

Interactive comment on “The influence of Atlantic climate variability on the long-term development of Mediterranean cold-water coral mounds (Alboran Sea, Melilla Mound Field)” by Robin Fentimen et al.

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Reply to Referee #2

Manuscript title: The influence of Atlantic climate variability on the long-term development of Mediterranean cold-water coral mounds (Alboran Sea, Melilla Mound Field)

submitted to Climate of the Past

response by: Robin Fentimen et al.

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In the following document, the responses to the comments made by Referee #2 are addressed one by one.

General comment

Comment Referee #2: the interpretation of the X-ray fluorescence (XRF) is a bit problematic for the production of decisive environmental interpretations, considering that the principal aim of this study is to define the climatic processes susceptible of affecting the coral mound formation. I suggest the addition of some figures and more importantly, to either tonedown some of these interpretations, or if possible, to add (or compare to) more records that support the environmental reconstructions presented in this article.

Response: We agree with Referee #2 that interpretations linked to the XRF records need to toned down. This is also in agreement with the points raised by Reviewer #1 (see comment and reply). Following these comments the section from Lines 581 to 602 has been deleted in the new version of the manuscript since the XRF data did not sufficiently support the interpretations made. Furthermore, we agree that down toning interpretations linked to the XRF in other parts of the manuscript is needed.

Regarding additional figures, the manuscript is already figure-rich. In addition, the comments and suggestions of Reviewer #1 (and also Reviewer #2, e.g. foraminiferal plates) will require the addition of extra three figures: 1. adding the CT visualization of fragments bigger than 2 cm; 2. adding the Van Krevelen diagram (see reply to Referee #1); 3. adding a plate illustrating the most important foraminifera species. As such, we consider that the manuscript will hopefully be adequately illustrated (see below for specific comments).

Specific comments

Reviewer comment: In the introduction it might widen the scope of the manuscript to add that the selected coral mound is (strategically) located at the interface between

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different water masses as showed in fig. 2.2. It is later an important aspect of the discussion.

Response: Comment integrated in the revised version of the manuscript.

Reviewer comment: The Oceanography section could be improved by describing water masses from shallower to deeper, if possible. More Calls to the figures in this section would be helpful. It would be helpful to add in fig.2 the water masses flowing directions (crosses and dots, in and out of the paper for example). Could you please elaborate whether Alboran Sea gyres strengths and structures display seasonal changes, as you mention that they are non-permanent?

Response: Modifications made in the revised version of the manuscript. Details concerning the seasonal changes to the Alboran Sea gyres have been added.

Reviewer comment: Concerning the benthic foraminifera faunal assemblages in section 3.5, I was wondering whether you checked the 63-150 μm fraction? By doing so, you could (qualitatively at least) assess if there is a bias on small species (e.g. the opportunistic specie *Epistominella exigua*)?

Response: The fraction 63-125 μm (we used a 125 μm mesh) was not investigated in this study. It was intended to exclude the smaller forms which are more likely to be displaced by bottom currents (e.g. Lutze and Colbourn, 1984) which govern cold-water coral environments. Moreover, the inclusion of the finer fraction would make the data less comparable to other important benthic foraminiferal studies in the area (e.g. Schönfeld, 2002; Milker & Schmiedl, 2012; Stalder et al., 2015; 2018; Fentimen et al., 2020). However, we do agree that this approach has its drawbacks, noticeably the underestimation of smaller opportunistic species, and we strongly advocate for the investigation of the finer fraction in areas with weaker bottom currents and for the living assemblages. The authors have considered this methodological point in a study of the Moira Mounds - NE Atlantic CWC mounds (Fentimen et al., 2020 Marine Micropal). Taking into account the pros and cons of integrating the finer fraction (63-125 μm), we

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decided to only focus on the larger fraction for this precise setting (this approach was also used in a high energy setting by Schönfeld (1997).

Reviewer comment: Also, you did not mention on which literature you based your foraminifera species identifications? This should be added in the material and methods.

Response: The identifications were based on a selection of benthic foraminiferal atlases (thus cross-referencing), essentially: Jones (1994), Murray (2003), Margreth (2010) and Milker and Schmiedl (2012). This has been added to the Material and Methods. A full list of the literature used for identifying foraminifera species (+ the original description of the given species) has been added as Annex 2 (see attached file).

Reviewer comment: I am aware of the difficulties due to the inconstant depositional processes in this area (which is also a problem in this study but I won't insist on it as you dealt with it fairly in the manuscript), but did you estimate the accumulation rates of benthic foraminifera (BFAR), and compared it to the foraminiferal density and TOC?

Response: We did estimate the BFAR (see attached Annex 1 and Supp. Figure 1) but decided to not include it in the manuscript. We decided to avoid using the BFAR since in such environments, which show intermittent sedimentation and erosive events, we believe it to be an untrustworthy proxy (see response to reviewer 1).

Reviewer comment: It would be very helpful to add pictures of the benthic foraminifera cited in the text, especially those selected for any geochemical measurements. Providing pictures should be generalized in the literature as confusions persist.

Response: We agree with the Reviewer that adding a plate with the most abundant foraminifera species would avoid potential confusions and allow comparison to other studies (and identifications, since these may indeed vary slightly from one person to another). Our identifications are in agreement with Jones (1994), Murray (2003), Margreth (2010) and Milker and Schmiedl (2012) (the literature used; see response to the

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comment below and the newly added Annex 2), that clearly illustrate these species. For this reason, and since the manuscript is already figure-rich, we had first decided not to add a plate. However we agree that adding a plate is important to document our identifications so this will be done in the revised version.

Reviewer comment: I don't have access to the Annex, but you could add there a reference list to the original description of each foraminifera specie, at least for those cited in the main text.

Response: This has been added in Annex 2 (in the same table as the list of literature used for identifications). Annex 1 (foram counts) has also been attached to the reply as it was previously missing, we apologize for the inconvenience.

Reviewer comment: It is hard to get decisive and conclusive interpretation from the TOC, according to the curve presented in fig. 4. It would be interesting to compare this curve with the BFAR. In any case, the interpretation should be toned down.

Response: We agree with the Reviewer that these interpretations need to be toned down (also in agreement with Reviewer 1's comments). A curve comparing TOC to the BFAR is attached "Fig. BFAR _ TOC". This can be added as Supp. Material to the manuscript if wished. However, BFAR in cold-water coral environments is biased by bottom current dynamics and may rather reflect hiatuses. Also, as mentioned in the reply to Reviewer 1, a number of micropaleontological studies have pointed out the reasons why the BFAR is potentially biased (see the review Jorissen et al., 2007). Noticeably, Naidu and Malmgren (1995) showed that in low oxygen environments, BFAR does not reflect surface-water productivity. Since we suspect that the seafloor at BR1 was at times depleted in oxygen, we further avoided to use the BFAR as a productivity proxy. Moreover, taphonomic processes, which directly impact BFAR, are not well constrained (see for example Murray, 2006; Stefanoudis et al., 2017; Capotondi et al., 2020; Fentimen et al., 2020).

Reviewer comment: If available, other proxies of primary productivity changes would

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be appreciable. If the sieved samples are still available, the fastest (but not the best) way to try check this would be to count the planktonic foraminifera and calculate their accumulation rates?

Response: This approach would indeed be the fastest but as mentioned, not the best. Indeed, the accumulation rate of planktonic foraminifera is considerably biased in such settings by the strong currents affecting the area (see also response to comment above). We believe that the accumulation rate of planktonic foraminifera would probably reflect sorting by bottom currents rather than productivity (see for example, Fentimen et al., 2020, Marine Micropal) and as such should be avoided. For this study we had started assessing planktonic foraminiferal assemblages but decided against further investigations for the following reasons: (1) planktonic foraminifera are more likely to be allochthonous than benthic foraminifera, especially considering the setting, and (2) planktonic foraminifera from the study site were concentrated essentially within the smaller sized material (63-150 μm), thus further increasing the probability of a high contribution of allochthonous foraminifera.

Reviewer comment: Section 4.3: You only show the section 1 in the figure, will the other sections be added in supplementary? This would be interesting to illustrate your description line 320.

Response: Other sections will be added to the manuscript (also in agreement with the comment by Reviewer 1) together with the CT visualization of fragments bigger than 2 cm. We are currently working on producing this.

Reviewer comment: I agree that Zr/Al and Rb/Al differ from Ti/Al and Si/Al but mostly in the fact that the first two are harder to interpret than the later. Unfortunately, I am not sure that any conclusive strong interpretation can be extracted from these curves.

Response: The interpretations linked to the XRF will be toned down, as previously suggested. We aim to use this dataset rather as supporting information for the macrofaunal and microfaunal assemblages. We will make this clearer in the revised version

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of the manuscript.

Reviewer comment: In the discussion it would be very helpful to have a figure showing TOC + benthic forams assemblages (at least the species that have a “significant ecological meaning”) + $\delta^{13}\text{C}$, especially to illustrate the discussion lines 555 - 560.

Response: We agree with this, however this was not done to avoid overloading Figure 7 which already contains a lot of information. Thus we would prefer not to combine or present a new figure, especially since the manuscript already contains quite some figures (+ the ones that will be added following both reviewers' comments: see reply general comment and reply to reviewer 1).

Reviewer comment: Line 412-413: You only described *B. spathulata* as opportunistic (line 394), but you used the plural form “taxa”. Do you consider all the fresh organic matter-feeding species as opportunistic?

Response: Indeed this is not clearly stated in the sentence, precision is added in the revised version. Indeed, we consider *B. spathulata* as opportunistic but also *Bulimina* spp., following the observations made by Eichler et al. (2014) or Lutze and Coulbourn (1984).

Reviewer comment: Also, it has been suggested that relatively small shelled species that rely on fresh organic matter show a faster (/stronger?) response to seasonality changes (Fontanier et al., 2006). Are there any signs of *E. exigua* in the samples?

Response: There was no *E. exigua* in the samples. There were very scarce occurrences of *Alabaminella weddellensis* (a species sharing the same ecology, i.e. responding rapidly to periods of increased phytodetritus input). See also attached Annex 1. We agree that *E. exigua* (and other opportunistic phytodetritus-feeding species) are generally small and are essentially found in the smaller fraction (63-125 μm), so variations in this species abundance are possibly missed. Other studies at BR 1 did not either report the presence of *Epistominella exigua* and *E. vitrea* (Stalder et al., 2015;

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2018). This question and response go together with the discussion to integrate or not the finer fraction (63 - 125 μm) - see above, previous comment.

Reviewer comment: Since we are in the seasonality topic, are there any past and present evidences of seasonal changes affecting the surface stratification in the area and also the primary productivity? This would be the right place in the article to discuss this topic, and eventually support the benthic foraminifera findings (especially for what is observed at MIS 5 and 7). This addition would be a solution to balance the ““weakness”” of the XRF trends. You could also add a figure showing XRF + benthic forams for the runoff hypothesis.

Response: Primary productivity in the Alboran Sea is controlled by a number of variables: the formation of Western Mediterranean Deep Water in the Gulf of Lions which would itself be influenced by varying atmospheric conditions (for studies on the topic, see for example Ausin et al. (2014; 2015) and references therein). Moreover, the influence of entering Atlantic Water (which enters as a jet at the Strait of Gibraltar) on primary productivity is also important and is subject to seasonal changes (the strength of the jet at the Strait of Gibraltar will have an effect on the strength of both Western and Eastern Alboran Gyres). For literature, see for example: Heburn and La Violette (1990), Oguz et al. (2014). So seasonal changes do indeed affect surface stratification at the study site. However, considering the location of BR 1, the benthic foraminiferal assemblages, TOC (see Van Krevelen diagram attached) and at a lesser extent XRF results, we believe that BR 1 is essentially impacted by variations in terrestrial input, and secondly by water mass rearrangements (see discussion section 5.1.2). Again we would prefer to avoid adding an extra figure for the reasons already mentioned above (i.e. high number of figures already presented in the manuscript).

Reviewer comment: Is there any occurrence of deep infaunal and/or dysoxic species such as Globobulimina spp, Chilostomella, etc ?

Response: Deep infaunal species can be considered rare. The most abundant deep

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infaunal species is Chilostomella oolina (max. abundance 5%, av. abundance ca. 1 to 2 %). Globobulimids are even less abundant (max. abundance 2 %, av. abundance approximately 0.5 %). See Annex 1 (attached). In order to estimate oxygen content variation, we used the formula proposed by Schmiedl et al. (2003): $(OH / (OH + LO) + Div) * 0.5$, with OH = relative abundance of high oxygen indicators (e.g. Cibicides pachyderma, Gyroidina orbicularis, Hanzawaia boueana, Lenticulina spp., Pyrgo spp., Quinqueloculina spp., and Sigmoilopsis schlumbergeri), LO = relative abundance of low oxygen indicators (Bolivina spp., Bulimina spp., Cassidulina carinata, Chilostomella oolina, Globobulimina spp., Melonis barleeanus, Nonionella turgida, Praeglobobulimina ovata, Trifarina spp., and Uvigerina spp.) and Div = normalized benthic foraminifera diversity. We decided however not to include this in the manuscript since it essentially reflects the abundance of buliminids, and was hence redundant.

Reviewer comment: Section 5.1.2 It is very difficult to see systematic interglacial SS increases supporting seafloor turbulence. Also, the abundances of *T. angulosa* which is often associated with strong bottom water energy do not support this here. The steady fluvial increase during MIS 5 suggested by SS figure 8 might be plausible, but it is still hard to observe a systematic strong glacial/interglacial signal.

Response: We agree with the Reviewer that this statement is misleading and does not match with the the foraminiferal assemblages. The sentence has thus been reworked in the revised version of the manuscript. The sentence now reads: "This would promote the formation of internal waves and would have favoured coral proliferation by increasing lateral food availability (Fig. 10)". This is also in better agreement with the title of the section.

Reviewer comment: The second half of section 5.1.1 is well argumented. But I still have questions about the fate of these runoffs. How can we be sure that this "material" is not displaced laterally by the currents, away from the studied area? Line 621: I am not sure that there is enough arguments in the discussion to dismiss water mass rearrangements yet.

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Response: The results of the RockEval analyses indicate that the origin of the organic matter preserved in the sediment at Brittlestar Ridge 1 is of terrestrial origin. The newly added figure (Oxygen Index vs. Hydrogen Index diagram, see attached) demonstrates this. If the material resulting from terrestrial run offs were to be displaced laterally by currents, one would expect the signal of the organic matter preserved at BR 1 to be rather marine in origin. This is not the case. The statement Line 621 is not intended to dismiss or exclude water mass rearrangements, it rather suggests that they are of secondary importance at BR 1 when compared to fluvial input. This is especially true in comparison to other CWC environments, for example the extensively studied CWC mounds in the Northeast Atlantic (Irish margin), where water mass rearrangements are believed to drive almost exclusively cold-water coral growth dynamics.

Reviewer comment: Line 646: What about seasonality changes?

Response: No seasonality changes were documented in this study, so we cannot make any conclusions about these (nor can we confirm anything).

Reviewer comment: For the conclusion and also as a conclusive remark for all the manuscript; I suggest toning down the terrestrial inputs implications as the XRF data far from being clear enough to give decisive interpretations. I also suggest keeping the door open to other processes such as water mass reorganization or maybe the impact of primary productivity changes due to seasonality variations (affecting the gyres?).

Response: We agree that the XRF data needs to be toned down (as mentioned previously and in the reply to Reviewer 1). However the conclusion that fluvial input plays a decisive role in coral development is especially based is also and especially supported by benthic foraminiferal assemblages (see section 5.1.1). The influence of water mass rearrangements is also highlighted in the conclusion (e.g. "Increased fluvial organic matter inputs are driven by the increased impact of warm and moist Atlantic air masses with intensified Western and Eastern Alboran Gyres that lead to more important turnover between surface and intermediate water masses. This phenomenon

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is promoted by enhanced Modified Atlantic Water inflow at the Strait of Gibraltar"). We do not exclude in this final section the effect of water mass rearrangements and do not develop the impact of seasonality variations since we have little indications about this in this study. Hence we would prefer to keep the conclusion brief and as such.

Other suggestions

Reviewer comment: Lines 24 – 36 I would suggest a reorganization of the second half of the abstract, as it feels that information are randomly presented, which might confuse some readers.

Response: This has been reworked in the revised version of the manuscript.

Reviewer comment: Line 134 isn't it northwest instead of northeast that the MAW enters the Alboran Sea?

Response: Indeed, this has been corrected.

Reviewer comment: Line 226 I think that it is important to mention in section 3.5 the error on the benthic foraminifera relative percentages. With ~ 300 specimens counted, variations of less than $\pm 5\%$ are not very trustworthy. For more statistics of the sorts you can check and cite Patterson and Fishbein, 1989 and Fatela and Taborda, 2002.

Response: This has been mentioned in the latest version of the manuscript.

Reviewer comment: Lines 300 – 304 are a bit too interpretative and should be placed and developed in the discussion.

Response: We agree with the Reviewer. This has been placed and further developed in the discussion (in addition to the OI vs. HI diagram which illustrates this statement, see attached figure).

Reviewer comment: Line 309 The sentence "This trend is mirrored in GS (Fig. 4)" is not useful as you are describing both SS and GS starting at line 306. - I think that you could place figure 5 in supplementary information, and put the sentences lines 310-

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313; "The percentage of sortable silt (SS%) increases with...and SS% is indicative of a sorting process induced by bottom currents (Fig. 5)", in the discussion, to support your (toned-down) interpretations.

Response: We agree with the Reviewer on both these points. Figure 5 has been moved to the Annexes (making room for other figures, as stated in the general comment). Reviewer comment: Line 323, is it possible to indicate quantitatively the dominance of *B. dichotoma* ?

Response: Yes this is possible and has been added in the revised version of the manuscript. *B. dichotoma* makes up for over 95% of all counted bryozoans.

Reviewer comment: Could sentences lines 327-329 be simplified by just saying that bryozoan and coral content is generally inversely correlated?

Response: This could be done but we believe that it would possibly be a case of over simplification. Coral and bryozoan content are indeed anti-correlated during MIS 5 and MIS 2 but the distribution pattern of both organisms does not always follow such a pattern (see for example MIS 6).

Reviewer comment: Lines 346onward, it would be helpful to display the mean percentages of each species within the Bulimina grouping. Just out of curiosity, is the offset between the 3 species relatively stable down-core? I would also displace the diversity sentences (lines 343 – 345) to the end of the 4.4 section.

Response: The mean percentages of Buliminid species can be found in Annex 1. The most abundant species is *B. marginata*, followed by *B. striata* and then *B. aculeata*. *B. aculeata* shows the strongest offset with the two other species (noticeably during the last glacial). Moving lines 343 - 345 to end of the section has been done in the revised version of the manuscript.

Reviewer comment: Line 359 I think you meant *T. angulosa* which is the one showing ~30 % abundance during MIS 6. I can't see 30 % for *D. coronata* during MIS 4 in

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figure 7. I advise to recheck the description of this figure in general.

Response: This sentence needs indeed to be corrected. *D. coronata* reaches ca. 20 % during MIS 4. Corrections have been made.

Reviewer comment: Section 4.5 The Holocene is not mentioned, yet it is among the periods showing the most changes.

Response: We rather chose to highlight the changes at the transition between the last glacial and the Holocene: “The passage from MIS 2 to MIS 1 is marked by a sharp decrease in planktonic and benthic $\delta^{13}\text{C}$ (from -1.2 ‰ to -2.2 ‰ and from 1.8 ‰ to 1.0 ‰ respectively)”. We chose not to insist too much on the Holocene in this study as this core is not the best suited to study this time interval at BR 1 (cores studied by Fink et al., 2013; Stalder et al., 2015; 2018 are better examples).

Reviewer comment: Line 398 I would replace “support” by “suggest”. As mentioned before it would be better to tone down the interpretation. It is also a good spot in the text to put the origin of the TOC.

Response: We agree with the Reviewer. This needs to be toned down and the origin of the organic matter (OI vs. HI diagram) integrated at this stage of the discussion.

Reviewer comment: Line 428 could you please precise where these inputs occur?

Response: Precision added.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2020-82>, 2020.

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Species list and quantitative data of benthic foraminifera from core MD13-3462G.

Depth (cm)	2	12	22	32	42	52	62	72	82	92	102	112	122	132	142	152
Sum counts	582	438	413	620	616	639	434	394	341	374	315	282	301	332	314	341
Split	16	4	4	2	8	16	4	16	32	16	64	128	64	128	128	64
Total (Sum counts x Split)	9312	1752	1652	1240	4928	10224	1736	6304	10912	5984	20160	36096	19264	42496	40192	21824
Fraction weight (g)	0.8	1.07	0.86	0.32	0.76	2.39	0.62	1.69	1.46	1.01	5.01	6.47	3.04	10.85	10.38	10.32
Foraminifera/g	11640	1637	1921	3875	6484	4278	2800	3730	7474	5925	4024	5579	6337	3917	3872	2115
<i>Adelosina laevigata</i>	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
<i>Alabaminella weddellensis</i>	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
<i>Ammonia beccarii</i>	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0
<i>Amphistegina lessonii</i>	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
<i>Amphycoena scalaris</i>	6	14	8	0	1	0	0	0	0	0	0	1	0	0	0	0
<i>Anomalinaoides globulus</i>	0	0	0	0	0	0	1	0	0	0	0	1	0	1	1	1
<i>Astronion antarcticus</i>	0	0	1	2	0	0	0	0	0	0	1	0	0	0	0	0
<i>Astronion gallowayi</i>	0	0	0	3	0	0	2	0	0	0	0	0	0	0	0	0
<i>Astronion stelligerum</i>	0	0	0	22	0	7	19	29	6	0	17	15	1	2	12	0
<i>Bigenerina nodosaria</i>	1	3	0	0	0	0	0	0	0	1	0	0	0	0	0	0
<i>Biloculinella depressa</i>	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
<i>Biloculinella inflata</i>	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0	0
<i>Biloculinella lobata</i>	0	1	1	1	0	1	0	0	1	0	0	0	0	0	0	0
<i>Bolivina obliqua</i>	5	2	2	1	5	2	0	0	0	0	4	0	0	2	3	3
<i>Bolivina difformis</i>	2	2	1	0	0	0	0	0	0	0	0	0	0	0	0	0
<i>Bolivina pseudoplicata</i>	0	0	2	1	3	0	1	2	1	1	3	1	0	0	0	0
<i>Bolivina spathulata</i>	1	3	19	0	9	4	4	4	10	37	7	3	4	2	6	6
<i>Bolivina spinescens</i>	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0
<i>Bolivina striatula</i>	2	2	3	6	9	11	12	13	3	0	8	2	13	12	5	4
<i>Bolivina subspinosca</i>	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
<i>Bolivina variabilis</i>	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
<i>Bulimina aculeata</i>	10	5	11	7	8	16	1	4	0	2	3	4	4	7	12	10
<i>Bulimina marginata</i>	23	44	31	3	5	4	2	2	2	8	3	2	0	2	1	1
<i>Bulimina striata</i>	7	20	27	3	1	0	0	0	0	0	0	0	0	0	0	0
<i>Cancris auricula</i>	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
<i>Cassidulina carinata</i>	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0
<i>Cassidulina crassa</i>	6	18	10	2	22	8	2	2	9	2	3	4	4	8	3	3
<i>Cassidulina laevigata</i>	52	17	32	96	60	68	45	42	61	20	34	23	48	43	33	66
<i>Cassidulina reniforme</i>	0	3	0	2	4	0	0	4	4	1	0	0	0	0	0	0
<i>Cassidulinaoides bradyi</i>	2	1	2	0	0	2	0	0	0	0	1	1	0	1	0	0
<i>Chilosiphonella oolina</i>	2	8	6	0	0	0	0	0	0	0	0	0	0	0	0	0
<i>Cibicides aravaensis</i>	3	17	10	1	10	6	1	2	6	2	0	1	0	2	2	0

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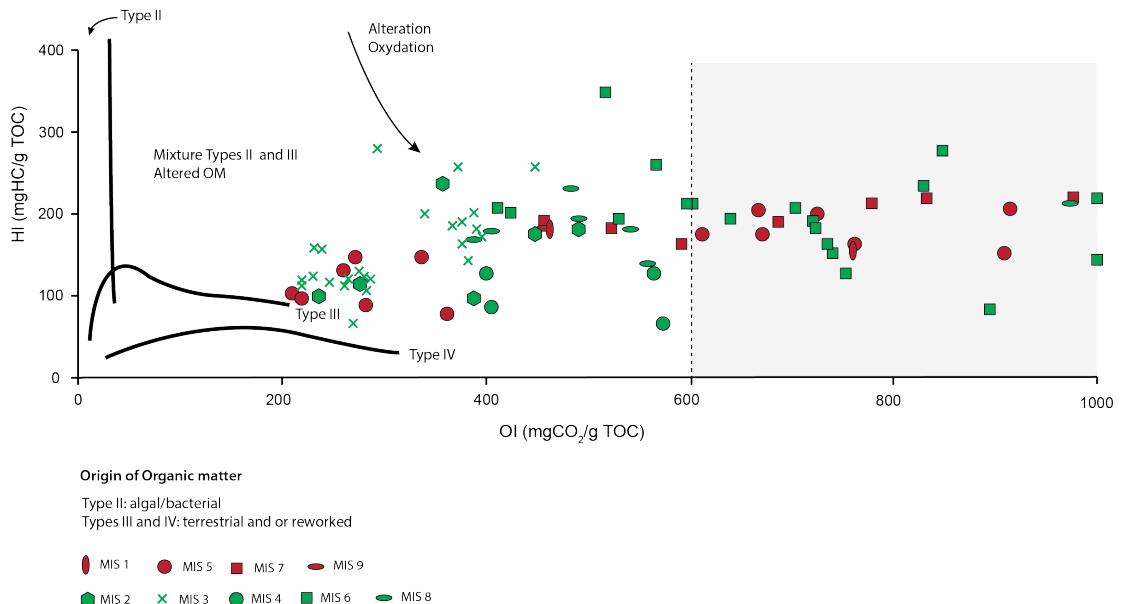
Annex 1. List of all benthic foraminfera species identified in this thesis, together with the references used for identification (see below for reference list).

Species	Original name	References used for identification
<i>Adelosina laevigata</i> d'Orbigny, 1826	<i>Adelosina laevigata</i> d'Orbigny, 1826	Milker and Schmiedl, 2012 (Fig. 12, 18-19)
<i>Alabaminella weddellensis</i> (Earland, 1936)	<i>Eponides weddellensis</i> Earland, 1936	Erden and Schönfeld, 2017 (Fig. 8, 24); Setoyama and Kaminski, 2015 (Fig. 5, 3)
<i>Ammonia beccarii</i> (Linnaeus, 1758)	<i>Nutilus beccarii</i> Linnaeus, 1758	Milker and Schmiedl, 2012 (Fig. 27, 1-2)
<i>Amphistegina lessonii</i> d'Orbigny, 1826	<i>Amphistegina lessonii</i> d'Orbigny, 1826	Hottinger, 1993 (Pl. 184, Fig. 1-11)
<i>Amphicyrina scalaris</i> (Batsch, 1791)	<i>Nutilus scalaris</i> Batsch, 1791	Murray, 2003 (Fig. 5, 1); Milker and Schmiedl, 2012 (Fig. 18, 22-25)
<i>Anomalinaea globulosus</i> (Chapman and Parr, 1937)	<i>Anomalinaea globulosus</i> Chapman and Parr, 1937	Margreth, 2010 (Pl. 39, Fig. 1)
<i>Astronion antarcticus</i> Parr, 1950	<i>Astronion antarcticus</i> Parr, 1950	Margreth, 2010 (Pl. 37, Fig. 4)
<i>Astronion gallowayi</i> Loeblich and Tappan, 1953	<i>Astronion gallowayi</i> Loeblich and Tappan, 1953	Margreth, 2010 (Pl. 37, Fig. 3)
<i>Astronion stelligerum</i> (d'Orbigny, 1839)	<i>Nonionina stelligera</i> d'Orbigny, 1839	Cimerman and Langer, 1991 (Pl. 84, Fig. 13-15)
<i>Bigerinera nodosaria</i> d'Orbigny, 1826	<i>Bigerinera nodosaria</i> d'Orbigny, 1826	Margreth, 2010 (Pl. 5, Fig. 5); Milker and Schmiedl, 2012 (Fig. 10, 10-11)
<i>Biloculinella depressa</i> (d'Orbigny, 1826)	<i>Biloculina depressa</i> d'Orbigny, 1826	Margreth, 2010 (Pl. 8, Fig. 3); Murray, 2003 (Fig. 4, 2-3)
<i>Biloculinella globulus</i> (Borremann, 1855)	<i>Biloculina globulus</i> Borremann, 1855	Margreth, 2010 (Pl. 8, Fig. 2); Milker and Schmiedl, 2012 (Fig. 16, 19)
<i>Biloculinella inflata</i> (Wright, 1902)	<i>Biloculina inflata</i> Wright, 1902	Milker and Schmiedl, 2012 (Fig. 16, 20)
<i>Biloculinella labiata</i> (Schlumberger, 1891)	<i>Biloculina labiata</i> Schlumberger, 1891	Milker and Schmiedl, 2012 (Fig. 16, 21-22)
<i>Bolivina alata</i> (Seguenza, 1862)	<i>Vulvulina alata</i> Seguenza, 1862	Margreth, 2010 (Pl. 24, Fig. 1)
<i>Bolivina afformis</i> (Williamson, 1858)	<i>Textularia variabilis</i> var. <i>afformis</i> Williamson, 1858	Margreth, 2010 (Pl. 24, Fig. 6); Milker and Schmiedl, 2012 (Fig. 19, 28-29)
<i>Bolivina pseudoplicata</i> Heron-Allen an Earland, 1930	<i>Bolivina pseudoplicata</i> Heron-Allen an Earland, 1930	Milker and Schmiedl, 2012 (Fig. 19, 22-23); Murray, 2003 (Fig. 5, 17)
<i>Bolivina spathulata</i> (Williamson, 1858)	<i>Textularia variabilis</i> var. <i>spathulata</i> Williamson, 1858	Milker and Schmiedl, 2012 (Fig. 20, 1-2)
<i>Bolivina spinescens</i> Cushman, 1911	<i>Bolivina spinescens</i> Cushman, 1911	Margreth, 2010 (Pl. 24, Fig. 7)
<i>Bolivina striatula</i> Cushman, 1922	<i>Bolivina striatula</i> Cushman, 1922	Margreth, 2010 (Pl. 24, Fig. 5); Milker and Schmiedl, 2012 (Fig. 20, 3)
<i>Bolivina subspinescens</i> Cushman, 1922	<i>Bolivina subspinescens</i> Cushman, 1922	Margreth, 2010 (Pl. 24, Fig. 8); Milker and Schmiedl, 2012 (Fig. 19, 24)
<i>Bolivina variabilis</i> (Williamson, 1858)	<i>Textularia variabilis</i> Williamson, 1858	Milker and Schmiedl, 2012 (Fig. 19, 25-26)
<i>Bulimina aculeata</i> d'Orbigny, 1826	<i>Bulimina aculeata</i> d'Orbigny, 1826	Margreth, 2010 (Pl. 27, Fig. 8); Milker and Schmiedl, 2012 (Fig. 20, 19)
<i>Bulimina marginata</i> d'Orbigny, 1826	<i>Bulimina marginata</i> d'Orbigny, 1826	Milker and Schmiedl, 2012 (Fig. 20, 23); Murray, 2003 (Fig. 6, 4-5)
<i>Bulimina striata</i> d'Orbigny, 1826	<i>Bulimina striata</i> d'Orbigny, 1826	Frontalini et al., 2014 (Fig. 6, 5); Margreth, 2010 (Pl. 27, Fig. 10)
<i>Concis auricula</i> (Fichtel and Moll, 1798)	<i>Nutilus auricula</i> Fichtel and Moll, 1798	Milker and Schmiedl, 2012 (Fig. 21, 14-15); Murray, 2003 (Fig. 6, 6-7)
<i>Cassidulina carinata</i> (Silvestri, 1896)	<i>Cassidulina laevigata</i> var. <i>carinata</i> Silvestri, 1896	Margreth, 2010 (Pl. 25, Fig. 5); Milker and Schmiedl, 2012 (Fig. 20, 5)
<i>Cassidulina crassa</i> d'Orbigny, 1839	<i>Cassidulina crassa</i> d'Orbigny, 1839	Jones, 1994 (Pl. 54, Fig. 4); Margreth, 2010 (Pl. 26, Fig. 3)
<i>Cassidulina laevigata</i> d'Orbigny, 1826	<i>Cassidulina laevigata</i> d'Orbigny, 1826	Margreth, 2010 (Pl. 25, Fig. 4); Murray, 2003 (Fig. 6, 8-10)

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**Fig. 3.**[Printer-friendly version](#)[Discussion paper](#)

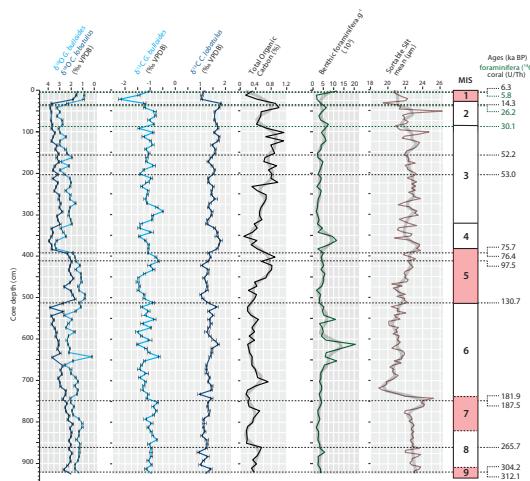
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Fig. 4.