11th Jun 20

Dear Dr Zoccarato,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "The Holocene imprint on future evolution of tidal marshes". It 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(that is, either to incorporate the suggestions or provide a compelling argument why the point made by the reviewer is not valid, or relevant to the editorial threshold as outlined below)
AND
• meet our editorial thresholds as outlined below,

then we would be happy to look at a substantially revised manuscript. However, please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

In the following, we list our main editorial concerns:

****Provide a robust model of the role of shallow sediment compaction in the resilience of coastal salt marshes with a compelling application to the vulnerability of the San Felice marshland****

****Restructure and rewrite the article so that
* the objectives of the work are clearly laid out in the introduction,
* a brief summary of methods and data are provided in the main text,
* all terminology is explicitly defined and used consistently throughout,
* your results, interpretations and their implications are clear and easy to understand, and
* the Methods and Supplementary Methods sections together provide sufficient detail to make the work reproducible****

In addition, please supply point-by-point responses to all the reviewers' comments.

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 understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

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 referees' comments (which should be in a separate document to any cover letter) and any completed checklist:

[link redacted]

** 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 me 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,

Joe Aslin

Associate Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

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

For your information, you can find some guidance regarding format requirements summarized on the following checklist:
Communications Earth & Environment formatting checklist

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

In this paper, the authors build a physical model in order to quantify sediment compaction in the San Felice marshland, Venice Lagoon. The shallow compaction of marshland and accelerated rate of sea-level rise will exacerbate risks of erosion and flooding of low elevation coastal zones. However, there are very limited physical models of sediment compaction, mostly due to the difficulty to acquire model's parameters and accurate measurements of compaction for validation. This paper presents an exciting model, that can be widely used in marshlands when e.g., incorporating sediment consolidations in projection of relative sea-level rise. Therefore, this study has the potential of making a major impact on risk assessment of marshlands. I recommend reconsidering the manuscript for publication after conducting major revisions.

My first main comment concerns the lack of clarity of the writing. The text is difficult for a reader with a general geoscience background to understand and lacks minimum explanations of monitoring techniques and modeling approach. The paper is written well in "Introduction", the writing breaks down at page 4 "Contribution of below-ground deformation to net accretion rates" and at Figure 3. I took a full day reading this but I never reached a full understanding. The authors use too many acronyms and abbreviations that make the text hard to follow.

The author uses a current of rate 1.2 mm/yr at a tide gauge in the northern Adriatic Sea for their relative sea-level projections which fully disregards accelerating sea-level rise associated with different global warming scenarios. I suggest the authors consult the literature that focuses on sea-level projections using tide gauge data (see e.g., Kopp et al. 2014; Jevrejeva et al. 2016). Kopp et al. (2014) provides valuable sea-level projections under three different RCP scenarios for a global set of tide gauge records.

The authors distinguish between displacement and deformation, as presented I foresee confusion. The leveling technique and artificial radar reflectors, both allow measuring vertical displacement (roughly normal to a reference datum). Are the InSAR rates in "line of sight" direction or locally tied to GPS time series in a geodetic reference frame? This is not clear in the text and needs more explanation.

Section 2 "contribution of below-ground deformation to net accretion rates" doesn't read well. It's difficult to find connections between sentences, and the reader becomes confused how these rates are calculated. I suggest moving "Subsidence and accretion dataset at San Felice marshland" section from "Material and Methods" to the main text, incorporating this part into section 2 and rephrasing this section,

A uniform deep subsidence rate of 1 mm/yr is assumed for the projection task but its origins are not discussed in the manuscript. Deep subsidence could be related to several shallow and crustal processes varying in space and time.

It's not clear throughout the text that what InSAR rates exactly represent. Are they measures of surface elevation change and/or vertical land motion? In Figure 2, deep-grounded reflectors facilitate the measure of vertical land motion, but for natural reflectors InSAR most likely represents surface elevation change since sediment accumulations may change natural reflectors. The authors need to clarify these confusing points.

Figure 1: The InSAR map (Fig1B) shows many pixels with positive rates. Can authors explain these rates? For natural reflectors positive rates could be due to sediment accretion. But how this can be

described if reflectors come to be artificial ones being grounded in depths (if there exist positive rates associated with these reflectors?). Also the large limit of color bar doesn't allow to easily distinguish positive and negative rates. I suggest the authors adding a (histogram) plot showing distribution of InsAR rates for San Felice Island next to Figure 1B.

Figure 2 is very useful and informative as it nicely illustrates time spans of each monitoring techniques and their foundation depths. However, it would be more useful for the reader if the authors show geographic locations of these instruments, so the reader could get an overview of spatial distributions of monitoring sites. I suggest adding these sites to Figure 1B.

Figure 3 is not very informative. It shows too many details. What do the vertical lines represent? I suggest the authors using a simpler illustration to spell out the message (Fig 3 is barely touched on in the text!). For instance, a scatter plot with rates from each technique versus depths could be an alternative display.

Table 1:
"deformation and displacement" – Again please be careful with terminology. What is the difference? Both refer to the same quantity: changes in vertical distance along the line from the Earth's surface to the reference datum (geoid or ellipsoid).
Please report rates (mm/yr) and their uncertainty instead of standard error of displacement, and specify the range of TCR rates in the text (Ln 55)

The authors should add a map or at least info showing the thickness of Holocene strata for their study area. This info should be presented in the text explaining the shallow measurement as a function of depth in the second section "Contribution of below-ground deformation to net accretion rates". For example, in Ln 56-57, thickness of Holocene sediment is quite relevant and is a main factor for understanding and modeling shallow compaction.

Minor comments:

Ln 17: Please leave out the geographic coordinates of study area.

Ln 27: "with average values of 3–4, 1–2 and 2–3 mm/year in the northern, central and southern portions..." >> with average values of 3–4, 1–2 and 2–3 mm/year, respectively, in the northern, central and southern portions…

Ln 32-33: "Usually, formation and evolution of salt-marshes are studied with models that does not explicitly account for consolidation of the marsh sediments." Can authors give some reasons and add relevant references here?

It hasn't explained anywhere within the text what SE-SET and MO-SET measurements are and what SE and MO stand for!

Ln 50: Please clarify in one sentence here which projection scenarios are used for MSL rise and sediments rate?

Ln 51: change "by the end of the century" to "2100"

Ln 143: “in particular the 1m-top soil where major consolidation takes place”. Can you cite some relevant works here? Or I’d suggest changing this to “in particular within top several meters of soil where major consolidation takes place”.

The author mention use of GPS time series in the Acknowledgment. Do I miss something here? I couldn’t find any GPS results in the manuscript!

Please explain $\sigma z,0$ in the formula given in the Supp.

Reviewer #2 (Remarks to the Author):

This is a potentially very interesting paper, unfortunately it contains so many flaws that the results are extremely difficult to understand. The objectives of the work are not clearly stated, so the reader does not know what to expect. Does the paper focus on the formulation of a new model for salt marsh soil compaction/deformation? If the answer to this question is yes, then I am afraid but this work does not sufficiently describe the model characteristics and how the parameters were determined. In case this work makes use of a model already formulated/validated and applied to salt marshes, then it should be clarified in the text, citing the source, and the authors should specify the objective of this new application/research in the Introduction.
As for the message, the paper does not deliver it in a clear and sounded way. My impression is that the “Materials and Methods” section was originally thought to be included in the main part of the paper, and then, since Nature Communications requires the Methods section to be at the end, it was simply moved. This is not acceptable. The main part of the paper must be clear and self-sustaining without the need for the reader to jump to the Methods section in order to understand what she/he is reading. The same concept applies to “Supporting Information” that should in fact be “of support”, not essential to understand the results presented by the authors, as in this manuscript.
I believe that the data set presented in this work is of great value, but unfortunately, it is poorly described: no details about the acquisition of measurements are included, no descriptions of the sensors or methodologies are listed. The authors do not even explain how they calculated the uncertainties associated with the measurements performed with different sensors/methods.
I suggest the authors to completely revise the structure of the manuscript, clearly listing the objectives of their work, and describing their findings for all readers – not just for experts or those who have gone through the Methods section at the end of the paper. I also suggest to better describe the data set and the methods used to collect the measurements with each instrument, including a detailed description of the calculations applied in order to quantify the displacements for each data set. Finally, some paragraphs are very difficult to read/understand: a thorough revision of English language is needed.
I have several more questions/comments/corrections, please see below:
Abstract: the questions included in the Abstract are not pertinent: this work does not deal with vegetation processes, nor with organic matter deposition - at least not explicitly. Moreover, no erosion processes are accounted for (lateral or surficial). Please include only questions that deal with the actual objectives of this work.

Abstract: “Elevation changes of a tidal platform strongly…” “…sea-level rise (SLR) is highly dependent…”

Lines 13-14: “…the delicate equilibrium that these complex systems provide to wildlife and plants.”

Line 15: “…of the world … perspective that implies a deep understanding…”

Fig. 1 : change to “northern”. What do you mean with "specific behavior"? How did you calculate the SEC values? If such values are taken form others, please specify and describe how they were obtained.

Fig. 2: change to “… over the time interval …”. In the caption there is FMH, in the figure it is FHM.
Line 17: please make a choice about the way you call salt marshes (not salt-marsh) or marshlands. The use of both terms throughout the paper is misleading.
Lines 18-19: “… representative of microtidal marsh environments. The main focus of this research is to investigate how the …”
Line 21: delete “seasons’
Lines 21-25: This is incorrect. Venetian salt marshes are flooded also in Spring and Summer, almost every day, during regular high tides – even the highest portions are flooded every month of the year during spring tides.
Line 29: “The vertical displacement of natural landforms … by the availability …”
Line 30: “ … contributing to deposition, the consolidation … self-weight, the erosion …”
Line 31: “external forcings …”
Lines 31-32 : here it is not clear if RSLR denotes the sum of all the factors mentioned in this paragraph: please specify or rephrase.
Line 32: “… that do …”
Line 33: “Conversely, this process…”
Line 34: delete “However”
Lines 35-36: “… may be critical to accurately quantify the dynamics of the surface elevation at centennial to illenial time scale, allowing …”
Line 37: Delete “Moreover” and put “has been proven significant”
Line 38: “… providing valuable insights …”
Line 39: “… driving the resilience of marshes to SLR”.
Line 42 “…observations obtained with a …”
Lines 43-44: “The model is calibrated using data collected in the period 1993-2013 through a network of instruments and sensors deployed on the marshland (Fig. 2), even tough …”
Line 45: please add a map that includes the actual location and distribution of all these instruments across the San Felice salt marsh. In Fig. 2, TCRs go down to three different depths. Please list here the three depth values, and describe how many TCRs belong to each depth value. How many TCRs are located over the San Felice salt marsh?
Line 46: Please, briefly describe what is a SET. Moreover, specify their characteristics in terms of depths, number, location, etc. Do the same for all the instruments.
Line 47: specify the type of the natural reflectors. How many were used? what are they?
Line 49: “further details on the measurement dataset are available in Materials and Methods section”
Line 52: This chapter is poorly organized, and it does not make sense. You need to include a list of variables (i.e. VD, DE, etc.) that are estimated using the different instruments/ sensors, and describe how they are calculated. I understand that a detailed description is included in the Methods section, however the reader should not be required to jump to the Methods section in order to understand what you have done. A Table might help.
Line 54: Do you refer to TCR measurements only? please specify
Lines 58-59: I suggest to use + for accretion and - for negative displacement values, and please make this clear in the text.

Line 59: It is not clear to me why you start this paragraph saying that the "Holocene compaction" refers to 0 to -4m and then you calculate VD averaging all the values coming from TCRs, HPL, MO_SET and SE_SET that go down to -8m. Could you please explain?
Lines 60-66: I am sorry but I cannot follow this description. Please rephrase. Please include comments to Fig. 3 so that it will be easier to visualize the results that you are describing. The way the results are presented and explained is very confusing.
Fig. 3 : I think this figure is extremely difficult to interpret. I suggest rethinking it completely. In the new figure, please include the error bars. Why there are three panels? Are the first two panels (above) referred to two different data sets or two different locations? Is the third panel (below) a sort of summary? Please add the unit of the y axis of the third panel. In the caption use “accretion” instead of “expansion” and “… which is in agreement with the PT observation.”
Line 79: Have you defined ω? I see the definition in the Methods section, but you must define all the variables before you mention them.
Line 90: “… 5mm/year respectively”
Line 91: delete actual and use “current’
Line 93: have you defined δ?
Line 102: “threatened”
Lines 103-111: these sentences seem to say that the only input of sediments available for the salt marshes comes from the sea. I doubt so, and I suggest the authors to verify and rephrase or bring evidence that support this hypothesis. Even though the Venice lagoon is known to lack sediment inputs, resuspension of sediments from channels and tidal flats is very high, increasing suspended sediments that are then deposited on marshes. This has nothing to do with the MOSE system.
Line 115: “source” of what?
Line 123: “Our findings show that salt marsh CO2 storage capacity must account for the compaction…”
Line 125: “… highly impact the San Felice marshland resilience if sedimentation is maintained…”
Line 128: “Our results confirm previous …”
Line 140: “…provided by biomass…”
Line 157 : “At the San Felice ….” “… displacement and deformation measures, and specifically deformation (DE) is … and 0.5 m, while vertical …”

Supporting Info: I suggest to move the content of the Supporting Info to the Materials and Methods section because it is important to understand the results.
Table 1: “Details of measured deformations, …” Where does the standard error come from? No description in the main text. Please add a description
Table 2: please specify what type of sediments you are considering. I think that the listed parameters, as for example the specific weight, correspond to sand. However, it would be good to show results also for finer sediments, silty and clayey sediments, since salt marshes usually have sediments finer than send. If this is exclusively referred to the specific studied salt marsh, please say so. Moreover, what would be the results for sediments rich in organic matter? Salt marsh soils tend to have organic matter content higher than 20%. What would be the behavior in this case?
Hydro-geomechanical properties and model set-up
it is not clear to me if this model has been already published in a previous paper or not. If the answer is yes, the paper should be cited and a discussion about the uncertainty should be included. In case the answer is no, then a comprehensive work on calibration/validation of the model must be included in order to show its feasibility.

Reviewer #3 (Remarks to the Author):

Review of “The Holocene imprint on future evolution of tidal marshes” by Zoccarato & Da Lio (CEE-20-0086)

Zoccarato & Da Lio present a tidal marsh model, constrained by a fairly unique set of field data, that explicitly incorporates subsurface processes (sediment compaction) with the ultimate goal to predict future marsh vulnerability within the context of climate change. There are several widely used marsh models in the literature, but as the authors point out (lines 32-34), very few (if any) fully capture subsurface processes. The authors correctly stress the fact that this is of particular importance when considering longer timescales, an issue that is often overlooked in the marsh literature. As such, this is a welcome contribution that builds on many years of geotechnical modeling advances within the Padua group, such as the adaptive mesh approach published by the lead author in 2017. The present study is tied to a range of monitoring techniques in the Venice Lagoon that complement one another and serve to capture subsidence processes in a depth-integrated fashion. I am particularly pleased to see that these measurements are all carried out with respect to well-defined reference points (anywhere between the wetland surface and a given depth in the subsurface), something that is too often neglected. There are very few marshes worldwide that have seen this type of monitoring, placing this study at the forefront of the field. Nevertheless, I do see ample opportunities to further improve this work, especially in terms of presentation. My main concerns revolve around the treatment of marsh accretion and sea-level projections, along with the use of terminology and some documentation issues. Please note that I am not a modeler and my expertise in the geotechnical aspects of the work is limited; I assume others can comment in more depth on those elements.

While the primary strength of this study is the dynamic incorporation of sediment compaction, a limitation is the fact that marsh accretion is simulated as a constant value, despite the nonlinear feedbacks that exist between RSLR, sediment accretion, and biogenic production (see Morris et al., 2002 and many subsequent studies, including D’Alpaos et al., 2012). The authors are aware of this (lines 131-132, 138-139) but it may be advisable to be a little more upfront about it by pointing out this caveat at the outset. Equally important is the nature of the material that accumulates on the marsh surface. Is this clastic, organic, or a mix of both? One would expect that this plays a large role in dictating the effective stress and the resulting amount of compaction in the three scenarios. I partly ask this because of what is stated in line 110: closure of the MOSE system would reduce the sediment flux to zero (which I take to be a hypothetical scenario, because I assume there are no plans for permanent closure of the barriers). This would imply that there is no organic contribution and that all loading is due to clastic material? The statement in line 140, as well as the information in Table 2 in the SI, suggest that the present study did not consider organic matter. This needs to be mentioned more explicitly, because it constitutes a significant limitation in the model predictions.

The definition of RSLR (lines 31-32) needs some thought. The authors mention “increasing MSL and deep subsidence,” but how about shallow subsidence? Note that Jankowski et al. explicitly included all three components in their definition of RSLR. Elsewhere (lines 54-56) the authors specifically mention the 0 to 4 m depth range as the one where shallow compaction operates, strikingly similar to the 0 to 5 m depth range as identified by recent work in coastal Louisiana (Keogh & Törnqvist, 2019, Ocean Sci). In other words, they may want to define RSLR as encompassing all subsidence components, not just the deep component, or they may end up excluding the largest player. To be clear, I don’t mean to prescribe this, but if the authors choose a different approach they need to be very explicit about this and provide a precise definition of what exactly the deep subsidence component constitutes (the Pleistocene surface and deeper? or strata deeper than 8 m, the maximum anchor depth in the cartoon in Fig. 2?).

I was puzzled about the sea-level scenarios of 1.2 mm/yr for the remainder of this century. Note that this number is now widely considered to be the global average for the 20th century (recent studies by Hay, Dangendorf, and colleagues); the present-day rate is well over 3 mm/yr (WCRP Global Sea Level Budget Group, Earth Syst Sci Data, 2018), and future rates are almost certain to be higher. Are the conditions in the Adriatic expected to be that much different? This would need to be justified. A more conventional approach might be to use global projections from the 2019 IPCC-SROCC report, assuming, of course, that those numbers are appropriate for the Adriatic/Venice Lagoon. It seems plausible that using larger (and more realistic) numbers for future geocentric sea-level rise will increase marsh vulnerability.

The manuscript contains tons of acronyms, several of which are not widely used in the literature and some are not intuitive. For example, artificial radar corner reflectors are referred to as TCRs. It would be helpful to limit acronyms wherever possible and to spell out those terms that are only used a few times in the manuscript. In the figures, please avoid acronyms when you can, or at least ensure that they are all explained in legends or captions, in a systematic fashion. For example, the authors mention PSI measurements in Fig. 1, but PSI does not feature in Fig. 2. I found myself constantly having to look up what the acronyms stand for; but even then, the associated terminology is a bit confusing. For example, the first equation in the Materials and Methods indicates that vertical displacement (VD) is equivalent to shallow subsidence as defined in the seminal paper by Cahoon et al. (1995, Mar Geol), but in Fig. 3 the impression is given that this is represented by deformation (DE). I got lost here. Also, please explain SE-SET and MO-SET; these are apparently two different varieties of surface elevation tables that I was not familiar with (the cartoon in Fig. 2 suggests that the difference relates to the length of the rod?). And what is the difference between “accretion rate (AC)” and “sedimentation rate (ω)?” More broadly, the terminology needs attention. For example, one of the opening sentences in the Materials and Methods section mentions “displacements and deformations,” but I don’t think it is clear to all readers that these are associated with different measurement methods. To illustrate the importance of unambiguous terminology, consider the heading (line 52) “Contribution of below-ground deformation to net accretion rates.” Given the nature of this study, I would argue that this should be reversed; what the authors show is that accretion drives subsidence rather than vice versa. In lines 65-66, the impression is given that SEC observations are somehow obtained from marker horizon measurements, which is incorrect (they are obtained from SET measurements only). Things like these must be ironed out, or the reader will end up being confused.

Related to this, I spent quite some time looking at Fig. 3, but I had difficulty understanding what exactly this figure is trying to convey. Is it meant to illustrate the actual rates that are observed at different depths below the surface? For example, the uppermost two panels (the difference between the two escaped me; it isn’t explained) include vertical purple lines that connect data points (squares) that are said to refer to high-precision leveling data. In the left-hand panel, the deepest of these lines shows a value of about –0.5 mm/yr and the right-hand panel appears to show the same thing. Why is this duplicated? And more importantly, what does it mean? That the sediment column between 2 and 4 m depth is subsiding at this particular rate according to the leveling data? Table 1 in the SI indicates that high-precision leveling data were obtained with respect to depths at 0.5, 2.4, and 8.0 m, which seems at odds with Fig. 3. I’m sure I must be missing something, but frankly, I got lost here. I believe the presentation of this information needs quite a bit of work. Without a doubt, Fig. 3 is critical since it presents the data that are used to constrain the model.

In Fig. 4, I assume that the red and blue curves are observations, but why do they extend (and abruptly reverse course) above the land surface? This looks strange. It also looks like only the SET-data are used for comparison with model results; what happened to the leveling data, for example? And what is the purpose of the inset with the tidal channel?

More information is needed regarding the subsidence dataset, in particular regarding the number of observations. For example, with few exceptions it isn't clear how many SETs, corner reflectors, etc., were used. This should be added to Table 1 in the SI.

The English writing needs work, as illustrated, for example, by the first sentence of the Introduction. I have not attempted to edit the manuscript, but I assume a copy editor will do so.

A few more specific comments:
Line 17: please be more specific; what exactly is "long-term time scale?"
Line 17: avoid mentioning lat/long data in the main text.
Line 18: my understanding has always been that the Mediterranean is nearly always microtidal, but perhaps conditions are different here? Could you simply provide the tidal range?
Line 29: be careful with the terminology here; aggradation and subsidence refer to different things (i.e., the counterpart of aggradation is erosion; the counterpart of subsidence is uplift). I think what is meant here is elevation gain/loss.
Line 57: please be careful, aggradation can also go hand in hand with loss of elevation, as shown by Cahoon et al. (1995) and many subsequent studies.
Lines 58-59: more specificity needed here; is surface displacement uplift or subsidence? Please be more direct in your wording to avoid confusion.
Lines 115-124: this paragraph fits better in the Introduction, because it provides the broader context and justification for this study.
Line 143: why is the first meter singled out here? The basis for this isn't entirely clear, given the earlier findings that highlighted the top 4 meters.
Fig. 1A-B: the study area (B) shows numerous point observations that exhibit surface-elevation gain, and several more with rather modest elevation loss, which seems different from the general pattern as shown in (A). This needs to be addressed.
Fig. 1C-D: please provide a little more detail about these photos; I assume they are supposed to illustrate a tidal channel and a marsh platform, respectively?

In summary, the two main substantive issues that I believe need to be addressed (RSLR and the role of organic matter) can be expected to have opposite effects on the model predictions (upward revision of rates of RSLR will increase the marsh accretion deficit, while incorporating organic matter might do the opposite). On balance it may come out in a wash, but that is of course rather speculative, which should be acknowledged. Despite these issues I believe this could become a valuable contribution, but plenty of work is necessary to effectively communicate the findings to the largest possible audience. I would strongly encourage the authors to do so.

Torbjörn Törnqvist

Response to Reviewers' comments

The lines indicated in the response refer to the final version of the manuscript without trackchanges.

Reviewer #1

In this paper, the authors build a physical model in order to quantify sediment compaction in the San Felice marshland, Venice Lagoon. The shallow compaction of marshland and accelerated rate of sea-level rise will exacerbate risks of erosion and flooding of low elevation coastal zones. However, there are very limited physical models of sediment compaction, mostly due to the difficulty to acquire model's parameters and accurate measurements of compaction for validation. This paper presents an exciting model, that can be widely used in marshlands when e.g., incorporating sediment consolidations in projection of relative sea-level rise. Therefore, this study has the potential of making a major impact on risk assessment of marshlands. I recommend reconsidering the manuscript for publication after conducting major revisions.

My first main comment concerns the lack of clarity of the writing. The text is difficult for a reader with a general geoscience background to understand and lacks minimum explanations of monitoring techniques and modeling approach. The paper is written well in "Introduction", the writing breaks down at page 4 "Contribution of below-ground deformation to net accretion rates" and at Figure 3. I took a full day reading this but I never reached a full understanding. The authors use too many acronyms and abbreviations that make the text hard to follow.

Generally speaking, the manuscript has been completely re-word in section "*Contribution of below-ground deformation to net accretion rates"*. Note that the section title has been changed to "*Salt marsh vertical dynamics: contribution of below-ground deformations and accretion rates*" according to the comment of reviewer #3. A new section "*Measurements dataset at the San Felice salt marsh*" has been added to better describe the monitoring techniques. The modelling approach has been made clearer by adding details on its implementation in the main text. The number of acronyms has been reduced but we kept them to describe the various instruments/sensors.

The author uses a current of rate 1.2 mm/yr at a tide gauge in the northern Adriatic Sea for their relative sea-level projections which fully disregards accelerating sea-level rise associated with different global warming scenarios. I suggest the authors consult the literature that focuses on sea-level projections using tide gauge data (see e.g., Kopp et al. 2014; Jevrejeva et al. 2016). Kopp et al. (2014) provides valuable sea-level projections under three different RCP scenarios for a global set of tide gauge records.

A new paragraph has been added to the main text (lines 155-165) to deal with projection rates by different global warming scenarios. In particular we accounted for the global mean sea level projections under the Representative Concentration Pathway RCP2.6 and RCP8.5 scenarios, with a median sea level rise of 440 mm and 840 mm expected by 2100. We estimate the salt marsh loss in vertical land elevation. However, in the case of the Norther Adriatic Sea, arm of the Mediterranean Sea, projections need to consider the particular configuration as semi-closed basin, exchanging water with the Atlantic Ocean through only the Gibraltar Strait. Also, NAO induced changes in atmospheric pressure, temperatures, and salinity variation, may provide regional sea level trends different from the global mean sea level projections. Nowadays these projections are still uncertain. Moreover, a second important factor is the relationship between mean sea level rise and accretion rates. Indeed, the deposition dynamics over a marsh surface is strictly related to the relative elevation of the marsh surface with respect to the water level. For high rates of sea level rise, a more complex model is needed including the dynamics of the surface processes (e.g., Marani et al., 2007).

Marani, M., D'Alpaos, A., Lanzoni, S., Carniello, L. & Rinaldo, A. Biologically-controlled multiple equilibria of tidal landforms and the fate of the Venice Lagoon. Geophys. Res. Lett.34, 1–5, DOI: 10.1029/2007GL030178 (2007).

The authors distinguish between displacement and deformation, as presented I foresee confusion. The levelling technique and artificial radar reflectors, both allow measuring vertical displacement (roughly normal to a reference datum). Are the InSAR rates in "line of sight" direction or locally tied to GPS time series in a geodetic reference frame? This is not clear in the text and needs more explanation.

Distinction between displacement and deformations has been made clear by adding a completely new section on available observations, including a full explanation of the variables provided by each monitoring technique. Displacement refers to absolute displacement whereas deformation is the relative displacement between two points in the space. This is provided at lines 67 and 78 and depicted in Fig. 3A. Since SAR interferometry provides land displacements along the line-of-sight (LOS), the CGPS and SAR motions must be referred to the same coordinate system. Thus, we have projected the three dimensional CGPS vectors (i.e. local east, north, and vertical motion used to calibrate the SAR solution) in the SAR LOS. Notice that Venice coastland is moving in the horizontal direction almost uniformly (18-20 mm/yr northward and 15-17 mm/yr eastward with reference to IGS08) and therefore the local east and north components of the CGPS are much smaller than the local vertical component referred to the same datum. Moreover, the difference in the CGPS local vertical displacement projected along the LOS is very small (less than 1 mm/yr thus almost negligible). We provide further information at lines 66-70.

Section 2 "contribution of below-ground deformation to net accretion rates" doesn't read well. It's difficult to find connections between sentences, and the reader becomes confused how these rates are calculated. I suggest moving "Subsidence and accretion dataset at San Felice marshland" section from "Material and Methods" to the main text, incorporating this part into section 2 and rephrasing this section.

The authors fully take this suggestion from the reviewer. We introduced a new section on the monitoring techniques explaining measurement type, number of locations, depth, etc. Table 1 of SI appendix is also introduced here and modified to include more information. The section on interpretation of data is also completely re-word to make it clear to readers. To do this, Figures 3 and 4 have been completely revised. Figure 3 only shows the available data, whereas Figure 4 provides the interpretation of them in terms of vertical dynamics of the salt marsh.

A uniform deep subsidence rate of 1 mm/yr is assumed for the projection task but its origins are not discussed in the manuscript. Deep subsidence could be related to several shallow and crustal processes varying in space and time.

Deep subsidence related to long-term geological processes (millions to thousand years) is considered time-independent for the projection purpose (80 years). We used the regional pattern of land vertical movement as derived by Carminati and Di Donato (1999) for the norther Po plain area. We added the following reference to the manuscript:

Carminati, E., Di Donato, G.,1999. Separating natural and anthropogenic vertical movements in fast subsiding areas: the Po plain (N. Italy) case. Geophys. Res. Lett. 26 (15),2291–2294.

It's not clear throughout the text that what InSAR rates exactly represent. Are they measures of surface elevation change and/or vertical land motion? In Figure 2, deep-grounded reflectors facilitate the measure of vertical land motion, but for natural reflectors InSAR most likely represents surface elevation change since sediment accumulations may change natural reflectors. The authors need to clarify these confusing points.

We totally agree on this comment. A clear explanation of what each single monitoring sensor is measuring is given in Table 1, Figures 3A and B, and in the main text of section "*Measurements dataset at the San Felice salt marsh*". As mentioned by the reviewer, deep-grounded reflectors measure of vertical land motion (VD) and for natural reflectors InSAR represents the surface elevation change (SEC).

Figure 1: The InSAR map (Fig1B) shows many pixels with positive rates. Can authors explain these rates? For natural reflectors positive rates could be due to sediment accretion. But how this can be described if reflectors come to be artificial ones being grounded in depths (if there exist positive rates associated with these reflectors?). Also the large limit of color bar doesn't allow to easily distinguish positive and negative rates. I suggest the authors adding a (histogram) plot showing distribution of InsAR rates for San Felice Island next to Figure 1B.

Fig. 1 illustrates the ground movements of the Venice coast basing on Persistent Scatterer Interferometry technique. Focusing on San Felice salt marsh, some pixels show positive rates, mainly located close to the creeks, that are likely due to local accretion process controlled by the settling out of sediments in suspension originated by erosion of the close salt marsh edges and tidal flats (Da Lio et al., 2018). Positive rates associated with artificial reflectors grounded in depths are not detected in this study (see new Table 1).
The colour bar has been changed to allow better distinction of positive and negative values. A histogram has been also added to Fig. 1 to show the distribution of the InSAR rate.

Da Lio, C., Teatini, P., Strozzi, T. & Tosi, L. Understanding land subsidence in salt marshes of the venice lagoon from261SAR interferometry and ground-based investigations.Remote. Sens. Environ.205, 56 – 70, DOI: https://doi.org/10.1016/j.262rse.2017.11.016 (2018).

Figure 2 is very useful and informative as it nicely illustrates time spans of each monitoring techniques and their foundation depths. However, it would be more useful for the reader if the authors show geographic locations of these instruments, so the reader could get an overview of spatial distributions of monitoring sites. I suggest adding these sites to Figure 1B.

We agree with the reviewer on the necessity of introducing the geographic locations of the instruments. The sites are added to Fig. 2 (top-right subpanel). We prefer Fig. 2 instead of Fig. 1B for convenience on description purposes (description of instruments is after reference to Fig.1).

Figure 3 is not very informative. It shows too many details. What do the vertical lines represent? I suggest the authors using a simpler illustration to spell out the message (Fig 3 is barely touched on in the text!). For instance, a scatter plot with rates from each technique versus depths could be an alternative display.

Figure 3 has been completely rethought according to this suggestion. Panel A shows the depth of the instruments and the type of measurement they provide (absolute vertical displacements, deformations, surface elevation change, accretion). Panel B shows the scatter plots with rates from each technique versus depth, including their uncertainty. The plots also provide information on terminology used for positive and negative values of each measurement type, e.g., positive vertical displacements refer to land uplift whereas negative values refer to land subsidence.

Table 1:
"deformation and displacement" – Again please be careful with terminology. What is the difference? Both refer to the same quantity: changes in vertical distance along the line from the Earth's surface to the reference datum (geoid or ellipsoid).
Please report rates (mm/yr) and their uncertainty instead of standard error of displacement, and specify the range of TCR rates in the text (Ln 55)

As mentioned before, the clarification on definition of displacements and deformations has been provided in the new section on available observations in the San Felice site. The average values of the rates and their uncertainty have been reported in Table 1.

The authors should add a map or at least info showing the thickness of Holocene strata for their study area. This info should be presented in the text explaining the shallow measurement as a function of depth in the second section "Contribution of below-ground deformation to net accretion rates". For example, in Ln 56-57, thickness of Holocene sediment is quite relevant and is a main factor for understanding and modeling shallow compaction.

We address this point by adding info on the thickness of the Holocene strata in the study area. In particular, we refer to previous works by Tosi et al. 2007 and 2009, where a map of Holocene thickness for the Venice Lagoon is obtained by combining data from stratigraphic, sedimentological, and radiochronological cores with high resolution seismic surveys. In the study area, the Holocene deposits are about 10 m thick. However, for the purpose of this analysis we consider only depths in the range 0-4 m (see lines 105-108).

Tosi, L. et al. Note illustrative della carta geologica d'italia alla scala 1: 50.000 foglio 128 venezia. APAT - Dipartimento287Difesa del Suolo, Servizio Geologico d'Italia (2007).

Tosi, L., Teatini, P., Carbognin, L. & Brancolini, G. Using high resolution data to reveal depth-dependent mechanisms that285drive land subsidence: The Venice coast, Italy.Tectonophysics474, 271–284, DOI: 10.1016/j.tecto.2009.02.026 (2009).

Minor comments:

Ln 17: Please leave out the geographic coordinates of study area.

OK, done.

Ln 27: "with average values of 3–4, 1–2 and 2–3 mm/year in the northern, central and southern portions..." >> with average values of 3–4, 1–2 and 2–3 mm/year, respectively, in the northern, central and southern portions…

OK, done.

Ln 32-33: "Usually, formation and evolution of salt-marshes are studied with models that does not explicitly account for consolidation of the marsh sediments." Can authors give some reasons and add relevant references here?

The relevant references are cited in the text, e.g., Marani et al., 2010, Da Lio et al., 2013. Here we stated that the 'surface' models do not explicitly account for the consolidation because until now coupled models of consolidation and accretion were not available. Subsidence rates are usually introduced with constant values or through empirical relationships.

It hasn't explained anywhere within the text what SE-SET and MO-SET measurements are and what SE and MO stand for!

OK, done. Line 79.

Ln 50: Please clarify in one sentence here which projection scenarios are used for MSL rise and sediments rate?

OK, done.

Ln 51: change "by the end of the century" to "2100"

OK, done.

Ln 143: "in particular the 1m-top soil where major consolidation takes place". Can you cite some relevant works here? Or I'd suggest changing this to "in particular within top several meters of soil where major consolidation takes place".
OK, agree.

The author mention use of GPS time series in the Acknowledgment. Do I miss something here? I couldn't find any GPS results in the manuscript!
We added in the manuscript the use of GPS time series to calibrate the SAR solution.

Please explain σz,0 in the formula given in the Supp.
OK, done.

Reviewer #2

This is a potentially very interesting paper, unfortunately it contains so many flaws that the results are extremely difficult to understand.

The objectives of the work are not clearly stated, so the reader does not know what to expect. Does the paper focus on the formulation of a new model for salt marsh soil compaction/deformation? If the answer to this question is yes, then I am afraid but this work does not sufficiently describe the model characteristics and how the parameters were determined. In case this work makes use of a model already formulated/validated and applied to salt marshes, then it should be clarified in the text, citing the source, and the authors should specify the objective of this new application/research in the Introduction.

The main objective of this work is to investigate how the Holocene strata compaction influences the current and future surface elevation change (lines 26-28). We aimed to do so by integrating available data at the San Felice salt marsh and a proper geomechanical framework, which consider the potentially highly compressible sediments characterizing these landforms. The model approach is not new and originally published in *Zoccarato and Teatini (2017).* Later the model was successfully applied to real-world cases in the Mekong delta (Zoccarato et al., 2018) and in the Mississippi river wetlands (Zoccarato et al., 2020). Specific applications to salt marshes in the Venice Lagoon can be found in Zoccarato et al., 2019 where the coupled dynamics between surface and subsurface processes is implemented. The model is here calibrated for the specific characteristic of the San Felice salt marsh using available hydro-geomechanical properties of the sediments. This is generally stated at lines 49-56: *We propose a first-of-its-kind dataset interpretation with a physically-based model of soil consolidation originally developed by Zoccarato and Teatini (2017). The elevation dynamics of the San Felice-natural salt marsh is accurately described taking advantages of available observations obtained with a comprehensive monitoring network of subsidence, accretion at different spatio-temporal scales. The model is calibrated using data collected in the period 1993-2013 through a network of instruments and sensors deployed on the salt marsh.*
We provide more detailed information about calibration of the model results at lines 133-139.

As for the message, the paper does not deliver it in a clear and sounded way. My impression is that the "Materials and Methods" section was originally thought to be included in the main part of the paper, and then, since Nature Communications requires the Methods section to be at the end, it was simply moved. This is not acceptable. The main part of the paper must be clear and self-sustaining without the need for the reader to jump to the Methods section in order to understand what she/he is reading. The same concept applies to "Supporting Information" that should in fact be "of support", not essential to understand the results presented by the authors, as in this manuscript.

The paper was originally thought in the present form since the beginning of its first draft. However, we understand that it is difficult to appreciate the model approach by simply reading the main text without the Material and Methods section. For this reason, we added some more details on the geomechanical model approach in the main part of the manuscript (lines 123-129) to support readers with a general geoscience background. Full description of the model with a comprehensive sensitivity analysis can be found in Zoccarato and Teatini, 2017.

Zoccarato & Teatini. Numerical simulations of Holocene salt-marsh dynamics under the hypothesis of large soil deformations. Ad. Water Res.110, 107–119, DOI: 10.1016/j.advwatres.2017.10.006 (2017)

I believe that the data set presented in this work is of great value, but unfortunately, it is poorly described: no details about the acquisition of measurements are included, no descriptions of the sensors or methodologies are listed. The authors do not even explain how they calculated the uncertainties associated with the measurements performed with different sensors/methods.

We introduced a new section on the monitoring techniques explaining measurement type, number of locations, depth, etc. Table 1 of SI appendix is also introduced here. Figure 3 has been completely rethought to this purpose. It shows a cartoon of the available instruments on the field site and the measured rates (mm/year) with their associated uncertainty as standard deviation to the mean. In this section we provide a brief description of each monitoring technique, including how these rates are calculated.

I suggest the authors to completely revise the structure of the manuscript, clearly listing the objectives of their work, and describing their findings for all readers – not just for experts or those who have gone through the Methods section at the end of the paper. I also suggest to better describe the data set and the methods used to collect the measurements with each instrument, including a detailed description of the calculations applied in order to quantify the displacements for each data set.

We took this suggestion from the reviewer and the manuscript has been substantially changed to make the text clearer to convey the message to a broader audience. According to comments and suggestions from the three reviewers of this paper, we generally provide substantial changes to the following issues:

- description of the dataset and the methods to collect the measurements (a new section is added in the main text for this purpose)
- description of the calculation employed to quantify displacements (see lines 108-114)
- description of the numerical model in a simpler way, not just for experts (lines 123-129).

Finally, some paragraphs are very difficult to read/understand: ***a thorough revision of English language is needed****.*

The manuscript has been revised by an English Language Editing through the Nature Research Editing Service but notice that we did not keep track changes of these corrections to maintain a readable manuscript.

I have ***several more questions/comments/corrections****, please see below:*

Abstract: the questions included in the Abstract are not pertinent: this work does not deal with vegetation processes, nor with organic matter deposition - at least not explicitly. Moreover, no erosion processes are accounted for (lateral or surficial). Please include only questions that deal with the actual objectives of this work.

We agree on the fact that we did not model superficial process as coupled dynamics between inorganic sedimentation and organic matter decomposition (e.g., Marani et al., 2007). However, the sedimentation rates we use in model can be though as values that are the sum of the two processes and the hydro-geomechanical properties used to characterize the sediments are available from lab compression tests on surficial sediments (first top meter).

We have been adjusted the abstract according to the reviewer comment and re-worded according to the limit of maximum of 150 words.

Marani, M., D'Alpaos, A., Lanzoni, S., Carniello, L. & Rinaldo, A. The importance of being coupled: Stable states and catastrophic shifts in tidal biomorphodynamics. J. Geophys. Res. - Earth Surf.115, 1–15, DOI: 10.1029/2009JF001600301, (2010)

Abstract: "Elevation changes of a tidal platform strongly…" "…sea-level rise (SLR) is highly dependent…"
OK, done.
Lines 13-14: "…the delicate equilibrium that these complex systems provide to wildlife and plants."
OK, done.
Line 15: "…of the world … perspective that implies a deep understanding…"
OK, done.
Fig. 1 : change to "northern".
OK, done.
What do you mean with "specific behavior"? How did you calculate the SEC values? If such values are taken form others, please specify and describe how they were obtained.
In the section on dataset this is stresses and made clearer.
Fig. 2: change to "… over the time interval …". In the caption there is FMH, in the figure it is FHM.
OK, done.
Line 17: please make a choice about the way you call salt marshes (not salt-marsh) or marshlands. The use of both terms throughout the paper is misleading.
Thanks, term "salt marshes" is kept in the manuscript.
Lines 18-19: "… representative of microtidal marsh environments. The main focus of this research is to investigate how the …"
OK, done.
Line 21: delete "seasons'
OK, done.
Lines 21-25: This is incorrect. Venetian salt marshes are flooded also in Spring and Summer, almost every day, during regular high tides – even the highest portions are flooded every month of the year during spring tides.
In this paragraph we meant to bring to the reader attention the Aqua Alta (high tide) phenomenon that is the exceptional high tide (defined as greater than 110 cm above mean sea level) usually occurring during autumn and winter under specific meteorological conditions which hamper the astronomical tide. We specified in the text the definition of 'Aqua Alta'.
Line 29: "The vertical displacement of natural landforms … by the availability …"
OK, done.
Line 30: " … contributing to deposition, the consolidation … self-weight, the erosion …"
OK, done.
Line 31: "external forcings …"
OK, done.
Lines 31-32 : here it is not clear if RSLR denotes the sum of all the factors mentioned in this paragraph: please specify or rephrase.
We eliminate the previous definition at line 33 and we introduce the 'correct' definition at (relative sea-level rise = deep (regional) subsidence + shallow subsidence (0-4m) + sea level rise).
Line 32: "… that do …"
OK, done.
Line 33: "Conversely, this process…"
OK, done.
Line 34: delete "However"
OK, done.

Lines 35-36: "… may be critical to accurately quantify the dynamics of the surface elevation at centennial to millenial time scale, allowing …"
OK, done.
Line 37: Delete "Moreover" and put "has been proven significant"
OK, done.
Line 38: "… providing valuable insights …"
OK, done.
Line 39: "… driving the resilience of marshes to SLR".
OK, done.
Line 42 "…observations obtained with a …"
OK, done.
Lines 43-44: "The model is calibrated using data collected in the period 1993-2013 through a network of instruments and sensors deployed on the marshland (Fig. 2), even tough …"
OK, done.
Line 45: please add a map that includes the actual location and distribution of all these instruments across the San Felice salt marsh.
The location of the instruments and sensors is provided in the new panel of Figure 2.
In Fig. 2, TCRs go down to three different depths. Please list here the three depth values, and describe how many TCRs belong to each depth value. How many TCRs are located over the San Felice salt marsh?
The depth values of the TCRs and their number is provided in Table 1, which has been moved from supplementary material to the main text and modified to include more information. In the table is clear the number of available TCRs for each depth over the San Felice salt marsh.
Line 46: Please, briefly describe what is a SET. Moreover, specify their characteristics in terms of depths, number, location, etc. Do the same for all the instruments.
This is done in the new section "Measurement dataset at the San Felice salt marsh", where each method/sensor is described. Depths, numbers, locations are provided in Table 1 and Fig. 2.
Line 47: specify the type of the natural reflectors. How many were used? what are they?
This is done in the new section "Measurement dataset at the San Felice salt marsh".
Line 49: "further details on the measurement dataset are available in Materials and Methods section"
This sentence has been removed from the manuscript.
Line 52: This chapter is poorly organized, and it does not make sense. You need to include a list of variables (i.e. VD, DE, etc.) that are estimated using the different instruments/ sensors, and describe how they are calculated. I understand that a detailed description is included in the Methods section, however the reader should not be required to jump to the Methods section in order to understand what you have done. A Table might help.
As mentioned before, the description of the different type of measurements provided by each monitoring technique is given in "Measurement dataset at the San Felice salt marsh". The variables (VD, DE, SEC, AC) are described in the text and depicted in Figure 3A. Details on their calculation are included. The description given in Methods section has been removed. Table 1 is also introduced to help the reader with a summary of available information. Moreover, Figure 3B (new) shows the scatter plots with rates from each technique versus depth, including their uncertainty. The plots also provide information on terminology used for positive and negative values of each measurement type, e.g., positive vertical displacements refer to land uplift whereas negative values refer to land settlement.
Line 54: Do you refer to TCR measurements only? please specify
We refer to all available measurements. The text has been changed to add more details on data used to calculate the vertical dynamics of the salt marsh from available observations. Lines …
Lines 58-59: I suggest to use + for accretion and - for negative displacement values, and please make this clear in the text.
OK, we take this suggestion. We highlight terminology for positive and negative values of each type of variable in the text (lines 66-87) and in Figure 3B.
Line 59: It is not clear to me why you start this paragraph saying that the "Holocene compaction" refers to 0 to -4m and then you calculate VD averaging all the values coming from TCRs, HPL, MO_SET and SE_SET that go down to -8m. Could you please explain?

The way we compute the vertical land movement from marsh surface down to 4m is now explained in more detail in the section "Salt marsh vertical dynamics: contribution of below-ground deformations and accretion rates". The two profiles of the below-ground vertical dynamics provided in Figures 4A and 4B are obtained using data from the 4m-depth TCR, the HPL, the SE-SET (only Figure 4A) and the MO-SET (only Figure 4B). For the purpose of this analysis, we considered only depths in the range of 0 to 4 m rather than the all Holocene thickness because we consider a negligible compaction of the layer between 4 and 8 m. Since the 4 m-depth TCR quantifies deep (regional) subsidence values, the deformation measured by the HPL between 8 m and 2.4 m is entirely attributed to the layer between 4 m and 2.4 m.

To obtained the vertical displacement of the land surface ($VD\_{surface}$) the following relationship is applied $VD\_{surface} = VD\_{TCR(4m)} + DE\_{HPL(4\text{-}2.4m)} + DE\_{SE\text{-}SET(2\text{-}0m)}$ for Figure 4A, whereas $VD\_{surface} = VD\_{TCR(4m)} + DE\_{HPL(4\text{-}2.4m)} + DE\_{HPL(2.4\text{-}0.5m)} + DE\_{MO\text{-}SET(0.5\text{-}0m)}$ holds true for Figure 4B. Assuming deformation uniformly distributed within each monitored depth interval, the displacements change linearly. By using the profiles of Figures 4A and 4B, it results in $VD\_{surface}$ = -2.4 mm/year and $VD\_{surface}$ = -2.1 mm/year, respectively. Employing data from FMH, with accretion rates of the order of 3 mm/year, it means that land uplift less than 1 mm/year is found for this area, confirming the analysis obtained with Persistent Scatterer Interferometry on natural PTs.

All this information are available at lines 100-117.

Lines 60-66: I am sorry but I cannot follow this description. Please rephrase. Please include comments to Fig. 3 so that it will be easier to visualize the results that you are describing. The way the results are presented and explained is very confusing.

Figure 3 has been completely rethought: it is now showing a cartoon of the monitoring network installation depth (with measurement type) and data (rates and associated uncertainties). Description of this figure is given at lines 88-97. The previous panels of Figure 3 are now shown in Figure 4, which is described in section "Salt marsh vertical dynamics: contribution of below-ground deformations and accretion rates".

Fig. 3 : I think this figure is extremely difficult to interpret. I suggest rethinking it completely. In the new figure, please include the error bars. Why there are three panels? Are the first two panels (above) referred to two different data sets or two different locations? Is the third panel (below) a sort of summary? Please add the unit of the y axis of the third panel. In the caption use "accretion" instead of "expansion" and "… which is in agreement with the PT observation."

We have changed this figure completely (see previous comment) as suggested. The panels of Figures 4A and 4B refer to different dataset (one uses TCR, HPL, SET-SET and the other uses TCR, HPL, MO-SET). This is now clear in the text at line 109. The unit of the y axis has been added in Figure 3A.

Line 79: Have you defined ω? I see the definition in the Methods section, but you must define all the variables before you mention them.

The variable ω was defined in the main text "ω (Fig. 4, top subpanel), which defines the *accretion rate function* sampled from a log-Normal distribution".

However, there is no conceptual difference between "accretion rate (AC)" and "sedimentation rate (ω)", thus we substitute the occurrences of *sedimentation rate* with *accretion rate,* as it was also suggested by reviewer #3.

Line 90: "… 5mm/year respectively"

OK, done.

Line 91: delete actual and use "current'

OK, done.

Line 93: have you defined δ?

δ is defined in the caption of Table 2 (previous table 1). In the main text we avoid to use δ but it clear from the text the reference to the table.

Line 102: "threatened"

OK, done.

Lines 103-111: these sentences seem to say that the only input of sediments available for the salt marshes comes from the sea. I doubt so, and I suggest the authors to verify and rephrase or bring evidence that support this hypothesis. Even though the Venice lagoon is known to lack sediment inputs, resuspension of sediments from channels and tidal flats is very high, increasing suspended sediments that are then deposited on marshes. This has nothing to do with the MOSE system.

We agree on this comment. The scenario we formulate is certainly a pessimistic scenario assuming that any sedimentation (inorganic and organic) is prevented. We add a paragraph in the discussion section (lines 176-189) to better explain our hypothesis and underlying the fact that input of sediments in the lagoon is also available from resuspension of sediments and from organic matter decomposition.

Line 115: "source" of what?

We eliminate 'source'

Line 123: "Our findings show that salt marsh CO2 storage capacity must account for the compaction…"

OK, done.

Line 125: "… highly impact the San Felice marshland resilience if sedimentation is maintained…"

OK, done.

Line 128: "Our results confirm previous …"

OK, done.

Line 140: "…provided by biomass…"

OK, done.

Line 157 : "At the San Felice …." "… displacement and deformation measures, and specifically deformation (DE) is … and 0.5 m, while vertical …"

OK, done.

Supporting Info: I suggest to move the content of the Supporting Info to the Materials and Methods section because it is important to understand the results.

We do not move all the content of Supporting info into the Material and Methods section. Table 1 has been moved in the main text. The information in the Supporting Info can help to replicate results and better get into the model details.

Table 1: "Details of measured deformations, …" Where does the standard error come from? No description in the main text.

We meant standard deviation associated to the mean value.

Please add a description Table 2: please specify what type of sediments you are considering. I think that the listed parameters, as for example the specific weight, correspond to sand. However, it would be good to show results also for finer sediments, silty and clayey sediments, since salt marshes usually have sediments finer than send. If this is exclusively referred to the specific studied salt marsh, please say so. Moreover, what would be the results for sediments rich in organic matter? Salt marsh soils tend to have organic matter content higher than 20%. What would be the behavior in this case?

We add a description in the main text to specify the type of sediments we are considering in this contribution. We consider only one sediment type because few hydro-geomechanical properties are available for sediments generally characterized by high porosity and low density. Indeed, the sediments that usually characterize these landforms are difficult to sample for lab-compression tests. We underline that the sediment characterization does not distinguish between inorganic or organic material but consider average values obtained by lab tests. However, we also stress the fact that the geomechanical properties vary with the effective stress, thus with the depth (i.e., sediments become stiffer after consolidation). Obviously, if the organic content would change, the properties will also change and the marsh behaviour will be different. Some attempts to characterize highly-rich organic sediments with in-situ field tests are ongoing in this area (Teatini et al., 2020).

P. Teatini et al., Characterizing marshland compressibility by an in-situ loading test: design and set-up of an experiment in the Venice Lagoon. In: Living with Subsidence - Proc. X Int. Symp. on Land Subsidence, P. Fokker and G. Erkens eds., Proc. IAHS, 382, 345-351, https://doi.org/10.5194/piahs-382-345-2020, 2020.

Hydro-geomechanical properties and model set-up it is not clear to me if this model has been already published in a previous paper or not. If the answer is yes, the paper should be cited and a discussion about the uncertainty should be included. In case the answer is no, then a *comprehensive work on calibration/validation of the model must be included in order to show its feasibility.*

The main contribution to the modelling approach used in this study to calculate consolidation in a large deformation formulation can be found in the following papers:

- Gambolati, G., Giunta, G. & Teatini, P. Numerical modeling of natural land subsidence over sedimentary basins undergoing large compaction. In Gambolati, G. (ed.) CENAS - Coastiline evolution of the Upper Adriatic Sea due to sea level rise and natural and anthropogenic land subsidence, no. 28 in Water Science and Technology Library, 77–102 (Klwer Academic Publ., 1998).
- Zoccarato, C. & Teatini, P. Numerical simulations of Holocene salt-marsh dynamics under the hypothesis of large soil deformations. Ad. Water Res. 110, 107–119, DOI: 10.1016/j.advwatres.2017.10.006 (2017).

Later the model was successfully applied to real-world cases in the Mekong delta (Zoccarato et al., 2018) and in the Mississippi river wetlands (Zoccarato et al., 2020). Specific applications to salt marshes in the Venice Lagoon can be found in Zoccarato et al., 2019 where the coupled dynamics between surface and subsurface processes is implemented.

In this contribution the model is calibrated with available data from the instruments/sensors at the San Felice salt marsh. The model has been run with 100 different values of accretion rate (Fig. 4C) and the best-fitting displacement curve is selected according to measurements (Fig. 4A-B). The uncertainty propagation from model input to model output is provided by the Monte Carlo simulation (Fig. 4C).

Reviewer #3

Review of "The Holocene imprint on future evolution of tidal marshes" by Zoccarato & Da Lio (CEE-20-0086)

Zoccarato & Da Lio present a tidal marsh model, constrained by a fairly unique set of field data, that explicitly incorporates subsurface processes (sediment compaction) with the ultimate goal to predict future marsh vulnerability within the context of climate change. There are several widely used marsh models in the literature, but as the authors point out (lines 32-34), very few (if any) fully capture subsurface processes. The authors correctly stress the fact that this is of particular importance when considering longer timescales, an issue that is often overlooked in the marsh literature. As such, this is a welcome contribution that builds on many years of geotechnical modeling advances within the Padua group, such as the adaptive mesh approach published by the lead author in 2017. The present study is tied to a range of monitoring techniques in the Venice Lagoon that complement one another and serve to capture subsidence processes in a depth-integrated fashion.
I am particularly pleased to see that these measurements are all carried out with respect to well-defined reference points (anywhere between the wetland surface and a given depth in the subsurface), something that is too often neglected. There are very few marshes worldwide that have seen this type of monitoring, placing this study at the forefront of the field. Nevertheless, I do see ample opportunities to further improve this work, especially in terms of presentation. My main concerns revolve around the treatment of marsh accretion and sea-level projections, along with the use of terminology and some documentation issues. Please note that I am not a modeler and my expertise in the geotechnical aspects of the work is limited; I assume others can comment in more depth on those elements.

While the primary strength of this study is the dynamic incorporation of sediment compaction, a limitation is the fact that marsh accretion is simulated as a constant value, despite the nonlinear feedbacks that exist between RSLR, sediment accretion, and biogenic production (see Morris et al., 2002 and many subsequent studies, including D'Alpaos et al., 2012). The authors are aware of this (lines 131-132, 138-139) but it may be advisable to be a little more upfront about it by pointing out this caveat at the outset.

We agree with the reviewer on the fact that strong non-linear dynamics characterize these complex transitional systems. We are aware of the works by Morris et al., 2002 and D'Alpaos et al., 2012, and a recent paper has been also published on the coupled surficial-subsurficial dynamics (and its effect) in salt marshes (see Zoccarato et al., 2019). However, in this contribution we mainly focus on the impact of the Holocene sediments on the resilience of salt marshes by 'simplifying' the surficial models and taking a constant accretion rate value as input to the geomechanical model. Further developments are certainly encouraged by implementing more complex models. However, model calibration will become more difficult due to the increased number of model parameters to be tuned. We have further underlined this limitation in the discussion (lines 186-189).

Equally important is the nature of the material that accumulates on the marsh surface. Is this clastic, organic, or a mix of both? One would expect that this plays a large role in dictating the effective stress and the resulting amount of compaction in the three scenarios. I partly ask this because of what is stated in line 110: closure of the MOSE system would reduce the sediment flux to zero (which I take to be a hypothetical scenario, because I assume there are no plans for permanent closure of the barriers). This would imply that there is no organic contribution and that all loading is due to clastic material? The statement in line 140, as well as the information in Table 2 in the SI, suggest that the present study did not consider organic matter. This needs to be mentioned more explicitly, because it constitutes a significant limitation in the model predictions.
We add a description in the main text to specify the type of sediments we are considering in this contribution (lines 176-186). We consider only one sediment type because few hydro-geomechanical properties are available for sediments generally characterized by high porosity and low density. Indeed, the sediments that usually characterize these landforms are difficult to sample for lab-compression tests. We underline that the sediment characterization does not distinguish between inorganic or organic material but consider average values obtained by lab tests. However, we also stress the fact that the geomechanical properties vary with the effective stress, thus with the depth (i.e., sediments become stiffer after consolidation). Obviously, if the organic content would change, the properties will also change and the marsh behaviour will be different. Some attempts to characterize highly-rich organic sediments with in-situ field tests are ongoing in this area (Teatini et al., 2020).

The definition of RSLR (lines 31-32) needs some thought. The authors mention "increasing MSL and deep subsidence," but how about shallow subsidence? Note that Jankowski et al. explicitly included all three components in their definition of RSLR. Elsewhere (lines 54-56) the authors specifically mention the 0 to 4 m depth range as the one where shallow compaction operates, strikingly similar to the 0 to 5 m depth range as identified by recent work in coastal Louisiana (Keogh & Törnqvist, 2019, Ocean Sci). In other words, they may want to define RSLR as encompassing all subsidence components, not just the deep component, or they may end up excluding the largest player. To be clear, I don't mean to prescribe this, but if the authors choose a different approach they need to be very explicit about this and provide a precise definition of what exactly the deep subsidence component constitutes (the Pleistocene surface and deeper? or strata deeper than 8 m, the maximum anchor depth in the cartoon in Fig. 2?).

We understand the point of the reviewer and welcome the suggestion to keep the terminology in line with previous works, e.g., Jankowski et al. 2017. In order to accomplish with this, we eliminate the previous definition at line 33 and we introduce the 'correct' definition at lines 39-40 (relative sea-level rise = deep (regional) subsidence + shallow subsidence (0-4m) + sea level rise). A total number of 5 occurrences of the acronym RSLR has been detected in the manuscript, thus we preferred to spell out the term. At line 33, we also provide definition of what is meant for deep subsidence (the compaction-free Pleistocene basement).

I was puzzled about the sea-level scenarios of 1.2 mm/yr for the remainder of this century. Note that this number is now widely considered to be the global average for the 20th century (recent studies by Hay, Dangendorf, and colleagues); the present-day rate is well over 3 mm/yr (WCRP Global Sea Level Budget Group, Earth Syst Sci Data, 2018), and future rates are almost certain to be higher. Are the conditions in the Adriatic expected to be that much different? This would need to *be justified. A more conventional approach might be to use global projections from the 2019 IPCC-SROCC report, assuming, of course, that those numbers are appropriate for the Adriatic/Venice Lagoon. It seems plausible that using larger (and more realistic) numbers for future geocentric sea-level rise will increase marsh vulnerability.*

A new paragraph has been added to the main text (lines 155-165) to deal with projection rates by different global warming scenarios. In particular we accounted for the global mean sea level projections under the Representative Concentration Pathway RCP2.6 and RCP8.5 scenarios, with a median sea level rise of 440 mm and 840 mm expected by 2100. We estimate the salt marsh loss in vertical land elevation. However, in the case of the Norther Adriatic Sea, arm of the Mediterranean Sea, projections need to consider the particular configuration as semi-closed basin, exchanging water with the Atlantic Ocean through only the Gibraltar Strait. Also, NAO induced changes in atmospheric pressure, temperatures, and salinity variation, may provide regional sea level trends different from the global mean sea level projections. Nowadays these projections are still uncertain. Moreover, a second important factor is the relationship between mean sea level rise and accretion rates. Indeed, the deposition dynamics over a marsh surface is strictly related to the relative elevation of the marsh surface with respect to the water level. For high rates of sea level rise, a more complex model is needed including the dynamics of the surface processes (e.g., Marani et al., 2007).

Marani, M., D'Alpaos, A., Lanzoni, S., Carniello, L. & Rinaldo, A. Biologically-controlled multiple equilibria of tidal landforms and the fate of the Venice Lagoon. Geophys. Res. Lett.34, 1–5, DOI: 10.1029/2007GL030178 (2007).

The manuscript contains tons of acronyms, several of which are not widely used in the literature and some are not intuitive. For example, artificial radar corner reflectors are referred to as TCRs. It would be helpful to limit acronyms wherever possible and to spell out those terms that are only used a few times in the manuscript. In the figures, please avoid acronyms when you can, or at least ensure that they are all explained in legends or captions, in a systematic fashion. For example, the authors mention PSI measurements in Fig. 1, but PSI does not feature in Fig. 2. I found myself constantly having to look up what the acronyms stand for; but even then, the associated terminology is a bit confusing.
For example, the first equation in the Materials and Methods indicates that vertical displacement (VD) is equivalent to shallow subsidence as defined in the seminal paper by Cahoon et al. (1995, Mar Geol), but in Fig. 3 the impression is given that this is represented by deformation (DE). I got lost here. Also, please explain SE-SET and MO-SET; these are apparently two different varieties of surface elevation tables that I was not familiar with (the cartoon in Fig. 2 suggests that the difference relates to the length of the rod?). And what is the difference between "accretion rate (AC)" and "sedimentation rate (ω)?" More broadly, the terminology needs attention. For example, one of the opening sentences in the Materials and Methods section mentions "displacements and deformations," but I don't think it is clear to all readers that these are associated with different measurement methods. To illustrate the importance of unambiguous terminology, consider the heading (line 52) "Contribution of below-ground deformation to net accretion rates." Given the nature of this study, I would argue that this should be reversed; what the authors show is that accretion drives subsidence rather than vice versa. In lines 65-66, the impression is given that SEC observations are somehow obtained from marker horizon measurements, which is incorrect (they are obtained from SET measurements only). Things like these must be ironed out, or the reader will end up being confused.

We completely agree with reviewer with the fact that terminology needed a complete revision. We have done so trying to:

- provide a clear description of the terminology in Fig. 3B, relating the expressions (i.e. uplift vs settlement) with the specific observations.
- limit the acronyms or better explain them in legends and captions

- give a full description on the difference between displacements and deformations in relation to the type of instruments/sensors
- defining the difference between SE-SET and MO-SET
- removing flaws

These modifications required to introduce a new section in the main text (Measurement dataset at the San Felice salt marsh), originally placed in the Material and Methods, and a complete revision of the section ‘Contribution of below-ground deformation to net accretion rates’. The title of the latter has been changed to ‘Salt marsh vertical dynamics: contribution of below-ground deformations and accretion rates’ to accomplish with the reviewer claim about drivers of subsidence. We agree on the fact that *accretion drives subsidence*. In turn, subsidence drives net vertical elevation gain, i.e., the difference between accretion and land settlement.
Moreover, there is no conceptual difference between “accretion rate (AC)” and “sedimentation rate (ω)”, thus we substitute the occurrences of *sedimentation rate* with *accretion rate*.

Related to this, I spent quite some time looking at Fig. 3, but I had difficulty understanding what exactly this figure is trying to convey. Is it meant to illustrate the actual rates that are observed at different depths below the surface? For example, the uppermost two panels (the difference between the two escaped me; it isn’t explained) include vertical purple lines that connect data points (squares) that are said to refer to high-precision leveling data. In the left-hand panel, the deepest of these lines shows a value of about –0.5 mm/yr and the right-hand panel appears to show the same thing. Why is this duplicated? And more importantly, what does it mean? That the sediment column between 2 and 4 m depth is subsiding at this particular rate according to the leveling data? Table 1 in the SI indicates that high-precision leveling data were obtained with respect to depths at 0.5, 2.4, and 8.0 m, which seems at odds with Fig. 3. I’m sure I must be missing something, but frankly, I got lost here. I believe the presentation of this information needs quite a bit of work. Without a doubt, Fig. 3 is critical since it presents the data that are used to constrain the model. In Fig. 4, I assume that the red and blue curves are observations, but why do they extend (and abruptly reverse course) above the land surface? This looks strange. It also looks like only the SET-data are used for comparison with model results; what happened to the leveling data, for example? And what is the purpose of the inset with the tidal channel?

This was a major critical point of this review. We tried to completely revise Figs. 3 and 4 to better convey the message. Figure 3 has been completely rethought: it is now showing a cartoon of the monitoring network installation depth (with measurement type) and data (rates and associated uncertainties). A full description of this figure is given in the *Measurement dataset at the San Felice salt marsh* section.
The previous panels of Figure 3 are now shown in Figure 4, which is described in section “Salt marsh vertical dynamics: contribution of below-ground deformations and accretion rates”.
Panels A and B of Figure 4 refer to reconstruction of vertical displacements by using different dataset (Fig4A: **TCR, HPL, SET-SET** and Fig. 4B: **TCR, HPL, MO-SET**). This is now clear in the text at lines 108-117. For the purpose of this analysis, we considered only depths in the range of 0 to 4 m rather than the entire Holocene thickness because we consider a negligible compaction of the layer between 4 and 8 m. Since the 4 m-depth TCR quantifies deep (regional) subsidence values, the deformation measured by the HPL between 8 m and 2.4 m is completely attributed to the layer between 4 m and 2.4 m. To obtained the vertical displacement of the land surface ($VD\__{surface}$) the following relationship is applied $VD\__{surface} = VD\__{TCR(4m)} + DE\__{HPL(4\text{-}2.4m)} + DE\__{SE\text{-}SET(2\text{-}0m)}$ for Figure 4A, whereas $VD\__{surface} = VD\__{TCR(4m)} + DE\__{HPL(4\text{-}2.4m)} + DE\__{HPL(2.4\text{-}0.5m)} + DE\__{MO\text{-}SET(0.5\text{-}0m)}$ holds true for Figure 4B. Assuming deformation uniformly distributed within each monitored depth interval, the displacements change linearly. By using the profiles of Figures 4A and 4B, it results in $VD\__{surface}$ = -2.4 mm/year and $VD\__{surface}$ = -2.1 mm/year, respectively. Employing data from FMH, with accretion rates of the order of 3 mm/year, it means that land uplift less than 1 mm/year is found for this area, confirming the analysis obtained with Persistent Scatterer Interferometry on natural PTs.

The model calibration is performed by comparing model result in term of mean sedimentation rate and vertical displacements (yellow line in Fig. 4) with the reconstruction from available dataset. Fig. 4C shows Monte Carlo model results, providing the uncertainty distribution over the mean value. We remove the inset with the tidal channel because inappropriate.

More information is needed regarding the subsidence dataset, in particular regarding the number of observations. For example, with few exceptions it isn't clear how many SETs, corner reflectors, etc., were used. This should be added to Table 1 in the SI.

We thank the reviewer for this comment. Table 1 in the SI has been moved to the main text and integrated with requested information such as the number of observations. The location of the instruments is also provided in Fig. 2 (top-right subpanel).

The English writing needs work, as illustrated, for example, by the first sentence of the Introduction. I have not attempted to edit the manuscript, but I assume a copy editor will do so.

The manuscript has been revised by an English Language Editing through the Nature Research Editing Service but notice that we did not keep track changes of these corrections to maintain a readable manuscript.

A few ***more specific comments****:*
Line 17: please be more specific; what exactly is "long-term time scale?"
Ok, done. We mean millennial time scale.
Line 17: avoid mentioning lat/long data in the main text.
Ok, done.
Line 18: my understanding has always been that the Mediterranean is nearly ways microtidal, but perhaps conditions are different here? Could you simply provide the tidal range?
This is true, we provided the tidal range (average about 1 m) at line 26.
Line 29: be careful with the terminology here; aggradation and subsidence refer to different things (i.e., the counterpart of aggradation is erosion; the counterpart of subsidence is uplift). I think what is meant here is elevation gain/loss.
We thank for this correction. The manuscript has been revised to avoid this terminology flaws.
Line 57: please be careful, aggradation can also go hand in hand with loss of elevation, as shown by Cahoon et al. (1995) and many subsequent studies.
We agree with the reviewer. We replaced 'aggradation' with 'uplift' in this case.
Lines 58-59: more specificity needed here; is surface displacement uplift or subsidence? Please be more direct in your wording to avoid confusion.
This paragraph has been completely re-worded according to a thorough revision of the terminology.
Lines 115-124: this paragraph fits better in the Introduction, because it provides the broader context and justification for this study.
OK, done. We agree on this suggestion (sentence modified lines 15-23).
Line 143: why is the first meter singled out here? The basis for this isn't entirely clear, given the earlier findings that highlighted the top 4 meters.
We actually meant the 'top several meters' here. We changed it accordingly.

Fig. 1A-B: the study area (B) shows numerous point observations that exhibit surface-elevation gain, and several more with rather modest elevation loss, which seems different from the general pattern as shown in (A). This needs to be addressed.
Figure 1A illustrates the Venice coastal subsidence trend detected by SAR-based interferometry on TerraSAR-X images 2008-2013 showing the PTs scattered on the several environments describing the study area (e.g. salt marshes, urban, industrial, agricultural areas).
Focusing only on the salt marsh environment of the Venice lagoon, Da Lio et al., 2018 saw that the mean subsidence values were −3.0 ± 2.9 mm/yr, −2.6 ± 2.6 mm/yr, and −1.8 ± 3.3 mm/yr for the north, central and southern portions, respectively.

Figure 1B shows the PTs scattered on the San Felice salt marsh, with a mean subsidence rate of -0.2 ± 2.4 mm/year, in accordance with Da Lio et al., 2018. To note that the standard deviation is also quite large, highlighting a significant variability and noise level.

Fig. 1C-D: please provide a little more detail about these photos; I assume they are supposed to illustrate a tidal channel and a marsh platform, respectively?
This is correct. Little more detail has been added in the caption of the figure.

In summary, the two main substantive issues that I believe need to be addressed (RSLR and the role of organic matter) can be expected to have opposite effects on the model predictions (upward revision of rates of RSLR will increase the marsh accretion deficit, while incorporating organic matter might do the opposite). On balance it may come out in a wash, but that is of course rather speculative, which should be acknowledged. Despite these issues I believe this could become a valuable contribution, but plenty of work is necessary to effectively communicate the findings to the largest possible audience. I would strongly encourage the authors to do so.

26th Nov 20

Dear Dr Zoccarato,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "The Holocene imprint on the future evolution of tidal marshes". It has now been seen by our reviewers, and I include their comments at the end of this message. As you will see, reviewers 1 and 2 now recommend publication with only minor revisions, but reviewer 3 has identified several important issues that must be resolved. We continue to be 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, for publication in Communications Earth & Environment to be appropriate we will need you to:

**Ensure that your projections of subsidence are compelling, and discuss uncertainties (that is, comprehensively address reviewer 3's concerns raised in the paragraph starting "Figs. 4A and B are based on a synthesis")

**Utilise appropriate estimates of sea level rise and accretion (that is, comprehensively address reviewer 3's concerns raised in the paragraph starting "I appreciate that the authors now consider rates of SLR")

We invite you to revise your manuscript along these lines, and resubmit together with a point-by-point response. 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) 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.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me 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,

Joe Aslin

Associate Editor,
Communications Earth & Environment
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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

I read the revised version of the manuscript and I am satisfied with the authors' revisions. The authors have well responded to my comments and those of other reviewers. The manuscript reads well now, with good and useful graphics. I recommend its publication in Communications Earth & Environment, after the following revisions.

I appreciate the effort in writing a broad title, but I am afraid that it will not get enough attention. I would rather put more stress on the innovative physical model, data sets and location of study area (Venice Lagoon, Italy).

Ln 20: (SRL) >> SLR

Ln 22-24: Add some references.

Ln 28: Define the term "Holocene" at its first appearance: e.g. "in the last few millennia".

Ln 89: CGPS >> continuous Global Positioning System

Fig 1A & B and 2B: Define units of X’ and Y’s axes (meter?)

Fig 1C: Define %PTs!

Line 198-208: The authors incorporated global sea-level projections under RCP 2.6 and 8.5 scenarios into their revised analysis. As suggested in my previous revision (and also by reviewer 3) and stated by the authors in their revision, local and regional trends in SLR projections are important; and still the authors couls make use of local sea-level projection data sets provided by Kopp et al. (2014, Earth's Future) at nearby gauges to reduce the projection uncertainty.

Ln 202: sea level change >> sea-level change

Ln 204: define NAO.

Ln 121-122: ...in the variability of the measured subsidence rates in the Venice Lagoon and "in areas of increased sediment deposition" (e.g., Törnqvist et al., 2008 Nature Geoscience; Karegar et al., 2015, Geology; Brain et al., 2012, Earth and Planetary Science Letters).

Ln 249 – 252: Compaction of late Holocene coastal wetlands in the Mississippi Delta and North West of Europe have been recently quantified using continuous GPS observations (see Karegar et al., 2020, GRL).

Reviewer #2 (Remarks to the Author):

The revised version of the manuscript has greatly improved, and I believe that now it is suitable for publication. I have some minor further suggestions to the authors, please see below.
Abstract: "… inorganic sedimentation contributes to …"
Line 20: SLR instead of SRL
Line 21: decrease instead of decreases
Line 54: I suggest to substitute "with" with "performed using" or other term
Line 58: " … in the period 1993-2013 … deployed on the San Felice …"
Line 95-96: I suggest to change this sentence, which is not very clear, and write that "accretion corresponds to the thickness of the sediment layer deposited over the feldspar"
Line 221: delete "might'
Line 225: as far as I know, the presence of Spartina alterniflora has never been recorded in the Venice lagoon (luckily!), where there is Spartina maritima instead.
Line 234: use "show" instead of "demonstrate"
Line 172: "salt marsh restoration"

Reviewer #3 (Remarks to the Author):

Review of revised manuscript "The Holocene imprint on the future evolution of tidal marshes" by Zoccarato & Da Lio (CEE-20-0086B)

Zoccarato & Da Lio have made substantial revisions to their originally submitted manuscript, resulting in significant improvements. For example, I appreciate that the authors now mention the limitation that they are not using a coupled geomechanical-morphodynamic marsh model. It would be even better if this is stated upfront in the Introduction, so it is clear to the reader what they will get and what they won't get. Within this context, I think the paper would be stronger if there is more mention of morphodynamic marsh model studies in the relevant paragraph (lines 39-48). For example, the recent study by Mariotti (2020, Adv Water Res) comes to mind. Going back to the main concerns that I raised in my first review, the authors have addressed several issues while other elements still need attention, including the connection between the InSAR-derived data and other subsidence measurements. I also think more can be done to clarify and simplify terminology.

Figs. 4A and B are based on a synthesis of subsidence measurements from different methods and serve as the key test for the modeling. What is shown here makes sense, with subsidence rates that decrease with depth. However, there appears to be an unexplained conflict with the InSAR-derived PT data that show a value of –0.2 ± 2.4 mm/yr (with a mode around –1 mm/yr). Figs. 4A and B show total subsidence rates at the surface that are just over –2 mm/yr. The authors then argue that this is consistent with the PT data when accretion (3 mm/yr) is taken into account. However, as indicated elsewhere (e.g., the caption of Fig. 1) the PT data do not capture accretion and provide subsidence/uplift directly. This seems odd – am I missing something here? Much of this appears to come down to the basic question of what the PT reflectors really are. I would assume that these are objects that do not accumulate sediment (as illustrated by Fig. 2F)? After all, how else could they be useful reflectors? On the other hand, I also note the response to a comment by Referee #1 that suggests that some PT sites do record accretion, which would be at odds with what is suggested in the manuscript. This is fundamentally important and since much of the study appears to hinge on these things, it must be resolved. An important related issue is that data uncertainties are often not considered. I partly mention this because there appear to be only two sites with vertical accretion measurements, which is not much to characterize a complex marsh environment. One would expect plenty of spatial variability, so fully incorporating errors is essential.

I appreciate that the authors now consider rates of SLR for the remainder of this century in association with RCPs, in addition to their "background" value of 1.2 mm/yr (which really should be seen merely as a reference scenario with no predictive meaning – see below). However, they need to add the outcomes of the RCP-related scenarios to Table 2. These things said, I do not find their arguments that rates of SLR in the Northern Adriatic Sea will be lower than the global average compelling. The present-day flow rate through the Strait of Gibraltar can approach 1 Sv (Soto-Navarro et al., 2010, JGR-Oceans), i.e., at first sight quite adequate to transfer Atlantic Ocean waters into the Mediterranean (I haven't done any further calculations to ascertain this, but that should be straightforward). In addition, thermosteric contributions to SLR occur independently of such mass exchanges, so it is on the authors to show that these effects are very small in the Adriatic, today and in the future. Likewise, I am not convinced by the NAO argument; as far as I know this is a largely cyclic phenomenon, so absent strong evidence that a distinct future trend in the NAO is expected this does not support their argument (besides, the authors would have to explain how exactly the NAO affects sea level in this region). So unless I am off base here, I recommend the authors delete this portion of text – it may raise unnecessary red flags. And just to be clear: they are using a rate that is about one third of the present-day rate of global mean sea level rise. As indicated above, they should stress that the 1.2 mm/yr scenario can merely serve as a reference point against which to compare the RCP-derived sea-level scenarios. (On this issue, I would add that Referee #1 raised similar concerns, so it seems fair to assume that this is a real thing.) Put differently, 1.2 mm/yr scenarios cannot be predictions for the future, but rather model experiments that can aid understanding about the role of shallow subsidence in marsh evolution and serve as a backdrop for the RCP-based predictions.

The use of acronyms has improved, but some issues remain. For example, why is a natural radar reflector (Fig. 2) or natural reflector (line 61) referred to as a PT and not as a NRR or NR? And I still see no unambiguous explanation about the difference between MO-SET and SE-SET (i.e., what MO and SE means remains unclear). Table 1 and Fig. 2 suggest that this is associated with the depth of the rod. Please note that the wetland literature often uses separate acronyms (SET versus RSET or rSET) for instruments with or without a deep rod. Why not simply adopt what many readers are already familiar with? Also, instead of referring to the difference between accretion rate and SEC as deformation (DE), the mainstream literature simply talks about shallow subsidence (shallow compaction would be fine too), so why not stick with well-established terminology?

A few more specific comments:

Line 20: SRL should be SLR.
Line 30: note that "a few centimeters of subsidence" means very little without a timescale. If this is a value for the remainder of this century it would mean something entirely different than if it is an annual value.
Lines 76-77: a much easier way to describe this is to simply say "thickness of deposits on top of the FMH." Invoking height would require placing the measurements in a geodetic framework, which is unnecessary.
Lines 94-95: this needs to be clarified; I assume the "–" is intended to be "minus"? Please say so or readers may be confused. Perhaps more importantly, you really want to include uncertainties here; they are considerable!
Lines 101-102: while this statement sounds plausible, it is immediately contradicted by the claim that subsidence rates below 4 m are negligible.
Line 116: not sure what is meant with "land uplift" here… given the context, I assume they mean surface-elevation gain? These are fundamentally different things that should not be confused.
Lines 171-172: "lagoon inlets" should be "tidal inlets."
Line 199: this should be the Mississippi Delta, Louisiana.
Lines 218-219: The term "aggradation rate" is commonly used as equivalent to "accretion rate." As mentioned earlier, note that the sum of accretion and subsidence is surface-elevation change.

Without a doubt, the authors have made significant improvements regarding the presentation of their findings including, for example, Fig. 3. However, these improvements also enabled me to spot the apparent conflict between the PT-data and the other subsidence data. I believe it is imperative that this issue be resolved.

Torbjörn Törnqvist

Response to Reviewers' comments

The lines indicated in the response refer to the version of the *manuscript with trackchanges*.

Reviewer #1

I read the revised version of the manuscript and I am satisfied with the authors' revisions. The authors have well responded to my comments and those of other reviewers. The manuscript reads well now, with good and useful graphics. I recommend its publication in Communications Earth & Environment, after the following revisions.

I appreciate the effort in writing a broad title, but I am afraid that it will not get enough attention. I would rather put more stress on the innovative physical model, data sets and location of study area (Venice Lagoon, Italy).

Thanks for the suggestion. We agree on that, the title has been changed to: The Holocene imprint on the future evolution of tidal marshes in the Venice Lagoon (Italy) revealed by an innovative dataset interpretation

Ln 20: (SRL) >> SLR
Ok done.

Ln 22-24: Add some references.
Thanks for the suggestion. The following references have been added to the manuscript:

Brain, M. J. Past, present and future perspectives of sediment compaction as a driver of relative sea level and coastal change. Curr. Clim. Chang. Rep. 2, 75–85, DOI: 10.1007/s40641-016-0038-6 (2016).

Marani, M., D'Alpaos, A., Lanzoni, S. & Santalucia, M. Understanding and predicting wave erosion of marsh edges. grl DOI: doi:10.1029/2011GL048995 (2011).

Houser, C. Relative importance of vessel-generated and wind waves to salt marsh erosion in a restricted fetch environment. J. Coast. Res. 230–240, DOI: 10.2112/08-1084.1 (2010).

Anderson, F. E. Effect of wave-wash from personal watercraft on salt marsh channels. J. Coast. Res. 33–49 (2002).

Day, J. et al. Sustainability of mediterranean deltaic and lagoon wetlands with sea-level rise: the importance of river input. Estuaries Coasts 483–493, DOI: 10.1007/s12237-011-9390-x (2011).

Nicholls, R. J. & Cazenave, A. Sea-level rise and its impact on coastal zones. Science 328, 1517–1520, DOI: 10.1126/290 science.1185782 (2010).

Schuerch, M., Spencer, T. & Temmerman, S. e. a. Future response of global coastal wetlands to sea-level rise. Nature 292 231–234, DOI: 10.1038/s41586-018-0476-5 (2018).

Ln 28: Define the term "Holocene" at its first appearance: e.g. "in the last few millennia".
Ok done.

Ln 89: CGPS >> continuous Global Positioning System
Ok done.

Fig 1A & B and 2B: Define units of X' and Y's axes (meter?)
Ok done. We have modified the related caption adding the coordinate system UTM33, WGS84.

Fig 1C: Define %PTs!
Ok done. We have modified the related caption adding the definition. The caption now reads: "Panel A shows the average surface elevation change, SEC (mm/year), of the Venice coastal area located on the northern Adriatic Sea (Italy) obtained by Persistent Scatterer Interferometry on TerraSAR-X images acquired from March 2008 to November 2013 (modified after1). Positive values indicate uplift, negative land subsidence. The enlargement in panel B shows the SEC behaviour of the San Felice salt marsh whereas panel C shows the frequency distribution of the SEC rates (i.e. the percentage of PTs in each SEC class-rate over the total amount of PTs detected on the marsh). Panels D-F show typical marsh environments with creeks, vegetated and muddy areas. The base maps in panels A and B are courtesy of Esri, Maxar, GeoEye, Earthstar Geographics, CNES/Airbus DS, USDA, USGS, AeroGRID, IGN, and the GIS User Community. Coordinate system: UTM33, WGS84"

Line 198-208: The authors incorporated global sea-level projections under RCP 2.6 and 8.5 scenarios into their revised analysis. As suggested in my previous revision (and also by reviewer 3) and stated by the authors in their revision, local and regional trends in SLR projections are important; and still the authors could make use of local sea-level projection data sets provided by Kopp et al. (2014, Earth's Future) at nearby gauges to reduce the projection uncertainty.
Accordingly to comment of reviewer #3, we have updated Table 2 to incorporate the results under RCP-derived scenarios. In the main text we have reported local values of MSL projections in Venice, as derived in the recent works of

Zanchettin, D.et al. Review article: Sea-level rise in venice: historic and future trends. Nat. Hazards Earth Syst. Sci. Discuss. 2020, 1–56, DOI: 10.5194/nhess-2020-351 (2020).

Thiéblemont, R., Le Cozannet, G., Toimil, A., Meyssignac, B. & Losada, I. Likely and high-end impacts of regional sea-level rise on the shoreline change of european sandy coasts under a high greenhouse gas emissions scenario. Water 11, 2607 (2019).

The difference between these values and the global mean sea level projections differs of about 10% (Zanchettin et al., 2020).

Ln 202: sea level change >> sea-level change
Ok done.

Ln 204: define NAO.
Ok but we remove this sentence accordingly to comments by reviewer #3.

Ln 121-122: ...in the variability of the measured subsidence rates in the Venice Lagoon and "in areas of increased sediment deposition" (e.g., Törnqvist et al., 2008 Nature Geoscience; Karegar et al., 2015, Geology; Brain et al., 2012, Earth and Planetary Science Letters).
Thanks. We have modified and introduced suggested references.

Ln 249 – 252: Compaction of late Holocene coastal wetlands in the Mississippi Delta and North West of Europe have been recently quantified using continuous GPS observations (see Karegar et al., 2020, GRL).
Thanks. We have introduced the suggested reference.

Reviewer #2

The revised version of the manuscript has greatly improved, and I believe that now it is suitable for publication. I have some minor further suggestions to the authors, please see below.

Abstract: "… inorganic sedimentation contributes to …"
Ok thanks.

Line 20: SLR instead of SRL
Ok done.

Line 21: decrease instead of decreases
Ok done.

Line 54: I suggest to substitute “with” with “performed using" or other term
Ok done.

Line 58: “ … in the period 1993-2013 … deployed on the San Felice …”
Ok done.

Line 95-96: I suggest to change this sentence, which is not very clear, and write that “accretion corresponds to the thickness of the sediment layer deposited over the feldspar”
Ok thanks.

Line 221: delete “might’
Ok done.

Line 225: as far as I know, the presence of Spartina alterniflora has never been recorded in the Venice lagoon (luckily!), where there is Spartina maritima instead.
Absolutely. We have corrected our mistake. *Spartina maritima*, *Spartina townsendii*, *Spartina anglica* are the Spartina-species identified in the Venice Lagoon as mixed patches (Lo V.B. et al., Marine Environmental Research, 2017; Silvestri S. et al., Estuarine, Coastal and Shelf Science, 2005; Scarton F. et al., Proceedings of the Sixth International Conference on the Mediterranean Coastal Environment, MEDCOAST 03., 2003). Therefore, we broadly refer to them as *Spartina*.

Line 234: use “show” instead of “demonstrate”
Ok done.

Line 172: “salt marsh restoration”
Ok done.

Reviewer #3

Review of revised manuscript “The Holocene imprint on the future evolution of tidal marshes” by Zoccarato & Da Lio (CEE-20-0086B)

Zoccarato & Da Lio have made substantial revisions to their originally submitted manuscript, resulting in significant improvements. For example, I appreciate that the authors now mention the limitation that they are not using a coupled geomechanical-morphodynamic marsh model. It would be even better if this is stated upfront in the Introduction, so it is clear to the reader what they will get and what they won’t get.
Thanks for this suggestion. We stress this concept in the introduction (lines 63-65).

Within this context, I think the paper would be stronger if there is more mention of morphodynamic marsh model studies in the relevant paragraph (lines 39-48). For example, the recent study by Mariotti (2020, Adv Water Res) comes to mind.
We thank the reviewer for the comment. We introduced marsh model studies as suggested:

Mariotti, G. Beyond marsh drowning: The many faces of marsh loss (and gain). Adv. Water Resour.144, 103710, DOI:295https://doi.org/10.1016/j.advwatres.2020.103710 (2020).

D’Alpaos, A. & Marani, M. Reading the signatures of biologic-geomorphic feedbacks in salt-marsh landscapes. Adv. Water Resour.93, 265–275, DOI: 10.1016/j.advwatres.2015.09.004 (2016).

Fagherazzi, S.et al. Numerical models of salt marsh evolution: Ecological, geomorphic, and climatic factors. Rev. Geophys. 50, DOI: https://doi.org/10.1029/2011RG000359 (2012).

Going back to the main concerns that I raised in my first review, the authors have addressed several issues while other elements still need attention, including the connection between the InSAR-derived data and other subsidence measurements. I also think more can be done to clarify and simplify terminology.
Figs. 4A and B are based on a synthesis of subsidence measurements from different methods and serve as the key test for the modeling. What is shown here makes sense, with subsidence rates that decrease with depth.
However, there appears to be an unexplained conflict with the InSAR-derived PT data that show a value of –0.2 ± 2.4 mm/yr (with a mode around –1 mm/yr). Figs. 4A and B show total subsidence rates at the surface that are just over –2 mm/yr. The authors then argue that this is consistent with the PT data when accretion (3 mm/yr) is taken into account. However, as indicated elsewhere (e.g., the caption of Fig. 1) the PT data do not capture accretion and provide subsidence/uplift directly. This seems odd – am I missing something here? Much of this appears to come down to the basic question of what the PT reflectors really are. I would assume that these are objects that do not accumulate sediment (as illustrated by Fig. 2F)? After all, how else could they be useful reflectors? On the other hand, I also note the response to a comment by Referee #1 that suggests that some PT sites do record accretion, which would be at odds with what is suggested in the manuscript. This is fundamentally important and since much of the study appears to hinge on these things, it must be resolved. An important related issue is that data uncertainties are often not considered. I partly mention this because there appear to be only two sites with vertical accretion measurements, which is not much to characterize a complex marsh environment. One would expect plenty of spatial variability, so fully incorporating errors is essential.
We understand the reviewer point on this issue. The problem is again on definition of terminology, but the results presented in the manuscript are coherent.
PTs measure Surface Elevation Change (SEC), thus accounting also for accretion over the marsh surface. Indeed, PTs are defined in the manuscript as portions of the marsh surface keeping a coherent response or, for example, large wood remnants deposited on the ground (Fig. 2F). Note that some pixels show positive rates, mainly located close to the creeks, that are likely due to local accretion process controlled by the settling of sediments in suspension originated by erosion of the close salt marsh edges and tidal flats (Da Lio et al., 2018).
The misunderstanding arises in the definition of 'subsidence' and 'land uplift' when dealing with InSAR-derived PT data. According to the authors (see Figure 3), these terms are defined as positive SEC values (uplift) and negative SEC values (subsidence). Thus, subsidence and uplift are the comprehensive response of the "actual" marsh surface, which account for both compaction/expansion of sediments plus accretion. This is also the reason why the authors do not call the 'shallow compaction' as subsidence (see minor comments).
This choice is directly linked to the type of measurements available from InSAR-derived observations. Indeed, from these observations it is not possible to distinguish the two separate components of accretion and subsidence/uplift as they furnish an 'integral' behaviour over time of the marsh surface, accounting for all physical processes.
We think that the reviewer has in mind another definition of land subsidence/uplift that accounts for the movement (positive or negative) of the marsh surface at a reference (initial) time without the contribution of accretion.
We provide a clarification of this concept in the manuscript (lines 96-100) to avoid confusion and put all readers at the same terminology level.

Da Lio, C., Teatini, P., Strozzi, T. & Tosi, L. Understanding land subsidence in salt marshes of the venice lagoon from261SAR interferometry and ground-based investigations. Remote. Sens. Environ.205, 56 – 70, DOI: https://doi.org/10.1016/j.262rse.2017.11.016 (2018).

We incorporate errors of SET and FHM measurements, as suggested (see lines 108-110). Moreover, we underline that the Monte Carlo analysis is here essential to incorporate uncertainty in the model input (e.g., accretion rates) and thus account for the possible variability of the model response.

I appreciate that the authors now consider rates of SLR for the remainder of this century in association with RCPs, in addition to their "background" value of 1.2 mm/yr (which really should be seen merely as a reference scenario with no predictive meaning – see below). However, they need to add the outcomes of the RCP-related scenarios to Table 2.
These things said, I do not find their arguments that rates of SLR in the Northern Adriatic Sea will be lower than the global average compelling. The present-day flow rate through the Strait of Gibraltar can approach 1 Sv (Soto-Navarro et al., 2010, JGR-Oceans), i.e., at first sight quite adequate to transfer Atlantic Ocean waters into the Mediterranean (I haven't done any further calculations to ascertain this, but that should be straightforward). In addition, thermosteric contributions to SLR occur independently of such mass exchanges, so it is on the authors to show that these effects are very small in the Adriatic, today and in the future. Likewise, I am not convinced by the NAO argument; as far as I know this is a largely cyclic phenomenon, so absent strong evidence that a distinct future trend in the NAO is expected this does not support their argument (besides, the authors would have to explain how exactly the NAO affects sea level in this region). So unless I am off base here, I recommend the authors delete this portion of text – it may raise unnecessary red flags. And just to be clear: they are using a rate that is about one third of the present-day rate of global mean sea level rise. As indicated above, they should stress that the 1.2 mm/yr scenario can merely serve as a reference point against which to compare the RCP-derived sea-level scenarios. (On this issue, I would add that Referee #1 raised similar concerns, so it seems fair to assume that this is a real thing.) Put differently, 1.2 mm/yr scenarios cannot be predictions for the future, but rather model experiments that can aid understanding about the role of shallow subsidence in marsh evolution and serve as a backdrop for the RCP-based predictions.
We now add the scenarios based on RCPs projections in Table 2 as suggested and modified scenarios SC-1, SC-2, and SC-3 with a SLR reference scenario of 1.2 mm/year, meant to understand the role of shallow subsidence in marsh evolution. We first explain this at lines 163-164 and stress the concept again in the discussion at lines 219-223. We also highlight this fact in the introduction (lines 63-66). We think is now clear for the reader how we perform our projections, underlying the need for more sophisticated model with high SLR rates. The model must account for the dependency of sediment deposition based on MSL over the marsh surface.
Moreover, we add two recent references (Zanchettin et al, 2020, Thiéblemont et al, 2019) to include projections of sea-level rise in Venice. Regional/Local effects provide for estimation with a likely range of MSL rise between 210 mm (low RCP2.6) and 1000 mm (high RCP8.5) by 2100 in Venice relative to 1986-2005.

Zanchettin, D.et al. Review article: Sea-level rise in venice: historic and future trends.Nat. Hazards Earth Syst. Sci. Discuss. 2020, 1–56, DOI: 10.5194/nhess-2020-351 (2020).

Thiéblemont, R., Le Cozannet, G., Toimil, A., Meyssignac, B. & Losada, I. Likely and high-end impacts of regional357sea-level rise on the shoreline change of european sandy coasts under a high greenhouse gas emissions scenario. Water 11, 2607 (2019).

The use of acronyms has improved, but some issues remain. For example, why is a natural radar reflector (Fig. 2) or natural reflector (line 61) referred to as a PT and not as a NRR or NR?
We change the definition of PT as Point Target consisting of natural radar reflectors. This way the acronym corresponds to the defined variable. PT is the standard way used in SAR interferometry to refer to something on the land surface persistently and coherently reflecting the radar waves. Therefore, we prefer to maintain this acronym
And I still see no unambiguous explanation about the difference between MO-SET and SE-SET (i.e., what MO and SE means remains unclear).
Table 1 and Fig. 2 suggest that this is associated with the depth of the rod. Please note that the wetland literature often uses separate acronyms (SET versus RSET or rSET) for instruments with or without a deep rod. Why not simply adopt what many readers are already familiar with?

The abbreviations MO and SE refer to the companies that operate to install the instrumentation. We think is not necessary to insert the full description of the companies in this case. We thank the reviewer for the suggestion to move to more general acronyms RSET and rSET that we have been substituted in the manuscript (and in Figures).

Also, instead of referring to the difference between accretion rate and SEC as deformation (DE), the mainstream literature simply talks about shallow subsidence (shallow compaction would be fine too), so why not stick with well-established terminology?
That is true, but here we used the more general term “deformation” to also include *expansion* among possible deformation processes (Figure 3).

A few more specific comments:

Line 20: SRL should be SLR.
Ok done.

Line 30: note that “a few centimeters of subsidence” means very little without a timescale. If this is a value for the remainder of this century it would mean something entirely different than if it is an annual value.
This is certainly true. We have changed to ‘land elevation loss relative to the Adriatic mean sea level’.

Lines 76-77: a much easier way to describe this is to simply say “thickness of deposits on top of the FMH.” Invoking height would require placing the measurements in a geodetic framework, which is unnecessary.
Ok thanks, we have modified also according to Reviewer #1.

Lines 94-95: this needs to be clarified; I assume the “–” is intended to be “minus”? Please say so or readers may be confused. Perhaps more importantly, you really want to include uncertainties here; they are considerable!
We specified that “–” is intended as a minus and uncertainties are included as suggested (lines 108109).
Lines 101-102: while this statement sounds plausible, it is immediately contradicted by the claim that subsidence rates below 4 m are negligible.
We consider negligible consolidation below that threshold based on available measurements, which show about 1 mm/year (i.e. deep subsidence of this area).

Line 116: not sure what is meant with “land uplift” here… given the context, I assume they mean surface-elevation gain? These are fundamentally different things that should not be confused.
Thanks for this correction. We changed to surface-elevation gain that is what we meant.

Lines 171-172: “lagoon inlets” should be “tidal inlets.”
Ok thanks.

Line 199: this should be the Mississippi Delta, Louisiana.
Ok thanks.

Lines 218-219: The term “aggradation rate” is commonly used as equivalent to “accretion rate.” As mentioned earlier, note that the sum of accretion and subsidence is surface-elevation change.
Ok thanks. We changed to accretion rate to be consistent with previous terminology used in the paper.

Without a doubt, the authors have made significant improvements regarding the presentation of their findings including, for example, Fig. 3. However, these improvements also enabled me to spot the apparent conflict between the PT-data and the other subsidence data. I believe it is imperative that this issue be resolved.

9th Feb 21

Dear Dr Zoccarato,

Your manuscript titled "The Holocene imprint on the future evolution of tidal marshes in the Venice Lagoon (Italy) revealed by an innovative dataset interpretation" has now been seen again by our reviewer, whose comments appear below. In light of their advice I am 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 reviewer. 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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REVIEWERS' COMMENTS:

Reviewer #3 (Remarks to the Author):

Review of revised manuscript "The Holocene imprint on the future evolution of tidal marshes…" by Zoccarato & Da Lio (CEE-20-0086C)

I have examined this second revision of the Zoccarato & Da Lio manuscript. My impression is that their work is now close to being suitable for publication; I only have a few remaining requests for changes that should be easy to implement.
I read the response by the authors to the concerns I had raised about the PT data. To be clear, I don't think there is any misunderstanding as to how the PT data are interpreted in this paper – I understand the author's intent that the PT data record SEC (as opposed to subsidence/uplift). However, I remain a bit puzzled how a discrete radar target can continue to produce a coherent signal under conditions of ongoing sediment accretion (line 93). One would think that this must eventually lead to burial and obliteration of the target in question. If there is anything the authors could add to alleviate this concern, I would welcome it.

In the previous round, I suggested the inclusion of a few references regarding morphodynamic modeling in the Introduction (lines 41-43). I appreciate that this was done, but I did not ask for Jankowski et al. (2017) to be dropped – this reference should be maintained because it speaks to consolidation/subsidence as mentioned in this paragraph (unlike most of the other papers that are cited here).

I may inadvertently have caused some confusion by mentioning rSET and RSET in my previous review (Table 1 and line 83). Please note that these are the same things (they have just been written differently in different publications); both refer to SETs equipped with a rod of appreciable depth. So my recommendation would be to use RSET (or rSET) for the instrument with the 2-m-deep rod, and SET for the one with the 0.5-m-deep rod.

In summary, the authors present a relatively rare but much needed study that integrates different subsidence monitoring techniques within a modeling framework, very much in the spirit of what was recommended in the recent review article by Shirzaei et al. (Nature Reviews Earth & Environment, v2, p40-58, 2021) which they may want to cite. I look forward to seeing this paper come out in the literature.

Torbjörn Törnqvist

Response to Reviewer' comments

The lines indicated in the response letter refer to the final version of the manuscript.

Reviewer #3

Review of revised manuscript "The Holocene imprint on the future evolution of tidal marshes..." by Zoccarato & Da Lio (CEE-20-0086C)

I have examined this second revision of the Zoccarato & Da Lio manuscript. My impression is that their work is now close to being suitable for publication; I only have a few remaining requests for changes that should be easy to implement.

I read the response by the authors to the concerns I had raised about the PT data. To be clear, I don't think there is any misunderstanding as to how the PT data are interpreted in this paper – I understand the author's intent that the PT data record SEC (as opposed to subsidence/uplift). However, I remain a bit puzzled how a discrete radar target can continue to produce a coherent signal under conditions of ongoing sediment accretion (line 93). One would think that this must eventually lead to burial and obliteration of the target in question. If there is anything the authors could add to alleviate this concern, I would welcome it.
Selection of PTs were been performed using a specific PSI approach able to maximize the information retrieved from the long-term temporally-regular stack of TerraSAR-X scenes acquired between 2008 and 2013. Adopting a "short-term candidate pixel" criterion, candidate pixels that exhibit PT-like behavior were selected. Taking advantage of the large number of acquisitions per year, candidate PTs were estimated within stacks spanning one year only, in order not to discard pixels that were coherent only over a short period, as it is the case for specific plots of the salt marsh surface, as example. Then, the various lists of annual candidate PTs were merged together.
For further details on this specific PSI approach, please refer to Da Lio et al., 2018.
We have introduced a brief reference to this concept at line 88 of the manuscript.

In the previous round, I suggested the inclusion of a few references regarding morphodynamic modeling in the Introduction (lines 41-43). I appreciate that this was done, but I did not ask for Jankowski et al. (2017) to be dropped – this reference should be maintained because it speaks to consolidation/subsidence as mentioned in this paragraph (unlike most of the other papers that are cited here)
The reference was taken out for mistake. It is now back in the manuscript (line 42).

I may inadvertently have caused some confusion by mentioning rSET and RSET in my previous review (Table 1 and line 83). Please note that these are the same things (they have just been written differently in different publications); both refer to SETs equipped with a rod of appreciable depth. So my recommendation would be to use RSET (or rSET) for the instrument with the 2-m-deep rod, and SET for the one with the 0.5-m-deep rod.
Ok, the manuscript has been revised and modified accordingly.

In summary, the authors present a relatively rare but much needed study that integrates different subsidence monitoring techniques within a modeling framework, very much in the spirit of what was recommended in the recent review article by Shirzaei et al. (Nature Reviews Earth & Environment, v2, p40-58, 2021) which they may want to cite. I look forward to seeing this paper come out in the literature.
We are aware of such publication in Nature Reviews Earth & Environment, we introduced this citation to stress the need of our modelling approach in the introduction (line 45).