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 recognized the value of this manuscript. However, it is unfortunate that this manuscripts have 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 massing. 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.

(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 (75º24'30"N, 63 º59'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).

(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 Artic 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.

(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.

(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

Biogenscience readers. For example, you wrote "a warm Atlantic species" in the abstract, but this species live in the seawater of 0.5 to 4 ºC 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.

(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. trudidae*, *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."

(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 *Stylochlamydium venustum* and *Ceratocyrtis borealis* in the Arctic, but this is not a good reason because these two species are deep-water species 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.

(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.

(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.

(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. historicosa* 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.

Reviewer's suggestion

Major revision.

Detailed comments

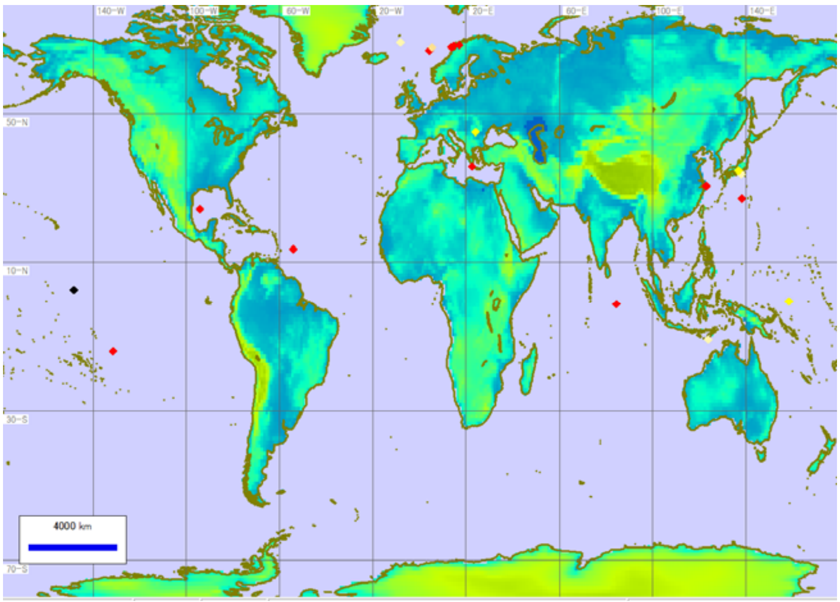
0. Title and abstract

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"

Comment 0-2. Abstract

The sentences about *Ceratocyrtis histricosus* 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 0.5ºC to 4ºC (Itaki et al., 2003). Can you say "a warm" species, cannot you? The second point is "Atlantic water species." According to Takahashi & Honjo (1981), these species was trapped in the 988 and 3755 m water depths in the equatorial Atlantic Ocean. Thus, this species is NOT a warm species. 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. I briefly listed the occurrence points of this species as well.



Red: plankton sample. Yellow: Holocene sediment, black age unknown.

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)

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

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

[equatorial Pacific]

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

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

[Okhotsk]

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)

[equatorial Atlantic]

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

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.

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 antactícus* above the Lomosonov Ridge, the Arctic Ocean. This is the practical evidence about your mention.

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.

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.

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

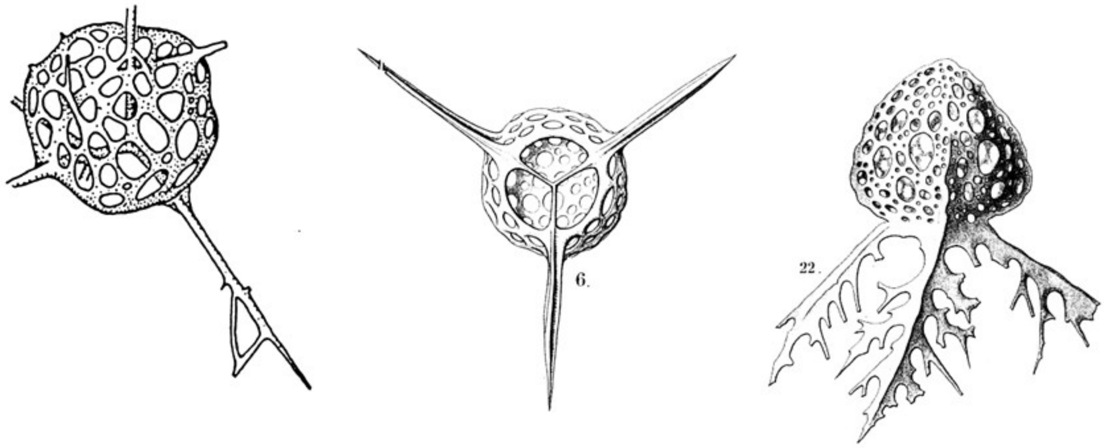
The "to avoid complications" is no scientific reason. If you want get them together, you can select "marine siliceous-test Rhizaria"

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.

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.



Left: *Tholospyris gephyristes*, middle: The type species of *Archibursa*, Right: The type species of *Tripodiscium*.

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.

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.

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.

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.

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.

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 Artic a 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.

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.

You 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 regards 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 2σ (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.

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 complied for this purpose, because you may find extinct species in the Arctic Ocean, although you must take care wrongly identified specimens as well.

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.

Comment 5-5 p. 16659, Lines 22 – 25. Inflow... from ... Pacific... negligible... *Stylochlamydium 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. *Stylochlamydium 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!

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

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 *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 manusript.

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. How to note the existence of this genus and this species in the previously published references? In my personal experience, I often saw *Joergensenium* specimens in the North Pacific. (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 georgi*, and *Actinomma turidae*. The high diversity of actinommids and *Joergensnium* 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.

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.

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

What do you want to say? 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.

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 DLETED or thoroughly changed with caution.

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.

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).

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.

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.

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

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

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.

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.

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. historicosa* 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!

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 (~ 0.05°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°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.

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*.

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 *Ceratospyris 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°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 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."

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

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.

Comment 5-22 p. 16664, Lines 9 – 10.

See the comment shown above.

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).

Comment 5-24 p. 16664, Line 28; p. 16665, Line 1. "Actinommidae"

"Actinommidae" → "the actinommids", because the Actinommidae regarded in your paper is very limited species. Please check your "Actinommidae" throughout the text.

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

See the general comment. It may be wrong.

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 defense your opinion against the following possibility? The different nutritional niches between *Amphimelissa setosa* and the adult actinommids are easily presumed from the cell size. The skeletal diameter of the adult actinommids is 120–300 µm in diameter, whereas the length and width of *Amphimelissa setosa* are 65 µm and 50 µm, respectively. The cell volume of the former ranges from 9.05×10^5 mm^3 to 1.41×10^7 mm^3 while that of the latter is 2.16×10^5 mm^3. Thus, the cell volume of the adult actinommids 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 actinommids 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.

5.4.2 year difference

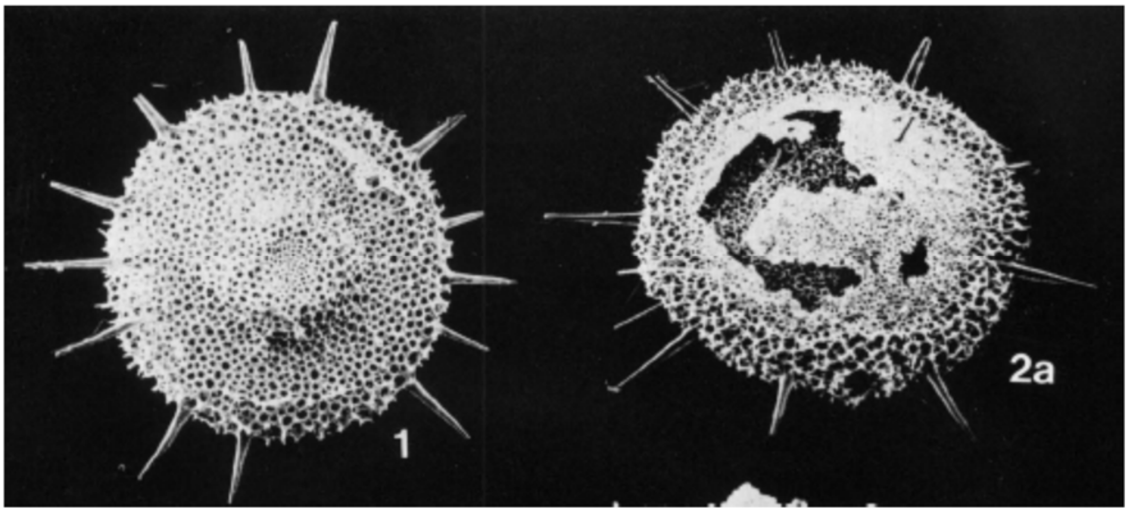
Comment 5-27 p. 16667, Lines 4 – 20.

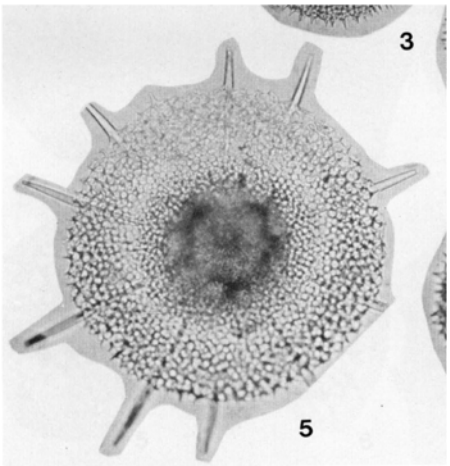
Must discuss the result of Dolan et al. (2014). In similar to your results, the abundance of *Amphimelissa setosa* is significantly lower in 2012 than 2011. You said that "*Amphimelissa setosa*… not changed before and after the cold eddy passage." You need to consider your discussion when you see Dolan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

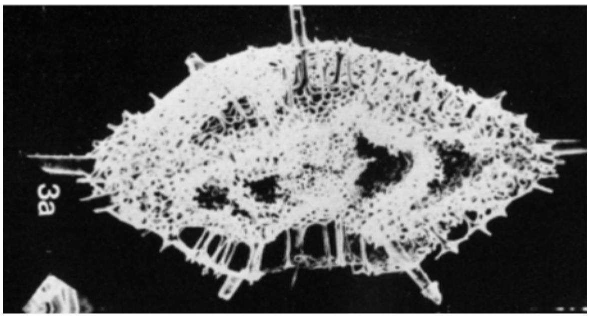
Taxonomy

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).







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