<u>Reviewer #1 Re-Review (Anonymous)</u>

[[I am afraid this revised version of the manuscript with the reference "egusphere-2023-1307" and entitled "Implications for the Resilience of Modern Coastal Systems Derived from Mesoscale Barrier Dynamics at Fire Island, New York" does not show significant improvements with relation to the original version. I can understand to some extent, as the authors received relatively positive feedback during the first round. Yet, some improvements could have been done and were asked to be done to the results section, in relation to the GPR data interpretation, and to the discussion section.]]

+ We believe the original comments were constructive and helped improve the manuscript significantly, particularly in terms of framing the Discussion. We are open to (and have made) further modifications based on particulars brought up in this review.

[[I continue to find that the work is fine; however, it is not easy to follow as many names to specific locations are continuously referred in the manuscript and the number of figures is high but with different scales of representation and focus areas, so that the reader gets easily lost. This turns makes it difficult to enjoy the reading of the manuscript. As suggested by other reviewers, the authors could have tried to reduce the number of figures. In relation to the figures with the islands, I would recommend trying to condense more the information and if possible use the same limits or scales. Figure 4, for example presents a very different scale than previous figures, why don't you show the same extent as in Figure 2? It does help that the sections in figure 3 are noted in figure 2.]]

+ Unfortunately, we cannot use the same limits and scales when relating the full structure of the island (51 km long) versus our individual study areas. The other problem is that our figures are already dense with information, and we believe further condensing would create even more confusion.

This review also pointed out that aspects of our investigation, such as our GPR profiles, are unnecessary, which would save figure space. However, we strongly disagree with this sentiment (see next comment).

[[Regarding the GPR, it is still not clear to me if it is actually needed. The authors made the effort to justify its contribution, however, I still have some doubts as with the interpretations from older maps and morphology, the authors reconstruct the history of the islands with a good level detail. Also, the information that the GPR brings overlaps with the information on evolution you can obtain from the DTM models that you have.]]

+ We take this as a compliment that we were able to relate our desktop study of the landscape so well that it seemed we did not even need even to perform a GPR investigation to confirm our suspicions. However, our use of GPR follows standard protocols for investigating the evolution of a barrier island. As Dougherty et al. (2019) states, "once the surface morphology is analysed, the next step to determine how a barrier formed is to study the history preserved in the shallow subsurface. The lidar data should be used to make informed decisions on where best to acquire detailed stratigraphy using geophysics." Without GPR, it would be impossible to understand the full lateral extents and thicknesses of various deposits, which is information we used to help guide the placement of our sediment cores. Ultimately, the combination of cores and GPR allowed us to chronologically constrain the barrier's geomorphology and provide insight into the types of environments the island previously supported.

Reference:
Dougherty, A.J., Choi, J.H., Turney, C.S., & Dosseto, A: Optimizing the utility of combined GPR, OSL, and Lidar (GOaL) to extract paleoenvironmental records and decipher shoreline evolution, Climate of the Past, 15(1), 389-404, 2019. https://cp.copernicus.org/articles/15/389/2019/

[[Besides, and as suggested in my previous comments, I still believe the data processing and interpretation could improve. It is not clear what the authors try to highlight in the interpreted sections, as they do not define clear patterns and radar facies. The use of black lines in the interpreted sections are intended to note the presence of prominent reflections related to washover and beach sediments. This should be better explained as sometimes the lines seem the result of a rather random interpretation of the radargrams. Are the lines intended to separate major units? Like major boundary surfaces related to erosive events? Or do they show the internal configuration of facies?]]

+ Highlighted reflections show the internal configuration of the facies in the subsurface. To make this distinction clear, and to highlight the extents of individual facies types, we now add consistent color highlight on the inferred/discernable limits of facies. Legends are provided in figures to aid interpretation.

[[I still do not understand very well the relationship between the water table (figure 7) and the mean sea level. It seems that the MSL is close to 0 m (NAVD88), however the WT is 2.5 m below this level, is that normal? It does not seem logic at all. Did you interpret the WT based on ground truth observation? I would revisit this as it seem a bit odd. Also, considering the fact that the barrier is so narrow in some areas, it is even easier to imagine a high water table, also supported by the penetration of salt or marine waters across the barrier, this also shows the intrusion above the level of 0 m. I am asking about this because the elevation of the WT determines the model for depth conversion from nano seconds to meters. Usually, the presence of water reduces the velocity to half of it and that would imply that the depth below that WT level should be expanded and for instance, reflections interpreted as being close to 5 m depth would be around 2.5 m. Therefore, I insist on making sure this is not an issue. If the WT is where it is usually (around MSL) the depth correction should be different above and below this level.]]

+ "Water Table" should be "Saltwater Table". There is a freshwater lens sitting on top of that boundary, which created some challenges for the sand auger. We are reasonably confident in our velocities inferred from hyperbola analysis since the linearly decompacted cores show good correlation with the facies placement seen in our GPR profiles. Additionally, our GPR lines over Whalers Inlet located a reflection at around -5 m which is consistent with the depth of the antecedent Pleistocene surface as reported by Schubert (2010) and mentioned in the manuscript.

Please note that the method we employ to for determining an average radar wave velocity from hyperbola analysis has been used by other authors:

Hede, M.U., Bendixen, M., Clemmensen, L.B., Kroon, A., and Nielsen, L.: Joint interpretation of beach-ridge architecture and coastal topography show the validity of sea-level markers observed in ground-penetrating radar data, The Holocene, 23(9), 1238-1246, 2013.

Layek, M.K., Debnath, P., Sengupta, P., & Mukherjee, A.: Delineation of sedimentary facies and groundwater-sea water disposition in an intertidal zone of the Bay of Bengal using GPR and VES, Journal of Environmental and Engineering Geophysics, 23(2), 235-249, 2018.

[[Another issue that I would like the authors to explain better is the problem with the age of the peat or organic horizons and the disparity with the plant materials. Also, it would be interesting to elaborate on the increase in age seaward from C1 to C3, also, how do you explain the occurrence of overwash processes in C1 and C3? If I understand this well, if C3 is overwashed, that would mean that C1 could hardly be as they are about 300 m apart. But even more difficult, is to understand the age or moment when that would happen as the organic level in C1 is half the age than in C3. Also, considering the GPR record, this is not easy to imagine. In the GPR line in figure 11, where these cores are located, the authors include dashed black lines to note dune surface buried by beach facies. Could the authors explain a bit better what do they mean with that and the associated implications? I imagine at least a large gap in the sedimentary record if the dunes are developed on top of beach sediments as suggested from the GPR lines to explain the jump in the sea level of about 2 m. Also, make sure the reader can understand those cases where an overwash buries a dune that formed above a beach without eroding it.]]

+ The disparity with the age of the peat versus plant remains arises because of root intrusion into existing peat deposits. All the radiocarbon samples analyzed for this study showed evidence of well-preserved roots/rootlets, so plant remains are expected to be generally younger than the bulk sediment age. The only sample where this age disparity was not present was in Core C9, where the organic sediment and plant ages overlapped in time. That particular sample comes from a thin organic horizon that probably represents an exceptional case (for Fire Island), where marsh was very quickly created and destroyed due to inlet proximity (Long Cove Inlet).

Regarding the issue at Point O' Woods, the interpretation of the reviewer is correct. There is a large gap in time due to the area around Point O' Woods being truncated by erosion at some point, probably after progradation well seaward of C3's location. As for the overwash noted at the rear of the island, past storms have been able to spot breach the relict dune lines (which are now topographically subdued due to island drowning). This is evident in the morphology, and Figure 3d highlights some prominent washover channels that penetrate the barrier from front to back. Additionally, post-storm imagery from the 1930s to the present also shows spot breaching in areas where relict dunes are topographically subdued.

[[Then, one more major issue with the manuscript is related to the discussion. I had suggested the authors not to insist on the unknowns, mostly because I found it too speculative and reduced on their interpretations of what could happen by not including or considering other possible processes and thus, trajectories. However, the authors decided not to change the document following the suggestion. As a result, this subsection of the discussion is mostly

filled with conjectures and with the strange believe that having a large dune is negative for the resilience of a coastal barrier.]]

+ We discuss the 'unknowns' because the morphological trends we highlight on Fire Island (narrowing, loss of marshes, drowning of barrier interior) have been seen in modeling and historically observed. These negative effects associated with large dunes are discussed by Timmons et al. (2010) and Dolan (1972). Additionally, the modeling by Mariotti and Hein (2022) demonstrating lag dynamics resulting from geomorphic capital are an extension of this.

Regarding the issue of resilience versus resistance, large dunes specifically increase barrier island *resistance* to landscape change over short-term annual to decadal scales. Counterintuitively, this short-term resistance comes at a price for barrier island *resilience* over longer timescales (decadal to centennial) by limiting (or eliminating) overwash that is required to build height/width while translating the barrier landward so that it can keep up with rising sea levels. In this context, because Fire Island remains mostly static, its backbarrier ecosystems are being degraded by erosion and drowning. In addition to slowly breaking down the island's ecological resilience, erosion and drowning is narrowing the barrier and potentially making it less physically resilient to future transgression in a time of increasing sea level and maybe increasing storm frequency/intensity.

To address the lack of clarity regarding both points, significant modifications were made to the Discussion, which are noted in the response to the reviewer's final comment (see below).

[[Well, this is very strange to read and only based on a narrow view of the evolution of a system. The authors assume that dunes can only be eroded, they do not consider the possibility that these features may adapt and even contribute sand to the backbarrier. For the authors, barrier inland migration can only occur driven by overwash. If a dune is able to build up, it is because sediment budget is high (as in this case helped by human interference on the evolution of tidaly inlets) and thus, it is resilient. If a change in the conditions happens, then, the shoreline will react and thus will do the dune. There are examples showing how dunes are able to move landward while maintaining or even gaining elevation and transferring sediment inland (example in some work by Davidson-Arnott and colleagues). Of course, this is not possible along armored coastal stretches, but also not considered at all as a possibility by the authors of this work. Then, all of a sudden we end understanding that only a coastal barrier on a unstable state (usually associated with a rollover phase) is more resilient than a barrier with a tall dune that it is still unknown which adaptation trajectory may follow.]]

+ This crux of this comment oversimplifies the dynamics at play, cites examples of transgressional dunes that may not be relevant in this system, and ignores many other well-documented cases where large dune systems have been completely destroyed over decadal timescales. The Virginia Eastern Shore barrier islands, for example, offer multiple cases of dunes and almost entire islands being lost (e.g. Hog Island and Cobb Island, among others). Additionally, this comment conflates the idea of 'resistance' with 'resilience', which we addressed in the response to the previous comment. As we noted, Fire Island's very tall dunes make it resistant to change, but not necessarily resilient in the long term. This loss of resilience is already being realized in sections of the barrier that have remained static for hundreds of years. We modified the Discussion heavily based on the previous comment, which addresses the concerns here. Our modifications are shown in response to the reviewer's final comment (see below).

[[In this regard, I must confess that I very much regret my initial positive comments to the manuscript, as I can hardly understand this discussion. In this regard, I suggest considering rearranging it. I do believe the noted change in state is worth studying and paying attention to understand the evolution of the system, but I do not understand the suggestion that the system may be close to a tipping point.]]

+ Ultimately, the reviewer's insights proved helpful to improving the manuscript, although this most recent round of comments demonstrated our Discussion required further refinement. We believe our revisions to the Discussion (see below, in response to final comment) should clear up the lack of understanding.

[[Yet, the authors should also consider changing the title of the manuscript, as it is not clear what does it mean. Implications of what? The implications are the unknown, so, do you want to state that in the title? It is like setting a question you cannot answer. The only thing you do know is that the state, and thus the dynamics and processes dominating the morphology (avoid using eco as you do not assess that component), have significantly changed and that may determine the future of the islands, but maybe only that the moment for barrier rollover will have a greater delay.]]

+ The reviewer correctly points out the main implication of the manuscript, which is that barrier rollover may have a greater delay, and we do not yet understand exactly what that means in the context of long-term resilience for an island which is now experiencing a greater rate of sea-level rise than at any time in the last 5,000 years. We can, however, point out that the short-term trend of bayside and interior drowning is likely to continue due to the presence of large overwash-resistant dunes, and we can also point to modeling studies and observations in other systems to show that a transition to rapid transgression and possibly even island drowning is possible. As our title states, these will have implications for resilience, and over multiple timescales.

[[Still in the title, what do you mean by coastal systems? Maybe it is too broad.]]

+ We currently qualify this with "Barrier Dynamics" in the title. An alternative could be to write "Coastal Barrier Systems," but we decided against this due to redundancy.

[[In the introduction section, lines 33 to 35, I would recommend the authors to revise this definition if they are actually using the one chosen by Masselink and Lazarus. The authors present a large discussion about the difference between the engineering or static approach to resilience and the ecologic/dynamic one, choosing the second to define the resilience of natural and not so natural systems, as the ones able to respond to disturbances maintaining their functions. I would recommend the authors to also include those approaches where dunes are included within the system, adding complexity and processes but more aligned with what their study area presents.]]

+ We add context to clarify our definition in the first paragraph of the Introduction: "In a general sense, Masselink and Lazarus (2019) define coastal resilience as the capacity of the landscape to maintain its ecological functions in the presence of disturbance. Here, we focus this definition and describe morphologic resilience as the ability of coastal barriers to redistribute sediment in a way that maintains the integrity and distribution of landscape components through time (Masselink and Lazarus, 2019 *c.f.* Long et al., 2006), with state shifts comprising threshold changes in system morphology that cannot be easily recovered to a previous configuration (Kombiadou et al., 2019)."

[[In the results section, I found difficult to understand the vertical relationships between the facies mentioned by the authors. So, how can dunes be overwashed? If I imagine the second to happen at the beach level…how are dunes buried by overwash? Then, if sea level is rising across your record, would you expect to be able to note the gradual rise?]]

+ It is confusing due to the long timescales involved. Since sea level is increasing, older dunes produced at a lower sea level can be overwashed due to an increase in the surface elevation of the beach. Additionally, washovers channelize over the dune and do not always cut down to the level of the sea in calm conditions. So, together, this allows for some preservation of the dune base beneath washover facies.

[[Make sure the length of the cores is well represented within the GPR lines.]]

+ Annotations added noting the decompacted core lengths.

[[Are the lines in figure 7 running parallel? If so, why do you need to show both?]]

+ This was done mostly to show the loss of the foredune in profile from 2016 to 2021, which is now more relevant to the Discussion based on our most recent edit. We use this comparison to demonstrate the concept of resistance.

[[In the discussion, the whole paragraph from line 533 to line 549 is very difficult to follow and understand and again, it presents a barrier that is passive and not able to react at all. Why do the authors assume that the current resilience of the island is low? Why it has to continue decreasing? This sentence is impossible to understand: Loss of morphological resilience is likely to be exacerbated by reserves of naturally available sediment in reaches of relict progradational dunes, which could enhance Fire Island's short-term resistance. I would think all the way around, because this means that the width of the barrier is high, how is that a not resilience system?]]

+ The width of the barrier is not necessarily high in places where there are multiple dunes present. In fact, it is quite narrow in places like Davis Park, where there at least two to three dune ridges present (see Figure 9; compare with Figure 3 for plan view). However, the barrier (especially the modern foredune) in such places is tall, and due to dunes blocking washover deposition, there is no longer a fringing marsh, and the bay is directly eroding into the base of the most landward relict dunes, destroying terrestrial ecosystems. In this sense, the dunes themselves may be resilient (for now) and provide a level of resistance against rapid landscape change, but at the scale of the whole barrier island the lack of washover is an impediment to the barrier's ability to adapt to changing environmental conditions. Thus, Fire Island could not be described as a resilient coastal system per the definition of Masselink and Lazarus (2019)—Fire Island's landscape is presently not adapting to long-term disturbance in a way that allows it to maintain its functions.

Since the reviewer's comments make it clear the Discussion was unsuccessful in relating this critical point, we attempt to fix this by breaking up the third paragraph of the Discussion into two separate paragraphs and completely rewriting them. Here, the first paragraph of the reworked section defines why we know Fire Island is presently experiencing a loss of resilience and that we think this process is likely to continue. The second paragraph explains why this loss of resilience is being enhanced by short-term resistance (which is defined with an example in Ho-Hum Beach):

"Regardless of the processes interacting or competing with inlet activity, our results suggest that if Fire Island were capable of sustaining inlets at the decadal scale—as it once did—it would display periodic barrier rotation accompanied by a greater prevalence of overwash. The latter is the primary process sustaining barrier island adaptability to disturbance over decadal timescales (Masselink and Lazarus, 2019). Instead, as sea level increases, the modern island with its nearly continuous high-foredune system is more likely to undergo gradual frontal erosion combined with drowning of the interior and bayside, which is already underway (Art, 1976; Nordstrom and Jackson, 2005; Sirkin, 1972). This process is well-documented historically (Dolan, 1972) and in modeling studies (Lorenzo-Trueba and Mariotti, 2017, Miselis and Lorenzo-Trueba, 2017; Magliocca et al., 2011; Rogers et al, 2015), the latter demonstrating that the lack of washover deposition from high, maintained foredunes can perpetuate a feedback loop of island narrowing and marsh destruction.

Although counterintuitive, loss of morphological resilience at the decadal scale may be exacerbated by reserves of naturally available sediment in places where relict progradational dunes exist, as well as by shallow antecedent topography. In the short term (years to decades), both features likely enhance Fire Island's resistance (see Kombiadou et al. 2019) against landward migration. However, over longer timescale (decades to centuries), modeling by Mariotti and Hein (2022) demonstrates that sediment reserves or "geomorphic capital" may increase the barrier's long-term commitment to retreat, as well as the rate of retreat when it eventually occurs. Shallow antecedent topography can also temporarily pin the barrier in place, further contributing to a long-term lag in sea-level driven retreat (Shawler et al., 2021a). At Fire Island, Ho-Hum Beach appears to provide an example of resistance imparted by both large dunes and shallow antecedent topography. Specifically, Figure 7 shows how the foredune at Ho-Hum Beach in 2016 provided resistance to barrier retreat from shoreline erosion induced by Wilderness Inlet. By 2021, the foredune was destroyed by the inlet's downdrift erosion shadow, but the geomorphic capital provided by this foredune and the relict ridge behind it resulted in frontal erosion of the barrier rather than full-scale migration. Additionally, Figure 7 reveals that the antecedent Pleistocene surface may be relatively shallow at -3 m elevation, which suggests the barrier could be at least partly pinned at this location."