could be expanded to capture more of the detail and significance of this work , e.g. *Our hypothesis is that there are significant seasonal variations in inter-tidal seagrass extents and cover and that the timing and magnitude of these differ across continental scales. Understanding these variations is essential for development and implementation of seagrass conservation and management legislation.*
115-116	Delete as this is out of scope
118-119	To be understood and assessed by readers there needs to be statement about the validation data used - e.g. source, number of sites and how it was matched spatially and temporally with the Sentinel2 mapping results.
Figure 3	Change the symbols and link to caption to clearly show which is minimum and maximum points, as at present it is very hard to tell the difference and interpret graphs a) and b) . OR for 3a) present each site as bar, with + or - std error on each end. This shows the latitudinal difference more clearly.
155-189	This section is very rough and appears as a first draft in terms of its lack of structure, overall narrative and key points. Suggest the authors read more widely on the range of global, high spatial resolution mapping and monitoring programs in place using Sentinel 2 and Planet data sets, as well as considering NDVI limitations, then re-write this in a form that has a clear main message and easy to follow structure.
156-158	It is not clear if this is actually over the entire continental scale, or it is just from 12 sites over a continental extent?
160-163	This does not reflect the current state of EO capabilities for monitoring coastal and inter-tidal areas at high spatial resolution(< 5m pixels)m, from multiple daily collections, with suitable spectral bands (e.g. S2, Planet Super Dove) and high radiometric resolution that are consistent calibrated over time - AND all available from global archives , located next to geospatial processing and compute capabilities (GEE, AWS, Planetary Computer, SentinelHub etc) .
164	Note that NDVI is not a suitable index to use where you are attempting to estimate vegetation cover on background substrates that are dark in colour and/or wet it has a highly non-linear response to cover at low (< 30% cover) and high (> 80% cover) which have been shown consistently across range of vegetation communities globally. As other indices, e.g. SAVI , EVI d not remove these biases there has been a shift to direct estimates of cover or fractions (e.g. see the approaches used by kelp mapping and monitoring communities) using satellite data set as indices are not suitable.
190-208	Suggest delete this section as it does not add any new knowledge and does not take in the work already done on building close to global scale inter-tidal, seagrass and coral reef monitoring systems.
209-228	Similar comment to last paragraph, the purpose of this paragraph, in terms of how it builds on the results presented and their potential application in seagrass restoration is not clear -if results obtained were directly linked to restoration project planning and monitoring requirements this would help.
230-233	Change sentences to keep in scope of sites used" *This research has provided the first full phenology assessment of intertidal seagrass* using 12 sites across a latitudinal gradient at *continental scale using satellite remote sensing. Contrary to established assumptions, we find that there are substantial differences in timings, magnitudes and rates of change of seagrass extents at 12 sites across 23◦ of latitude along the North-East Atlantic coastline.*

236-241	Requires more refinement based on more thorough assessment and synthesis of published seagrass and intertidal mapping and monitoring.
Methods section	This needs an introductory section with detailed flowchart explaining in text and graphically the sequence of operations used to collect the S2 data, drone data, and photo-quadrats and to then process each data set to produce the output seagrass extent and cover maps. The individual sections contain a lot of detail, but it is not clear how they fit together. This introduction/overview should also include details of how the satellite/drone/validation data are matched in terms of location and time, and pixel/sample size to generate validations statistics. The methods should have sufficient detail and logical structure that they could be replicated by another group with similar skills - this essential information is lacking.

Review for COMM EARTH & ENVT

Revision of Davies et al., A sentinel watching over inter-tidal seagrass phenology across Western Europe and North Africa (previously 'A sentinel watching over intertidal seagrass phenology')

Based on the revision and rebuttal letter provided by the authors as well as the three reviews I have now carefully weighed this revision. I still think that this is a wonderful data set. I also think that the authors have usefully added an analysis of potentially important drivers (light and temperature). However, I still have two main concerns which make me advise a further round of revision, and I have a few additional points.

1. Style of the written text. Both myself (reviewer 3) and reviewer 1 were quite critical about how the paper is structured and written. I cite from rev 1 '.. needs significant extra work to its structure, content, narrative and key findings..'. I am afraid this issue has not been resolved sufficiently yet. The authors do not respond to this in their rebuttal and appeared to have only 'repaired a bit here and there', which is their good right, but it has not convinced me of the maturity of this manuscript. In my first review I argued for a shorter and more factual introduction and asked for hypotheses – since the authors have been publishing more on this data set (e.g. Davies et al, 2023 RSE 290, 113554, same first and several other authors), their work should have allowed the formulation of such hypotheses. This may feel artificial when the work has been started out as mainly exploratory, but it simply helps in a clear structure and it allows the removal of generic bits of text improving the readability.

   I am glad that the authors have now presented a hypothesis in the into, with the following formulation 'Our hypothesis is that there are significant seasonal variations in inter-tidal seagrass extents and cover, and that the timing and magnitude of these differ across continental scales'. I would propose that the authors can be far more specific – hence make the hypothesis testable or even rejectable. Now, when read alone, this hypothesis is largely confirmed by textbooks in seagrass ecology (Hemminga & Duarte; Larkum et al) and would not need any assessment of seasonality in spatial extent with hyperspectral sensor data.
2. The distinction between Ulva-like greens and angiosperm seagrasses. Together with reviewer 2, I had difficulty in understanding how the spectra of these two groups with similar chlorofyll a and b were separated. The authors respond in their rebuttal that they 'detail more clearly..' It may be my limited understanding of the matter, but I could not find how they separated exactly these two classes in the current paper. Fortunately they refer to the Davies et al paper (2023, RSE), which has this question as one if its subjects across a range of sensor platforms. Davies et al (figure 5) show that only a handheld ASD spectrophotometer and the PRIMS satellite with a hyperspectral sensor on board actually do a really good job, the other sensors tested are substantially worse to really poor. Unfortunately the Sentinel 2 (with 10 m resolution) also did not differentiate very well and the paper then also carefully and rightfully concludes that 'our work demonstrates the potential of discriminating intertidal seagrasses from Ulvophyceae'. However, the current paper uses Sentinel 2 with 10 m resolution. I think this issue should be clarified in a simple and straightforward text.

Additional points arriving form the revision

3. (a) The inclusion of light and temperature as potential drivers is a clear improvement. But this could also have provided the basis for hypotheses to structure the paper. For example, it

is pretty counter-intuitive that light has a negative effect on seagrass bed extent (Fig 3.). Is this due to the analytical approach chosen where light and temperature are GAM-regressed as independents explaining the extent (E, L 144 suppl material explaining methods), but without their interaction. This joint analysis could cause a stronger apparent effect of temperature, possibly. Also, the authors do not present any statistics of goodness of fit other than the confidence intervals, I would be interested in an overall RMSE or r2 and p level of the GAM model for this whole dataset in the caption of figure 3. Then, could it be that the light dose range for Z. noltei between a minimum (for survival) and a maximum (saturating growth rates) is surpassed by most sites studied here? Intuitively I would find it unlikely. (b) And there is one caveat: the light units the authors have downloaded are not PAR, and of course they are not the net available PAR over the tidal cycle (in mol /m2/d). Now the time window the authors use is about two weeks and this is close to half a tidal cycle, so periods of darkness under turbid water during daytime high tide will likely be averaged out and the light in units used here will be useful anyway. But if they had Secchi disk measurements – or would have done a separation of the spectral profiles during high and low tide, their new figure 3 may have benefitted. A further, easily quantifiable driver, tidal amplitude, could have helped in approximating light availability, but may also be important in itself as it governs tidal flow rates and hence erosivity. (c) the authors have downloaded temperature data, but are these water temperatures or air temperatures? Intertidal water temperature is far more variable than subtidal water temperature and possibly than air temperature, but the authors should at least specify which temperature they include.

4. Minor issues.

   (a) Title: I would rather use 'North East Atlantic', than 'Western Europe and North Africa', just like you do in most of the text.
   (b) References in text and supplementary material: quite some capitals and italics are missing.
   (c) The notion in the summary paragraph and main text that intertidal seagrass beds also provide services to terrestrial ecosystems (L30 revision) is odd to me. First, ecosystems provide services to humans (see e.g. IPBES, or a vast proportion of the ecosystem services literature), and second, what services to land do you think of? Marba et al have provided evidence that Caribbean seagrass beds reduced damage and erosion after a hurricane passed by, but that is again, a service to mankind. If we have to think of Z. noltei as a major food source to for example brent geese, then which part of the food web is then terrestrial? I would simply remove this part of the introduction, as I already argued for in the first round. Please be precise when using ecosystem services as a concept, or as precise as possible, and just sticking to the functions of these plants in the coastal ecosystem(s) should suffice.
   (d) A.o. on L89 the authors argue that the community has been biased to the period of peak growth (literally: 'a focus of the community on the morphometrics or density of seagrass during these so-called periods of 'peak' growth or 'peak' extent'). I do not see why the limitations of other work has to be used so extensively to justify your work. Also, your justification derives from for example the papers by Soissons et al – and their work did not have the aim to unravel the spatial variation across seasons, but rather the seasonal variation in mechanical strength and carbon storage. I would be so much more pleased

without the rhetorics that ‘others have not done something so obvious’ so that now you can do it. I challenge you to rephrase your work in a positive way.

(e) Somewhat similar to my previous point I respond to the argument in your intro that this type of remote sensing of extent is needed for restoration. I really wonder whether it truly is. Your argumentation runs as follows ‘To be successful at scale, these efforts, especially restoration, require accurate and detailed information on spatiotemporal variability in the population structure of dominant habitat-forming species. Regular mapping is still lacking for most inter-tidal seagrasses.’. I largely agree but would counterargue that such remote sensing results only produce the coarse outlines of the extent of these seagrass beds, whilst ignoring the low density ones, whereas understanding the interplay of light requirements, clonal branching and (low) seedling establishment success versus losses due to the abrasive intertidal flow environment as well as herbivory would be far more helpful to understand what you call population structure.

(f) Figure 5 in my view is still redundant. Figure 4: really illustrative of the difference in seasonality and the complexity of the local coastal geomorphology, but I wonder whether your areal estimate with three decimals is justified by the method. I would have chosen 2.1 km2 rather than 2.087. Figure 4: for some of the colourblinds among us (including me) the colours are very difficult to distinguish. I understand you chose a warm-cold gradient from S to N and that helps a lot since it aligns with the vertical axis. Can you, in addition, give the ranges in temperature and light that exist across the range studied? And explain what -1 0 +1 as population level estimate actually means in km2? It will make the units more understandable for me.

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Review for COMM EARTH & ENVT

Second revision of Davies et al., A sentinel watching over inter-tidal seagrass phenology across Western Europe and North Africa (previously 'A sentinel watching over intertidal seagrass phenology')

Based on the second revision and elaborate rebuttal letter provided by the authors I have now come to the conclusion that this paper can be accepted for publication. I have a few remaining issues that can be dealt with by the editor(s).

I have the following remaining minor issues – and I use the comment numbering of the rebuttal that follows my numbering.

Comments 1 and 2: revision and response accepted.

Comment 3: (a) MINOR but RELEVANT: I do not think I argued for the use of W/m2 or KW, but rather for mol or E/m2/d, as it is the photons that enter into the photosystem rather than the energy they contain. One could argue that this is hairsplitting, but a plant physiologist would gretly prefer the flux unit of mol quanta /m2 /unit time. (b) MINOR BUT also RELEVANT. I understand the justification that the factor location will cover some of the effect of tidal amplitude, but it is less quantitative than the amplitude itself, and it may explain why some intertidal beds that share the same tidal amplitude share the same seasonal variation in extent. I have not done the analysis but I would guess the explanatory strength of a model incorporating tidal amplitude rather than just location would be stronger.

Comment 4: on the title. I leave this to the editor. A matter of taste what is more elegant. The currently proposed title certainly is more lengthy.

Comment 5: If the authors have followed the instructions of the journal I wonder how the journal looks at the spelling of geographical names or journals. New Zealand would not be spelled with lower case, would it? Refs 37-40 are examples as are ref 35 vs 36 for the odd use of 'et al'. Just do a careful manual/visual check of all references please.

Comment 6-8: OK!

Comment 9: I maintain my point. Fig 5 is redundant and just occupies precious journal space. But this of course is to the discretion of the editor.

Small issues in the text

Title: drop the stop at the end please

L 58 but several places in both 'summary' and 'main': the use of the ';' is very odd to my feeling of how English is properly written. But I am not a native speaker and leave this to the editors of the journal.

L63 how these may change = how this may change

L80 confusions = confusion

L87 rewrite like: '.. monitoring of the temporal variation in extent and distribution' simpler and easier to read.

L101: please remove 'for the first time' You do not need such claims and what if someone falsifies it? Just rely on the content of your work rather than on such rhetorics.

L105: S2 I understand, but I do not think you explained that S2 = Sentinel 2. Jus introduce this abbreviation somewhere in the text.

L109: 'a myriad of' = several. By now you get my point on style I guess.

L127: drop 'highly'

Caption fig 3: lower case on Seagrass in first line. And seriously reconsider the unit KW/m2.

Fig 4. The 'a' and 'b' have a too small font size. The percent cover scale for seagrasses need sto mentioned only once. The same is true for the words latitude and longitude along the axes.

L209: 'other assessments': such as? Add a reference. And Ref 45 is NOT on Rio de Aveiro but on New Zealand.

L226: 'both showed differing effects' = 'showed contrasting effects'.

L227: drop 'vitally'

L243 and 244: timings , magnitudes = use singular, timing, magnitude

L249: drop 'strong'

Finally, depending on your own taste and style, you may consider to thank the reviewers – zero obligation, so only if you sincerely think we were helpful ((((:

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