Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-75-RC1, 2018

Ocean Science
Discussions

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 August 2018

25 August 2018

Comments on "Measuring rates of present-day relative sea-level rise in low-elevation coastal zones: A critical evaluation" by Keogh and Tornqvist (OSD)

This paper makes use of a data set of benchmark (BM) depths at tide gauges and GPS stations in Louisiana, which enables the authors to come to conclusions regarding the ability of tide gauges to make accurate measurements of relative sea level rise in this and similar deltas. They make some recommendations on how such measurements

might be done better.

This is short paper which is mostly written well with decent figures. I am sure that the topics addressed have been discussed by these and other authors previously. Also they do not produce any actual new results on relative sea level trends in the area. Nevertheless the BM data set does result in a nice couple of plots which enable them to make their main point well. So I would have no objection to seeing this paper published eventually, although I do have some comments on their arguments and on the way some of the text is written.

One comment is a technical issue to do with the way that NOAA works. The authors say correctly that there are typically half a dozen BMs at each tide gauge site. Many of these are deep ones and Table 1 lists the depths of the deepest in each case. If the datum of the tide gauges is defined relative to one of these deep marks, then I can understand the arguments of the authors that relative sea level rise could be underestimated.

However, sometimes there are also surface (or near surface) marks which can appear as 'zero depth (N/A setting)' in Table S1 of the paper. Now, the NOAA web site (https://tidesandcurrents.noaa.gov/datum_options.html#STND) states explicitly that:

"Station datum is referenced to the primary bench mark at the station for the definition of the tide gauge station datum".

So, if the designated primary mark is a surface mark, the Station Datum at the gauge will have been defined by the land surface and their arguments will not apply.

Now, the only important site in the delta with a decent long record is Grand Isle. That has data from 1947 and its benchmark sheet (available from the NOAA web site) shows that the primary mark is BM10 which is a "survey disk on the sea wall" (again shown as zero depth and N/A setting in Table S1). This is a surface mark so the arguments of the authors do not apply here.

I looked at the information on the NOAA web site for all 31 NOAA stations given in Table 1 of the paper (i.e. the 35 stations listed minus 4 USACE stations). The NOAA web site information is essentially the same as in Table S1. Of the 31, 6 have primary marks which are surface (or very near surface) marks: Caminada Pass, East Bay, Grand Isle, Lafitte, Martello Castle and Weeks Bay. If the authors agree with this then I think their text should mention it.

Just in case, I checked my interpretation about the way NOAA works with the CO-OPS Technical Director (Dr. Peter Stone) and Chief Scientist (Dr. Greg Dusek). They replied:

"We control the water level observation primarily off of one primary bench mark (PBM) and then ensure the stability of that mark by using the remaining 9 or so marks. On occasions when we see substantial and/or continual differential movement between the PBM and the other marks, we adjust the PBM to a different mark determined to be stable relative to the remaining marks."

So that confirms what is on the NOAA web site, and confirms that my comments about the six mentioned above, and Grand Isle in particular, are correct. They do not fit into the main argument of the paper, so there should be some extra wording to handle that.

As for the other 25 stations in Table 1 for which the primary mark is a deep one, then I agree with their comments, but only in principle, and only at a time way into the future when these stations will have acquired records long enough for trend estimation. Stone and Dusek remarked:

"The large number of tide gauges used in the analysis is very perplexing. The NOAA gauges [mentioned in Table 1] (which were installed by CO-OPS) were installed for wide ranges of time. Two of the gauges (Shell Beach and Grand Isle) were installed for decades and we have calculated relative sea level change rates. The others have only been installed for a few months or years and do not have enough data to calculate statistically significant RSLR [relative sea level rise]."

Now, Grand Isle I have already mentioned. In fact, Shell Beach has a deep primary mark, so I accept that the argument of the authors applies for that. But as Shell Beach has data (in the PSMSL) only for 2008-2017, it is hardly yet a long record.

So I think some care should be taken in the text between explaining what could happen IN PRINCIPLE regarding tide gauges with deep primary marks, and what is the real situation at the moment in the delta.

This takes me to two mentions of the PSMSL in the paper. At line 50 the authors state that there are 5 PSMSL stations in Lousiana but do not give their names. They are Eugene Is (data 1939-1974), Grand Isle (1947-2017), South Pass (1980-1999), Shell Beach (2008-2017) and New Canal Station (2006-2017). As mentioned above, Grand Isle is the only important one for sea level trends. The PSMSL defines RLR datum at Grand Isle (and other NOAA sites) using the Station Datum information in each case that NOAA provides. Therefore, I think there should be a mention somewhere in the paper to the effect that the sea level rate at Grand Isle provided by the PSMSL record is not likely under-estimated as the text presently implies.

The other mention of the PSMSL is in the paragraph at lines 250-261. It again mentions only 5 PSMSL stations in the area. Why? The PSMSL cannot be expected to databank the density of stations that the authors need, so to somehow conflate the PSMSL with that requirement seems strange to me. In fact, what the PSMSL would be happy with in an area this size is a single tide gauge station with GPS and good BM control. Anyway, the authors show potentially they have many more than 5 so what is their problem? Also the paragraph says that of the 5 'only a few' have RSET nearby. If one takes 'a few' as meaning 3 or similar then one could read this sentence as saying that most PSMSL stations have RSET, which I think is opposite to what the authors want to say! So this paragraph needs rewording and I can't see why the PSMSL is being dragged into it at all.

Conclusions - so I see the problems that the authors raise about deep BMs, in principle.

However, I do not buy the suggestion that, instead of tide gauges, a better job could be done using RSET-MH data which seems to me to be a very rough and ready method, combined with GPS for deep submergence, combined with altimetry. RSET, GPS and altimetry data all have their own nuances and problems, and in particular altimetry until fairly recently has had problems getting very close to the coast.

Tide gauges could do the job you want if you have at least one surface mark at each site, and if there is ongoing monitoring of the relative heights between surface and deep BMs. That would solve your problem; a conversation with NOAA is required about constructing a history of the evolution of relative heights between benchmarks at each site.

Stone and Dusek commented to me also that "The authors' did not address the mounting of the water level sensors on different structures they were comparing and how those structures can be affected by settling. Some of the stations used in their comparisons are probably installed on piers where the pilings may only be sunk a few feet. Others, like the water level stations at Shell Beach and Calcasieu Pass, LA are mounted on massive steel structures driven into consolidated sediments (which we refer to as SPIPs). The type of installation can be relevant to consider when attempting to accurately assess sensor movement relative to bench marks on land, and presumably in the cases of the SPIPs, our leveling data could indicate if deep rod marks and the shallower rod or concrete marks show variable long-term trends relative to the water level observations."

So, while accepting the general main point of the authors, I think the main thing is to have access to histories of all the relevant surveying information at a site.

A last comment about the Conclusions is an obvious one, that the correct scientific approach is to make use of data from all techniques and see eventually how they compare, not just suggest rejecting tide gauges (which NOAA pay for, given that they are anyway needed for monitoring transient events such as storm surges) by adopting

an 'alternative approach'.

The Conclusions also makes some comments about deltas elsewhere around the world and lists some in Table 3. How many have deep BMs like in Louisiana? I suspect most do not, but at best have surface marks in which these arguments will not apply. It would be interesting to know.

So for the reasons above I think some rewriting of the text is required.

Detailed comments:

line 17 and elsewhere - GPS is better denoted at GNSS (Global Navigation Satellite System) these days.

line 35 - a reference to long tide gauge records in N Europe and N America could be Woodworth et al. (Surveys in Geophysics, 2011). The longest US record was claimed for many years to be Key West (Maul and Martin, GRL, 1993) but I guess now one should also mention Boston (Talke, JGR, 2018).

line 49 - the PSMSL should be referenced by its web site and journal (http://www.psmsl.org and Holgate et al., J Coastal Res, 2013)

line 50 - give the names of the five (see above)

line 89 - these references should also include the IOC Manuals, see http://www.psmsl.org/train_and_info/training/manuals/

line 125 - a reference is needed for where you got the Pleistocene surface information from.

line 169 - 'because all tide gauge benchmarks'. This is not true, see above.

line 172 - I would be grateful if you did not use the word 'eustatic' which means different things to different people (there is a recommendation about this in one of the IPCC reports). I suggest this is reworded:

... deep subsidence plus the component of RSLR associated with changes in real ocean level.

(or something like that). And drop 'as well .... effects'.

line 186 - I would reword:

... includes subsidence of that part of ... sediments deeper than the BM depth.

line 207 - reword to avoid eustatic:

.. adding the historic rate of real (geocentric) sea-level rise ..

There is a reference to Ericson et al. (2006) in the context of not using tide-gauge data. But the sea level rise value in that paper was just the global average taken from the IPCC (1.5 mm/yr) which hardly seems to me to be superior to using local tide gauges where available. I realise why Ericson et al. had to do that in their paper but it is not to be recommended in your case.

line 223 - a reference is needed for the InSAR mention, preferably for its use in deltas.

230-233 - SWOT is only one of several efforts to improve coastal altimetry. A general reference, in which there is mention of SWOT, would be:

Vignudelli, S., Kostianoy, A., Cipollini, P and Benveniste, J. (eds). 2011. Coastal altimetry. Berlin: Springer Publishing. 578pp.

237 - 'commonly too noisy'. From what I have read of the method I'm not surprised!

References - need doi's adding

453 - there should be an accent over the 'i' in Miguez

Figure 1 - What are the short and long vertical lines beneath the tide gauge in each panel supposed to be showing. I'd remove them. The point is that the datum of a tide gauge is determined by levelling to the BM nearby, so I would have the horizontal red line for the tide gauge at the same level as the BM and a dotted line between them.

I think this figure may have been adapted from Figure 1 of Webb et al. (2013) which in their case has a short vertical band which I think is supposed to be indicating a float gauge (of which there are fewer around), and a longer vertical band which I think has the same function as the vertical red line for the BM in the present case. Anyhow, please lose the vertical lines under the tide gauges in this case.

Figure 2 and 3 - could the lat/lon ticks and annotation face outside the map to be clearer?

Figure 3 - could the colour scale on the right be labelled Pleistocene Depth and the insert headed FD above PS or similar? As mentioned above, a reference is needed in the caption for the Pleistocene depth information. The black lines for the shoreline are hard to see.

There is no point mentioning ENG1 and 2 if they don't appear on the plot. But perhaps say (ENG1 andd ENG2, see Table 2).

Figure 4 - nice plot.

Figure 6 - put mm/yr after mean, standard deviation

Tables 1 and 2 - head the column 'Maximum benchmark foundation depth (m)'

Figure S1 - I don't see the point of this figure. It has no more information than Figure 6. Doesn't do any harm I guess.

I hope these comments are useful. I have no objection to my identity being revealed. I am very grateful to Peter Stone and Greg Dusek for information which helped me complete this review.

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