

2014.2.17

Dear Dr Ikenoue

Thanks for extensive revision. You have done a lot of work. Revised version looks fine except for a part of discussion.

I think two referees are deeply interested in this paper and see the MS has high potential to be published but after extensive revision. I agree with them. The authors responded well to most of the comments, but failed to do so to some of comments or critics. Reviewers suggest that authors did discussion with very weak evidence or logic at some points, 5.2 for an example (as in the comment by reviewer 2). Both reviewers indicated that discussion includes some unscientific “opinions”. You do not need to give “opinions” in the scientific paper. I believe these points of discussion may not be essential for this paper. I suggest authors to reconsider the critics on these discussions. I do not say your opinions are wrong but I am not convinced with these.

Introduction looks nice. Authors clearly describe the aim of this MS as “We present radiolarian depth distributions and flux variations in the western Arctic Ocean, and discuss their seasonality and species associations in relation to the environmental conditions (temperature, salinity, depth, sea-ice concentration, and downward shortwave radiation)”. Thus I again recommend that the authors focus on this exciting question that can be answered clearly using your original data while authors should try to shorten, remove or tone down the discussion of the origin, evolution (including adaptive change of morphological variation to stressful conditions), historical change of the distribution of the species. All these are also interesting questions, I agree. But these are not essential questions that can be answered by your original data given in this paper only. I believe the main critics of the reviewers are on the discussion of the these questions and these will go to the other papers.

The followings are the lists of replies by authors where I found flaws. For others the responses looks fine for me. My suggestions are in red. I strongly encourage authors to consider further revision, thanks. Revised version will be reviewed by subject editor and may or may not go to reviewers.

Yutaka Watanuki

COMMENTS FROM REVIEWER 1

1) LOCAL SPECIATION

Page 16660, line 8

Or: "... suggested the possibility of endemism for..."

We accepted your suggestion, and revised lines 7-13 as follows: "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 Actinommididae might be ongoing in the central Arctic Basin. Our results support this hypothesis, and suggest that local speciation or morphogenesis took place not only in the central Arctic basin, but also in the western Arctic Ocean."

<Endemism may be OK, but it does not necessarily indicate local speciation. I do not understand how does your results support Kruglikova et al. I believe your original results (8 species list at a single point in two years) may not increase the understanding of the species distribution and history so much.

2) DIET OF JUVENILE ACTINOMMIDS AND *A. l. leptodermum*

Page 16661, line 18

That's a very thin logical link. In the absence of information specifically on these species (or at the very least the genus), I would delete this as too hypothetical.

According to your comment and comment 5-14 of reviewer 2, we added vertical profiles of chlorophyll a at station 32 and 56 to figure 2. "Small spumellarians might be herbivorous (Anderson 1983) so Actinommididae 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 Actinommididae 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 actinommids and A. l. leptodermum may be bound to the euphotic zone, and so can be herbivorous.”

<I agree that this is very weak evidence. Many terrestrial carinivorous predators live in the place with high density of glasses but they do not eat glass. You need more strong evidence. High overlap of two species does not necessarily indicate prey-predator relationships.

3) OCCURRENCE OF *Ceratospyris histricosus* IN 2000

Page 16663, line 1-3

That's very slim evidence... that would translate to ca. 0.5 degree over last century or so. Do you really think that half a degree affects the distribution of a species that much? I would tend to rather believe that the studies from the 50s/60s simply did not pick it up, so there is more a sampling bias than a "warming trend" observation.

Thanks for your comment.

According to your comment and comments 5-16 and 5-19 of reviewer 2, we replaced Page 16662, line 21-22 as follows: “This species has not been observed in the Canada Basin during the 1950s and 1960s (Hülseman 1963, Tibbs 1967).” was changed to “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 “.

We added the following text to the end of the text on line 3 in page 16663:7

“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.”

<I understand that water flow play important roles in determining the species distribution rather than water temperature itself. Then how do you know the historical increase of flow rate of AIW? Does this slight increase of temperature indicate the increase of flow rate? Global warming just increases the temperature but may not influence the flow. You need references.

4) TRANSPOTR OF DECOMPOSITION MATERAIL AS FOOD OF RADIORATIANS IN DEEP

Page 16668, line 13-15

I will mention this here, but similar examples occur at other spots in the text (essentially ALL your conclusions/hypotheses trying to link radiolarian abundances/flux and feeding habits): I do realise that you use mitigating words as "maybe", "probably", "suggest", etc, but these hypotheses are not based on anything factual, not a shred of data, and remain highly controversial. Just because a few deep-living radiolarian species increase in abundance, it does not necessarily mean that organic matter export to depth increases (and so on, and so on). It's just a conjecture, maybe not even an hypothesis. If you had measurements of organic matter at various levels in the water column, and through time, and you would see some correlation to species abundance, you might have been able to formulate such an hypothesis. Even then, as you surely know, correlation is not causality, and such a coincidence between organic matter and species data would not prove the hypothesis.

Thanks for your comments. We revised the text as follows: 9 “This probably indicates that decomposing material from the primary production during the sea-ice free season was transported to great depths, giving nutrition to the deep water radiolarian fauna.” was changed to “This probably indicates that decomposing material from the primary production during the sea-ice free season was transported to great depths and might also act as a substrate for bacterial growth, providing the deep water radiolarians with sufficient food elements. We have no data to support this but in the Chukchi Sea, moderately high rates of bacterial production at the end of the growing season (July-August) have been found (Cota et al., 1996; Wheeler et al., 1996; Rich et al., 1997).”

<I totally agree with reviewer's comment; it does not necessarily mean that organic matter export to depth increases. Authors mentioned that "some lateral advection at a depth lower than 180m" play some role. Authors are suggested to remove this part from discussion. As you have no data to support (or reject) the hypothesis, you do not need to discuss.

COMMENTS FROM REVIEWER 2

1) DIVERSITY INDEX, EVOLUTION AND ORIGIN

(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.

<Reviewer give general question; not specific to specific taxon. Authors did the best. That is OK. But authors should be careful. I suggest that the it will be safer to tone down the discussion on species diversity and endemism of this groups of organisms where no good agreement of taxonomy has been made.

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

<I agree with this general comment. I made a similar comment at the early stage of the review. Is [Arctic Ocean radiolarian fauna was introduced after the last Glaciation] your opinion or hypothesis generally accepted by the scientists working on this group of the organisms? If this "opinion" has been proposed already, your original data does not contribute much to the further understanding of the origin of this group as you give a data of species community at a single location not distribution, including historical, data. You do not need to discuss deeply where you do not have sufficient original data.

Comment 5-19 A suggested discussion for your 5.3.3.

<Sorry. I still do not understand the logic of your response here. SEE 3)
OCCURRENCE OF *Ceratospyris histricosus* IN 2000 (REVIEWER 1)

2) DISTRIBUTION AND ORIGIN

TO RESPOND TO THE COMMENT OF “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 0.5°C to 4°C (Itaki et al., 2003), **AUTHORS REPLY AS** “this species is originating from the Norwegian Sea and has been transported by the “warm Atlantic water”.

<Authors need good references. Your original data presented in this MS does not say anything about this. I do not understand why the authors can say that “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.” **Does it simply mean that the suitable temperature range of this species is broader than believed before?**

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. histricosus* 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. histricosus* is not only an Atlantic species, but it is still a question for the Arctic Ocean *C. histricosus* population, from where did they come?

5.2 Characteristic and ongoing speciation...

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

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.

<The MS is not such a review paper discussing history of the distribution of the species. Your original data do not contribute much to such question. Such discussion can be made in the other paper where you review all studies on taxonomy and distribution of this group or organism.

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. 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 is being recruited from the North Atlantic.

<This is possibly given by other researchers and will be correct. But authors should give strong evidence basing on your results supporting this “opinion”. Do not use much space just introducing the hypothesis provided by other people. How does your data given in this MS support or reject the hypothesis? I do not believe that the presence of species at single location (that is the data you give) contribute much for understanding the origin and distribution.

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.

<Sorry, I'm very confused. You mentioned in the METHOD section that you did sampling at single location in the western Arctic Sea. Why can you talk about your results not presenting in this MS? Are these shown in the other papers? Give references.

3) SECTION 5.2

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. Our results support this hypothesis, and suggest that local speciation or morphogenesis took place not only in the central Arctic basin, but also in the western Arctic Ocean.

< I do not understand what of your results support which hypothesis? (Endemism arose? Radiolarians had been rapidly evolving under the stressful conditions? Speciation or morphogenesis within the family Actinommidae might be ongoing in the central Arctic Basin?). Why does the occurrence of what we interpret as new and still undescribed Actinomma species support the some of these hypothesis? I suppose that a different structure of the medullary shells may support this but how?

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.

<Why do you believe that the low temperature and variable quality of the water induce speciation? Give rationale and evidence. How does your original data given here support this idea? As you say if this is only speculation, remove this. We do not need speculation without any evidence.

OTHER COMMENTS

P2L3 Cold and well mixed water mass based on summer ice edge were [seemed to be: <EVIDENCE IS NOT SO SYTRONG] essential for high reproduction and growth of A. setosa.

<P9L26-28 Try to move references to METHODS and minimize the usage of references in RESULTS section. If you believe you can't, that will be OK.