

Author's response to reviewer #2

We are grateful to your comments and useful suggestions that improved our manuscript greatly. As described below, we have revised our manuscript. Please note that the expression in blue colored letters are the ones provided by you whereas those in black are our replies.

### Summary of reviewer's comment

This paper is the key paper to understand the marine siliceous-test bearing Rhizaria in the Arctic Ocean. The result is so interesting that potential readers to Biogeoscience will recognize the value of this manuscript. However, it is unfortunate that this manuscript has many problems: (i) this manuscript has forgotten citing many important references in the Arctic polycystines; (ii) some terminologies are not precise more or less; (iii) discussion includes many unscientific opinions; and (iv) some points leave scope for misunderstanding as an act of injustice. Although I am positive to be published, these four points must be revised for acceptance. I will make comments and suggestions to help the authors accept this manuscript.

### Summary of the comments

#### (i) Insufficient citation of the previous publications

Although the papers regarding on the Arctic polycystines are a few, several important papers are missing. Bernstein (1931, 1932, 1934) and Meunier (1910) are very informative for your study. Dolan et al. (2014) is of particular importance. Dolan et al. (2014) studied the surface water plankton samples from summer 2011 and 2012 in the Chukchi Sea and this paper noted the abundance of radiolarians (*Amphimelissa setosa*) is quite low in 2012, compared with 2011. **You must refer this paper and discuss something in your manuscript because the studied period is overlapped each other.**

Kosobokova et al. (2002) is also much related with your manuscript.

Bernstein 1931 is listing from the Kara Sea: Two Acantharia species, *Amphimelissa setosa*, *Dictyophimus clevei*, *Dictyophimus* sp. *Plectacantha oikiskos?* and *Sticholonche* sp. This does not give us very much information. No quantitative data.

Bernstein 1932 is listing is making comments on: *Amphimelissa setosa*, *Plectacantha oikiskos*, *Dictyophimus gracilipes* (*clevei* is rejected herein), *Dictyophimus tetracanthus* (but not identified properly), *Doralapsis heteropora* (Acantharia), *Sticholonche zanglea*. This does not give us very much information. No quantitative data.

Bernstein 1934 is listing two Acantharia species, *Plectacantha oikiskos*, *Phormacantha hystrix*, *Dictyophimus gracilipes*, *Dictyophimus multispinus*, *Amphimelissa setosa*, *Actinomma leptodermum*, *Sticholonche zanglea*. Still no quantitative data, and not very much information.

Bernstein's three papers does not report on anything important rather than abundant Acantharia and Taxopodia (*Sticholonche zanglea*).

We refer to Bjørklund and Kruglikova (2003) and they refer to Bernstein (1931, 1932, 1934) so we perfectly well know about this literature. However, Bernstein is not a radiolarian specialist and with all the stations and material she has studied, and only listing a handful (VERY FEW) of species and in most cases only finding VERY FEW individuals, it is obvious that her species list, is not optimal, nor is her material. The only significant

information is her 1932 discussion of *D. gracilipes* and her new *D. multispinus* (in 1934). This is also a fact we are reporting on in Bjørklund and Kruglikova (2003) and Bjørklund et al. (2013), as well as in our present MS with many morphological variations of *Dictyophimus*. We do not yet know what this variation means.

Meunier (1910) does not give us any significant information except a short note on two new species: *Amphimelissa stenostoma* and *Sticholonche ventricosa*. No quantitative data and not very much information to use.

The three Bernstein papers and the one by Meunier have been added to our text, but no information of any significance can be extracted from these papers. We have not used the *A. stenostoma* concept as there are transitional forms in addition to the forms with round versus reticulate pores as discussed by Bjørklund and Swanberg 1987. Meunier mention that his *A. (Bortyostrobos) setosa* is approximately similar to Jørgensen fig 83 on plate XVI. There is not at all any similarity as the species Meunier refer to is *Lithomelissa setosa*. *Amphimelissa setosa* is on the other hand illustrated on pl XVIII fig 109 a-b. So this Meunier paper is not very helpful or informative for our study. However, they have now been included. Thanks for reviewer.

We inserted the following text in page 16649 between lines 21 and 22:

“Bernstein (1931, 1932, 1934) reported on six Polycystina, two Acantharia and two Taxopodia species, but did not give any information on abundance in the Barents Sea and Kara Sea for the Polycystina, but for the Acantharia and Taxopodia she reported them to be abundant, with a maximum occurrence in the deeper and warmer Atlantic water. Meunier (1910) also reported on Acantharia, Taxopodia and Nassellaria in the Kara Sea and the Arctic Ocean, but he stated (page 196) that his material was not rich in radiolarians.”

Dolan et al. (2014).

Thanks for your good recommendation.

Please see our response to your comment 5-10, 5-12, 5-23, 5-27.

Kosobokova et al. (2002)

Please see our response to your comment 1-2.

## **(ii) Some terminologies are not precise more or less**

### **(ii)-a “Radiolaria**

As the authors said, the term “Radiolaria” is problematic. The author used the term “radiolaria” which includes Phaeodaria (p. 16652, Lines 1- 3: To avoid complications...”, but this treatment has no scientific reason. Rather than, this still makes confusions to readers.

“Radiolaria” include not only polycystines but also Acantharia and Taxopodia. Furthermore, the term “Radiolaria” traditionally include the cercozoan Phaeodaria, or had been simply equal to Phaeodaria or Collodaria, in regardless of taxonomic long distance from polycystine. The different concept of Radiolaria for plankton studies has lead serious confusion among them, but polycystine, Acantharia, Taxopodia, and Phaeodaria **MUST BE CLEARLY** separated each since they have quite different ecology. **This clarification is important to your manuscript.** Bernstein (1931) reported abundant taxopods from 200–355 m water depths at Station 28 (75°24'30"N, 63 °59'E) and abundant acantharians from almost all the stations examined by Bernstein. As early as 1900's, Meunier (1910) also reported Acantharia and Taxopodia in the Kara and Barents Sea.

Thus, I strongly recommend to you that you MUST use “radiolarian polycystines and phaeodarians”, “Polycystina and Phaeodaria”, or “marine siliceous Rhizaria” See Suzuki & Aita (2011).

What the reviewer is suggesting does not look very good in our eyes:

- 1) “radiolarian polycystines and phaeodarians” If anything: polycystine radiolarians and phaeodarians
- 2) “Polycystina and Phaeodaria” Suzuki & Aita (2011) reject Polycystina as a taxon name.
- 3) “marine siliceous Rhizaria” Can be used like this: marine siliceous Rhizaria (Spumellaria, Nassellaria and Phaeodaria)

Thanks for your comment.

According to your comment, we refer to Suzuki and Aita (2011) and inserted the following text in page 16649 between lines 4 and 5 (1 Introduction):

“In our study we have analyzed only the siliceous forms of class Rhizaria and herein we have used the definition of Radiolaria as defined by Suzuki and Aita (2011). In their taxonomic scheme they include the following orders: Collodaria, Nassellaria, Spumellaria, Acantharia and Taxopodia. In addition we do include order Entactinaria which Suzuki and Aita (2011) reported getting extinct during the Permian, but Bjørklund et al. (2008) demonstrated its presence also in recent plankton and sediment samples. In this study we have excluded order Acantharia as they have a skeleton of SrSO<sub>4</sub> and Collodaria, a group that normally do not possess a skeleton or only with loose spines. Therefore, our study only includes forms with a solid skeleton of SiO<sub>2</sub>. In this paper we have chosen to include data also on order Phaeodaria which have not been assigned to Radiolaria but to Cercozoa in recent studies using molecular biology (Cavalier-Smith and Chao, 2003; Nikolaev et al., 2004; Adl et al., 2005; Yuasa et al., 2005). To make the text read well we therefore use Radiolaria, or radiolarians when appropriate, to also include Phaeodaria, this to make it possible for us to compare already published data from the north Pacific region (Okazaki et al., 2003, 2005; Ikenoue et al., 2010, 2012a).”

#### (ii)-b living radiolarians and dead radiolarians

This manuscript regarded the cells stained with Rose-Bengal as “living radiolarians”, but it is not precise. As Rose Bengal simply stains the cytoplasm of cells, the dead cell which still keeps unconsumed cytoplasm can be dyed red as well. In particular, the cytoplasm of dead cells may not dissolve in water columns because of very cool Arctic sea waters. Thus, you need to separate “living cells” from “dead cells with cytoplasm.” **However, it is practically difficult to do such things**, you need to add some careful implications throughout the text. In an opposite manner, some living cells cannot be stained with Rose Bengal. What did you treat these cases in your manu? **For added explanations, please refer to p. 262 of Okazaki et al. (2004). He carefully wrote as “stained specimens were counted as “Live”, and empty skeletons were counted as “Dead”. We determined that specimens were “Live” if their protoplasm stained clearly, to avoid false staining by other organisms.” Please do not copy and paste this sentence.**

Thanks for your comment.

The siliceous shells can easily be colored red by the bacteria, but then it is the bacteria that have been colored, not the siliceous shells.

According to your comment, we changed the sentence in page 16652, line 4-5 as follows:

“We determined that specimens were “living”, if their protoplasm was stained clearly, this to avoid false staining by other organisms such as bacterial growth).”

**was changed to**

“Plankton tow samples were stained with Rose-Bengal to discriminate between living and dead specimens. Specimens that clearly stained bright red were interpreted as living cells, while cells that did not stain red, or just barely indicated a red shine, were interpreted as dead because of the lacking protoplasm. This is also in accordance to Okazaki et al. (2004).”

**(ii)-c adult and juvenile**

You applied these terms for *Amphimelissa setosa* and Actinommidae for example. What kind of morphotypes was called as “adult” and “juvenile”? **You should define it anywhere in the manuscript.**

According to your comment, we inserted the following text in page 16656, lines between 6 and 7:

“We defined the 2-shelled forms of Actinommidae as juvenile. Then the 3 and 4 shelled forms will be adult. For the *Amphimelissa setosa* we defined those with cephalis only as juveniles. Those with a well developed cephalis and with a barely or well developed thorax are defined as adult.”

**(ii)-d comparative terms**

The authors repeatedly used “warmer”, “colder” and other comparative terms. But, the authors should concerns what kind of images will bring such comparative terms by Biogenesis readers. For example, you wrote “a warm Atlantic species” in the abstract, but this species live in the seawater of **0.5 to 4 °C** according to previous studies. Although this is apparently warmer in the Arctic, but it will be very difficult to figure out without knowledge to the potential authors.

Thanks for your comment.

According to previous studies *C. histricosus* in the Norwegian fjords do live in much higher temperatures. We do not have data here but we will be surprised if this species is not living in the upper water layers of Sognefjorden in temperatures up to 7 °C and probably higher. But this we cannot confirm now.

This is a comment we do not agree in. The warm water species you refer to is *C. histricosus* and this is normally a “warm water species” compared to other species living in the Arctic Ocean. However, the only real “cold water” species we have is *A. setosa*, all the other Arctic Ocean species (except *A. turidae* and *A. georgii* and some other forms) are also basically found in the Norwegian Sea and should therefore also be “warm water” species. Therefore, somewhere in our text we could state something like: As also reported by Itaki et al. (2003) we support the idea that *C. histricosus* is a new intruder in the PWW- water in the Chukchi Sea area.

Please also see our response to your comment 0-2, 5-11, 5-13, 5-19.

**(iii) Discussion includes many unscientific opinions**

When I carefully read the manuscript, I found may the intentions with ambiguous evidences, inappropriate reasons, and mistakes with insufficient review of the already published papers throughout the manuscript. Although I welcome attractive hypothesis and presumptions, I cannot connive the logically unsupported intentions.

(iii)-a the fear of artificial high diversity and endemism in the Arctic Ocean **As much is known to biologists and taxonomists, the diversity is significantly and artificially controlled by different taxonomic concepts. The artificial endemism is also created depended on the published years of new taxa.** Although you intention about the high diversity and strong endemism in the Arctic Ocean might be true, I have nothing to say that you manuscript is inevitably affected with your discussion. First of all, all the specialists with the exception of your group identified the adult *Actinomma* as only two species (*Actinomma boreale* group and *Actinomma leptodermum* group, rarely *Actinomma leptodermum longispinum*). They generally add the word “group” so that their identification gets together variable morphotypes of *Actinomma*. On the other hand, you group separated these 2 species into 7 taxa (*Act. boreale*, *Act. geogeri*, *Act. l. leptodermum*, *Act. l. longispinum*, *Act. truidae*, *Actinomma* sp. morphotype A, and *Actinomma* sp. morphotype B). Published years of new taxa is apparently effected to your discussion. *Act. geogeri* and *Act. turgidae* were described in Kruglikova et al. (2009), and the new genus *Joergensenium* was described in Bjørklund et al. (2008). As the authors also well recognize, **there are many un-illustrated undescribed species to *Actinomma* and *Joergensenium* in the North Pacific.** Under such circumstances, nobody say whether your opinion in the higher diversity and endemism is correct or not. At least, the absence of *Joergensenium* in the North Pacific is apparently wrong. **You should add the comment as “Our opinion is, however, needed to be tested with re-examination of *Actinomma*-specimens in the North Pacific and is also awaited to describe *Joergensenium* species in other regions.”**

Dear reviewer, we hope you are aware that Dr. Kruglikova has been working with radiolarians in the North Pacific since the 1960’s. Can we then assume that she more or less do know the radiolarian fauna in this area? Can we assume that she has enough material from several stations in the North Pacific? When she published on *A. georgii* and *A. turidae* (to mention only these two) she stated in the description that these two species had not been observed outside the Arctic Ocean. They are not so far observed in the Norwegian Sea proper, but *A. turidae* is found in the Cleve plankton material but in tows from ca 2600-0m, probably in the deep part, as it was not found in Cleve’s shallow hauls. Nowhere in the World Ocean do we have a concentration of Actinommidae as high as in the deep polar basins, 70-90%. Nowhere do we have such a high variability in the skeletal shapes and forms as in the Arctic Ocean. This has been stated in Bjørklund and Kruglikova (2003) paper and is further expressed herein. The forms *Actinomma* sp. morphotype A, and *Actinomma* sp. morphotype B are both found in the Chukchi Sea material only, not in the material the lead author worked up from the North Pacific and the southern Bering Sea. In other words they are by us, in the material we have available, only found in the Arctic Ocean proper, and that is why we define them as endemic to the Arctic Ocean.

The reviewer say: At least, the absence of *Joergensenium* in the North Pacific is apparently wrong.

As far as we can understand we have in our MS never stated that *Joergensenium* does not exist in the North Pacific, but we state that *Joergensenium* sp. A and *Joergensenium* sp. B (not described yet but in progress) is only found in the Arctic Ocean. The lead author has **not** observed this species in his sediment trap material from the North Pacific and southern Bering Sea. Also we see some other *rare* (very few) forms of *Joergensenium* in our material, also supportive to our statement that there is a big variability among the actinommid, and similarly also among the entactinarians, in the Arctic Ocean.

(iii)-b the origin of the Arctic polycystine species



It is interesting because the people who studied the North Atlantic tends to say the origin from the North Atlantic (Petrushevskaya, 1979; Kruglikova, 1999) while those who studied the North Pacific said the North Pacific origin to the Arctic species (Motoyama, 1997, Mar Micropal, 30, p. 45–63 ; Matul & Abelmann, 2005). However, you only cited the papers in the North Atlantic origin. You discarded the North Pacific origin hypothesis by the absence of *Stylochlamydidium venustum* (in Atlas of Boltovskoy et al 2010, this species is listed at 3 stations with 1-3% in plankton above 150m. We regard this a shallow.) and *Ceratocyrtis borealis* (in Atlas of Boltovskoy et al 2010, this species is listed at 5 stations with 5-10% in plankton above 150m, we regard this a shallow.) in the Arctic, but this is not a good reason because these two species are deep-water species (they are not) which cannot pass through the shallow Bering Strait. The origin of the Arctic species should be discussed with the shallow-water species which potentially can pass through the Bering Strait. In addition, each species can be derived from the North Atlantic or the North Pacific, or the both. **As your paper does not focus on the origin of the Arctic species, unconcluded opinions are better not to be used in your manuscript as much as possible.**

We are saying that the present day Arctic Ocean radiolarian fauna was introduced after the last Glaciation, or very early in the Holocene (Kruglikova et al 2009). We are of the opinion that very few radiolarian elements are at present being introduced to the Arctic Ocean from the Pacific. To our knowledge we have no Pacific radiolarian species in the Chukchi Sea fauna that has established a local population.

If you as a reviewer tell us that the two above species are “deep water dwellers”, then we have different opinions on what deep water really is. To us plankton collected at depth shallower than 150 m represent shallow water. When one species occur with >10% above 150m, then we tend to believe that this is good enough evidence for a very limited RECENT transport of Pacific polycystins into the Arctic Ocean via the Bering Strait. Your reference to Matul and Abelman (2005) (*Amphimelisa setosa*) is definitely a shallow water species, but its migration is not a recent event.

#### (iii)-c Presumptions about food preferences to each taxa

The authors tried to determine food preferences of your concerned polycystine taxa. I can agree about “ice-algae” and other ice-organisms in ice as a source of food to the polycystines, but the author should take care on the point that it does not directly imply phytoplankton feeder or the abundance of the polycystines is controlled by the abundance of phytoplankton. The ice-organisms in ice are the importance source of organic matter in principal. If you want to insist on your herbivorous hypothesis, two kinds of data are essential: (i) The seasonal change of chlorophyll a and (ii) the sediment trap data where your concerned polycystines increase and decrease. Without these data, imprudent imagine should not be said, avoiding from unscientific confusion.

According to your comment, we revised our manuscript.

Please see our response to your comment 5-12, 5-14, 5-15, 5-23, 5-25.

#### (iv) Some points leave scope for misunderstanding as an act of injustice

##### (iv)-a Title

I believe you did it by accident, the title of your manuscript is very similar to that of Dolan et al. (2014). Dolan et al. (2014) published the Arctic radiolarians and tintinnids entitled “Microzooplankton in a warm Arctic: a comparison of tintinnids and radiolarians from summer 2011 and 2012 in the Chukuchi Sea” (Acta Protozoologica, 53: 101 – 113). In consideration with Dolan (2014), **the word “microzooplankton” in your title is too general**

**than your objects. Thus, the term “microzooplankton” must be deleted from the title at least.**

One of substitute titles is “Flux variations and vertical distributions of **Polycystina and Phaeodaria (marine siliceous Rhizaria)** in the western Arctic Ocean: environmental indices in a warming Arctic.” Please consider it.

According to your comment, we changed the title as follows:

Our current title: Flux variations and vertical distributions of microzooplankton (Radiolaria) in the western Arctic Ocean: environmental indices in a warming Arctic

**was changed to**

New title: “Flux variations and vertical distributions of siliceous Rhizaria (Radiolaria and Phaeodaria) in the western Arctic Ocean: indices of environmental changes”

Please also see our response to your summary comment (ii)-a Radiolaria.

(iv)-b Insufficient citation

I was also much surprised but the nearly identical important sentence and interpretations have been already clearly written in previous paper (Itaki et al., 2003). Itaki et al. (2003, p. 1519, Right column, Lines 23 – 25) wrote “No information on *C. historicosus* was reported from many plankton samples **from the Canadian Basin in the 1950s and 1960s (Hülsemann, 1963; Tibbs, 1967)**”. On the other hand, you wrote on p.1662, Lines 21–22 as “This species has not been observed in **the Canadian Basin during the 1950s and 1960s (Hülsemann, 1963; Tibbs, 1967)**”. **So, the priority of this notice has Itaki et al (2003) but not you.** This is unallowable because this mention brought the distinguishing discussion in your manuscript. **It is better for the authors to check such mistakes throughout the manuscript.**

Thanks for your comment.

This can be fixed by adding a reference after our statement. We had no intention to take this as our observation as that was Itaki. Please see our response to comment 5-16.

## **Reviewer’s suggestion**

### **Detailed comments**

#### **0. Title and abstract**

##### **Comment 0-1. Title**

Avoiding from unexpected doubt, I suggest a substitute tile such as “**Flux variations and vertical distributions of Polycystina and Phaeodaria (marine siliceous Rhizaria) in the western Arctic Ocean: environmental indices in a warming Arctic**”

Thanks for your suggestion.

Please see our response to your summary comment (iv)-a Title above.

##### **Comment 0-2. Abstract**

The sentences about *Ceratocyrtis historicosus* will bring a misunderstanding to readers. The authors said “**a warm Atlantic water species**”, but this mention is wrong. First of all, this species favors on the seawater of **0.5°C to 4°C** (Itaki et al., 2003). Can you say “a warm” species, cannot you? The second point is “Atlantic water species.”

Thanks for your comment.

We deleted the text about *Ceratocyrtis histicosus* in Abstract but we discussed this species in section 5.3.3 Upper AW association.

Please see our comment below and also see our response to your comment 5-16, 5-17, 5-18, 5-19, 5-20.

The reviewer must know that the intention with our text is to say that this species is originating from the Norwegian Sea and has been transported by the “warm Atlantic water”. Therefore its presence in the Chukchi Sea (0.5°C to 4°C) is not the temperature, which this species favors, but it has adapted to this temperature in the Chukchi Sea. In the Norwegian coastal water where the temperature at summer is significantly warmer and in the fjords, where it is even warmer, this species is present in low numbers at temperatures up to at least 7°C (Swanberg and Bjørklund 1987), and may be even higher.

According to Takahashi & Honjo (1981), these species was trapped in the 988 and 3755 m water depths in **the equatorial Atlantic Ocean**.

Please look on his plate. This is not the real *C. histicosus*. as defined by Jørgensen.

Thus, this species is **NOT a warm species**.

1<sup>st</sup>. This is not *C. histicosus*.

2<sup>nd</sup>. You refer to traps at 988 and 3755 m. What does a trap do? We thought it collected material from the depth above each sampling depth (from 988m to 0m and from 3755m to 0m). By this technique you have no information from where the bugs in the traps do originate from. They may all come from the photic zone or greater depths, we do not know from which depth the collected material lived at.

This species is a **cosmopolitan species**, including the southern oceans. Please see the distribution map of occurrence data shown below. Thus, this is **NOT an Atlantic species**.

Yes the reviewer is correct, this is probably a cosmopolitan species. What we really mean when we say an “Atlantic water species” is that this species is being introduced to the Arctic Ocean through the Fram Strait, and that it is a member of the Norwegian Sea *C. histicosus* population that has been transported northwards by the Atlantic warm water (Gulf Current). We should try to restructure our statements on this.

It is correct as the reviewer #2 state that *C. histicosus* is not only an Atlantic species, but it is still a question for the Arctic Ocean *C. cisticosus* population, from where did they come?

I briefly listed the occurrence points of this species as well.

I made an occurrence list of this species as below.

[North Pacific]

plankton from Vityaz' St. 3518 (27° 12' 3" N - 138° 17' 8" E) by Petrushevskaya (1971a).

surface sediments from China Station (30°30'N, 123°E, the year of 1959) by Tan & Tchang (1976)

Based on the line drawings this is similar to the *C. histicosus* as defined by Jørgensen but we are not 100% sure.

sediments from Stations VS-R-115a, -116b, and -60a by Benson (1983)



Our co-author worked with Benson when he entered his data into Radiolaria.org. They settled on *C. histricosus* but it was different from the original form, but concluded to use this name.

surface sediments from Sample NPNT 17-1 (33° 45' 0" N - 138° 0' 0" E), by Nishimura & Yamauchi (1984a)

This time Nishimura is as far away from *C. histricosus* as it is possible to come (if you refer to her pl. 24 fig 9). This is not even close to the real form and has nothing to do with *Helotholus histricosus* Jørgensen.

[equatorial Pacific]

plankton from RIS St. 52 (14° 1' 0" S - 131° 26' 0" W) by Petrushevskaya (1971a)

Petrushevskaya do know the Norwegian Sea fauna and her identification is probably correct.

Core RC12-66 (2° 37' 0" N - 148° 13' 0" W) by Nigrini & Lombardi (1984)

[Okhotsk]

Specimen on Pl 15 fig 6 does look like *C. histricosus* but it is broken and very doubtful. We are not at all convinced on this identification.

surface sediments from Vityaz' St. 6691 by Kruglikova (1975)

[Indian Ocean]

DSDP 27-262-3 (10° 52' 11.4" S - 123° 50' 46.8" E) by Kling (1977)

On Kling's plate 1, fig 6 another not complete and well preserved specimen is illustrated. Cephalis and upper part of thorax should be furnished with needle shaped spines, none can be seen. This specimen has only the outline of *C. histricosa*, not the characteristic spines. We would not take this identification for granted.

[equatorial Atlantic]

sediment trap at the PARFLUX Mark II, Station E (13° 32' 12" N - 54° 6' 0" W), by Takahashi & Honjo (1981).

What is shown in their plate 7 figs 5-7 is not *Helotholus histricosus* as defined by Jørgensen. What is illustrated here is not even close to the real Norwegian Sea and fjord forms, which we know very well. See Dolven et al 2013 where the Jørgensen type collection is discussed and the real *C. histricosus* specimens are illustrated.

## 1. Introduction

Comment 1-1. p. 16648. Line 15: Particle flux play important roles in the carbon export.

As your manuscript treated not only polycystines but also phaeodarians, **Lampitt et al. (2009) may be cited** if you have no objections and no doubt. If you want to put emphasis on polycystines, this paper is inappropriate for this purpose.

Thanks for your suggestion.

We also treat phaeodarians, but not put emphasis on them, so we don't cite the reference in this study.

Comment 1-2. p. 16648, Line 26-27 Microzooplankton.. a key component of pelagic food webs.

Not only Calbert and Landry (2004). **Kosobokova et al. (2002) is better to be cited** because this paper shows quantitative data of “food” from the gut of a mesopelagic copepods, *Spinocalanus antacticus* above the Lomonosov Ridge, the Arctic Ocean. This is the practical evidence about your mention.

Thanks for your good recommendation. We add this reference.

Comment 1-3. p. 16649, Lines 18-25.

Should refer **Bernstein (1931, 1932, 1934)**. This paper is of particular important to know the vertical distribution of marine protists before the World War II.

**Meunier (1910) may be cited either**, because a new taxopod species is described in the Arctic.

Thanks for your suggestions.

We add these four papers as the references.

These papers do not tell us very much as they are all from the eastern part of the Arctic Ocean (Barents and Kara Seas). We do not talk about Taxopodia and also this one is from the eastern part of the Arctic Ocean. What is more important in Meunier (1910) is that Meunier described a new *Amphimelissa* species. However, we have not separated between these two as it is only possible to separate the two in well-developed specimens. *A. setosa* has a lateral flatten cylindrical skeleton, Meunier’s new species is inward curved at the terminal end. All the in-between forms cannot be identified properly. We have not used Meunier’s species and all forms have been identified as *A. setosa*.

Please see also our response to your summary comment (i).

## **2. Oceanographic setting**

Excellent!

## **3. Materials and methods**

### **3.1 Plankton tow samples**

Comment 3-1. p. 16651, Line 14 CTD

a CTD observation ---> a CTD (**Conductivity Temperature Depth profiler**) observation I know CTD, but readers may not know it.

We agree with your comment. We changed the text as follows:

“a CTD observation”

**was changed to**

“a CTD (Conductivity Temperature Depth profiler) cast”

Comment 3-2. p. 16652, Lines 1 – 3. To avoid complications...

The “to avoid complications” is no scientific reason. If you want get them together, you can select “**marine siliceous-test Rhizaria**” In addition, **this manuscript must note that “Acantharia and Taxopodia did not examined in this study” anywhere else, because they apparently belong to Radiolaria. If you use the term “marine siliceous Rhizaria”, you only note about Taxopodia.**

Thanks for your comment.

We deleted the following text:

Phaeodaria have not been recognized as Radiolaria but as Cercozoa in recent studies using molecular biology (Cavalier-Smith and Chao, 2003; Nikolaev et al., 2004; Adl et al., 2005; Yuasa et al., 2005). To avoid complications we dealt with the phaeodarians as one of the radiolarian groups according to the classical taxonomy (Anderson et al., 2002; Takahashi and Anderson, 2002).

And we inserted a revised explanation in page 16649 between lines 4 and 5 (1 Introduction): Please also see our response to your summary comment (ii)-a Radiolaria.

### 3.4. Taxonomic notes

#### Comment 3-3. Tripodiscium gephyristes

It is like to use the genus Archibursa (Type species: Archibursa tripodiscus Haeckel, 1887, subsequently designated by Campbell, 1954) rather than Tripodiscium. Just suggestion. **This does not constitute the essential point for acceptance.**

Thanks for your suggestion. However, we retain “Tripodiscium gephyristes” as it is.

## 4. Results

### 4.1. Radiolarians collected by plankton tows.

#### Comment 4-1. No collodarians

**MUST comment “No Collodaria have been found” or “We did not concern about skeletonless Collodaria” here. This information also should be added on Section 4.2 “Radiolaria collected by sediment trap”** The presence or absence of visible Collodaria has been a critical issue in the Arctic since the probable Collodaria were detected in an environmental molecular sequence data in the Arctic (See Lovejoy et al., 2006; Lovejoy & Potvin, 2011). Lovejoy et al. (2006) wrongly cited Collodaria as Spumellarida. Please take care about it. Collodaria always harbor algal symbiont so far as known (Suzuki and Aita, 2011), thus the implication of Collodaria will be focused in near future.

According to your comment, we inserted the following text:

Page 16655 between lines 12 and 13 (section 4-1)

“We have observed taxopodians, but they have not been identified according to the two species as defined by Meunier (1910), nor have they been quantified. Furthermore, we have not been able to observe any collodarian individuals although we cannot exclude their presence in the Arctic Ocean (Lovejoy et al., 2006; Lovejoy & Potvin, 2011).”

Page 16657 between lines 15 and 16 (section4-2)

“We have observed taxopodians, but they have not been identified nor quantified. Furthermore, we have not been able to observe any collodarian individuals.”

#### 4.1.1 Standing stock and diversity of radiolaria

Good.

#### 4.1.2 Vertical distribution of radiolarian species and environment

##### Comment 4-2. p. 16656, Lines 11-12.

You must **show criterion for selected 14 species for Biogeoscience readers**, although I can easily understand your criteria by my experience.

According to your suggestion, we add a following sentence:

“The selected taxa were radiolarian taxa with 1% or higher relative abundance through the upper 1000 m of the water column at either of the two stations and with high relative abundance in each water depth.”

#### 4.2.1. Radiolarian flux and diversity in the upper trap

**Comment 4-3. p. 16657, Lines 21-22.**

Prior to document the numerical total radiolarian flux, the author should explain the **strong distinctive seasonality in the total radiolarian flux at the first.**

According to your comment, we changed the text as follows:

“Total radiolarian flux in the upper trap varied from 114 to 14,677 specimens  $\text{m}^{-2} \text{day}^{-1}$  with an annual mean of 2,823 specimens  $\text{m}^{-2} \text{day}^{-1}$  (Fig. 5). The highest fluxes were observed during the beginning of sea-ice cover season (November in 2010 and 2011,  $>10,000$  specimens  $\text{m}^{-2} \text{day}^{-1}$ ). The fluxes were higher during the open water season (August–October in 2011,  $>5,000$  specimens  $\text{m}^{-2} \text{day}^{-1}$ ) and around the end of sea-ice cover season (July–August in 2011,  $>4,000$  specimens  $\text{m}^{-2} \text{day}^{-1}$ ) than those during the sea-ice cover season (December–June, mostly  $<800$  specimens  $\text{m}^{-2} \text{day}^{-1}$ ).”

**was changed to**

“The highest total radiolarian fluxes in the upper trap were observed during the beginning of sea-ice cover season (November in 2010 and 2011,  $>10,000$  specimens  $\text{m}^{-2} \text{day}^{-1}$ ) (Fig. 5). The fluxes were higher during the open water season (August–October in 2011,  $>5,000$  specimens  $\text{m}^{-2} \text{day}^{-1}$ ) and around the end of sea-ice cover season (July–August in 2011,  $>4,000$  specimens  $\text{m}^{-2} \text{day}^{-1}$ ) than during the sea-ice cover season (December–June, mostly  $<800$  specimens  $\text{m}^{-2} \text{day}^{-1}$ ). The fluxes varied from 114 to 14,677 specimens  $\text{m}^{-2} \text{day}^{-1}$  with an annual mean of 2,823 specimens  $\text{m}^{-2} \text{day}^{-1}$ .”

**Comment 4-4 p. 16657 Lines 25 – p. 16658 Line 1.**

Should **show the average** of the total radiolarian flux in the intervals of **August-October in 2011 and December-June in 2012**, because you show the annual mean though your sampling intervals on Line 23, page 16657.

According to your comment, we showed the average of the total radiolarian flux as follows:

“(August–October in 2011,  $> 5000$  specimens  $\text{m}^{-2} \text{day}^{-1}$ )”

**was changed to**

“(August–October in 2011, average, 5,710 specimens  $\text{m}^{-2} \text{day}^{-1}$ )”

“(December–June, mostly  $< 800$  specimens  $\text{m}^{-2} \text{day}^{-1}$ )”

**was changed to**

“(December–June, average in 2011, 944 specimens  $\text{m}^{-2} \text{day}^{-1}$ ; average in 2012, 723 specimens  $\text{m}^{-2} \text{day}^{-1}$ )”

#### 4.2.2 Radiolarian flux and diversity in the lower trap

**Comment 4-5 p. 16658 Lines 18-19**

Should estimate the average of the total radiolarian flux in the intervals of May-September in 2012.

According to your comment, we showed the average of the total radiolarian flux as follows:

“(0–80 specimens  $\text{m}^{-2} \text{day}^{-1}$ ) during May–September in 2012.”

**was changed to**

“(average, 21 specimens  $\text{m}^{-2} \text{day}^{-1}$ ) during May-September in 2012.”

## 5. Discussion

### 5.1 Comparison between Arctic and North Pacific Oceans

Comment 5-1 p. 16659, Line 4. shell-bearing microplankton

Not precise. Lorica-bearing tintinnids show very high diversity and abundance in the Arctic Ocean (see Meunier, 1919, for example). Organic-walled dinoflagellates are also detected from the Arctic as well (Lovejoy and Potvin, 2011). **Should write “mineralized skeletal-bearing microplankton.”** How about planktic foraminifers? Some comment will be needed about it for readers, although the abundance of planktic forams has been reported few in many previous papers.

According to your comment, the following text:

”due to the low productivity of shell-bearing microplankton”

**was changed to**

” due to the low productivity of siliceous and calcareous microplankton”

Comment 5-2 p. 16659 Line 7–9. annual means and Fig. 8

I understand that the annual means are generally shown in these studies, but you need to explain what kind of scientific implication can be shown with the annual means in YOUR DATA. **Although I don’t say to delete the annual means, you must add more reasonable quantitative data, as commented below.** Your data show apparent two abundant seasons and two sparse seasons in a year. As long as you discuss the contribution of biogenic particle flux in the section 5.1 of this manuscript, are the sparse seasons needed to be averaged with abundant seasons? How long does the biogenetic opal flux make contributions to the carbon export in water columns or sea-floor? Six months? A week? **You should carefully consider the efficient duration of your concerned opal biogenetic fluxes.**

I strongly recommend you that you must regard only the flux of the direct efficient duration, calculating becomes more complex:

Procedures as follows:

- (i) The abundant seasons in your concerned locations are decided. By using parametric statistics, the low values out of  $2\sigma$  (for example) are regarded as “less contributing duration”.
- (ii) The intervals of contributing season (duration) are specified by the procedure (i).
- (iii) You calculate the mean in this limited interval. The unit “week” may be better, because the organic carbon of a given opal flux will completely consume with a week.

**I imagine this will reveal a significantly large contributions of polycystines and diatoms in the Arctic than any other North Pacific Ocean.**

According to your comment, we revised the text as follows:

“The biogenic opal collected in this study mainly consisted of radiolarians and diatoms, therefore siliceous skeletons of radiolarians and diatoms might play important role to export biogenic silica to the deep Arctic. Relatively high flux of radiolarians in arctic microplankton might contribute to substantial part of the POC flux.”

**was changed to**

“However the radiolarian fluxes in the upper trap showed an apparent abundant season (July–November) and a sparse season (December–June) in a year, and that the lower trap also showed an extremely low flux during May–September 2012. Therefore we regarded the period when radiolarian fluxes were higher than  $1\sigma$  (3,489: upper trap; 5,675: lower trap) as a contributing period. As a result, the mean of radiolarian fluxes during the contributing period in the western Arctic Ocean showed a higher value (7,344: upper trap; 11,871: lower trap) than at any other stations in the North Pacific Ocean (Table S5). The biogenic opal collected



in this study mainly consisted of radiolarians and diatoms based on our microscopic observations. Other siliceous skeletons (silicoflagellate skeletons, siliceous endoskeleton of dinoflagellate genus *Actiniscus*, chrysophyte cysts, ebridian flagellate, and palmas) are minor components in the same trap samples (Onodera et al., 2014), therefore siliceous skeletons of radiolarians and diatoms might play an important role to export biogenic silica to the deep Arctic. Onodera et al. (2014) also estimated the diatom contribution to POC flux at station NAP, but more than half of the contribution to total POC has not been explained yet. Relatively high flux of radiolarians in arctic microplankton might contribute to a substantial part of the POC flux.”

## 5.2 Characteristic and ongoing speciation...

Comment 5-3 p. 16659 Lines 17 – 19 close affinity to the Atlantic fauna

You need data. Must make a compiled species list to the Bering Sea, Arctic Ocean, Norwegian Sea & Denmark Strait, and Baffin Bay & Davis Strait. And then, the number of overlapped species in the Arctic Ocean with the Pacific and North Atlantic oceans will be documented in the manuscript. **The references MUST BE SELECTED from the papers with ILLUSTRATIONS. Please ignore the papers with wrongly identified taxonomic names.**

The papers on the Arctic oceans are also compiled for this purpose, because you may find extinct species in the Arctic Ocean, although you must take care wrongly identified specimens as well.

We can see many papers talking about the radiolarian fauna in the Bering Sea (Blueford, 1983 sediment) and in the two trap stations (one in the Bering Sea one in the North Pacific; Ikenoue et al., 2012a);, and most lately the Kruglikova et al. (2013) with a detailed list also in the North Pacific and Bering Sea. Those lists are quite different from the species lists from the Norwegian, Greenland and Iceland Seas. This list is well known, and maybe we can just refer here to Bjørklund and Kruglikova (2003) and we also think Itaki et al. (2013) refer to the arctic radiolarian fauna to be of an Atlantic affinity.

Blueford, J. R.: Distribution of Quaternary radiolaria in the Navarin Basin geologic province, Bering Sea. *Deep Sea Research Part A. Oceanographic Research Papers*, 30, 763-781, 1983.

Kruglikova S. B.: Radiolaria-Polycystina from the bottom sediments of the World Ocean as bioindicators of environmental fluctuations. Moscow, GEOS, 2013. - 231 p.

We have never found any extinct polycystine species in the surface sediments of the Arctic Ocean. However, in the Barents Sea some rare individuals can be found. However, we do not know about any papers reporting on reworked or extinct species or specimens in the Arctic Ocean. How can you find extinct radiolarian species in the Arctic Ocean? If you refer to *A. seosa* this is a species that is still living, and one of the dominant species in the Arctic Ocean. Do you have any examples of extinct radiolarian species in the Arctic Ocean?

It is enough here to refer to other peoples conclusions.

We changed the text as follows

“The radiolarian fauna observed in this study of the western Arctic Ocean was found to have a close affinity to the Atlantic fauna, and the family Cannobotryidae and Actinommidiae were dominant in the western Arctic Ocean.”

**was changed to**

“The radiolarian fauna observed in this study was characterized by high dominance of two families, the Cannobotryoidae and the Actinommidae (Fig. 3). *Amphimelissa setosa* is the dominant species, while the actinommids make a species association with a close affinity to the Norwegian Sea fauna. *Amphimelissa setosa* first appeared in the North Pacific and migrated into the North Atlantic through the Arctic Ocean during the Pleistocene interglacial optima (Matul and Abelmann, 2005). This species became extinct in the North Pacific close to the MIS 4/5 boundary (Kruglikova, 1999; Matul et al., 2002; Ikenoue et al., 2011), and has not been observed in recent materials in the North Pacific (Ikenoue et al., 2012a).”

Comment 5-4 p. 16669, Lines 18 – 22. Petrushevskaya (1979).. Bjorklund and Kruglikova (2003)...

This is NOT based on your data. **You must add the discussion BASED ON YOUR DATA.**

No, it is enough here to refer to other peoples conclusions, however we did add a line and refer to our species list in Table 3.

We inserted the following text in page 16659 lines between 22 and 23:

“This is also supported by the species listed in Table 3, they all occur in the Norwegian Sea, except for the taxa that we at present classify as endemic to the Arctic Ocean.”

Comment 5-5 p. 16659, Lines 22 – 25. Inflow... from ... Pacific... negligible...*Stylochlamydidium venustum*, and *Ceratospyris borealis* are absent in the western Arctic Ocean.

**MUST DELTE THIS SENTENCE AND CHANGE EVIDENCES.** This verification is ridiculous. As the deepest point in the Bering Strait is 42 m water depths at the present. Even if the sea level raised in warmer periods than the present such as MIS 5 77-110 ka), MIS 9 (300 – 330 ka), MIS 11 (375-420 ka), and MIS 19, the deeper-water species are primarily unable to intrude into the Arctic Ocean. *Stylochlamydidium venustum* and *Ceratospyris borealis* lives in the 50–100 m and 100–300 m water depths (Okazaki et al., 2005, p. 2252). Okazaki et al (2005) studied the south of the eastern Aleutian Islands, the most adjacent region to the Bering Sea **but not the Okhotsk**, suggesting that these two species live in similar water depths in the Bering Sea. **Thus, these species have never used to prove the no effect of the North Pacific Waters to the Arctic Ocean, unless you have data these two species live in shallower than 42 m water depths in the BERING SEA!**

We changed the text as follows:

”Inflow of radiolarians with waters from the northern part of the Pacific Ocean is probably negligible since the most abundant and typical radiolarian species in the North Pacific such as *Stylochlamydidium venustum*, and *Ceratospyris borealis* are absent in the western Arctic Ocean.”  
**was changed to**

”Inflow of radiolarians with waters from the northern part of the Bering Sea is probably negligible since the most abundant and typical radiolarian species in the recent Bering Sea such as *Stylochlamydidium venustum*, and *Ceratospyris borealis* are absent in the western Arctic Ocean. These two species are surface dwellers in the North Pacific (Tanaka and Takahashi, 2008) and are major species in the recent Bering Sea (Ikenoue et al., 2012a).”

Dear reviewer, you are right in one thing, we do not have to state anything about the inflow of radiolarians with water from the northern Pacific. What we are trying to say is that the fauna in the Chuchi Sea and the Pacific part (western part) of the Arctic Ocean is MAINLY recruited by fauna elements originating from the Norwegian Sea in the Early Holocene and

now being brought around in the Arctic Ocean by the Gulf Stream, or with other words, Atlantic warm water. There are no RECENT typical Pacific/Bering Sea polycystine species that has established a planktonic population in the Chukchi Sea. You refer to Okazaki et al (2005) Table 6 at p. 2252. Do you really believe that the species listed in the Surface dweller column only live in the 50-0 m zone? Do you similarly exclude the species in the second column (100-50m zone) not to live in the 50-0 m zone? According to your reference to Table 6 *Spongostrochus glacialis* should only live in the 50-0m zone. Is this the case? Hülsemann reported this species to be common/abundant at great depth in her material from the Arctic Ocean! As you know, we still lack the evidence that Pacific polycystines have established populations in the Chukchi Sea or elsewhere in the Arctic Ocean. All the species we are listing in Table 3 all occur in the Norwegian Sea, except for two, *Cornutella strylophaena* and *Cornutella longiseta*. However, we know that the Norwegian Current entering the Arctic Ocean through the Fram Strait is rather rich in polycystine species. We do not think there is any doubt that the Arctic Ocean polycystines mainly are being recruited from the Norwegian Sea during early Holocene time. The Norwegian Sea fauna is again being recruited from the North Atlantic.

If you want to say as such, you must show the evidence from the species which live in shallower than 42 m water depths.

We do not know about any paper reporting on living polycystines in the shallow (northeastern) part of the Bering Sea. Also the sediments are barren or at best low in biogenic opal in this part of the Bering Sea. Of the 0.8 Sv of Pacific water flowing into the Arctic Ocean we do not know about any papers reporting on a definitive Pacific polycystine establishment.

Why do you ignore Matul and Abelmann (2005)? This paper said that *Amphimelissa setosa* appeared in the Sea of Okhotsk, and **crossed the Bering Strait at MIS 5e**.

This means that *A. setosa* cannot be transported today, and it is the present day transport we are discussing.

If you read Matul and Abelman (2005), you will see that they refer to “Bjørklund personal communication” that *A. setosa* was observed in DSDP site at MIS 10 time (40-60% *A. setosa*). At MIS 5e *A. setosa* was already established in the North Atlantic. We still miss data on its first occurrence in the North Atlantic though. So far the Pacific has the oldest recorded occurrence.

This means that *Amphimelissa setosa* at least is originated from the North Pacific, differing from Petrushevskaya (1979). **This contradiction MUST BE EXPLAINED in your manuscript if you need to say about the origin of the species in your manuscript.**

How was the radiolarian fauna in the Arctic Ocean during the last glaciation? No data tell us that the Arctic Ocean was holding a radiolarian population. All cores from the Arctic Ocean show barren of opal, the same is in the Norwegian Sea glacial period. After *A. setosa* had been introduced to the Arctic Ocean from the Pacific, this species was then established on the Atlantic side of the Arctic Ocean. On the onset of Holocene the North Atlantic Radiolarian fauna was first observed in the Norwegian Sea at about 12000 14C yrs BP, *A. setosa* was the dominant species in Norwegian Sea Younger Dryas sediments. At the Glacial/Holocene boundary the fauna changed significantly, drop in *A. setosa* and a jump in new species introduced with the Holocene establishment of the warm trans-Atlantic Current, the Gulf

Stream. The first major Fauna shift has been estimated to reach the Fram Strait in early Holocene 9800 14C yrs BP. Many of the species living in the Norwegian Sea and that once in a while is accompanied with fauna following the strong pulses of intruding Atlantic water, does not make it all the way to the most remote places of the Arctic Ocean. Therefore, only a handful of species can adapt to the harsh arctic environments. Those species reaching the Chukchi Sea are essentially all in the Norwegian Sea. The Arctic Ocean radiolarian fauna is today very young in geological terms, and the fauna has adapted accordingly. *Actinomma* has evolved in a special way and new forms have developed. Not necessary to repeat our endemic species, but as far as we can judge, based on the material we have available from the Arctic Ocean, Nordic Sea, Barents Sea and the North Pacific, Sea of Okhotsk and the Bering Sea, we have not observed our endemic species elsewhere than in the Arctic Ocean.

But you are right we do not need to talk about the origin of the species. At least two of the radiolarian workers on this MS feel it is a way to interpret our data.

**Comment 5-6 p. 16659, Lines 25 – p. 16660, Line 11.**

The authors insisted that *Actinomma* morphogroup sp. A, *Actinomma* morphogroup B, *Joergensenium* sp. A have not been reported in other areas in the Arctic Ocean, nor in the North Pacific and in the North Atlantic.” but this is nonsense. (i) **The genus *Joergensenium* was described in the year of 2008 (Bjørklund et al., 2008). As far as I know, NO PAPERS regarding on the Arctic radiolarians, except for Dolan et al. (2014), have been published AFTER to 2008.**

Bjørklund et al 2013 published on about 145 species of which ca 95 had a warmer water origin, the rest of a local boreal-arctic origin, of an Atlantic affinity.

We have not seen these forms in our sediment trap materials from the North Pacific and Bering Sea (Ikenoue et al., 2012a).

How to note the existence of this genus and this species in the previously published references?

Dear reviewer, we do NOT talk about analyzing previous papers! We talk about results from analyzing the material that we ourselves have available from the Arctic Ocean, Nordic Seas, Barents Sea, and the North Pacific, Sea of Okhotsk and the Bering Sea, Again, we do not discuss the genus *Joergensenium* but we discuss two very specific forms, *Joergensenium* sp. A and *Joergensenium* sp. B. These two forms HAVE NOT SO FAR, in our material, been observed outside the Chukchi Sea.

It could have been identified as Entactinaria gen. and sp. indet.

In my personal experience, I often saw *Joergensenium*-specimens in the North Pacific.

Again dear reviewer, what you will call Entactinaria today you earlier probably would classify as one or another kind of Actinommidae. But if you often have seen *Joergensenium*-species (Entactinaria) then you have seen something that Suzuki and Aita (2011) got extinct in the Permian(?). So why have you not call these forms for Entactinaria if you have often seen them? As you know, the central part of Actinommidae (*Actinomma*) is quite different from Entactinaria (*Joergensenium*).

(ii) The second point is that you must not use taxonomically confused groups for this purpose. Except for the papers with Kjell Bjørklund and his colleagues, almost all the papers use the

taxonomic names *Actinomma boreale* group and *Actinomma leptodermum* groups in the North Pacific, and they have never tried to distinguish your *Actinomma* morphogroup sp. A, *Actinomma* morphogroup sp. B, *Actinomma georgii*, and *Actinomma turidae*. The high diversity of actinommids and *Joergensenium* has still be owned by the difference on the taxonomic concepts unless someone try to look for them from the North Pacific and North Atlantic actinommids, although your interpretation is presumed to be true.

If so the “*Actinomma boreale* group” is a garbage can and cannot be used for any ecological interpretations, as we do not know the ecology of the different species that is included in this “group”. The same for “*Actinomma leptodermum* group” There is almost no morphological criteria that you can point on saying that this specimen is this species or that specimen is that species. The only way of a safe separation is via lots of hard work analyzing pictures and making statistical measurements on a whole set of specimens in a sample. All the species in the “*Actinomma boreale* group” and the “*Actinomma leptodermum* group” will make bad paleoecological reconstructions. What about *Actinomma boreale/leptodermum* group in the Norwegian Sea? What kind of ecological resolution do you get by grouping like this? When splitting in Adult *A. boreale* and *A. leptodermum leptodermum* you will see that in the Norwegian Sea the latter has its main population in colder water than the *A. boreale* population. What is included in the north Pacific “*Actinomma boreale* group” and the “*Actinomma leptodermum* group” is not known, but probably they are different from the real forms in the Nordic Sea, the home area from where they were described.

In the North Pacific you will never be able to find these four *Actinomma* species as they are, in our opinion, endemic to the Arctic Ocean. We have looked in relevant material and have not found them.

We changed the title of section 5.2 as follows:

“5.2. *Characteristic and ongoing speciation of radiolarians in the western Arctic Ocean*”  
was changed to

“5.2. *Characteristic and ongoing morphogenesis and speciation of radiolarians in the western Arctic Ocean*”

We changed the text as follows:

“In our results the radiolarian fauna in the western Arctic Ocean were characterized by a wide diversity of the family Actinommidae and high standing stock of *Joergensenium* sp. A in the PWW (Table S6). *Actinomma* morphogroup A (58 specimens), *Actinomma* morphogroup B (57 specimens), *Joergensenium* sp. A (1401 specimens) observed in the western Arctic Ocean in our study have not been reported in other areas in the Arctic Ocean, nor in the North Pacific and in the North Atlantic. Although we could not conclude yet, *Actinomma* morphogroup A and B and *Joergensenium* sp. A might be new species endemic for the western Arctic. Kruglikova et al. (2009) described two new species *Actinomma georgii* and *A. turidae*, and suggested the endemism hypotheses for these two species as a result that radiolarians had been rapidly evolving under the stressful conditions in the Arctic Ocean and that the central Arctic Basin might be the center of an ongoing speciation within the family Actinommidae.”

**was changed to**

“Our results suggest that the radiolarian fauna in the western Arctic Ocean was characterized by a wide morphologic variability in the skeletons within the family Actinommidae and high standing stock of *Joergensenium* sp. A in the PWW (Table S6). *Actinomma* morphogroup A (58 specimens), *Actinomma* morphogroup B (57 specimens), *Joergensenium* sp. A (1,401 specimens), has so far only been observed in the western Arctic Ocean in our study. We have



not seen this species outside the Arctic Ocean, neither in the North Pacific or in the North Atlantic. *Actinomma* morphogroup A and B and *Joergensenium* sp. A might be new species endemic for the western Arctic. Kruglikova et al. (2009) described two new species *Actinomma georgii* and *A. turidae*, and suggested the possibility of endemism for these two species. They also indicated a fifth group *Actinomma* indet. (their fig. 5, p. 32) which probably consists of still several undescribed species. Their argument was that the endemism arose as radiolarians had been rapidly evolving under the stressful conditions in the Arctic Ocean, and that speciation or morphogenesis within the family Actinommidae might be ongoing in the central Arctic Basin.”

Comment 5-7 p. 16660, Lines 11 – 13. Our result might support this hypothesis...

Why? How? You need explanation, in consideration with my comment shown above.

This was no problem for reviewer #1, and obvious not for us either. The many morphological forms and shapes in *Actinomma* and may be in *Joergensenium* too, in the Arctic Ocean, is a result of the stressed ecological conditions. This is not explained but discussed in Kruglikova et al. (2009).

**In our present paper we write, and we do not think we can say very much more at present:** “The reason for radiolarian species speciation in this area is still not understood but we can only speculate that this can be controlled by the harsh environmental stress (Allen and Gilooly, 2006; Kruglikova et al., 2009), particularly the extremely cold water masses under the sea-ice (−1.7°C) and the always-changing quality of the water masses, affected by the inflowing Pacific water.”

Comment 5-8 p. 16660, Lines 15 – 16. Joergensenium .. undescribed species...

What do you want to say?

That in the Arctic Ocean we still have some difficult and undescribed species both in genus *Actinomma* and *Joergensenium*.

As I repeatedly say, this genus was first described in 2008, and nobody tried to check the species belonging to this genus so far. *Joergensenium apollo* describe by Kamikuri (2010) is the only species after the first description of this paper. However, the existence of this genus has been known in many radiolarian specialists but no body illustrated in the publications.

We do not think this is known by many radiolarian specialists. Most 3 and 4 shelled specimens with more than 6 radial spines are dumped into the mysterious *Actinomma*-group, under different and incorrect names. So, what do you want to say? We cannot see anything wrong in our story and statement, nor could reviewer #1. When not present in the North Pacific and the Bering Sea we simply refer to Ikenoue et al (2012a) paper where they discuss the radiolarian fauna in sediment traps. The lead author knows the fauna and when he worked up the Chukchi Sea material and found the new forms, cited by us herein, he had not observed these forms in the trap material from the Bring Sea north the North Pacific. Therefore we claim that these forms are endemic to the Arctic Ocean as they are not found in the Nordic Seas either. Based on the pictures in Kamikuri (2010) our Arctic forms are different.

Comment 5-9 p. 16660, Lines 16 – 17. The reason for ... speciation.. is still not understood...

One of reasons is apparently caused by THE different taxonomic concept and insufficient knowledge on un-illustrated *Joergensenium*-species in the North Pacific.

**In conclusion, no supported your own evidences and reliable fact have been shown in the section 5.2, the reviewer strongly recommend the authors that this section MUST BE DELETED or thoroughly changed with caution.**

Why so? What is the problem? We do not discuss the North Pacific *Joergensenium* forms, nor the different *Actinomma* forms! If reviewer #2 is of the opinion that we cannot discuss these two genera in the Arctic Ocean before we know their occurrence in the North Pacific, how then can the study of radiolaria progress? Is the North Pacific the key area only? No, for the time being we will stick to our story and future work will justify if we are wrong or right. In our previous and present papers we are trying to open up the understanding that *Actinomma* is and has been a trash-can of problematic species. We have shown that from this trash-can you can extract *Joergensenium* as these forms used to be classified as *Actinomma* spp., in other words, a real trash-can. In the Arctic Ocean we have a majority of actinommids in terms of % values in the radiolarian skeletons in the surface sediments and with a high variability of shapes. This is the main argument to state that there is an active and ongoing speciation, or call it morphogenesis if you want, of actinommids in the Arctic Ocean. Our documentation of new forms and shapes are evidence that something special is going on in this area, this special thing we call “morphogenesis” or “speciation”. You do not like our expression “speciation”, we have now also added the term “morphogenesis”, but you cannot reject us to propose that this is how we interpret our data, namely that “morphogenesis” is a result of ecological changes, which again leads some of these forms to succeed being new species through “speciation”.

### **5.3 Vertical distribution**

#### **5.3.1 PSW and PWW association**

**Comment 5-10 p. 16660, Line 24 – 1661 Line 7. Amphimelissa setosa:**

The review about the ecology of *Amphimelissa setosa* is insufficient in your manuscript. Bernstein (1931) noted that this species live in the **-1.68°C to -1.29°C and 34.11 to 34.78 “permils”** in the Arctic Ocean, for example. I think this data is in concordant to the opinion in Matul and Abelmann (2005) (cold and saline) (p. 1661, Line 7). Dolan et al. (2014) also documented that *Amphimelissa setosa* occupies the radiolarian fauna in the Arctic and provides no clear indications of possible differences in microzooplankton prey abundances or compositions. You should make discussion with these previous studies. The important thing is these two papers regard the Arctic Ocean.

Thanks for your comment.

We inserted the following text page 16661 lines between 5 and 6 as follows:

“.....(Itaki et al., 2003). Bernstein (1931) noted that this species live in the cold (-1.68°C to -1.29°C) and saline (34.11 to 34.78) waters in the Arctic Ocean. Matul and Abelmann (2005) also suggested...”

As for Dolan et al. (2014), we refer to it later. Please see our response to your comment 5-12, 5-23, 5-27.

As the taxonomic scheme to *Amphimelissa setosa* is different by authors, you first make sure whether the same morphotype is called as the same species name. *Amphimelissa setosa* in Dolan et al (2014) is identical to that in Bernstein (1931).

Bernstein (1931) refer to Menuir (1910) where another *Amphimelissa* species is described. We do not have this available, but if Menuir is operating with two *Amphimelissa* species, then

Bernstein is either disregarding Menuir's new species or is not able to separate the two. So how can you state that *A. setosa* by Doland et al (2014) is identical to Bernstein (1931)? We guess we all know how to recognize *A. setosa*, but in this study we have not differentiated between the one with round pores and the one with reticulated pores, as defined by Bjørklund and Swanberg.

#### Comment 5-11 Comparative terms

The explanation of this manuscript is ambiguous. What degrees were “warmer temperature than Station 56”, “cold but moderate warm”? (See p. 16660, Line 27). 30 °C? 0.1 °C? **Readers cannot image it as you wish.**

Thanks for your comment.

We revised the text as follows:

“At Station 32, these two water masses exhibited warmer temperature than Station 56; indicating that cold but moderate warm, and well mixed water mass were more favorable for this species than the perennial cold water mass such as PWW (100–250 m).”

**was changed to**

“At Station 32, these two water masses exhibited warmer temperature (about one degree higher at the temperature peak) than Station 56; indicating that cold to moderately warm (-1.2 to 1.6 °C), and well mixed water mass were more favorable for this species than perennial cold water masses such as PWW (100-250 m).”

#### Comment 5-12 p. 16661, Line 1

“**More favorable**” (p. 16661, Line 1) **needs more deep discussion** because Dolan et al. (2014) found the abundance of this species is quite different between 2011 and 2012 (Fig. 3 of Dolan et al., 2014). Your interpretation about the ecology of *Amphimelissa setosa* can explain this paradox or not? You should mention something based on your data.

According to your comment.

We inserted the following text in **page 16661 lines between 1 and 2**:

“According to Dolan et al. (2014), *A. setosa* showed significantly lower abundances with higher chlorophyll *a* concentrations of 2012, the low sea ice year, compared to the year of 2011 with higher sea ice and lower chlorophyll *a* concentrations. Thus, the abundance of phytoplankton protoplasm with the remains of chlorophyll *a* is not related with the abundance of *A. setosa*. This is harmonious with our result that chlorophyll *a* was a little higher at Station 56 but the abundance of *A. setosa* at Station 56 was fairly lower than that at Station 32 in contrast to Actinommidae spp. juvenile forms, *Actinomma l. leptodermum*. Therefore the favorable condition for *A. setosa* is related to cold and well mixed water mass and any other organisms except for those from phytoplankton near the summer sea-ice edge.”

#### Comment 5-13 Actinommid and Spongotrochus glacialis (p. 16661, Lines 8 – 26)

colder (p. 16661, Line 16), “cold but water” (p. 16661, Line17). **See the comment 5-11.**

Thanks for your comment. We changed the text as follows:

“At Station 56, SML and PSW water masses were colder and more homogeneous than at Station 32; indicating that Actinommidae spp. juvenile forms and *A. l. leptodermum* preferred cold but warmer water than PWW.”

**was changed to**

“At Station 56, SML and PSW water masses were colder (-1.2 to 0.6 °C) and more homogeneous than at Station 32; indicating that Actinommidae spp. juvenile forms and *A. l. leptodermum* preferred slightly warmer water than PWW (-1.6 °C).”

Comment 5-14 p. 16661, Line 17 – 18: Small spumellarians might be herbivorous (Anderson, 1983).

What are you thinking? **See the summary of comments (iii)-c.** The knowledge of Roger Anderson is mostly based on the tropical collodarians and a few spumellarians. Please let me know if you know the papers which Roger regarded the cold water regions. The second, Roger has never studied Actinommidae in your sense. **I strongly comment to you that you properly read Anderson (1983) and his many papers.** At all, can herbivorous polycystines survive the long polar night when marine algae in the vegetative stage may not be present? If you insist that Actinommidae spp. juvenile forms and *A. leptodermum* are herbivorous euphotic taxa, it is better to write the sentence that their abundance increases in association with increasing in phytoplanktons.

According to your comment. We added vertical profiles of chlorophyll *a* at station 32 and 56 to figure 2. Please see the end of this file.

We revised the text as follows:

”Small spumellarians might be herbivorous (Anderson 1983) so Actinommidae spp. juvenile forms and *A. l. leptodermum* might therefore be bound to the euphotic zone where phytoplankton prevails.”

**was changed to**

“Our results show that Actinommidae spp. juvenile forms and *A. l. leptodermum* are most abundant in the upper water layers where phytoplankton also prevails (Fig. 2). It is most likely that the juvenile actinommidids and *A. l. leptodermum* may be bound to the euphotic zone, and so can be herbivorous.”

With this change, we revised the following text:

**Page 16651, lines 12 and 13**

“Hydrographical data (temperature, salinity)”

**was changed to**

”Hydrographical data (temperature, salinity, dissolved oxygen, and chlorophyll *a*)”

**Page 16652, line 9**

“Profiles of temperature, salinity, and dissolved oxygen”

**was changed to**

“Profiles of temperature, salinity, dissolved oxygen, and chlorophyll *a*”

**Page 16652, lines 23 and 24**

“Temperature, salinity, and dissolved oxygen show”

was changed to

“Temperature, salinity, dissolved oxygen, and chlorophyll *a* show”

**Caption of figure 2**

“Figure 2. The depth distributions of total dead and living radiolarians at stations 32 (a), and 56 (b) in comparison to vertical profiles of temperature, salinity, dissolved oxygen (Nishino, 2013), and living radiolarian diversity index (Shannon and Weaver, 1949). Also the different water masses are identified Surface Mixed Layer (SML), Pacific Summer Water (PSW), Pacific Winter Water (PWW), Atlantic Water (AW), and Canada Basin Deep Water (CBDW).”

**was changed to**

“Figure 2. Depth distributions of total dead and living radiolarians at stations 32 (a), and 56 (b) in comparison to vertical profiles of temperature, salinity, dissolved oxygen, and

chlorophyll *a* (Nishino, 2013), and living radiolarian diversity index (Shannon and Weaver, 1949). The different water masses are identified as: Surface Mixed Layer (SML), Pacific Summer Water (PSW), Pacific Winter Water (PWW), Atlantic Water (AW), and Canada Basin Deep Water (CBDW).”

We inserted the following text in **page 16652 lines between 23 and 24:**

“Chlorophyll *a* higher than 0.1 mg m<sup>-3</sup> was observed in 0-80 m depth.”

We added the following text after **page 16652, line 27:**

In 0-80 m depth, chlorophyll *a* was a little higher at Station 56 than at Station 32.

Guess you can ask the same question for any animal group in the Arctic Ocean. What do you think happens with the crustaceans, many of them feeding on phytoplankton exclusively? From where do the tintinnids get their winter food? Our traps indicate that radiolarians are present all through the year. This means that they do survive the winter! The same traps also indicate that phytoplankton is also present throughout the year (Onodera et al., 2014).

We do not insist, we carefully suggest that this is a possibility.

#### **Comment 5-15 p. 16661, Lines 24 – 26 *S. glacialis***

Okazaki et al. (2005) is also cited to show the water depths of *S. glacialis* because the study are is closer than the Okhotsk Sea of Okazaki et al. (2004). “*Spongotrochus glacialis* is associated with the phytoplankton production, but this does not simply mean herbivorous species. Casey et al. (1979) clearly wrote *Spongotrochus glacialis* is heterotrophic bacteria feeder (Fig. 5 of Casey et al., 1979). In conclusion, this paragraph should be revised in consideration with these comments.

May be not phytoplankton feeders, but never the less it would be very convenient to eat what is around you at any time. May be we should ask Casey what he base his statement on. Casey, as we do herein, simply suggests *S. glacialis* to be a “heterotrophic bacteria feeder”. He did not conduct any experiments to settle this. If you know to which experiment Casey used to make such a conclusion, please let me know.

The examples you refer us to are at least as weak as ours. Another taxonomic point: how many of us “radiolarian experts” do really understand the taxonomy of *S. glacialis*? I think this species also is a garbage-can where our colleagues put forms that are big, flat, spongy with spiny rim, with and without a pylome etc. etc. and do not pay attention to smaller differences and details. At present we do not know what is the real *S. glacialis* and the different forms that has been incorporated in this species are many. However, what we call *S. glacialis* fit Hülsemann’s description and as her and our material is from almost the same area we still accept her species concept. This at least to be consistent within our own papers.

#### **5.3.3 Upper AW association**

##### **Comment 5-16 p. 16662, Lines 21 – 22. “... the 1950s and 1960s.**

Itaki et al. (2003, p. 1519, Right column, Lines 23 – 25) wrote “No information on *C. historicos*a was reported from many plankton samples **from the Canadian Basin in the 1950s and 1960s (Hülsemann, 1963; Tibbs, 1967)**”. On the other hand, you wrote “This species has not been observed in **the Canadian Basin during the 1950s and 1960s (Hülsemann, 1963; Tibbs, 1967)**”. So, the priority of this notice has Itaki et al (2003) but **NOT YOU!**

Thanks for your comment.



We had no intention to take this as our observation as that was Itaki. We use your suggestion in your comment 5-19.

**Comment 5-17 p. 16662, Lines 26 – p. 16663, Line 1.**

**It may be hard for the potential readers to differentiate your new discovery from the results of Itaki et al. (2003), although you precisely wrote this point.** You noted that “according to McLaughlin et al. (2011), the mean temperature of the PWW within the Canada Basin increased slightly ( $\sim 0.05^{\circ}\text{C}$ ) from 2003 to 2007..” However, Itaki et al. (2003) has already showed a similar thing (though quite different), “According to Swift et al. (1997), the temperature of the AIW in 1994 at the Chukchi-Mendeleyev boundary is higher by at least  $0.2^{\circ}\text{C}$  than in the 1950s and 1960s.” In regardless of quite different, this makes an impression to say the exactly same things. I will propose a suggested solution later.

Thank you for your suggestion. We use your suggestion in your comment 5-19.

**Comment 5-18 p. 16663, Lines 1 – 3. the recent warming of the PWW and AW might induce the expansion of the habitat of *C. histricosa* into the PWW.**

**Itaki et al. (2003) commented that “Interestingly, this water temperature corresponds to the lower limit for survival of this species”** (p. 1520, in the Conclusion). Thus, if you consider the warming phenomena in the PWW led inversion by *C. histricosa* into this water, **you should show that the sea water temperature of the PWW exceeds the lower limit for survival of *C. histricosa*.**

We guess that you are perfectly well aware of that nobody knows the lower limit for survival of this species, not even the upper limit. We have added the following sentence as we do not think that the temperature itself is the reason for the areal expansion of *C. histricosus*:

“... expansion of the habitat of *C. histricosus* into the PWW. It is not so much the effect of the temperature itself that is causing the expanding distribution of *C. histricosus*, but the general temperature increase indicates that larger volumes of warmer AW is entering the Arctic Ocean. The increasing volumes of inflowing AW will therefore increase the chances for more exotic radiolarians to reach further and further into the Arctic Ocean and the Chukchi Sea.”

**Comment 5-19 A suggested discussion for your 5.3.3**

**“*Ceratocyrtis histricosus* occurred commonly in the upper AW (250 – 500 m) and rarely in the PPW. *Ceratocyrtis histricosus* is a species interpreted as being introduced from the Norwegian Sea, most likely during the early Holocene by the warm Atlantic water drifting through the Arctic Ocean (Kruglikova, 1999). Itaki et al. (2003) first noticed that *Ceratospyrus histricosus* has not been observed in the Canada Basin during the 1950s and 1960s and he pointed out that the common occurrence of this species in the Chukchi and Beaufort seas in 2000 may be the effect of the recent warming of the AIW. Itaki et al. (2003) also introduce that the temperature of the AIW in 1994 at the Chukchi-Mendeleyev boundary was higher by at least  $0.2^{\circ}\text{C}$  than in the 1950s and 1960s, from Swift et al (1997). Differing from Itaki et al. (2003), we first found this species in the PWW. According to McLaughlin et al. (2011), the mean temperature of the PWW within the Canada Basin increased slightly ( $\sim 0.05^{\circ}\text{C}$ ) from 2003 to 2007 and then remained constant until 2010. According to Itaki et al. (2003), *C. histricosus* can survive in the temperature range of  $0.5\text{--}4^{\circ}\text{C}$ . Although our data on the temperature of the PWW is apparently lower than the lower limit for survival of this species (Fig. 2), the rare existence of this species in the PWW may be caused by unobserved warming in the PWW or by appearance of other optimistic conditions for *C. histricosus*. However, the**

warming in the AIW has already been recognized in 1994 (Swift et al., 1997) and that in the PPW is also reported by McLaughlin et al. (2011), suggesting that the recent warming of the PWW and AW might induce the expansion of the habitat of *C. histricosus* into the PWW.”

Thanks to the reviewer 2. This made a good point. We use your suggestion and add a few sentences in response to your comment 5-18 to show that we simply do not think that these small temperature changes are that critical, but that the expansion is done due to increased volume of Atlantic water.

We revised the text as follows:

“*Ceratocyrtis histricosus* occurred commonly in the upper AW (250-500 m) and rarely in the PPW. This species is a species interpreted as being introduced from the Norwegian Sea, most likely during the early Holocene, by the warm Atlantic water drifting through the Arctic Ocean (Kruglikova, 1999). Itaki et al. (2003) first noticed that *Ceratospyris histricosus* had not been observed in the Canada Basin during the 1950s and 1960s and he pointed out that the common occurrence of this species in the Chukchi and Beaufort seas in 2000 might be an effect of the recent warming of the AIW. Itaki et al. (2003) also introduced that the temperature of the AIW in 1994 at the Chukchi-Mendelejev boundary was higher by at least 0.2°C than in the 1950s and 1960s, from Swift et al (1997). Differing from Itaki et al. (2003), we first found this species in the PWW. According to McLaughlin et al. (2011), the mean temperature of the PWW within the Canada Basin increased slightly (~0.05°C) from 2003 to 2007 and then remained constant until 2010. According to Itaki et al. (2003), *C. histricosus* can survive in the temperature range of 0.5–4°C. Although our data on the temperature of the PWW is apparently lower than the lower limit for survival of this species (Fig. 2), the rare existence of this species in the PWW may be caused by unobserved warming in the PWW or by appearance of other optimistic conditions for *C. histricosus*. However, the warming in the AIW has already been recognized in 1994 (Swift et al., 1997) and a warming in the PWW is also reported by McLaughlin et al. (2011), suggesting that the recent warming of the PWW and AW might induce the expansion of the habitat of *C. histricosus* into the PWW. It is not so much the effect of the temperature itself that is causing the expanding distribution of *C. histricosus*, but the general temperature increase indicates that larger volumes of warmer AW is entering the Arctic Ocean. The increasing volumes of inflowing AW will therefore increase the chances for more exotic radiolarians to reach further and further into the Arctic Ocean and the Chukchi Sea.”

**Comment 5-20 p. 16663, Lines 4 – 10**

Yes, the pulse of the tropical-subtropical radiolarian taxa into the Arctic Ocean is known, but **you need to cite Brady (1878) and Itaki & Khim (2007)**. Brady (1878) wrote the presence of tropical-subtropical polycystine species but has never illustrated these species. Itaki & Khim (2007) examined the samples of Brady (1878) and they first proved the existence of such tropical-subtropical species in the Arctic Ocean.

It is correct that they identified the species on Brady’s slides but in their discussion and their abstract they clearly conclude that these samples studied by Brady and identified by Haeckel should best be regarded as “sample contamination or misidentification of samples. These samples should according to Itaki be interpreted as “uncertain and should be regarded with suspicion”.

Because the pulse of the tropical-subtropical radiolarian taxa into the Arctic Ocean has already been known in the late 19th century.

This is also how we interpret Itaki's data, but Itaki is of the impression that this is not the case but that samples has been misidentified or mixed and should not be used or used with care.

In addition, **Bjørklund et al. (2012) clearly declared that the reported pulses may not be a consequence of global warming** (See the abstract of Bjørklund et al (2012)). This point is the important point in Bjørklund et al. (2012), you **MUST NOT WRITE BEING MISUNDERSTOOD AS A RESULT OF GLOBAL WARMING!**

Thanks for your comment.

We go through the MS and, make sure that we are not understood as we state that *C. histriosus* can be interpreted by the reader as a result of global warming. We rewrote the text about *C. histriosus*. Please see our response to your comment 5-18.

#### **5.3.4 Lower AW association**

No problem.

#### **5.4 Seasonal and annual radiolarian flux**

##### **5.4.1 Radiolarian fauna and seasonal sea-ice concentration**

###### **Comment 5-21 the necessity of a family name**

The family name "Cannobotryidae" is unnecessary to show in this section because only a single species constitutes this family.

According to your comment.

We delete Cannobotryidae or replace it with *Amphimelissa setosa*.

###### **Comment 5-22 p. 16664, Lines 9 – 10.**

See the comment shown above.

We agree with your comment.

###### **Comment 5-23 p. 16664, Lines 17 – 21. Swanberg and Eide (1992) ... correlated with chlorophyll a.**

**Dolan et al. (2014) found the opposite fact in the Arctic.** Swanberg and Eide (1992) regarded the Norwegian Sea. According to Dolan et al. (2014), *Amphimelissa setosa* was significantly lower abundances with higher chlorophyll concentrations of 2012, the low sea ice year, compared to the year of 2011 with significant sea ice and lower chlorophyll concentrations (p. 109 – 110, Dolan et al. 2014). **Thus, the abundance of phytoplankton protoplasm with the remains of chlorophyll a is not entirely related with the abundance of *Amphimelissa setosa*.** On the other hand, although Dolan et al. (2014) did not note, the summer ice edge is likely related with the abundance of *Amphimelissa setosa*. This will support your opinion in p. 16664, Lines 20-21. **Thus, it is better for the authors to change the discussion about the importance of phytoplankton, in consideration with Dolan et al. (2014).**

Thanks for your good recommendation. We revised the text as follows:

"Thus *A. setosa* prefer water masses near the summer ice edge for reproduction and growth."

was changed to

"Dolan et al. (2014), however, reported that the abundance of *A. setosa* was not entirely related to high Chlorophyll *a* with low sea-ice concentration as we have said in section 5.3.1.

Therefore we interpreted that cold and well mixed water mass based on summer ice edge and maybe other ice fauna were essential for high reproduction and growth of *A. setosa*.”

and with this change, we also revised page 16647, lines 12-15.

“*Amphimelissa setosa* was dominant during the open water and the beginning and the end of ice cover seasons with well-grown ice algae, ice fauna and with alternation of stable water masses and deep vertical mixing.”

**was changed to**

“*Amphimelissa setosa* was dominant during the season with open water as well as at the beginning and at the end of the seasons with sea ice cover. Cold and well mixed water mass based on summer ice edge were essential for high reproduction and growth of *A. setosa*. Our data indicate that *A. setosa* might have a three months life cycle.”

and further more, we inserted the following text in page 16664, lines between 14 and 15.

“Zasko et al. (2014) also reported that *A. setosa* was essentially absent in the plankton samples in the central polar basins.”

**Comment 5-24 p. 16664, Line 28; p. 16665, Line 1. “Actinommidae”**

**“Actinommidae” ---> “the actinommidids”, because the Actinommidae regarded in your paper is very limited species. Please check your “Actinommidae” throughout the text.**

According to your comment.

We replaced “Actinommidae” with “the actinommidids”.

**Comment 5-25 p. 16665, Lines 6 – 8. feeds on algae**

See the general comment. It may be wrong.

We changed the text as follows:

This might indicate that Actinommidae spp. juvenile form can feed on algae growing on the ice or other phytoplankton under the sea-ice. Therefore, *A. setosa* and the actinommidids might have different nutritional niches.

Please also see our response to your comment 5-26.

**Comment 5-26 p. 16665, Lines 9 – 20.**

**I can agree with your opinion about “Therefore, Amphimelissa setosa and Actinommidae have different nutritional niches.”, but I cannot completely understand your logic. First of all, why is the example of the Okhotsk Sea (Okazaki et al., 2003) needed to prove your opinion? Can you defend your opinion against the following possibility?** The different nutritional niches between Amphimelissa setosa and the adult actinommidids are easily presumed from the cell size. The skeletal diameter of the adult actinommidids is 120–300 µm in diameter (only for *A. georgii* and *A. turidae* and some undescribed forms of similar size, but majority are *A. boreale* and *A. lept. leptodermum* about 80 µm), whereas the length and width of Amphimelissa setosa are 65 µm and 50 µm, respectively. The cell volume of the former ranges from 9.05×10<sup>5</sup> mm<sup>3</sup> to 1.41×10<sup>7</sup> mm<sup>3</sup> while that of the latter is 2.16×10<sup>5</sup> mm<sup>3</sup>. Thus, the cell volume of the adult actinommidids is 4 to 65 times larger than that of Amphimelissa setosa. If the metabolism is the same each other, the required volume of feed at a given time is quite different. So, if they have the same food preference, Amphimelissa setosa has an advantage over the adult actinommidids in starving conditions. However, if food is sufficiently supplied enough to reach to the sea-floor, they did

not under starving conditions because these two polycystines are plankton. Thus, if you insist “different nutritional niches”, you probably need to show the data about the independent changes in the standing stocks or fluxes between these two taxa. Differences of reproduction rates between *Amphimelissa setosa* and the actinommids cannot be used for proving your opinion because we have no data on the number of survival daughter cells from a single (a couple of?) polycystine species. **In conclusion, the paragraph between Lines 9 – 20 on Page 16665 should be deleted unless you can show more scientific evidences.**

Thanks for your comment.

Because there is winter sea ice, comparable situation as in the Chukchi Sea.

Page 16665, lines 9-20 is not right as the reason for different nutritional niches between *Amphimelissa setosa* and Actinommidae but is right as the reason that the diversity indices were negative correlated with the total radiolarian fluxes in the Arctic Ocean on the contrary to that in the Okhotsk Sea.

Thus we deleted the text as follows:

“This study showed that the productivity of radiolarian was low but diversity was high under the sea-ice (Figs. 5 and 6). In contrast, radiolarian fauna in the sediment trap set in the Okhotsk Sea showed low diversity during the winter to spring when seasonal sea-ice covered the surface (Okazaki et al., 2003). The maximum total radiolarian flux during the summer season around the sea-ice edge and the open water is characterized by high dominance of *A. setosa* (> 90 %) in our area. Such high dominance of single species does not occur and major nine taxa contributed more than 60% to the radiolarian assemblage in the Okhotsk Sea (Okazaki et al., 2003). *Amphimelissa setosa*, which have small and delicate siliceous skeleton, might respond to primary production more directly and rapidly and develop earlier than Actinommidae, which have more robust skeleton. Therefore, *Amphimelissa setosa* and Actinommidae have different nutritional niches.”

**was changed to**

“This study showed that the productivity of radiolarians was high, but diversity was low, during summer season with low sea-ice concentration in the western Arctic Ocean (Fig. 5 and 6). In contrast, radiolarian fauna in the sediment trap set in the Okhotsk Sea showed high diversity during summer season (Okazaki et al., 2003). The maximum total radiolarian flux during the summer season around the sea-ice edge and the open water is characterized by high dominance of *A. setosa* (>90%) in our area. Such high dominance of a single species does not occur in the Okhotsk Sea, where the main nine taxa contributed with more than 60 % of the radiolarian assemblage (Okazaki et al., 2003). *Amphimelissa setosa*, which has a small and delicate siliceous skeleton, might respond to water mass conditions near summer ice edge both more directly and more rapidly. The contrast of seasonal diversity between these two areas was due to the difference of species composition and their response to water mass changes with low sea-ice.”

and we added the following text after p. 16665, Lines 6 – 8 as follows: “...under the sea-ice. Therefore, *A. setosa* and the juvenile actinommids might have different nutritional niches”

#### **5.4.2 year difference**

**Comment 5-27 p. 16667, Lines 4 – 20.**

**Must discuss the result of Dolan et al. (2014).** In similar to your results, the abundance of *Amphimelissa setosa* is significantly lower in 2012 than 2011. You said that “*Amphimelissa setosa*... not changed before and after the cold eddy passage.” You need to consider your discussion when you see Dolan et al (2014).



According to your comment, we revised the text as follows:

“*Amphimelissa setosa* was the most dominant (> 90 %) during this period and the radiolarian species composition was not changed before and after the cold eddy passage. Therefore the cold eddy in addition to seasonal water mass variations with sea ice formation would enhance the high radiolarian flux, but not diversity, in 2010.”

**was changed to**

“*Amphimelissa setosa* was the most dominant species (>90%) and showed the highest flux (13,840 specimens m<sup>-2</sup> day<sup>-1</sup>) during November 2010 in the upper trap. The flux of this species was about 3,500 specimens m<sup>-2</sup> day<sup>-1</sup> higher and kept the highest value half a month longer than that in 2011. The cold eddy passage would transport a cold and well mixed water mass, conditions favorable for *A. setosa*. Therefore the cold eddy passage in addition to seasonal water mass variations with sea ice formation would enhance the high radiolarian flux.”

### **Taxonomy**

**Comment 6-1 *Spongotrochus glacialis* ---->*Spongotrochus* aff. *glacialis***

I don't make sure whether the illustrated specimen was properly identified as this species, because I cannot recognize the presence of central empty sphere and the empty space between the circumferential ring and the central sphere. The most referable illustrations for *Spg. glacialis* are shown on pl. 60, fig. 5, and pl. 31, figs. 1, 2a and 3a of Nakaseko and Nishimura (1982).

We replaced *Spongotrochus glacialis* with *Spongotrochus* aff. *glacialis* only in plate 3, fig.9.

The specimens we have seen in the Arctic Ocean seem to follow Hülsemann's description fairly well. She did not show any photographs but her discussion of the species seems logical and safe. We also confer with Petrushevskaya 1968 on this species. We think we have used the *Spongotrochus glacialis* correctly, but the *Spongotrochus* aff. *glacialis* we do not know what to name rather than say it is close to the real one *Spongotrochus glacialis*. Therefore we continue to use “*Spongotrochus glacialis*”.

### **Errata**

We found several mistakes and corrected as follows.

#### **Page 16658, line 17**

“October-November”

**was changed to**

“November-December”

#### **Page 16658, lines 24-26**

“During July–September 2011, juvenile and adult forms of *A. setosa* were dominant during June–July and August–September, respectively.” was deleted.

#### **Page 16661, line 4**

80% was changed to 86%.

### **References**

We added the following papers to the references in our manuscript.

Bernstein, T.: Protist plankton of the North-west part of the Kara Sea, Transactions of the Arctic Institute, 3, 1–23, 1931 (in Russian with English summary).

Bernstein, T.: Über einige arktische Radiolarien, Arch. Protistenkunde, 76, 217–227, 1932.

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We inserted following two text.

In page 16669 lines between 3 and 4.

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In page 16669 lines between 6 and 7.

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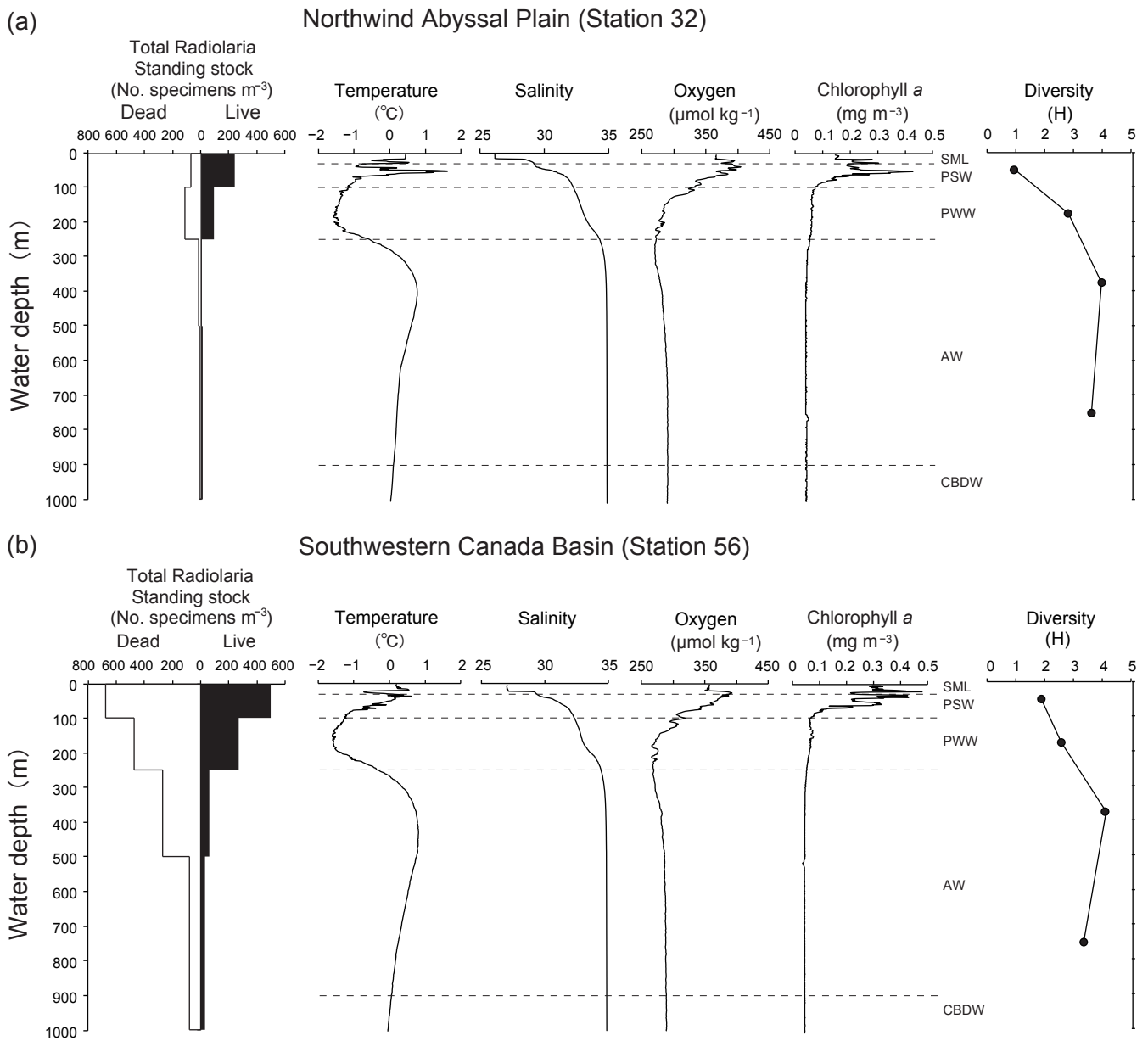


Fig. 2