

Permanent loss of barrier island resilience due to a critical transition in dune ecosystems

Corresponding Author: Dr Orencio Duran Vinent

This manuscript has been previously reviewed at another journal. This document only contains reviewer comments, rebuttal and decision letters for versions considered at Communications Earth & Environment.

Version 0:

Decision Letter:

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Dear Dr Duran Vinent,

Your manuscript titled "Critical transition in barrier islands in response to sea level rise" has now been seen by 2 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.

In particular, please address the following editorial thresholds:

* Provide clear and full justification for all your conceptual assumptions in your model and analysis (e.g. barrier island erosion, barrier island migration rates, dune recovery rates, dune morphology), based on peer-reviewed references.

* Present strong theoretical support for the simplifications in your modelling and place these in the context of other contradictory approaches.

* Discuss the robustness and limitations of your methods, and expand the discussion of your results with other published studies.

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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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,

Carolina Ortiz Guerrero
Associate Editor
Communications Earth & Environment

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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:

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REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

Dear Authors,

Congratulations on the manuscript! It is an exciting piece of research with high societal relevance. The relevance is clear for society, the content seems solid, and the results are interesting. The manuscript has much potential.

Nonetheless, I think some discussion must be added to place this work in the context of barrier island-related research, providing a better understanding to readers. Furthermore, it is also important to highlight the limitations of the study and their implications on the results presented. Please see my comments given below.

1) L17-18: There will always be a net erosion in both directions when barrier islands evolve. Hence, this sentence must be modified to avoid any ambiguous implications for a general audience.

2) L23-25: It is not always the barrier island elevation that controls its migration rate. The migration rate is related to the volume of sand that needs to be mobilised in the cross-shore direction. This barrier migration rate can be linked to its width since the energy needed for cross-shore sediment transport (i.e., sediment eroded from the barrier shoreface to the back-barrier area) may vary from one system to another. (e.g., Morton and Miller (2005); Twichell et al. (2013); Bamunawala et al. (2021)).

3) L129-137: This is a well-established relationship (i.e., barrier island height is the key factor in determining whether it would be drawn under SLR). Authors should compare their findings with similar findings available in the literature.

4) L142-143: It is meaningless to mention any SLR rate without future time horizons and projection scenarios considered. Indicate such relevant information here for better understanding.

5) L151-165: This is also related to the barrier migration controlled by its size and sediment availability for cross-shore migration. Authors should relate these findings to similar literature, making the manuscript more comparable.

6) L174-175: How significant is this shift, given that barrier island coasts are less than 10% (globally), with some surviving under rising sea levels? If the authors want to indicate a shift in the overall Carbon budget within barrier island systems due to potential barrier island rollovers, then it must be supported by relevant values/figures from this study. What is mentioned in L169-174 is only general information related to barrier island evolution under SLR.

7) L176-180: Authors should demonstrate how to couple anthropogenic impacts to the model presented before concluding such can be done with their method presented.

8) Robustness of model projections: It is also helpful to the readers to have an idea of the implications of the assumptions and simplifications (made during computations) on model predictions. Suggest authors to indicate such description when presenting the results to understand its context better.

A few references are given below (only as suggestions).

- Moore, L. J., and Murray, A. B. (2022). Islands on the move. *Nat Geosci* 15, 602–603. doi: 10.1038/s41561-022-01000-6.
- Morton, R. A., and Miller, T. L. (2005). National Assessment Of Shoreline Change: Part 2, Historical Shoreline Changes And Associated Coastal Land Loss Along The U.S. Southeast Atlantic Coast. doi: 10.3133/ofr20051401.
- Murray, A. B., and Moore, L. J. (2018). “Geometric Constraints on Long-Term Barrier Migration: From Simple to Surprising BT,” in *Barrier Dynamics and Response to Changing Climate*, eds. L. J. Moore and A. B. Murray (Cham: Springer International Publishing), 211–241. doi: 10.1007/978-3-319-68086-6_7.
- Bamunawala, J., van der Spek, A., Dastgheib, A., Murray, A. B., and Ranasinghe, R. (2021). An Integrated, Probabilistic Modeling Approach to Assess the Evolution of Barrier-Island Systems Over the Twenty-First Century. *Front Mar Sci* 8. doi: 10.3389/fmars.2021.755699.
- Mariotti, G., and Hein, C. J. (2022). Lag in response of coastal barrier-island retreat to sea-level rise. *Nat Geosci* 15, 633–638. doi: 10.1038/s41561-022-00980-9.
- Twichell, D. C., Flocks, J. G., Pendleton, E. A., and Baldwin, W. E. (2013). Geologic Controls on Regional and Local Erosion Rates of Three Northern Gulf of Mexico Barrier-Island Systems. *J Coast Res*, 32–45. doi: 10.2112/SI63-004.1.
- Hovenga, P. A., Ruggiero, P., Itzkin, M., Jay, K. R., Moore, L., and Hacker, S. D. (2023). Quantifying the relative influence of coastal foredune growth factors on the U.S. Mid-Atlantic Coast using field observations and the process-based numerical model Windsurf. *Coastal Engineering* 181, 104272. doi: <https://doi.org/10.1016/j.coastaleng.2022.104272>.
- Jay, K. R., Hacker, S. D., Hovenga, P. A., Moore, L. J., and Ruggiero, P. (2022). Sand supply and dune grass species density affect foredune shape along the US Central Atlantic Coast. *Ecosphere* 13, e4256. doi: <https://doi.org/10.1002/ecs2.4256>.

Reviewer #3 (Remarks to the Author):

Line 25: add refs from Mississippi delta which clearly shows the final developmental stage is a transgressive barrier island arc before total subsidence and becomes a shoal – plenty of refs on this. E.g.

Penland, S., Boyd, R., & Suter, J. R. (1988). Transgressive depositional systems of the Mississippi Delta plain; a model for barrier shoreline and shelf sand development. *Journal of Sedimentary Research*, 58(6), 932-949.

Line 27: “after-storm” should read “post-storm”

Line 37: “findings of a simplified process-based dune model 24.” The problem here is that ref 24 has been significantly criticised by Davidson-Arnott et al so this should be noted here and the D-A ref cited for completeness.

Lines 40-41: RE: “These control parameters determine whether barriers are in a ‘high-barrier’ state with well-developed dunes, a ‘low-barrier’ state devoid of dunes and a ‘mixed’ state in between “ These dunes are all low to very low compared to many coastal dunes in the world, even just foredunes which range up to 30m high in some instances. So you need to add a statement about this, and note that these are VERY relative terms. A reference on foredunes and their range of heights, morphology etc relative to your tiny ones would be valuable too. I think its Hesp 2002 that is THE review ref on this.

Line 46: RE: “and we 46 expect similar sand availability,....” This is clearly not the case. Many many studies show dune growth and size is related to sediment supply and surfzone-beach type so this is utter nonsense. In addition, you provide no refs to support such a notion. Either provide a rationale and refs to prove this or state that you have no idea about sand supply variations along the barriers, and that this factor could naturally lead to barrier volume differences.

Line 52: ‘on’ NOT ‘in’ a barrier island.

Line 55: RE: “Thus barrier elevation is assumed to be bounded by the maximum dune height H ...”. Many barrier islands have scattered dunes, often nebkha and remnant knobs, not continuous lines of relict foredunes. IF you have continuous alongshore dune ridges then this assumption is fine, BUT you mostly don’t, so assuming a single max dune height defines a barrier as high or low is pretty bad. Further you don’t state anything about the dune morphology, nor properly describe it for each barrier.

Lines 78-79: RE: “the alongshore variations in H and h_0 are random and can be approximated as a Gaussian noise with normal distribution N ” WHY? On what basis do you say this? What refs are there to back this up, and I mean refs using real world data not some further modelling data? You also need to explain “gaussian noise” so all readers can understand it.

Lines 93-94: RE: “in the absence of dune erosion, dunes grow up to the maximum elevation H and there is a single stable equilibrium in the system: $h = H$.” I do not find this reasonable at all. Why would all dunes grow to a maximum elevation especially when just above you state that its random?

Line 93. RE: “supported by the dune building dynamics 24,”. REF 24 has been proven to be fallacious, yet you still use it. WHY? On what basis?

Lines 96-97: RE: “On the other hand, in the absence of dunes, either because of lack of aeolian transport or back-beach vegetation” ADD (in caps) “On the other hand, in the absence of dunes, either because of lack of SEDIMENT SUPPLY, aeolian transport or back-beach vegetation...”

Lines 117-118: RE: “By definition, high-elevation barriers are characterized by fast dune recovery ($T + r \leq 1$), whereas in low-elevation barriers dune recovery is very slow” WHY? Where is your data to back this statement up? What refs apply? Dunes recover or rebuild at the same rates across all barriers within a regional climate regime. Dunes cant grow faster or slower due to elevation; their growth can be restricted due to the effects of salt water flooding or swash or washover on the

plants.

The conclusion adds nothing new to our knowledge of barrier islands.

I do not find this simplistic modelling compelling, and as noted above re line 46, there are drivers for which you have no data, and some of your assumptions are clearly fallacious (line 55; line 93) so its very difficult to believe your results.

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Author Rebuttal letter:

Response to Reviewer 2

Congratulations on the manuscript! It is an exciting piece of research with high societal relevance. The relevance is clear for society, the content seems solid, and the results are interesting. The manuscript has much potential.

Nonetheless, I think some discussion must be added to place this work in the context of barrier island-related research, providing a better understanding to readers. Furthermore, it is also important to highlight the limitations of the study and their implications on the results presented. Please see my comments given below.

We thank this reviewer for her positive assessment of our work and the useful comments and suggestions.

1) L17-18: There will always be a net erosion in both directions when barrier islands evolve. Hence, this sentence must be modified to avoid any ambiguous implications for a general audience.

We agree. We modified the sentence to emphasize that we are referring to vertical erosion due to wave runup.

2) L23-25: It is not always the barrier island elevation that controls its migration rate. The migration rate is related to the volume of sand that needs to be mobilised in the cross-shore direction. This barrier migration rate can be linked to its width since the energy needed for cross-shore sediment transport (i.e., sediment eroded from the barrier shoreface to the back-barrier area) may vary from one system to another. (e.g., Morton and Miller (2005); Twichell et al. (2013); Bamunawala et al. (2021)).

We agree. We included barrier width as another control on migration. We also included the suggested references to highlight the potential impact of our work.

3) L129-137: This is a well-established relationship (i.e., barrier island height is the key factor in determining whether it would be drawn under SLR). Authors should compare their findings with similar findings available in the literature.

We agree that the barrier's height is crucial in determining whether it could drown under SLR. However, we are claiming something different: that what we define as barrier's base elevation (h0) controls the formation and post-storm recovery of coastal dunes and thus the transition to

a barren barrier. This transition doesn't necessarily lead to barrier drowning because a barrier island lacking dunes can still have a large marsh platform that prevent its drowning (e.g. its transition to a submerged shoal), as it's the case with some of the barren islands in the VBI. We are not aware of any previous quantitative description of this type of transition except for Duran & Moore (2013). We have added some of this discussion in a new section at the end of the revised manuscript.

4) L142-143: It is meaningless to mention any SLR rate without future time horizons and projection scenarios considered. Indicate such relevant information here for better understanding.

1
Done.

5) L151-165: This is also related to the barrier migration controlled by its size and sediment availability for cross-shore migration. Authors should relate these findings to similar literature, making the manuscript more comparable.

We agree. However, we don't currently have enough information about the precise nature of the relation between barrier migration and barrier elevation, and that analysis would be outside the scope of this work. Therefore, we only indicate a potential relation, and cite relevant literature. In fact, we have modified the title to emphasize our focus on barrier elevation and the presence/absence of a coastal dune ecosystem.

6) L174-175: How significant is this shift, given that barrier island coasts are less than 10% (globally), with some surviving under rising sea levels? If the authors want to indicate a shift in the overall Carbon budget within barrier island systems due to potential barrier island rollovers, then it must be supported by relevant values/figures from this study. What is mentioned in L169-174 is only general information related to barrier island evolution under SLR.

A detailed analysis of barrier migration and its effect on the carbon budget of the barrier system goes beyond the scope of this work. We thus move this part to the introduction to highlight the potential impact of our work on the topic.

7) L176-180: Authors should demonstrate how to couple anthropogenic impacts to the model presented before concluding such can be done with their method presented.

We agree. However, this goes beyond the scopes of this work, so we modified the text to clarify the speculative nature of our comment.

8) Robustness of model projections: It is also helpful to the readers to have an idea of the implications of the assumptions and simplifications (made during computations) on model predictions. Suggest authors to indicate such description when presenting the results to understand its context better.

We thank this reviewer for the helpful suggestions. We have added more details about model assumptions and simplifications, and their potential effect on the results, in the description of the model and in a new section about the robustness of its predictions.

A few references are given below (only as suggestions).

We thank the reviewer for the additional references. We included them in the revised version.

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Response to Reviewer 3

Line 25: add refs from Mississippi delta which clearly shows the final developmental stage is a transgressive barrier island arc before total subsidence and becomes a shoal - plenty of refs on this. E.g. Penland, S., Boyd, R., & Suter, J. R. (1988). Transgressive depositional systems of the Mississippi Delta plain; a model for barrier shoreline and shelf sand development. *Journal of Sedimentary Research*, 58(6), 932-949.

Done.

Line 27: after-storm should read post-storm

Done.

Line 37: findings of a simplified process-based dune model 24. The problem here is that ref 24 has been significantly criticised by Davidson-Arnott et al so this should be noted here and the D-A ref cited for completeness.

Although we acknowledge the criticism from Davidson-Arnott's work (Davidson-Arnott et al., ESPL, 2018), it didn't invalidate ref. 24's findings. Ref. 24 presented a complex process-based simulation that resolved sand transport, wind aerodynamics and its interaction with the topography, vegetation growth and surface change. We note that none of these processes, except surface change, were actually solved in Davidson-Arnott et al. (2018) phenomenological model.

The physical simulation in ref. 24 showed that dunes grow up to a constant maximum height that depends on external conditions. Therefore, that maximum height would remain constant if those external conditions do not change over time, i.e. no change in wind intensity and direction, no wave-driven erosion, no shoreline change, no seasonality in vegetation growth, etc. As discussed in the revised manuscript, this general pattern of dune growth has been found in data at different locations (see refs. 3 & 38). Davidson-Arnott's paper presented data showing that dune height continues to increase over decades, and used this longer-term variability to falsify the model in ref. 24. However, this is misleading as predictions from Ref. 24 are strictly valid only for constant external conditions, which is clearly not the case in Davidson-Arnott's decade-long data. Interestingly, dune height at most of their sites seem to have recently converged to a maximum around 10m, which would be consistent with Ref. 24's prediction of an upper limit for dune height under given local conditions. Indeed, ref. 3 found that post-storm dune data in different barrier islands seem to converge to a constant value over few years; something similar was found for coastal dunes in Oregon (ref. 38). Furthermore, ref. 24 predicted that the maximum dune height can change with external factors, such as shoreline position (as shown in ref. 38), and explained the dependence between dune height and wave regime found in the data. This physical model has continued to evolve to include more complex external conditions, as recently shown in Hovenga et al. Coastal Eng. (2023).

However, what is important for this work is not that we assume that the maximum dune height is constant over time, but rather that we measure it as such from data. Indeed, as shown in Fig. 2, we measure the distribution of maximum dune heights using elevation data with negligible change rate over the 3 years period. Furthermore, as discussed in the text in a new section about the robustness of the model, the precise value of the average maximum dune

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height is not relevant for the transition to a barren state, which mainly depends on the dune growth rate.

Lines 40-41: RE: These control parameters determine whether barriers are in a high-barrier state with well-developed dunes, a low-barrier state devoid of dunes and a mixed state in between. These dunes are all low to very low compared to many coastal dunes in the world, even just foredunes which range up to 30m high in some instances. So you need to add a statement about this, and note that these are VERY relative terms. A reference on foredunes and their range of heights, morphology etc relative to your tiny ones would be valuable too. I think it's Hesp 2002 that is THE review ref on this.

As noted above, it seems the reviewer mistook our terminology for barrier state (low and high-elevation barrier) as referring to relative differences in dune height, instead of the actual definition based on the presence of dunes (high state) or absence of dunes (low state). We have changed the terminology to high and barren barrier in the new version to avoid this misinterpretation. Note that the actual dune size is not very relevant for the transition to a dune-less barren state. Furthermore, we find that even a relatively small 1m increase of elevation above the beach leads to a reduction of the flooding frequency by a factor close to 30. Thus, even the ~2m dunes in our field site, referred by the reviewer as tiny, can offer significant protection against storm flooding.

More importantly, in this work we are not making any claims about the size of coastal dunes in general, we are just relating measured local dune characteristics to the transition to a barren state in our study site, and therefore we restrict ourselves to local dune sizes. Of course, the model is general and could be applied to other coastal regions with different dune sizes. This, however, is clearly outside the scopes of this work.

Line 46: RE: "and we expect similar sand availability." This is clearly not the case. Many many studies show dune growth and size is related to sediment supply and surfzone-beach type so this is utter nonsense. In addition, you provide no refs to support such a notion. Either provide a rationale and refs to prove this or state that you have no idea about sand supply variations along the barriers, and that this factor could naturally lead to barrier volume differences.

That sentence was just part of the motivation or problem statement, it was not a research assumption. In our investigation we didn't make any assumption about sediment supply or sediment availability, contrary to the reviewer statement. In fact, we measured the maximum dune growth rate, which should correlate with sand availability, for each island using elevation change data (see Table 1). Interestingly, we found that maximum dune growth rates do not explain the differences in barrier elevation. For example, Cedar, a dune-less and barren barrier island, has a similar growth rate as Hog, an island with well-developed dunes.

To avoid confusion, we changed the sentence to "Given the external conditions are similar for all these barrier islands, and ASSUMING similar sand availability." In the revised version, we also discuss the implications of our results regarding sand availability.

Line 52: "on a NOT a barrier island."

4
Done.

Line 55: RE: "Thus barrier elevation is assumed to be bounded by the maximum dune height H ." Many barrier islands have scattered dunes, often nebkha and remnant knobs, not continuous lines of relict foredunes. IF you have continuous alongshore dune ridges then this assumption is fine, BUT you mostly don't, so assuming a single max dune height defines a barrier as high or low is pretty bad. Further you don't state anything about the dune morphology, nor properly describe it for each barrier.

In this sentence we refer to dune height at a point, i.e. at a given alongshore position (we clarified this in the revised version). Of course, the maximum dune height can change alongshore, this is the reason why we introduce a distribution function describing the alongshore variations of the maximum dune height. Also note that the parameters of the distribution function, assumed to be Gaussian, change from island to island. By definition, this distribution function also characterizes the alongshore dune morphology in the absence of erosion and for long enough times. For example, for a continuous uniform ridge, as in Hog island, the distribution function would be narrow around the average height, whereas for a more heterogeneous pattern the distribution would be wider, as in South Metompkin.

More importantly, the alongshore distribution of maximum dune heights is measured from data using elevations with negligible rate of change over 3 years. As shown in Fig.2(k,l) the distribution can be well approximated by a Gaussian.

Lines 78-79: RE: "the alongshore variations in H and h_0 are random and can be approximated as a Gaussian noise with normal distribution N " WHY? On what basis do you say this? What refs are there to back this up, and I mean refs using real world data not some further modelling data? You also need to explain "gaussian noise" so all readers can understand it.

The reviewer didn't include the first word in that statement, which is "Assuming". We are assuming a gaussian distribution because it is a very common one that describes many different random processes, and because it simplifies the numerical model and its comparison to data. Of course, we are free to make any sensible assumption in the model as long as we can validate it with data. This is exactly what we did and indeed the data from VBI seems to confirm our assumption (see Fig.2k,l).

We removed the term "gaussian noise" to avoid jargon.

Lines 93-94: RE: "in the absence of dune erosion, dunes grow up to the maximum elevation H and there is a single stable equilibrium in the system: $h = H$." I do not find this reasonable at all. Why would all dunes grow to a maximum elevation especially when just above you state that it's random?

Here we are referring to a "point model", which means that the elevation is described at a given alongshore position (we clarified this in the new version (line 66)). Of course, the

maximum dune height can change alongshore to reflect variable conditions. An important clarification now added to the manuscript is that in the absence of water-driven erosion and for a non-zero sand supply from the beach, the model assumes that dunes form everywhere along a barrier island, and although their maximum height can change alongshore randomly,

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depending on local fluctuations, it cannot change over time, at least in sub-decadal timescales. These are very general assumptions that capture most relevant cases, in particular our field site at the VBI. We added more details about the model assumptions in the revised text (lines 65-127).

Line 93. RE: "supported by the dune building dynamics 24," REF 24 has been proven to be fallacious, yet you still use it. WHY? On what basis?

Given the lack of detail we don't know what this reviewer is referring to as "fallacious". Ref. 24 uses a physics-based eco-morphodynamic model that reproduced the formation of parabolic dunes (Duran et al., PRL, 2006), the formation of coastal dune fields (de M. Luna et al. Geomorphology, 2011), the formation of parallel coastal ridges on prograding shorelines (Moore et al., Geology, 2016), and was recently coupled with more complex beach morphodynamics models (Cohn et al, 2019; Hovenga et al., 2023). The eco-morphodynamic model of Ref. 24 is in turn based on a well-established morphodynamic model describing dune and dune field formation on different planetary environments and conditions (e.g. Duran et al. PRE, 2005; Parteli and Herrmann, PRL, 2007; Parteli et al., PNAS, 2009; Duran et al. ESPL, 2010; Parteli et al., PRL, 2011; etc.).

If this reviewer is referring to Davidson-Arnott et al., ESPL (2018), the implication of that work relative to Ref.24 was already described above. Furthermore, Davidson-Arnott (2018) never discussed, tested or falsified the actual processes and assumptions described in Ref.24, but just misinterpreted the results and implications of Ref.24 when analyzing a few observation points at a single field site. We are not responsible for the faulty application of the findings of Ref.24 and believe it is not ethical, and factually incorrect, to use that single work (Davidson-Arnott et al.) to invalidate the physical insight and potential value of Ref.24 (as demonstrated by nearly 300 citations it has received, 30 in the last year alone).

Finally, as already stated before, and emphasized in the new version (lines 80-102), the assumptions from Ref.24 that we are using in this work are general and supported by field evidence (e.g. Refs. 4, 3 and 38). In short, we assume that, in the absence of water-driven erosion, dunes initially grow at a given growth rate up to a maximum dune height that can change with the alongshore location within the barrier. More importantly, here we have measured all the relevant model parameters and tested the model predictions. The fact that we are able to reproduce, as shown in Fig.1, the alongshore distribution of the actual barrier elevation (which to be clear is a very complex system) with only four measured parameters is proof of the value of our approach. Nevertheless, given that Ref. 24 was never intended to explain all possible complexity of real coastal dunes, but to offer a basic physical insight to separate the effects of internal dynamics and external conditions, we changed the text in line 92 to "supported by an idealized dune building dynamics".

Lines 96-97: RE: "On the other hand, in the absence of dunes, either because of lack of aeolian transport or back-beach vegetation" ADD (in caps) "On the other hand, in the absence of dunes, either because of lack of SEDIMENT SUPPLY, aeolian transport or back-beach vegetation"

Done.

6

Lines 117-118: RE: "By definition, high-elevation barriers are characterized by fast dune recovery ($T + r \approx 1$), whereas in low-elevation barriers dune recovery is very slow" WHY? Where is your data to back this statement up? What refs apply? Dunes recover or rebuild at the same rates across all barriers within a regional climate regime. Dunes can't grow faster or slower due to elevation; their growth can be restricted due to the effects of salt water flooding or swash or washover on the plants.

As further explained below (and expanded in the new version of the manuscript), we are not making any claims regarding dune growth time as function of elevation, but just modeling the effect of water-driven erosion (via wave runup) in post-storm dune recovery. In fact, this is the

whole point of our stochastic model: to resolve the competition between dune growth and water-driven erosion that determines whether a barrier island has a dune or not.

To be clear, the terms "high-elevation" and "low-elevation" refers to two main states of a barrier island, having-a-dune and lacking-a-dune respectively, as predicted by our model, and not to any arbitrary dune size or barrier elevation. Again, whether a barrier has a dune or not depends on the competition between dune growth and water-driven erosion. For a given set of parameters, the outcome of this competition is predicted by our model in two forms, (i) the alongshore elevation distribution function, and (ii) the average post-storm recovery time T_r , rescaled by the average "undisturbed" dune formation time (Eq. M3). When this rescaled post-storm recovery time (T_r) is less than 1, then by definition dunes do recover after erosion, in which case the island is in a state of "high-elevation", i.e. with mature dunes, and his alongshore elevation distribution function has a single mode at the average maximum dune height (see Fig. 1b for Hog island). On the contrary, when the rescaled recovery time is much larger than 1, then by definition dunes, in average, take very long to recover after erosion. In that case, the island is barren, dune-less and in a state of "low-elevation" and his alongshore elevation distribution function has a single mode at the average "base elevation" height (e.g. Fig. 1b for Cedar island).

We changed the terms for island state to avoid confusion and added more details in this section (lines 192-206).

The conclusion adds nothing new to our knowledge of barrier islands. I do not find this simplistic modelling compelling, and as noted above re line 46, there are drivers for which you have no data, and some of your assumptions are clearly fallacious (line 55; line 93) so its very difficult to believe your results.

We respectfully disagree with the above statements. Indeed, it seems to us, from most of the comments by this reviewer, that she/he missed the basic elements of our modeling and conceptual approach, our research method and results, and even our problem statement. The transition from a barrier island with a thriving dune ecosystem to an essentially barren barrier is one of the most relevant problems in coastal research since barren islands are prone to flooding and are in many cases a prelude to barrier drowning (or becoming a shoal). Barriers are very complex systems (as further highlighted in the revised manuscript) and there are currently no models that can accurately predict their dynamics in response to external factors (more in the revised manuscript). The notable agreement between our model predictions and barrier data (shown in Fig.1) is, in our opinion, the best proof of the high value of our simplified but rigorous modeling approach in quantitatively explaining and predicting the transition to a

7

barren state, and thus the disappearance of the rich and complex dune ecosystem, using only a few widely available remote sensing data.

As explained in detail above, this referee's comments relative to lines 46, 55 and 93 are factually incorrect. Furthermore, this reviewer does not portray properly our actual model hypotheses, nor our data analysis or our comparison between data and model predictions.

8

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 Duran Vinent,

Your revised manuscript titled "Critical transition in barrier islands' dune ecosystem and the sudden loss of barrier's resilience" has now been seen by the original 2 reviewers, whose comments are appended below. You will see that reviewer #1 finds your work of some potential interest. However, it seems to us that a deadlock has been reached between your arguments and reviewer #2's, and we will, therefore, seek independent advice from an adjudicating referee. But before we do so, please revise your manuscript in response to the remaining concerns and supply us with a point-by-point response to referee #2's report. We will send your responses, the report, and the revised manuscript to the adjudicator.

We would also appreciate suggestions for adjudicators that you have not collaborated with over the past 5 years or so, and who you would trust to be fair as well as competent to evaluate your disagreement with the reviewer. Please supply at least six names — we cannot promise to use any of them, but we will do so if it seems possible and appropriate.

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

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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,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

Thank you for sharing the revised version of the manuscript. I have no further comments or suggestions.

Reviewer #3 (Remarks to the Author):

line 186- add "curve" or "distribution" after Gaussian.

line 193:ADD (in caps) between dune growth, vertical water-driven erosion AND SEDIMENT SUPPLY.

lines 258-260: RE:"This *****

also*****

suggests ***

the*****

barren*****

state**

is*

258 irreversible

as water-driven processes cannot increase the barrier mean *****

259 base elevation above the beach berm once dunes

260 are eroded following a large storm."

This is highly unlikely since even a small return of sediment to the beach following a storm can or could lead to an increase in barrier elevation.

lines 263-264 RE: "in the absence of sand

264 accretion at a scale larger than the dune width" Why are you picking dune width as a scale to determine whether a dune grows or not? Makes zero sense.

Line RE: "Barrier *****

elevation*****

state**

is*****

also *****

closely*****

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to ***

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rate,***

as**

a *****

barrier **

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330 barely

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shrinking *****

and *****

sinking *****

under *****

SLR*****

until*****

potentially*****

transitioning*****

into*****

the*****

'barren'*****

331 state" This is nonsense. Look at the Pensacola region barriers for example, after hurricanes and you clearly see massive landward migration even where the barrier is "high" and covered by significant dunes.

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Author Rebuttal letter:

Response to Reviewer 3

line 186- add "curve" or "distribution" after Gaussian.

Done.

line 193: ADD (in caps) between dune growth, vertical water-driven erosion AND SEDIMENT SUPPLY.

Dune growth, as a process, already includes sand supply. We added this explicitly to the text.

lines 258-260: RE: "This also suggests the barren state is irreversible as water-driven processes cannot increase the barrier mean base elevation above the beach berm once dunes are eroded following a large storm." This is highly unlikely since even a small return of sediment to the beach following a storm can or could lead to an increase in barrier elevation.

This post-storm increase in elevation forms a so-called beach berm, which we find has an elevation of less than 0.5m above the reference beach elevation (Fig. 2a,b,c). Our statement refers to the difficulty of increasing barrier elevation above that level from only water-driven transport.

lines 263-264 RE: "in the absence of sand accretion at a scale larger than the dune width" Why are you picking dune width as a scale to determine whether a dune grows or not? Makes zero sense.

Because, as we found from our validated & calibrated model, the barrier's elevation at spatial scales larger than individual dunes (what we call the base elevation) will determine the flooding frequency after dune erosion, which in turn controls the probability of dune recovery.

Line RE: "Barrier elevation state is also closely related to its migration rate, as a barrier in a high state would barely migrate, essentially shrinking and sinking under SLR until potentially transitioning into the barren state" This is nonsense. Look at the Pensacola region barriers for example, after hurricanes and you clearly see massive landward migration even where the barrier is "high" and covered by significant dunes.

Here we are applying the standard definition of barrier migration as the landward motion of the back-barrier boundary (in contrast to just shoreline transgression or the landward motion of the dunes). Therefore, by definition, in the absence of dune overtopping and consequent overwash (as is the case for high barriers), there is no transfer of sand from the beach and dune area to the back-barrier, and thus there is no effective barrier migration. In that case, shoreline and dune transgression would lead to barrier shrinking.

1

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 Duran Vinent,

Your manuscript titled "Critical transition in barrier islands' dune ecosystem and the sudden loss of barrier's resilience" has now been seen by 1 adjudicator reviewer you suggested. 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.

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.

In particular, please ensure that the revised manuscript meets the following editorial threshold:

***Expand your discussion to clarify how your hypotheses' conceptual and physical assumptions may not apply to all natural dune systems.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred.

Important: The response to reviewers must not include any figures, tables or graphs. If you wish to respond to the reviewer reports with additional data in one of these formats, please add them to the main article or Supplementary Information, and refer to them in the rebuttal. Due to current technical limitations, any figures, tables, or graphs embedded in your rebuttal will not be included in the peer review file, if published.

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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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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,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor
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and also in our style and formatting guide (<https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf>) >Communications Earth & Environment formatting guide .

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REVIEWER COMMENTS:

Reviewer #4 (Remarks to the Author):

The manuscript analyzes the possibility of a tipping point, and consequently a critical transition, in barrier island morphodynamics. Specifically, the authors argue that two stable states exist, characterized by a 'high dune' and a 'dune-less' landscape. The analysis is elegant and convincing within the hypotheses made. I do not have any issues with making stringent hypotheses, especially if the analysis leads to greater understanding and the hypotheses do not conflict with observational reality. The authors do an excellent job of proving their hypotheses, and I appreciate the comparison with field observations.

From my perspective, the research presented in the manuscript is relevant, novel, and interesting. However, I urge the authors to better address the relevance of their hypotheses. The authors should more clearly acknowledge that some natural systems might possess characteristics that violate some of the hypotheses, meaning the overall behavior of such systems might not be well represented by the proposed model. While this may seem like an obvious statement, it would help qualify the results, particularly for readers who work on such systems. This suggestion does not detract from the work proposed.

Examples of the assumptions I am referring to include the model's consistent erosion of the dune to the base elevation when high water events (HWE) exceed the dune crest, or the consideration of a constant growth rate of the dune. Additionally, although a standard definition of 'migration' is used, it is true that this definition may preclude an accurate representation of some systems, as pointed out by a previous reviewer.

I believe these points can be easily addressed by the authors. The changes I suggest do not relate to the core analysis and findings. I support the publication of this manuscript.

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Author Rebuttal letter:

Response to Reviewer 4

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Examples of the assumptions I am referring to include the model's consistent erosion of the dune to the base elevation when high water events (HWE) exceed the dune crest, or the consideration of a constant growth rate of the dune. Additionally, although a standard definition of 'migration' is used, it is true that this definition may preclude an accurate representation of some systems, as pointed out by a previous reviewer.

I believe these points can be easily addressed by the authors. The changes I suggest do not relate to the core analysis and findings. I support the publication of this manuscript.

We thank the reviewer for the positive evaluation and helpful comment. We included the following text near the end of the discussion section (lines 312-317):

âNote that the stochastic model is not applicable to natural systems that violate the model's assumptions, in particular where dunes are not eroded down to the base elevation when high water events (HWE) exceed the dune crest, or where the dune growth rate strongly changes spatially. These could include wide barriers with relatively complex and large dune systems, and barriers exhibiting spatially complex beach armoring. However, the model should capture the stochastic dynamics of most narrow single-dune barriers, which are also the most vulnerable ones.â

1

Version 3:

Decision Letter:

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Dear Dr Duran Vinent,

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