Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-75-AC2, 2018

Ocean Science
Discussions
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Interactive comment on "Measuring rates of present-day relative sea-level rise in low-elevation coastal zones: A critical evaluation" *by* Molly E. Keogh and Torbjörn E. Törnqvist

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Received and published: 25 October 2018

Response to the manuscript review by an anonymous referee

Reviewer comments are marked as "Reviewer"; author responses are labeled "Author"

Reviewer: General comments:

Reviewer: This study seems closely related to the work published in GSA Today (Nienhuis et al. 2017) in which the authors were involved. But it is not clearly stated how both relate together. Nienhuis et al. is quoted towards the end of the manuscript, just before the conclusions. The findings on the underestimation due to shallow subsidence are already present in Nienhuis et al. Hence, should this manuscript be considered as supplemental information to the GSA Today one? I think it is important that the authors clarify how both studies relate together from the beginning (introduction). In addition, the introduction and the conclusion ("we present", "we propose") suggest the approach is novel. However, later on we find expressions and references which suggest it is not. Overall the authors need to make an effort to unambiguously set their study in the scientific context.

Author: First, we would like to thank the anonymous referee for the thoughtful feedback regarding our manuscript. We have taken the referee's suggestions into account and feel that it has enabled us to make significant improvements.

Author: The reviewer is correct in noting that there is a brief mention in the Nienhuis et al. (2017) paper that benchmarks in coastal Louisiana are typically anchored at depth and thus the associated tide gauges do not capture shallow subsidence. However, Nienhuis et al. do not go into any detail about how this information was acquired or methods to remedy this issue. Instead, the paper is relatively narrowly focused on presenting a subsidence map for coastal Louisiana and it is not concerned with the methodology of measuring present-day RSLR in LECZs in a more general sense. In the present manuscript, we present and analyze benchmark depth data, discuss limitations of a variety of techniques for measuring RSLR, and suggest an alternative method of measuring RSLR in LECZs. While the scope of the Nienhuis et al. paper is strictly limited to coastal Louisiana, here we use coastal Louisiana as a case study for an issue that is likely global in scope. Thus, we hope to reach a much wider audience than the target audience for the Nienhuis et al. paper. Therefore, while the reviewer is correct that there are distinct elements that connect the two studies, these two manuscripts are otherwise separate and stand alone. We have clarified this connection in lines 117-120 of the manuscript.

Author: As for the novelty of our manuscript, the practice of using RSET-MHs to measure shallow subsidence is not new and we cite two studies using state-of-the-art RSET-MH methods: Webb et al. (2013) and Cahoon (2015). What is novel is the method of combining RSET-MH data with data from GNSS stations and satellite altimetry in order to produce robust measurements of RSLR. This method was first introduced by Jankowski et al. (2017), but for a different purpose (to evaluate the ability of coastal wetlands to keep pace with RSLR). Here we explore this new approach in much more detail and with the explicit objective to reach the large, multidisciplinary community concerned with obtaining better measurements of present-day rates of RSLR. We now clarify these points in lines 117-120 and 138-140 of the manuscript.

Reviewer: In my opinion, the manuscript suffers from a perspective bias “against” tide gauges. That is, the authors show that both the tide gauges and GPS antennas are similarly anchored deep below the surface (at almost equivalent depths). Thus, none of them can actually capture the shallow subsidence. The combination of satellite altimetry and GPS data or the use of tide gauges suffer from the same drawback. Consequently, the statement that the novel approach eliminates the need for tide gauge data (repeated several times in the manuscript) is not objectively supported, because the same criticism applies to GPS antennas, and hence to the combination of satellite altimetry and GPS data. From my understanding, tide gauges + RSET-MH can work as well as satellite altimetry + GPS + RSET-MH. The authors need to think about it, and provide arguments to support their claim in a more convincing way, or reconsider the presentation of their findings (which are anyway interesting, in my opinion).

Author: This is an excellent point. We have adjusted our wording throughout the manuscript to clarify that tide gauges are critical for many applications and that we are merely discussing a specific (yet important) context where tide-gauge data may not be the best option. In the abstract and conclusions (see lines 43 and 392), we now say that our proposed method of measuring RSLR in LECZs eliminates the need for tide-gauge data “in this context”. Tide gauges remain critical for measuring many processes, especially tides (the original and still-primary purpose of tide gauges) and event-scale phenomena such as storm surge, and they are invaluable in this regard. We also note that best scientific practices will make use of all available data and compare the results of various measurement techniques. See lines 311-315.

Author: Indeed, many of the issues affecting tide gauges also affect GNSS stations. Both types of instruments are generally anchored at depth and thus do not capture shallow subsidence. In principle, both GNSS stations and tide gauges could be used to measure deep subsidence and these data could then be combined with measurements of shallow subsidence (plus geocentric sea-level rise, in the case of GNSS data) to calculate RSLR. However, the tide gauges must have sufficiently long time series (at least 30 years) and known foundation depths to be useful in this context. In coastal Louisiana, the number of tide gauges that meet these criteria (n = 5) are fewer in number than GNSS stations with known foundation depths (n = 10). Additionally, GNSS data are less susceptible to short-term environmental conditions (i.e. wind speed and direction, tides, atmospheric pressure changes) than are tide gauge data. Thus, GNSS is the preferred method for measuring deep subsidence. This additional information is now included in lines 316-321 and 327-329 of the manuscript.

Reviewer: The manuscript (introduction) suggests an assessment of their findings in LECZs worldwide, but the authors do not provide evidence that the findings apply beyond their case study zone, except for some general considerations (sediment thick in different coastal areas of world from the literature). The authors should be aware that different countries (agencies) have different practices in building infrastructures (tide gauges or GPS antenna monumentations). The US case study is likely not representative of the wide range of practices elsewhere. They should consider reducing the scope of the claims, and develop a cautious discussion in extending the findings in LECZ worlwide. The title may be revisited too.

Author: We have acquired information on benchmarks in The Netherlands and now include them in the manuscript for comparison. From conversations with Dutch colleagues, we understand that tide-gauge benchmarks in The Netherlands are ∼5-25 m

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deep and generally anchored in the Pleistocene basement except in areas where the Holocene sediment thickness is greatest (see newly-added Table 4). In other words, conditions in The Netherlands are roughly comparable to those in the Chenier Plain of coastal Louisiana (and likely other “thin” LECZs): they do not capture the shallow subsidence component, but because benchmarks are generally anchored in a relatively stable substrate they are easier to interpret than many of the tide gauges in the Mississippi Delta (and likely other “thick” LECZs) where benchmarks are essentially “floating” in the Holocene succession. This additional information is included in lines 279-287. We expect that benchmarks in other LECZs are likely constructed in a broadly similar fashion to those in coastal Louisiana and The Netherlands: either attached to rods driven to refusal or mounted on existing structures with non-negligible foundation depths. Although we are fortunate to have acquired relatively precise benchmark data from The Netherlands, we have found that information on benchmarks in other LECZs is very difficult to come by. A global analysis of benchmark construction would be a valuable but massive undertaking and is beyond the scope of the present manuscript.

Reviewer: In line with the above comment, I would suggest a search in the literature about GPS station monumnetations to support the worldwide extension. I vaguely remember a talk a decade ago or so about GPS antenna monumentations within an IGS meeting or an IAG scientific assembly. The concern of the study was the ability of the different types of GPS antenna monumentations to estimate actual ground / crustal motions. I think it might be worth searching for the details of this study or later studies on this subject.

Author: Concerted efforts are currently underway to address the complexities regarding GNSS monumentation. At a newly-constructed subsidence superstation located in the lower Mississippi Delta, for example, three GNSS instruments were anchored at different depths in order to build a depth-integrated profile of subsidence (Allison et al., 2016). Novel approaches like these are expected to greatly improve our understanding of subsidence in LECZs in the future. This information is now mentioned in lines 322-

326.

Author: In addition, we now refer to the information available on hundreds of individual GNSS stations through the International GNSS Service (http://www.igs.org/network) and the National Geodetic Survey (https://www.ngs.noaa.gov/CORS/). See lines 127-129 in the manuscript. Site photos indicate that most GNSS stations are indeed mounted on existing buildings. Although the foundation depth of these buildings likely varies and tracking down foundation information for each building would require an enormous effort, it is likely that most (if not all) are anchored at some depth beneath the surface. Put differently, it is unlikely that these buildings are simply floating on the ground surface.

Reviewer: In addition to the above comment, the choice of the deepest benchmark in section 4 needs to be supported, especially regarding the leveling analysis and practice to maintain the tide gauge datum, which can differ from one country (agency) to another (agency). Furthermore, I think this methodological choice should not be presented / discussed in the "Results" section but in the methods section.

Author: Excellent point. For our original analysis, we chose to use the benchmark with the deepest known foundation in order to maximize the size of our dataset: 35 tide gauges have at least one benchmark with known foundation depth, but primary benchmark depths are known for only 23 tide gauges. For comparison, we have added an analysis of primary benchmark foundation depths. For benchmarks with known foundations depths (i.e. those mounted on steel rods driven to refusal), we find that primary and deepest known benchmarks are anchored an average of 21.4 $\pm$ 3.9 m and 21.5 $\pm$ 7.4 m below the surface, respectively. Note that for 8 of 23 tide gauges (35%), the primary benchmark is also the benchmark with the deepest known foundation. The mean foundation depth for all benchmarks is 21.0 $\pm$ 5.4 m. Thus, we see that primary benchmark foundation depths are indistinguishable from the dataset as a whole. We have improved the explanation of our methods (and agree that it fits better in the Methods section than in the Results) and added a description of this new analysis in lines

167-170 and 190-196 in the manuscript.

Author: In addition, we have acquired information on benchmarks in the Netherlands (see above for an in-depth discussion). Dutch benchmarks are constructed in a similar fashion as those in coastal Louisiana (i.e. mounted on steel rods, sheet piling, or concrete) and are also anchored at depth. Foundation depths range from 5 to 25 m. This information is now included in the manuscript in lines 279-287 and in Table 4.

Reviewer: The manuscript is overall well written with good illustrations (Figures). In my opinion, it needs to consider the above comments. My suggestion is therefore a major revision.

Reviewer: Specific comments & Technical corrections:

Reviewer: p.2, L17-18: The expression is confusing. That is, if the station is >14 m, it includes the surface, and thus can capture any land motion. Consider rephrasing, why not using the same form as with the tide gauges? (Simply remove ">").

Author: We compiled GNSS station foundation information from Dokka et al. (2006) and Karegar et al. (2015). In these papers, minimum (rather than exact) foundation depths are given for two of the GNSS stations. They are reported as >20 m (site BVHS) and >15 m (site HOUM) and we adopt this notation in our manuscript (see Table 2). We have now highlighted these sites more clearly in Figure 4. We use these minimum foundation depths for the BVHS and HOUM stations when calculating the mean foundation depth for all GNSS stations and then indicate that this mean value is in fact a minimum value. In line 208 (see also lines 39 and 377), we report that GNSS stations are anchored an average of >14.3 m below the land surface (i.e. the average foundation depth is no shallower than 14.3 m) and thus do not include processes occurring in the shallow subsurface.

Reviewer: p.2. L22: the need for tide gauge data is often multi-application. The authors should be careful with this claim, and state the context of it (eliminates the need for this specific application and LECZ situation). In addition, see major comment above, that is, the same concerns apply to the GPS monumnetation, hence both tide gauges and GPS show the same drawback.

Author: Please see above for an in-depth discussion of this issue. We have adjusted our wording throughout the manuscript to clarify that we are focused on a specific context where tide-gauge data may not be the best option. Tide gauges remain critical for measuring many processes and are invaluable in this regard. See lines 311-315.

Reviewer: p.2, L34: a reference to support this claim is missing. It could be Holgate et al. (2013) which describes a data bank or similar; it could be an (the) article(s) that rescued the historical data of the stations listed in brackets.

Author: We now refer to five of the longest tide gauge records and cite three papers that presented the historical data: Key West, USA (Maul and Martin, 1993); Brest, France; ÅŽwinoujście, Poland; New York, USA; and San Francisco, USA (Woodworth et al., 2011); Boston, USA (Talke et al., 2018). See lines 55-58.

Reviewer: p.2. L37: Watson et al. is a good paper but it is not relevant in the context of this sentence. (Its global sea-level rise estimate is based on satellite altimetry data). Maybe the reference can be used somewhere else.

Author: We have removed the reference to Watson et al. (2015) from this sentence.

Reviewer: p.3 L50. Consider adding the reference for the PSMSL (Holgate et al. 2013 in J. Coastal Res).

Author: Good suggestion, we have added a reference to Holgate et al. (2013) as well as the PSMSL web address (http://www.psmsl.org). See lines 72-73.

Reviewer: p.3. L53. What signals encompass "natural variability" here?

Author: In lines 78-81, we have clarified that the natural variability includes phenomena such as storms, El Niño-Southern Oscillation cycles, changes in the orbital declination

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of the moon, shifts in ocean currents, and atmospheric pressure variability (Pugh, 1987; Douglas, 1991; Shennan and Woodworth, 1992).

Reviewer: p.4, L93. A reference is needed to support this claim. I vaguely remember a talk several years (decade?) ago at an IGS or IAG meeting about GPS antenna monumentations (structure, depth. . .) with some statistics. The concern of the study was the ability of GPS antennas to estimate actual ground / crustal motions. I think it can be worth searching the literature, especially since L97 states the issue of the nature of GPS station foundations as an objective of the study.

Author: Please see above for an in-depth discussion of this topic. Efforts are currently underway to address the complexities regarding GNSS monumentation. In addition, we now refer to the information available on hundreds of individual GNSS stations through the International GNSS Service and the National Geodetic Survey.

Reviewer: p.4. L100. Confusing (see general comments above). The expression suggests the approach is novel, especially because in the previous sentence it is stated what is not the purpose of the study. However, there are two references at the end of the sentence. Is this study a refinement? Consider rephrasing and clarifying.

Author: Please see above for an in-depth discussion of this issue. In this manuscript, we present a novel method to measure RSLR in LECZs. We now clarify that the reader should see Webb et al. (2013) and Cahoon (2015) for descriptions of the RSET-MH method (see lines 138-140), which can be used to measure one component of RLSR (shallow subsidence).

Reviewer: p.5, L144-145. This choice needs to be supported, especially regarding the leveling analysis and practice to maintain the tide gauge datum, which can differ from one country (agency) to another (agency).

Author: Please see above for an in-depth discussion of this topic. In our original analysis, we chose to use the benchmark with the deepest known foundation in order to maximize the size of our dataset. For comparison, we have added an analysis of primary benchmark foundation depths. We find that primary benchmark depths are indistinguishable from the dataset as a whole. We have improved the explanation of our methods and added a description of this new analysis in lines 167-170 and 190-196 in the manuscript.

Author: For a better global context, we now include information on benchmarks in the Netherlands, which are constructed in a similar fashion as those in coastal Louisiana and are also anchored at depth. Discussion of this information is now included in the manuscript in lines 279-282.

Reviewer: p.6, L172 (L205 too). What is behind the term ‘eustatic’?

Author: The term “eustatic” has been removed from the manuscript and replaced with clearer terminology. We now refer to this phenomenon as “real (geocentric) sea-level rise”.

Reviewer: p.14. I cannot see whether there are squares and circles co-located. Consider using a different colour too, it may help.

Author: The color scheme in Figure 2 has been changed to improve readability. Tide gauges and GNSS stations are now shown as dark blue circles and light orange squares, respectively.

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