**Response to editorial review**

Reviewer 1

Need to define “SLR” upon its first usage.
SLR was changed to ‘sea-level rise’ and RSLR was changed to ‘relative sea-level rise’ throughout

P. 4. McLeod et al., 2011. Should be a lower case “l” in name.
Corrected

P. 5. Would you be willing to define “Sabkha” parenthetically next to its first usage. Not sure many readers will be familiar with that term.
Sabkha is defined immediately prior to its use (i.e. hypersaline flats/sandflats or sabkha)

P. 6. Need a period after “…Duarte 2016).”
Corrected

P. 6. Please define the abbreviation “SRTM” upon first usage, or just delete the abbreviation. Next line, “or” should be “of”.
SRTM abbreviation deleted.
“or” changed to “of”

P. 7. Re: the statement “… of at least 20 years before harvesting can occur”. Also, in the tropical cyclone belt, harvesting would be replaced by storm effects over a similar period of time. Might be worth incorporating. This is our biggest point of discussion in the Neotropics where harvesting is limited. I tend to equate this cyclone destruction to annual marsh senescence; instead of an annual cycle of aboveground C turnover, mangroves can have a two-decade cycle of turnover.
Additional text has been added to the end of this paragraph making the link between feasibility of restoration projects with a 20 year commitment when cyclone/storm intensity and frequency may be increasing. The following text has been added:
*“In addition, while intense cyclones and storms are reported to have a return interval of approximately 20 years (Elsner et al. 2006), aligning with the commitment period for restoration projects before harvesting can occur (Galik et al. 2022), the feasibility of restoration projects in regions with a propensity for cyclone activity may decrease should projected increases in the frequency and intensity of major storms eventuate (IPCC 2021).”*

P. 7. Presumption of synchronous C burial with aboveground C increment is interesting though. I feel like you have been able to document fairly well that this synchrony is not true for mangroves under rising sea-levels.
This text has now been modified to the following:
*When mangrove forests and saltmarshes have reached their threshold capacity for standing above-ground biomass, then increases in the standing living stock is largely achieved by lateral increases in extent as increases in plant density in mature forests will be resource limited. Increases in below-ground biomass are presumed to be limited by vertical space within substrates for net biomass additionality and addition of biomass will increasingly be offset by decomposition of below-ground biomass as substrates asymptote towards higher elevations; that is unless relative sea-level rise creates more vertical space for below-ground storage (Rogers et al. 2019). Critically, lateral increases in extent are constrained by the availability of land where conditions are favourable within the intertidal and supratidal zone.*

P. 7. Duplicate citation for “Saintilan 1997” on two occasions.
Corrected.

P. 7. Re: the statement “…where mangroves have expanded into saltmarsh zones positioned higher in the tidal frame” does not apply to the neotropics. I know that you have previously addressed this, but perhaps a caveat is needed here. Marshes are most often lower in the tidal frame than mangroves in the neotropics. You always step down when you transition from mangrove to marsh.
This statement is actually incorrect as the mangrovesare likely expanding to maintain their position in the tidal frame. This text has been corrected to be more globally relevant and doe not explicitly refer to expansion of saltmarsh into mangrove. The following text was included:
*Increasing mangrove extent with relative sea-level rise and warmer temperatures over the past few decades has been observed and is particularly notable in Australia, the Gulf and Atlantic US Coastline, Mexico, and South Africa where mangroves have expanded landward to higher elevations*

P. 9. Re: the statement “…oxidizing materials required for decomposition of soil organic matter.” Remember also that there are anaerobic pathways of soil organic matter decomposition. You address this immediately below, but I suggest instead of “required” consider a different word or phrase here.

This sentence has been corrected to:
*Initially, bedrock or basement geology delimits the zone in which sediments can accumulate, but as accommodation becomes increasingly infilled, or ‘realised’, via the accumulation of mineral and organic material, substrate elevations increase and become increasingly terrestrialised and exposed to the oxidising conditions underlying aerobic processes of soil organic matter decomposition.*

P. 9. Should be “decomposition of organic matter”. Missing the word “of”.
Corrected

P. 9. Line with the citation to Allen (2009); need to add space between “and” and “organic”.
Corrected.

P. 9. Hydroperiod also includes “depth,” or rather should be “depth, duration, and frequency”.
Corrected

P. 10. Should the phrase be “sand sheets”?
Corrected

P. 10. Be sure that “-1” is superscripted in 20 mm yr-1.
Corrected

P. 10. A fairly useful perspective from the Caribbean basin is provided by: Sherrod CL, McMillan C (1985) The distributional history and ecology of mangrove vegetation along the northern Gulf of Mexico coastal region. Contributions in Marine Science, 28, 129–140. This might help with comments by the Reviewer as well. Also, look up what Maggie Toscano, Debra Willard, or Miriam Jones has published on the Everglades region. Might strengthen a few of these statements on Page 10.
Additional context has been provided about changes occurring in the Carribbean basin and Golf of Mexico, with specific reference to the work or Sherrod and McMillan, Willard and Jones. The following text has been icnoroporated:
“However, the initiation of mangrove and saltmarsh transgressive phases is globally variable due to the influence of glacio-isostatic adjustment on varying rates of mid to late-Holocene relative sea-level rise (Ribeiro et al. 2018), and the modulating effect that other climatic variables (e.g., droughts and/or frequent storms) have on conditions that favour mangrove expansion or decline (Sherrod and McMillan 1985, Jones et al. 2019).”; and
“Evident from interbedded peats and marls, the onset of the transgressive phase occurred from about 3500 y BP (Scholl 1964, Parkinson et al. 1994, Jones et al. 2019), although the occurrence of continuous vertical peat growth is modulated in some locations by other climatic factors, including a period of cooling, that were not conducive to widespread mangrove expansion and vertical growth (Sherrod and McMillan 1985, Jones et al. 2019).”
Additional reference has also been made to other saltmarsh literature pertaining to the east coast of North America

P. 10. Please use “keep up” or ‘keep up’.
Corrected

P. 11. In lieu of “1.5 o/oo,” I would standardize to “p.p.t.” for consistency.
Corrected

P. 11. Should be “increases in atmospheric”. Need to add “in”.
Corrected

P. 11. Paragraph break before “Presentation...”?
Corrected

P. 11. Delete “,” after 3500 yr BP. Next line should be “reported to have”. Add the word “to”.
Corrected

P. 12. Re: “…exhibit high soils organic carbon…”. Should this be “soil”?
Corrected

P. 12. Mispelled “below-ground” as “belowgroun”.
Corrected

P. 12. Line immediately after subheading. Delete “t”.
Corrected

P. 13. Misspelled “livelihoods” and “security”.
Corrected

P. 13. Need a space after “markets”
Corrected

P. 13. Double citation of “Lovelock et al. 2022”
Corrected to Lovelock et al. 2022a and Lovelock et al. 2022b

P. 14. Re: “…whilst the 100 year timeframe aligns with what is regarded to be “sequestered carbon”.” Is there is citation for the usage of this time frame? I’ve often wondered if there was a citation for that assumption. It seems to be more widely known in the core world as 100 years.
This sentence has been slightly rephrased and a citation included, as per below.
*The 25-year permanence time frame aligns with the period for which woody vegetation is anticipated to reach maturity and exhibit high rates of carbon addition to substrates (Osland et al. 2020), whilst the 100 year timeframe aligns with what is regarded to be permanently sequestered soil organic carbon (i.e. permanence) (Dynarski et al. 2020)*

P. 15. Misspelled “flux” as “flx”. Next line, misspelled “atmospheric”.
Corrected

P. 15. Should be “outcomes are”.
Corrected

P. 16. Should this be “vary” instead of “varies”? Refers back to “magnitude and duration”. Two lines later, misspelled “between”.
Corrected

P. 17. Re: “…capacity of at the site scale…”. Need to delete the word “of”. Few lines later, need space between “and” and “policy”.
Corrected

P. 18. Misspelled “appreciate”. Also, should be “data were”.
Corrected

**Reviewer 2**

This work is interesting and timely and provides a nice, albeit geographically biased,  overview of the topic. There are however some shortcomings. There are quite a few orthographic and spelling mistakes throughout, which should be addressed prior to resubmission, without line or page numbers it is difficult to highlight, but the first line of the abstract doesn't make sense and should be rewritten, the other orthographic issues are on pg 2, 7, 9, 10 (a few),11, 12, 13 (a few), and 15 of the MS.

We appreciate the reviewer and editor (Krauss) identifying these errors, and hope they have now been corrected. It appears that spell check was not enabled when preparing the draft and the number of errors that we did not pick up is surprising.

These are the minor issues. Larger issues are that for a big review such as this there is not a lot of information on regions outside of the US, Australia and (a bit on some parts of Asia), particularly Europe and S. America, although there are a lot of articles on both SLR, and to a lesser extent, blue carbon from these areas.

We appreciate the reviewers’ concerns, but the selection of cited literature is an outcome of prioritising brevity when considering sea level rise literature, and not an author bias. This is addressed in more detail in later comments, and efforts have been put in place to broaden the reference to sea level literature beyond that from Zone II and Zone V in Clark et al. (1978).

On pg 4, when referring to sequestration in seagrasses, it should also be noted that seagrasses can export carbon to adjacent ecosystems including mudflats and in the final sentence of the first section on that page, it would be better to justify the focus of the paper on salt marshes and mangroves on the greater C storage capacity per area rather than solely on in situ storage.

Regarding export of carbon from seagrasses, this has already been addressed in the manuscript with the following text: “While seagrass meadows may be an exceptional carbon source for sequestration elsewhere, their capacity for in situ blue carbon storage is largely limited to that stored within the living biomass and some detritus.” It is unclear what the reviewer is suggesting when referring to focussing on greater carbon storage per area, rather than in situ storage – these are largely the same thing. No change was made to address this comment.

On page 5 the second from last sentence focusses on the geographical regions of N America and Australia (plus S Africa). Is this solely because there has been a greater focus of study written in English on these regions, or potentially just studies that the authors have read on these regions. there is at least one study in S. America that discusses this, and potentially more in other regions.
The reviewer is implying an author bias that is misplaced. Saintilan et al. emphasises that increases in mangrove extent near mangrove southern limits are not profound, and changes have been related to other factors such as warming and propagule dispersal on currents. This is further emphasised by the recent work of Rodrigues et al. (2022) demonstrating that ‘temperature is the primary factor regulating the mangrove distribution in south Brazil’. In China, recent mangrove expansion has been linked to recovery following clearance and whether this is related to sea-level rise remains an ongoing discussion. To broaden the scope of referencing to warming, Cavanaugh et al. 2014 citation has been replaced with Ximenes et al. 2016, Osland et al. 2017 and Godoy and Lacerda 2015.

On page 10, the first section (carried on from the previous page) is also focussed on particular regions. There are quite a few studies from groups in Europe (Netherlands, Spain, Germany, Fennoscandia, the Baltic States, UK, and France) and S America that are missing from this, including groups from the National University of Mar del Plata in Argentina, Marcelo Cohens group in Brazil, Luiz Drude de Lacerda's group in another part of Brazil, and groups from Santa Catarina and Sao Paulo in Brazil that work in this area as well as Sanders from the US who works in S. America. Lastly, the first paragraph of the conclusions finishes with a very broad generalisation of the southern hemisphere, which based on the review, really only accounts for Australia. It would be better to either broaden out the review to a wider geographical coverage or undertake a systematic review (this would be the best way forwards), that will truly account for global coverage, even if only English language publications.

Following from previous comments, the reviewer appears to be implying a literature bias that was not intentional, but was an outcome of focussing on literature where sea level has been relatively stable for millennia (i.e. Zone V in Clark et al. 1978 – Australia, SE Asia and Southern Africa) and regions where sea level has been rising for millennia (i.e. Zone II in Clark et al. 1978, USA and UK literature dominate) as these represent end members of late Holocene sea-level trajectories (i.e. sea level stability vs sea level rise). We agree with the reviewer that this focus is not comprehensive; however, it did meet the need for brevity for this review. In addition, it is beyond the scope of this article to incorporate remarkably more Holocene sea-level literature by undertaking a systematic review, although this would be an ideal focus for a different manuscript. To address the reviewers concerns regarding an implied bias, efforts have been put in place to reference literature from far-field locations in zone IV (i.e. South America) and locations in Europe from zone II. To clearly demarcate this focus on end members, we have been explicit in the text with the following sentence: “*For brevity, we focus on end members: far-field locations (Zone IV-V) where relative sea level has been relatively stable for millennia (or may have fallen); intermediate locations where relative sea level has been rising over the mid- to late-Holocene (Zone II-III); and near-field locations (Zone I) where relative sea level has been falling (Figure 4A).”* We have also explicitly referred to the zones identified in Clark et al. 1978 and incorporated mapping of these zones into figure 4.

Regarding broad generalisations for the southern hemisphere, they still generally hold true – the sea level history in zones IV and V are conducive for the development of broad coastal floodplains, and there is less coastal development across a lot of the coastline of the southern hemisphere. Irrespective, effort has been put in place to be more definitive with this generalisation and the statement has been adjusted to:
“The magnitude and duration of the negative feedback on climate vary between hemispheres. Climate change is expected to squeeze mangrove and saltmarsh area in the northern hemisphere between accelerating relative sea-level rise and hard barriers. In the southern hemisphere, opportunities for landward expansion of mangrove and saltmarsh may be available where late-Holocene sea-level history coupled with ongoing sediment supply and lower contemporary rates of relative sea-level rise has facilitated the development of broad coastal floodplains and where coastal squeeze effects are minimised.”