**Revision notes.**

**Manuscript ID TPAL-2019-0028. Palynology.**

**Navidi-Izad et al.**

Dear Editor,

Thank you for handling our manuscript entitled "*Colonial palynomorphs from the Upper Ordovician of northeastern Iran: spore “thalli”, coenobial Chlorophyceae (Hydrodictyaceae) or cyanobacteria?*"submitted to the journal *Palynology*.

In the revised version we highlight the changes we made in the manuscript by using the Track Changes mode in MS Word.

We went through the attached files and reviews very carefully considering every point made by the reviewers. Below, you will find a detailed report where we explain exactly how we have revised the manuscript. We accepted most of the remarks by the referees, that were extremely useful. In some cases, we explain why we were unable to make the requested changes.

Below, you will find our comments in red (underlined in yellow color) on the referee reports.

We first comment on the “Reviewer(s)' Comments to Author” that you sent us back with the referee reports (May 20th, 2019). Additionally, we comment on the pdf files of two referees that you sent us at the same time (‘system\_appen PDF\_proof\_hi-rw.pdf’, of RW: Reed Wicander; and ‘Review of Navidi.pdf’, by Paul Strother). We thank the referees in the "Acknowledgments" in the revised version.

Yours sincerely,

Navid Navidi-Izad (for all authors)

**“Reviewer(s)' Comments to Authors”**

**Reviewer: 1**
Recommendation: Accept

Comments:
I don't consider novelty as a necessarily good characteristic, or one that somehow makes a better paper.

Additional Questions:
null:

Clarity; economy; consistency: Excellent

Clarity; economy; coherency: Excellent

Impact; competency; success level: Good

Novelty: Average

Utility; impact; interdisciplinarity: Excellent

Quality; relevance; ordering: Poor

We thouroughly acknowledge this very positive review.

The ordering is now changed, in particular by naming the “Results” to “Observations and comparison”.

**Reviewer: 2**
Recommendation: Minor Revision

Comments:
I think that this paper was hard to write because of the different interpretations of acritarch and cryptospore definitions. But the authors partially succeeded in clarifying the text. I think that it would be useful to add "sensu ...." each time that those terms are used.

We now used the terminology ‘*sensu’* to make the text easier to read.

A short description of the palynomorphs would be welcome.

The descriptions are (now) in the text. We do not add a separate section.

The specimen 6 in Pl. 1 is fragmented and show isolated dyads or tetrads. They look very similar to the "cryptospores" described in the Cambrian by Strother et al (2004). Are they no relation between your material and the Strother's one?

We rearranged the text to clearly indicate that we do not work on the cryptospores from the Iranian sections, as they have been studied in detail previously.

However, the comparison with Cambrian cryptospores of unknown biological affinity is now also made.

Congratulation to the authors.

Additional Questions:
null:

Clarity; economy; consistency: Excellent

Clarity; economy; coherency: Good

Impact; competency; success level: Excellent

Novelty: Excellent

Utility; impact; interdisciplinarity: Excellent

Quality; relevance; ordering: Excellent

**Reviewer: 3**
Recommendation: Major Revision

Comments:
   The article shows several weaknesses, starting with the aim of it. Its objective is to report different morphtypes of colonial microfossils from the Upper Ordovician and discuss their nomenclature and biological affinities. However, only a few specimens lacking any detailed descriptions are illustrated on plates and, from the figures, it is unclear whether the specimens assigned to *Grododowon* *orthogonalis* Strother, 2017 correspond to this species and if the other colonies of algae actually represent different taxa or, more probably, whether they are artifacts of conservation.

Agreed—we now clearly indicate that we are only able to make tentative comparisons. Some of our material resembles the ‘thalli’ of Strother *in* Strotheret al 2017. Others resemble cyanobacteria, and again others are possibly of algal origin. We now make it clear that we do not know what they are. But they are surely not derived from land plants.

In view of this, the proposed discussion, although interesting, is in no way supported by the material that was therein presented.

There are inconsistencies in “Material and Methods” that should be fixed.
For example, why did you process 105 samples if you collected 160? Were all the samples treated in both laboratories? Why? Did you find any differences in the results? Otherwise, the samples processed in each institution should be indicated accordingly.

This paragraph has been rewritten.

We now put the sampling in a detailed context and clearly indicated which samples were processed in which laboratory.

   It is stated that all of the processed samples yielded acritarchs, chitinozoans, scolecodonts and cryptospores. No further information is provided on these important palynomorph groups and the study is focused only on the colonial palynomorphs which, according to Figure 3, come from 10 levels. Are the latter really relevant? What percentage of the total assemblage do they represent?

It was never stated that all samples yielded all the different groups.

We now provide information of percentages of the different groups (this took some time to count). Additionally, we also provide the data from Ghavidel-Syooki (2016), that are very different from our counts.With the additional information on sedimentary facies studies, this clarifies the palynological context of the Ghelli Formation.

   As for the repository, details should be provided as to which slides are deposited in each institution (in a table perhaps).

This is now indicated in the text.

   The “Results” are not consistent. There are some results with incomplete descriptions of the analyzed material, intercalated with results and interpretations produced by other authors.
   The relevance of the results obtained in this work are not evident.

We now changed the name of this section.

   In the “Results” section, it is stated that the palynological investigations throughout the Ghelli Formation confirm the presence of rich assemblages composed of acritarchs, chitinozoans, scolecodonts, cryptospores and trilete spores, but are these the results of previous papers (cited herein)? or are they the result drawn after analyzing the material included in this manuscript? It is not clear.

This is now explained clearly.

Previous studies are from another section, as now explained clearly in the text.

Here, new studies of 105 samples include all palynological groups. This work is still in progress and part of a PhD (which is now indicated in the manuscript).

However, in the presen tstudy, we only focus on the problematic ‘colonial’ forms, that have never been described in detail before from Iran. They occur in ten (out of 105) samples only.

   The relative abundances of different palynomorph groups should be calculated to confirm that the cryptospores are present in high proportion and that the depositional paleoenvironment was close to the land. There are no data of cryptospores, nor illustrations (except plate 1, fig.6). Some plates with cryptospores would prove interesting to know not only their abundance but also their diversity.

This has all been previously published and the references included (Ghavidel-Syooki and co-workers). This is not the objective of our study.

Nevertheless, we here add also our own (new) data.

Note that the ‘colonial’ palynomorphs are very rare.

And again, we do not focus on the cryptospores.

   Due to the unconvincing illustrations for the positive identification of *Grododowon* *orthogonalis*, a detailed description of this form and the different types of palynomorph colonies present should be provided.

Agreed—we cannot be absolutely sure if we have *Grododowon*. This is now better explained in the text.

We disagree that our illustrations are unconvincing. However, it is clear that the preservation of our material is not as good as the type material of *Grododowon.*

   The difference in the dimensions between the specimens herein assigned to *Grododowon orthogonalis* and those of the Middle Ordovician of USA is not duly justified. The differences in laboratory techniques argued by the authors should be explained.

This is now discussed in some more detail. See also below.

    “Sphaeromorph clusters, colonies, coenobia and ‘thalli’ in the Ordovician: acritarchs or cryptospores?” This is an exceedingly extended text for explaining the different meanings of the terms acritarch and cryptospore, as well as of colonies and their synonymies, according to different authors. There is no contribution from this work to shed light on the discrepancies in the use of these terms.

The terminology is very confusing, and we attempted to put some order in this (as acknowledged by Referee 2, see above). However, it is probably more interesting as to what the biological affinities are rather than how they are classified (see below).

   In "Possible biological affinities", the authors include a synthesis of previous articles, with definitions and descriptions of different groups of algae, colonies, miospores, acritarchs and cyanobacteria. There are even paragraphs on Botryococcus and Gloeocapsamorpha, which are not relevant for this work, concluding that part of the material, which is not indicated in the text or in the figures, looks like cyanophyte colonies.

*Botryococcus* and *Gloeocapsamorpha* are cited here because, as ‘colonial’ palynomorphs, they are very relevant in Upper Ordovician palynology, even if we do not have a definite identification of these groups in Iran.

   The manuscript is too extensive in terms of information and discussions that have already been proposed in previous articles, of which some are neither relevant for this contribution nor supported by the data, which are scarce and only analyzed to a shallow extent.

We are attempting to clarify a situation made complex by the misuse of terminology.

Two authors, namely Strother and Steemans, added a lot of confusion, by changing the original definition of the cryptospores, for example.

Further confusing terms, like ‘thalli’, have been introduced. We attempt to simplify and clarify.

More importantly, some scientists now believe that some of these weird fossils are derived from land plants, but they are not.

   The conclusion is too general, and it is neither sufficiently supported by the data, nor does it focus on the results of this work. The analysis of the results is not meticulous, and it is based mainly on cryptospores derived from terrestrial plants not included in this document (e.g. list of taxa, abundance, illustrations)

We now make it clear that we do not work in this paper on the cryptospores, but only on the ‘colonial’ palynomorphs. We also point out, despite tentative approaches by different authors, that we, like these authors, lack evidence, and we are not able to provide clear biological affinities to these microfossils. These ‘colonies’ might be different things, but they are clearly not derived from land plants.

Additional Questions:
null:

Clarity; economy; consistency: Good

Clarity; economy; coherency: Average

Impact; competency; success level: Average

Novelty: Average

Utility; impact; interdisciplinarity: Average

Quality; relevance; ordering: Average

**Comments on the pdf file of referee RW (‘\_system\_appenPDF\_proof\_hi-rw.pdf’)**

Reed Wicander (rw) provided some useful questions and comments in his annotated manuscript (pdf file). Here we comment on the changes we made.

Page 4: We now wrote clearly what Ghavidel-Syooki (2016) found in comparison to our study. Both sections are 40 km apart.

Page 4: we now explained the term “mélange”.

Page 5: Material and Methods section, we now clearly explain what we did in the two laboratories, with new counts to explain how abundant (rare) the different palynomorph groups are in our samples.

Page 6: the caption “results” was changed now. The previous studies by Ghavidel-Syooki are indeed not our results, but part of the comparison and discussion.

We now added percentages.

Page 7: we agree (and changed the text) that our material does not perfectly fits in the diagnosis of the material of Strother et al.

Preparation techniques are always different, different laboratories obtain different results… (see also below).

We make now clear (again) that this paper is not focused on cryptospores.

Page 9: the objective of this paper has now been re-explained. We hope that this is clear now.

**Comments on the pdf file of referee 3 ‘Review of Navidi.pdf’ (Paul Strother).**

Agreed. We now corrected *Gordodowon* into *Grododowon*, and cryptospore into cryptospores, preservation into preservations. Microfossils to microfossil.

Strother described alone the taxon *Grododowon* in Strother et al. (2017).

The text is corrected accordingly, and there was and is no mistake in the quotations.

Sorry, but Hydrodictyaceae is correct!

We now explain which samples are stored where.

It is very interesting to learn that the “sheets” of Strother et al. (2017) were based on palynological processing at Lille! We ignored that, and this is not specified in the original paper by Strother et al. (2017) who did also not describe the techniques used in the laboratory, nor the observation techniques.

However, these samples must have been processed by the former technician at Lille, who is no longer working in the department. This is inside information. So, there have been different techniques/technicians used!

We made now clear that ‘thalli’ is not related to ‘thallophytes’, to avoid confusion.

We never affiliated our ‘colonial’ forms to prasinophytes, and thus do not need to rule them out.

Yes, Paul Strother is right, we do NOT classify our objects neither as prasinophytes, nor cyanobacteria (and we never did this), but they most probably belong to the polyphyletic ‘algae’ group.

We never wrote that hydrodictyacean algae produce spores. However, we wrote that the spores of zygnemataceaen algae are spores: zygospores.

Regarding the microscopical techniques used, this is now also explained in the text.

We acknowledge the useful remarks concerning the number of ‘cells’ in *Grododowon* and *Muzivum*. Similarly to *Eomerismopedia*, none of our determinations can really be affirmative.

We accordingly modified the text.

We hope that these corrections will satisfy both the referees and Editor.