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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "*Lingulodinium machaerophorum* expansion over the last centuries in the Caspian Sea reflects global warming" *by* S. A. G. Leroy et al.

Anonymous Referee #1

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General comments

Unfortunately I cannot agree with the main conclusion of this paper, being that Lingulodinium machaerophorum abundance increases are mainly related to changes in sea surface temperature, and this on its turn related to global change. There are several reasons I consider this incorrect: a. there are several parameters that affect the abundances of this species in the sediments, being amongst others temperature, salinity and nutrients (Lewis and Hallett 1997; see Marret and Zonneveld 2003 for a summary). The paper does not sufficiently investigate all these parameters and mainly speculates that temperature changes are the main causal factor – even if these temperature changes are quite small indeed. Other factors such as preservation are not discussed. b. There is no proof for a causal link between temperature changes and abundances of the cysts in the Caspian Sea. This is needed to reach the main conclusion. I don't consider the surface sediment data convincing data for this. This could be proven however by e.g. culture experiments, where other factors such as salinity and nutrients should also be investigated in a similar matter. However, I would suspect that such a link would prove to be much more complicated by these other factors. Also, there is no clear relationship shown – i.e. regression - between the historical temperature record and cyst abundances, which could confirm such a causal relationship. c. There is no clear evidence for higher abundances of Lingulodinium machaerophorum cysts in warm-water conditions of globally distributed surface sediments (see e.g. Zonneveld et al. 2012). Like most dinoflagellate cysts, such a relation between temperature and abundance is likely unimodal (e.g. Dale and Dale 2002), which further complicates matters. I did find this paper by Leroy and co-authors relatively well-written. However, it does present too much data to reach the conclusions - there is a lot of superfluous data. I also did not find the graphs transparent and easy to read. Also, raw data is missing. In conclusion, I cannot recommend this paper for publication in Biogeosciences. It would set a precedent for similar work that relies too much on speculation and not hard data. Please find some specific and technical comments underneath that could be useful to the authors.

Specific comments

p. 1666 6

"The use of dinocyst-inferred reconstruction of past water parameters, such as temperature,salinity, and nutrient levels, is a powerful palaeoenvironmental approach that may contribute to understanding many of the problems listed above (Marret and Zonneveld, 2003)." I don't agree: first it is unclear what is meant here – why would this be powerful? – and second I am not convinced this statement is actually true. True, dinocyst assemblages provide semi-quantitative indications of changes in water conditions but is that equal to a powerful palaeoenvironmental approach? “Lm which is a euryhaline coastal planktonic species restricted to regions with summer temperatures above 10–12 _C (Marret et al., 2004) and winter temperatures above 0 _C (Marret and Zonneveld, 2003; Lewis and Hallett 1997)”

These references are incorrect. Although Marret et al. 2004 does mention such summer temperatures, this paper does not present such data. The only paper that mentions such temperatures is Lewis and Hallett 1997, p. 134 and this should be the only and correct reference in my opinion. Furthermore, Lewis and Hallett 1997 considered the summer temperature most important as they considered this the main season of the bloom. This should be made explicit.

The correct name for the motile stage is Lingulodinium polyedra (Stein 1883) Dodge 1989.

“Its motile form, Lingulodinium polyedra (Stein) Dodge 1989, is reported to cause harmful algal blooms (Howard et al., 2009)”

This is incorrect because Howard et al. 2009 is a paper about phylogenetics of yessotoxin producers and did not study blooms. It is still quite disputed whether this species causes harmful blooms (see Lewis and Hallett 1997, p110, for instance).

Technical corrections

p. 16666

The correct name for the motile stage is Lingulodinium polyedra (Stein 1883) Dodge 1989.

p. 16667 “four sites” – as these have not been specified yet, this is quite confusing. This is also the case on p. 16669 p. 16669 “The invasion of the comb jelly Mnemiopsis leidyi in the late 1990s caused a drop in zooplankton, which in turn favoured phytoplankton. But other factors such as overfishing, eutrophication and climatic change may also have played a role.”

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There should be a reference following this statement. I assume this is also the Kideys paper? It is unclear as it is.

p. 16671

change “genus” to “genera”

p. 16672

“organiccarbon free” – correct

p. 16673

l. 2 How did the flocculation take place – with boiling? How long were the acid treatments?

l. 9 reformulate “at the same time” l. 10-11 It is incorrect to group round brown cysts as Brigantedinium - this genus is used for species with a (sub)polygonal archeopyle (Reid 1977). The name “round brown cysts” should be used instead. l.23 it would be beneficial if briefly could be explained what form A, B and ss are.

p. 16674

l. 4 reference to Fig. 5 is wrong – should be Fig. 3

Tables

Table A1

The formatting of this table can be improved. There needs to be more consistency. Also, how can a grab be for phytoplankton purposes?

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