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Interactive comment on "Coastal primary productivity changes over the last millennium: a case study from the Skagerrak (North Sea)" *by* Anna Binczewska et al.

Anonymous Referee #1

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General comments

The paper presents a new data set from two high-resolution cores in the Skagerrak, spanning the last 1.1 thousand years, with the purpose of investigating historical changes in productivity. The data itself is a nice contribution to our understanding of recent changes in the ecosystem and forms a good follow-up to previous work from the same group and is very relevant to the topics covered by Biogeosciences. However, I have some concerns about the overall discussion and interpretation of the data. Additional data is not needed, but the existing data should be more carefully reconsidered. In particular, the main conclusions about changes in productivity appear to rely closely

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on only one part of the data set i.e. the benthic foraminifera records. The authors identify three periods of different productivity based on some good analyses of the changes in benthic assemblages, but then appear to try to match each of the other records (TOC, δ13C, Mg/Ca, δ18O, Mn/Ca) to these three periods, which in many cases does not seem appropriate based on the figures provided. For example, in description of the Mg/Ca results, the authors highlight high temperatures between 900 and 1200 AD, I suppose because 1200 AD is where they have identified a benthic change. However, in my opinion, for the core EMB046/10-4GC this period does not stand out, and for the core EMB046/20-3GC the temperatures seen in the earlier period continue until perhaps 1400. More careful analysis and description of this and the other records should be provided. In addition, some of the speculation regarding sources of water/nutrients to the area (particularly the Atlantic Water) needs some more thought, and could be improved by trying some additional calculations based on the existing data. Uncertainties in the data need to be better characterised in the text and figures. Also, I think the authors could use the introduction to better highlight the importance of their work for a reader not familiar with the Skagerrak.

Specific Substantial Comments

Motivation

As a reader unfamiliar with the Skagerrak, I felt that more was needed in the introduction to explain why the work is important and to highlight the interest of the work to a wider audience. There are references to changes in the ecosystem, but it would help the reader a lot to know more specifically why changes in production are important to the region (e.g. fisheries etc). Perhaps put this motivation up front before the more technical introduction.

Uncertainty

The uncertainties on each record need to be better characterised throughout the paper. In particular, uncertainties should be quoted for all methodologies, and the level of

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confidence indicated. At present it is unclear whether the errors that are quoted are at 1 or 2 sigma, and this makes a huge difference. It would be helpful to have a bar showing the uncertainty beside each record in the figures as well.

Pg 5 Line 20: What is the TOC uncertainty? Even though it is published elsewhere it wouldn't take much space to include it here. Just a sentence to say uncertainty is 0.01 % (confidence).

Pg 6 Line 3: What is the level of confidence for these uncertainties? And are they long term precision or just the precision for the runs?

Pg 6 Lines 19-27: Discussion of errors needs to be clearer in this section. Line 19: what is the 0.016 long term precision measured on (I assume a standard material) and what is it's Mg/Ca value? What is the confidence level? Line 20: Join these two sentences together (e.g. 'show a good reproducibility, with the pooled. . .'. Line 22: This is ok but the authors still need the confidence level. Lines 25-27: I would like to see an estimate of the typical temperature uncertainty obtained when propagating Mg/Ca ratios through the calibration equation here. According to the Hasenfratz paper, the 2σ uncertainty ranges from $\pm$ 0.6 to 2 oC depending on the temperature. I guess we might be looking at $\pm$ 1.5 oC, and most of the wiggles in Figure 3 might fit within these uncertainty bounds. The authors really need to make sure that the moving average record is real and not just random scatter.

Pg 6 Line 28: I am less concerned that errors on counts would be a big problem for the results, especially for those with the biggest signals. However, it would still be nice to have an estimate of the counting uncertainty for each species. Ie. Replicate a sample several times how close can you repeat the counts? There are also some more formal ways to estimate counting uncertainty. See e.g. (Heslop et al. 2011, Diagnosing the uncertainty of taxa relative abundances derived from count data, Marine Micropaleontology 79, pp 114-120).

Pg 7 Line 10: It would be useful to know what the typical age uncertainty for a given

depth might be for both cores. The period after 1900 looks well-constrained by Hg, but radiocarbon can be difficult to use to match two cores exactly. However, I don't think the age model should affect your results too much. My main concern would be in saying things like 'between 1550 and 1650 one core shows this while the other core shows that.' Can the cores really be associated to that level of accuracy?

Describing the records in terms of the three periods

This was my main area of concern when reading through the paper. Firstly, I note that the three time periods loosely correspond to different sampling resolutions (i.e. relatively high resolution for the early and late periods and relatively low resolution for the middle period). Could this be affecting where lines were drawn? I don't think it is a big problem but it could be worth some investigation or a note in the text.

Secondly, as I noted above, the most convincing case for separating the record into three periods seems to be the faunal data. If I were to ignore those data and to try to describe the temperature, TOC, δ13C and Mn/Ca I would not find the patterns described in the text. The description of the temperature signal does not seem very objective. I gave one example above, and I note again how the record for EMB046/10-4GC doesn't seem to have any period that is very different, especially once you include the temperature uncertainty. I can see a period of low temperature for the other core between maybe 1450 and 1650-1700, but I do not see why we would distinguish the first 200 years from the next 500. The temperature and oxygen isotope records need a more thorough and objective analysis of whether they do actually match the faunal data, especially as the authors use such a match to argue for changes in water mass driving the faunal changes.

On Pg 12, Line 20 the authors describe the TOC as low, during the period of low productivity. However, it is higher than in the first period (argued to be moderately productive) and where the authors have also argued for lower oxygen concentration based on Mn/Ca. I find it hard to see how you could have less production, more bottom water

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oxygen and yet higher TOC if TOC is being considered as a productivity proxy. How about the possibility that the sediment TOC gets less down the core simply because it is decaying over time?

Influence of different water masses

Given the issues I have outlined above, the inferences that different water masses are influencing the sites at different time (eg. Atlantic Water contributing to moderate productivity during the early period), are not well-supported. For example, the authors argue that warmer conditions (from Mg/Ca and δ18O) during the 900-1200 period can be explained by increased Atlantic Water. Firstly, I suggest that the authors need to robustly test their trends with appropriate errors as outlined above. Secondly, the arguments based on temperature (especially in this section, Pg 11) are split by a section on carbon isotopes, which confuses the flow of the paper. There seem to be two arguments for temperature change: increased Atlantic Water influence and generally high temperature during the MWP. I think the authors do combine these arguments by reference to NAO changes, but the link is not made very clear. In addition, the authors should at least address why the temperature in core EMB046/20-3GC remains high well into their second time period, when the productivity has already decreased, which they overall argue is a partly a response to less AW. There is some discussion of complicating factors that might affect the temperature/productivity link, but it is not very clear and should be restructured. To solidify the link to increased AW I would suggest using the Mg/Ca and δ18O to reconstruct the δ18O of the deep water (e.g. salinity), because the δ18O of the shells also has a temperature signal. Knowing the water δ18O might provide a more useful indicator of water provenance and is standard practice in most paleoceanographic work, although the uncertainty might again be very high in this case. The references used to support the conclusions here seem appropriate, and they may be the simplest and best way to argue that the changes seen are do in part to water masses.

δ13C records

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The authors attempt to put the δ13C records into the context of their productivity results, and they note correctly that the late increase in δ13C is due to the Suess effect. However, given that changes in water masses are invoked to explain changes in productivity, I would expect to see some analysis regarding whether their δ13C changes could be explained this way, rather than by changing productivity at the sites. I think the authors correctly conclude that the δ13C records do not correspond to the productivity records. Therefore, I would recommend removing much of the δ13C discussion, simplifying it to a sentence or two saying that not much coherence is seen. I also think the Suess effect is well known enough that such a long paragraph from Pg 16 line 10 is not really warranted. Cutting out the δ13C discussion would also help streamline the paper.

Specific Minor Comments

Pg 3 Line 6: Note that in general, not all organic matter makes it to the seafloor (in fact most doesn't), and much of it is remineralised in the water column. If this situation is different in the Skagerrak the authors should say so and why.

Pg 3 Line 9: Sentence reads a little strangely. Consider rephrasing to something like 'Via photosynthesis, primary producers can help to remove CO2 from the atmosphere as part of the biological pump'.

Pg 3 Line 11: Change to 'likely negatively impact'.

Pg 3 Line 13: Sentence needs rephrasing. Need to specify 'export of organic matter' rather than 'production'.

Pg 3 Line 18: Include Figure 1 reference here to guide readers who don't know anything about the Skagerrak.

Pg 3 Line 20: How does increased air-sea gas exchange lead to increased nutrients? Perhaps expand a little.

Pg 3 Line 23: 'positive impacts on growth'. Growth rate? Growth magnitude? Be more

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careful about saying ‘positive impacts’. Do you mean positive in the sense of being ‘good’, or simply ‘more’?

Pg 3 Line 24: Similar to above, what is a negative change in trophic levels? Fewer trophic levels? This needs to be more specific.

Pg 4 Line 7: At the first introduction of the Kattegat, provide reference to Figure 1.

Pg 4 Line 9: It might be better to say that ‘in the past the NAO has represented one of the leading modes of natural climate variability over the North Atlantic’. Or remove the word ‘natural’ altogether. I think it may now be difficult to say whether the NAO is wholly ‘natural’ anymore.

Pg 4 Line 12: Specify what you mean by BCE.

Pg 4 Line 26: ‘governed by’ could be more specific. How does the sill control the AW?

Pg 5 Line 2: ‘internal’ might not be the best word here. Internal to what? Perhaps say ‘In addition to the mean circulation, processes such as. . .’ Also as a general point, it helps the reader if all the references can be at the ends of sentences where possible. It is possible in this instance.

Pg 5 Line 16: There are two sentences here that are almost the same. Consider rephrasing to avoid repetition.

Pg 5 Line 25: Avoid the use of ‘proved’ (or proven). I think it is fine to just say ‘potential’.

Pg 6 Line 1: What grade of methanol was used?

Pg 6 Line 1: ‘For each measurement 2-4 specimens were used.’ How many measurements were done for each depth? Are 2-4 specimens enough to get a robust average isotope signal?

Pg 6 Line 10: I think the authors could be more specific about the grade of HNO3 used. What Ca concentration were the final solutions? Were these matched to the standard

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concentrations?

Pg 6 Line 14: These results do not indicate 'no contamination', only 'no systematic contamination'. The authors are correct in the following sentence where they indicate that because of their high Mn/Ca values, contamination may be an issue. No need for the sentence on Line 16, as it repeats what was already said.

Pg 6 Line 18: 'After every 8 samples'? 'For' might not be specific enough.

Pg 6 Line 18: Which standard solutions were used?

Pg 6 Line 28: 'The foraminiferal faunal analysis'

Pg 7 Line 14: Include 'IntCal' in the wording somewhere here.

Pg 7 Line 21: What is the detection limit for the TOC measurements? I think the results look fine but might be worth checking.

Pg 7 Line 26: Here is an instance where knowing what the relative age uncertainty when comparing the different cores could be important. 1550-1650 is not a very long time period in terms of typical radiocarbon uncertainties.

Pg 7 Line 28: To me 'long-term trends' implies a gradual change to different conditions over the whole record. Instead these records are quite flat until the Suess effect kicks in. I might say 'long-term variations'.

Pg 8 Line 6: Same comment as above but now for oxygen isotopes.

Pg 8 Line 26: I know that identifying planktonic forams is very difficult at the tiny sizes looked at here. I agree with the author's decision to discuss only the total number rather than species. Do the benthic ID's suffer from the same difficulty. I have less benthic experience to offer a full comment here.

Pg 9 Line 7: What is meant by 'less fluctuating'? Apart from one very large peak in 20-3GC the records look quite similar. Are the standard deviations very different?

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Pg 9 Line 9: How does changes in mass accumulation affect the results? When looking at forams per gram, two things can change the magnitude: 1) number of forams and 2) grams of sediment. Can the authors rule out changes in mass accumulation rate with their age model?

Pg 9 Line 18: This statement seems quite broad. Several of the low abundance benthics do not seem to show any trends (perhaps because of the counting uncertainty?). Some of them show different trends. I think B. skagerrakensis can be highlight more here, because it is really the main record that shows any large and consistent changes.

Pg 10 Line 6: This first sentence needs rephrasing. What is meant by 'quality'? Again avoid 'proven'. Is it the individual species or the 'factors' that have an association with organic matter? I don't think the wording makes it clear.

Pg 10 Line 15: The following 3 sections should be rewritten taking into account the substantial comments above, taking more care over the uncertainties in each record, and appreciating more that e.g. temperature and oxygen isotopes don't necessarily match the benthic productivity records.

Abstract and conclusion: Can be looked at again after more careful consideration of the data.

Figure 2: n g-1 I assume to mean number per gram. It could be misunderstood as 'per nanogram' so perhaps change to 'Number g-1'.

Figure 4: For the uninitiated like me, 'Factor loading' doesn't mean very much as an axis title. Perhaps explain a bit more in the text.

Technical Corrections

There are a large number of instances where the language could be tightened up in the paper that would help make it easier to read. I would suggest having the paper thoroughly proof read again. Previous papers from some of the same authors read well. I have put a very few points below.

Pg 5 Line 29: Change ‘since’ to ‘because’. I have had it pointed out to me before that ‘since’ can be a confusing word in a paper dealing with the past.

Pg 6 Line 12: Rephrase sentence to ‘Fe/Ca and Mn/Ca ratios show no correlation with measured Mg/Ca values.’

Pg 6 Line 26: Hasenfratz miss-spelled.

Pg 9 Line 8: Change ‘sticking out’ to something sounding more scientific.

Figure 3 caption: ‘5-point’ not ‘5-points’

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