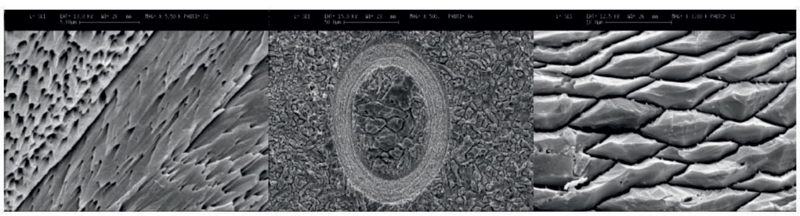
Permophiles

International Commission on Stratigraphy



Newsletter of the Subcommission on Permian Stratigraphy
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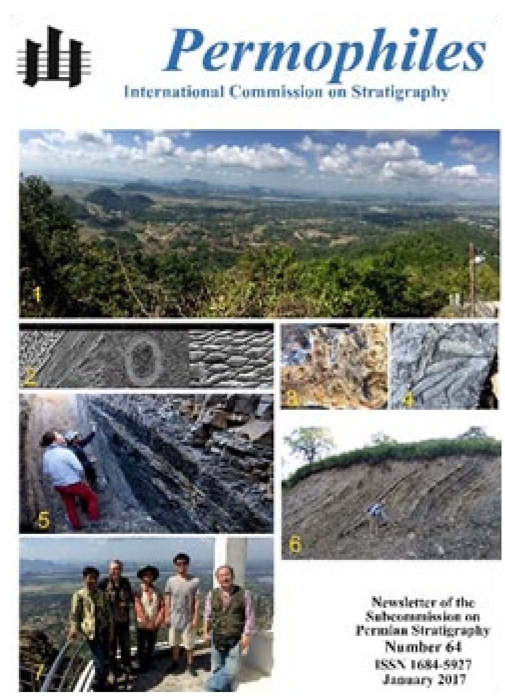

Photo 1: A distal view showing many small hills which are composed of Permian limestone blocks in southern Myanmar (courtesy S. Shen).
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EXECUTIVE NOTES

Notes from the SPS Secretary

Lucia Angiolini

Introduction and thanks

As you know, the publication of Permophiles 63 was delayed to October 2016 in order to report the voting of new members of the SPS during the 35th INTERNATIONAL GEOLOGICAL CONGRESS, 27 August - 4 September 2016, Cape Town, South Africa. With this short time between Permophiles 63 and the present issue I was concerned not to get enough material for this issue since everyone is so busy. However, soon it became clear that Permian scientists are very active and very keen to discuss a wide range of Permian topics. Amongst the most prolific contributors to Permophiles, I thank Charles Henderson, Spencer Lucas and Mike Stephenson. My warm thanks go also to the other contributors to this issue: Yuan Dong-xun, and co-authors, Petr Michaelsen and Claudio Garbelli.

I would like to thank Claudio Garbelli for his assistance in editing this and previous issues of Permophiles.

I would also like to keep drawing your attention to the new SPS webpage that Shuzhong Shen has provided at http://permian.stratigraphy.org/, where you can find information about Permophiles, what's going on in the Permian Subcommission, an updated version of the list with addresses of the SPS corresponding members and, very importantly, the newly updated Permian timescale.

Permophiles 64

A novelty of this issue is that it starts with the report of the emails exchanged by the SPS voting members in October and November 2016 to discuss the Base-Sakmarian GSSP proposal (Chernykh et al., Permophiles 63, p. 4). The main aim of this report is to illustrate in a transparent way the procedures that will ultimately lead to the vote for the GSSP of the base of the Sakmarian Stage.

This is followed by a related contribution by Yuan Dong-xun, and co-authors who propose to use the FAD of *Mesogondella monstra* as a marker for the Base-Sakmarian GSSP. Among the reported advantages in the use of *M. monstra,* are its wide distribution, its correlation potential being a deep-water species and its morphological features that make it easy to distinguish from allied species in the evolutionary lineage *Mesogondolella arcuata-M. uralensis-M. monstra*.

Charles Henderson, in a laudable attempt to stimulate debate inside the Permian community, presents his second harangue, which is indeed very interesting and very Permian-focused. In this harangue, Charles asks himself why *Hindeodus parvus* and the Global Stratotype Section and Point (GSSP) at Meishan are often referred to as "a mistake".

To answer this question, he goes through the concept of species (and the species is *H. parvus*) from a "population" perspective explaining clearly the significance of the findings of *H. parvus* in regions other than the stratotype. He underlines the importance of the base-Induan in Meishan being correlated with other markers, such as major carbon isotopic shifts, magneto-polarity zones and geochronologic ages. His philosophy, which is worth sharing, is that no section in the rock record can be perfect and we have to eventually accept GSSPs, because a GSSP definition is the prerequisite for further science.

In line with Charles' harangue, is the contribution by Mike Stephenson who wants to stimulate palynologists in the definition of Permian GSSPs, as a large amount of data on the topic is available. In fact Permian palynostratigraphy has been largely used by companies to correlate coal- and hydrocarbon-bearing rocks within basins and between basins. However, these palynological assemblages are difficult to correlate to the international Permian scale. Mike suggests revisiting key sections such as Meishan and Aidaralash Creek for high resolution palynological sampling, and to find candidate taxa to correlate the boundaries. This is a way to do further science after a GSSP definition.

The next contribution is by Spencer Lucas and co-authors who present a redefinition of the Newwellian substage of the Wolfcampian Stage, originally indroduced by Wilde (2002). In an effort to make its boundaries more precise, the substage is defined as the interval between the LOs of *Thompsonites*/"*Schwagerina"* and *Pseudoschwagerina* in the New Well Peak section. The new substage is correlated by fusulinid biostratigraphy in the USA, and it is approximately 4.8 Ma in duration based on cyclostratigraphy. The authors end their contribution by leaving open the question whether or not the Newwellian substage should be considered Carboniferous or Permian.

In his report, Per Michaelsen describes the coal-bearing middle part of a thick Permian-Triassic succession developed along the southern shores of the Mongolian Transbaikalian boreal seaway. Paleoclimatic indicators suggest Late Permian coal formation under boreal conditions, with cold winter months and development of peat-forming plants during summers. Syn-depositional faults, frequent sea-level changes and climate were the main factors controlling peat formation.

The last contribution is by Claudio Garbelli who sums up a recently published paper presenting a new approach to the events of the end-Permian mass extinction. The new methodology consists in the detailed study of the pattern of variation of brachiopod shell microstructures in the Late Permian and shows that brachiopods preferentially produced more organic-rich shells at the end of the period. This could be consistent with the hypothesis of ocean acidification in the latest Changhsingian.

Last but not least: remember to consult the newly updated Permian timescale at the end of this issue!

Future issues of Permophiles

The next issue of Permophiles will be the 65^{th} issue.

Contributions from Permian workers are very important to move Permian studies forward and to improve correlation and the resolution of the Permian Timescale, so I kindly invite our colleagues in the Permian community to contribute papers, reports, comments and communications.

The deadline for submission to **Issue 65** is **30th June, 2017**.

Manuscripts and figures can be submitted via email address (lucia.angiolini@unimi.it) as attachments.

To format the manuscripts, please follow the TEMPLATE that you can find on the new SPS webpage at http://permian.stratigraphy.org/ under Publications.

We welcome your contributions, your letters, comments, answers and advices to improve our communication as we move forward.

Notes from the SPS Chair

Shuzhong Shen

Time is flying. The Chinese New Year of the Rooster is coming quickly and this makes me busy in many end-year things. I wish our colleagues of the Permian community a wonderful 2017.

Thanks Lucia for organizing this issue. I have no much to say. We will send the Sakmarian proposal to all voting members for voting recently. However, I hope the proposal can be improved a little bit more before it can be voted. The other two Cisuralian GSSP candidate sections (Artinskian and Kungurian) have been excavated by our Russian colleagues. I herein call all colleagues again who are interested in working the GSSP candidate sections. SPS has a little money to support any activity related to the GSSP work.

Recently, the base of the Guadalupian Series in South China has been precisely calibrated by the high-precision U-Pb IDTIMS date (272.95 ± 0.11 Ma) based on the ash beds from the Kuhfeng Formation in South China (Wu et al., 2017). Thus, the new dates suggests that the Guadalupian Series in South China had a total duration of 13.85±0.52 myr given a Guadalupian-Lopingian boundary (GLB) age of 259.1±0.5 Ma (Shen et al., 2010; Zhong et al., 2014). The international Stratigraphic Chart will be updated in the next version.

There are very little data about the Permian in Myanmar. From last year, my colleagues and I have been to the Shan State of Myanmar twice and investigated the whole Permian. We have numerous discoveries of various fossils including fusulinids, forams, corals and brachiopods. In addition, Lower carboniferous brachiopods and conodonts, and abundant Ordovician brachiopod faunas and O-S graptolites have been collected too (see a cover photo). We hope we will publish those results shortly.

Shen, S. Z., Henderson, C. M., Bowring, S. A., Cao, C. Q., Wang, Y., Wang, W., Zhang, H., Zhang, Y. C., and Mu, L., 2010, High-resolution Lopingian (Late Permian) timescale of South China: Geological Journal, v. 45, no. 2-3, p. 122-134.

Wu, Q., Ramezani, J., Zhang, H., Wang, T. T., Yuan, D. X., Mu, L., Zhang, Y. C., Li, X. H., and Shen, S. Z., 2017, Calibrating the Guadalupian Series (Middle Permian) of South China: Palaeogeography, Palaeoclimatology, Palaeoecology, v. 466, p. 361-372.

Zhong, Y. T., He, B., Mundil, R., and Xu, Y. G., 2014, CA-TIMS zircon U–Pb dating of felsic ignimbrite from the Binchuan section: Implications for the termination age of Emeishan large igneous province: Lithos, v. 204, no. 0, p. 14-19.

SUBCOMMISSION ON PERMIAN STRATIGRAPHY

ANNUAL REPORT 2016

1. TITLE OF CONSTITUENT BODY AND NAME OF REPORTER

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2. OVERALL OBJECTIVES, AND FIT WITHIN IUGS SCIENCE POLICY

Subcommission Objectives: The Subcommission's primary objective is to define the series and stages of the Permian by means of internationally agreed GSSPs and establish a high-resolution temporal framework based on geochronologic and chemstratigraphical approaches, and to provide the international forum for scientific discussion and interchange on all aspects of the Permian, but specifically on refined intercontinental and regional correlations.

Fit within IUGS Science Policy: The objectives of the Subcommission involve two main aspects of IUGS policy: 1. The development of an internationally agreed chronostratigraphic scale with units defined by GSSP's where appropriate and related to a hierarchy of units to maximize relative time resolution within the Permian System; and 2. The establishment of framework and systems to encourage international collaboration in understanding the evolution of the Earth during the Permian Period.

3a. CHIEF ACCOMPLISHMENTS AND PRODUCTS IN 2015

A field excursion to the potential GSSP sections in southern Urals, Russia was organized during the ICCP 2015 in Kazan. Some SPS voting members attended the field excursion guided by the Russian colleagues. After the field excursion, a special SPS workshop was held to discuss the advantages and disadvantages of the potential GSSP sections. Most of the voting members agreed that the Sakmarian-base GSSP section at Usolka is good. However, the potential GSSP candidates for the base of the Artinskian and Kungurian stages are not well exposed, thus, both sections need to be excavated. The Russian colleagues organized a team in August, 2016 to dig two long trenches to make the strata of the Dalny Tulkas and Mechetlino Quarry sections fully outcropped now. The proposals of these two potential GSSPs will be prepared after the Sakmarian-base GSSP proposal is voted.

A revised and updated proposal of the Sakmarian-base GSSP has been published in *Permophiles* (2016, Issue 63). A group email was sent to all SPS voting members for one-month discussion before we organize a formal proposal for the subcommission. We have received a couple of comments and suggestions how to improve the proposal. We have also received a few comments about which conodont lineage to choose for the definition of the Sakmarian-base GSSP.

3b List of major publications of subcommission work (books, special volumes, key scientific paper)

Three issues of *Permophiles* (Issues 61-63) have been published since June, 2015. They are all avaialble on the SPS website (http://permian.stratigraphy.org/pub/pub.asp).

An updated Permian timescale has been published in these issues of *Permophiles*. A special issue titled "The Permian Timescale" has been organized by Spencer Lucas and Shuzhong Shen. More than ten papers have been available online. This will be published on the Special Publications of the Geological Society of London in early 2017.

3c. Problems encountered, if appropriate

We have encountered problems that discrepancies in conodont taxonomy and selection of the index species of the two proposals for Sakmarian-base and Artinskian-base GSSPs are present.

We also met a problem for the Lopingian-base GSSP which will be flooded after a dam established in 5 years for electronic power in the downstream of the Hongshui River in Guangxi, South China. We have extensively discussed with the local government and a detailed plan for searching the replacement of the GSSP section nearby the GSSP has been made. Field work to search replacement section in South China was carried out too during 2016.

4a. OBJECTIVES AND WORK PLAN FOR NEXT YEAR (2017)

The primary objectives are to complete the last three GSSPs (Sakmarian, Artinskian, and Kungurian stages). An updated proposal for the Sakmarian-base potential GSSP has been completed (see Permophiles 63). This proposal will be revised again after the one-month discussion during all SPS voting members. The Russian Stratigraphic Committee has excavated the Dalny Tukas and Mechetlino Quarry sections, then the SPS will organize an international joint field excursion to collect various samples in those sections.

4b. Specific GSSP Focus for 2017

The priority of 2017 for GSSP is to publish and send the updated Sakmarian-base GSSP proposal for discussion and voting in SPS.

5. SUMMARY OF EXPENDITURES IN 2016

We received an allocated budget 3000$ from ICS this year, of which 2850 US$ arrived at the SPS account after a bank processing fee was deducted. As planned in the 2015 annual report, this money was mainly used for supporting the SPS chair to go to Cape Town, South Africa to attend the ICS workshop during the 35th IGC, which was not enough.

6. BUDGET REQUESTS AND ICS COMPONENT FOR 2017

1. The Dalny Tukas and the Mechetlino Quarry sections for the Artinskian and Kungurian GSSPs have been excavated by the Russian colleagues. So, we plan to call all voting members for a field excursion on the three potential GSSP sections in southern Urals to collect samples. We will use a part of the 2017-year budget to support any voting member to go to southern Urals (3000US$).
2. A third field excursion for the three GSSPs of the Guadalupian Series in the Guadalupe National Park will be organized in 2017. This field excursion will be specially planned for working the three problematic Guadalupian GSSPs (1000US$).
3. SPS secretary Lucia Angiolini will be invited to Nanjing to edit the next *Permophiles* (1000US$).

In total: **US$5000**

APPENDICES

7. CHIEF ACCOMPLISHMENTS OVER PAST FIVE YEARS (2011-2016)

1) A new SPS website has been established.
2) Three GSSP bronze markers have been placed on the GSSPs in the Guadalupe National Park in USA.
3) A high-resolution timescale of the Permian system has been significantly refined (see SPS webpage Permian Timescale, also *Permophiles* 63).
4) Significant progress on the Sakmarian-base and Artinskian-base GSSP candidates has been made. Proposals for voting have been published and extensively discussed.
5) Two monuments have been built and a protected area has been established at Penglaitan, Laibin, Guangxi Province, China for the Wuchiapingian-base GSSP.
6) Ten formal issues and three supplementary issues of Permophiles have been published since 2010.
7) A Working Group on the Carboniferous-Permian transition between marine and non-marine sequences has been organized in 2015.

8. OBJECTIVES AND WORK PLAN FOR NEXT 4 YEARS (2016-2020)

1) Publishing the revised version of the proposals, organizing the field excursions and establishing the three (at least two) GSSPs for the Cisuralian.
2) Continue to work on the Guadalupian and global correlation for chemostratigraphy and geochronologic calibration. Publish the official papers for the three Guadalupian GSSPs.
3) Searching the replacement of the Lopingian-base GSSP nearby the stratotype section at Penglaitan, Guangxi, South China because the original will be flooded in 5-10 years by a dam for electronic power.

9. ORGANIZATION AND SUBCOMMISSION MEMBERSHIP

9a Names and Addresses of Current Officers and Voting Members

Five SPS voting members were replaced after August, 2016. We welcome Golubev, V.K., Mike Stephenson, Spencer Lucas, Mark Schmitz and Yichun Zhang become new SPS voting members, and we also thank Vladimir Davydov, Clinton Foster, Galina Kotlyar, Xiangdong Wang and Bruce Wardlaw for their great contributions to the SPS.

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9b List of Working (Task) Groups and their officers

1) Sakmarian-base and Artinskian-base GSSPs Working Group; Chair-Valery Chernykh.
2) Guadalupian Series and global correlation; Chair-Charles

Henderson.

3) Correlation between marine and continental Carboniferous-Permian Transition; Chair-Joerg Schneider.

4) Neotethys, Paleotethys, and South China correlations; Chairs Lucia Angiolini and Yue Wang.

9c Interfaces with other international project

SPS interacts with many international projects on formal and informal levels. SPS chair Shuzhong Shen organized an international cooperative project on the correlation of the Guadalupian Series between South China and Mt. Guadalupe in Texas, USA, which has been approved by NSFC.

REPORTS

Discussion on the updated version of the Sakmarian-base GSSP proposal published in *Permophiles* 63 (October, 2016)

In this contribution, we decided to include the discussion that went on by email in October and November 2016 about the questions raised by the SPS chair Shuzhong Shen on the updated version of the Sakmarian-base GSSP proposal (Chernykh et al., Permophiles 63, p. 4).

We think that it is very important for the Permian community to follow step by step and in a transparent way the process that will ultimately lead to the vote for the GSSP of the base of the Sakmarian Stage by the SPS voting members. This discussion is a very good example of how to promote research and achieve goals in stratigraphy.

The contributions to the discussion are presented in order of date of their arrival, starting from the call sent by Shuzhong Shen on 15th October 2016 and ending with the summary of Joerg Schneider on 12th December 2016 and the last remark of Valery Chernykh about the best marker to use to define the basal boundary.

15th October 2016, from Shuzhong Shen

Dear SPS voting members:

I would draw your attention to the fact that we are going to move forward in the procedure for the potential GSSP of the base of the Sakmarian Stage. In *Permophiles* 63 (http://permian.stratigraphy.org/pub/pub.asp), we circulated the updated version of the Sakmarian-base GSSP proposal based on that published in 2013. The updated version contains more conodonts, discussions and new figures. I would like to call a discussion on the following aspects for the proposal from all voting members. Your comments and suggestions are greatly appreciated. Please return your comments within one month:

1. Is the Usolka section good enough to be proposed as the GSSP section for the base of the Sakmarian Stage?
2. Are the palaeontological, geochronologic and geochemical data presented sufficient to be integrated as a formal proposal for SPS and ICS voting? If not, what other work or data need to be added?
3. If you agree with the Usolka section as the candidate of the GSSP, which lineage should we choose to define the GSSP? The *Mesogondolella* lineage or the *Sweetognathus* lineage?
4. Which index species we should choose to define the GSSP? Specifically, *Mesogondolella uralensis* or *M. monstra* (if we use the *Mesogondolella* lineage)? The former species has not been known from outside of the southern Urals. Or *Sweetognathus merrilli* or *S. binodosus* (if we use the *Sweetognathus* lineage)?
5. Other suggestions to improve the proposal?

16th October 2016, from Charles Henderson

I assume that, as a discussion and not a vote, a 'reply-all' is appropriate. I add a few comments below.

1. Is the Usolka section good enough to be proposed as the GSSP section for the base of the Sakmarian Stage?

I think the Usolka section is a good section for the GSSP. It generally has continuous sedimentation in what is probably a slope setting. It is punctuated with turbidites or tempestites that seem to transport some shallow water forms, but the background sedimentation of thin-bedded carbonate mudstones with some siliciclastic component dominates the section. The section has several ash beds that yield good ages that seem to demonstrate continuous sedimentation. Isotopic data do not seem to be diagenetically altered. It is an accessible section and will be protected and it honours earlier agreements to place the boundary in Russia. There are a number of potential ways to correlate.

2. Are the palaeontological, geochronologic and geochemical data presented sufficient to be integrated as a formal proposal for SPS and ICS voting? If not, what other work or data need to be added?

I think the geochronologic and geochemical data are sufficient. There are some fusulinaceans and also some ammonoids. Palynology might be useful - didn't Mike Stephenson collect some material and or provide a preliminary report. There would be more fusulinaceans if we went to a shallower section, but then conodonts would be fewer. Usolka is the right place if we use conodonts as the definition. I am not sure there would be any value in paleomag as few if any reversals would be expected. Are there any more ash beds to be dated? There could be more discussion on sedimentology and sequence stratigraphy. Bed 25/3 might be a MRS, in which case we might want to look for the first good event above that.

3. If you agree with the Usolka section as the candidate of the GSSP, which lineage should we choose to define the GSSP? The *Mesogondolella* lineage or the *Sweetognathus* lineage?

This is a deeper water section, so it would be best to use the *Mesogondolella* lineage. They are far more common in the section. Shallow-water taxa like *Sweetognathus* are present, but rare. It is not a good idea to suggest the FO of a rare taxon in a section approximates the FAD. There are also taxonomic issues as well and I have seen more *Sweetognathus* than anyone else. It would be good if Valery discussed the *Mesogondolella* lineage. How is it determined? Simple relative occurrences is not really sufficient. I would like to see some transitional forms and populations with growth series. Are there really transitional forms of *M. uralensis* - it has a very distinct anterior carina that I don't see in other taxa. I wonder if *M. striata* makes a better ancestor for *M. monstra*? Valery has seen more of these than anyone.

4. Which index species we should choose to define the GSSP? Specifically, *Mesogondolella uralensis* or *M. monstra* (if we use the *Mesogondolella* lineage)? The former species has not been known from outside of the southern Urals. Or *Sweetognathus merrilli* or *S. binodosus* (if we use the *Sweetognathus* lineage)?

I am leaning to *M. monstra*, but I would like to hear more from Valery on the lineage.

5. Other suggestions to improve the proposal?

The current proposal is now showing more correlation of the section and point, but this could still be improved.

23th October 2016, from Mike Stephenson

Dear All,

Thanks for the interesting discussion of the proposed site.

Like Charles, I am generally in favour of the Usolka section.

The main reason for this email is to reply to Charles' question about palynology. I visited the Usolka and Dalny Tulkas sections in July 2007.

The palynology report that Charles refers to was about Dalny Tulkas and was published in Permophiles 50 (see attached). The palynomorphs of Dalny Tulkas were quite well preserved, but in my opinion no obvious markers were present that might be used to correlate a boundary beyond the section using palynomorphs.

I have slides for the Usolka section (see sample list, photos of sample levels, and my positioning of the sample levels on the standard log that we used in 2007), but I don't think that I looked at them in detail.

I think now is the time! I will find the slides and report back.

23th October 2016, from Mark Schmitz

Everyone,

This is my first communication as a voting member of the Subcommission, so first of all let me introduce myself and express my pleasure at being invited to participate in this community and its deliberations. I know many of you on the Subcommision, but haven't met all of you - briefly, I am a geologist and isotope geochemist with a particular interest and expertise in high-precision geochronology and its application across a variety of geoscience disciplines. It was my great fortune in the early 2000s to be introduced to the stratigraphic community and the Late Paleozoic in particular through my colleague at Boise State University, Dr. Vladimir Davydov. Over the past decade the problems of time scale definition, correlation, and calibration have been a significant component of our joint research program and the more general work of our Isotope Geology Laboratory at Boise State. I have also been a co-editor for radioisotope geochronology of the Geologic Time Scale 2012 and continue in that role for the upcoming volume in 2020, and thus have an intimate knowledge of nearly every radioisotopic constraint on the Phanerozoic (and Neoproterozoic) time scale.

Regarding the proposal for the base of the Sakmarian, our work on integrated quantitative biostratigraphic compositing and radioisotopic dating in the Usolka and other southern Urals sections has been published (Schmitz and Davydov, 2012) as referenced in the proposal. Given our exhaustive efforts to find and date volcanic ash beds globally throughout the Pennsylvanian-Cisuralian interval, I can confidently state that the Usolka section provides a unique opportunity for time scale definition from a radioisotopic perspective; we have found no other stratigraphic section and interval with the same intercalation of biostratigraphy and volcanism. As such the proposal appears first-rate in terms of this criterion for establishing a GSSP.

29th October 2016, from Spencer Lucas

All:

The proposal for the base Sakmarian GSSP is a good one.

The only obvious deficit of the Usolka section is its thin, condensed nature. However, the auxiliary GSSP at the Kondurovsky section compensates for this.

The strengths of the Usolka section are well detailed in the proposal--extensive conodont records, good fusulinid record and already completed chemostratigraphy and radioisotopic dates.

Clearly, the best (most correlate-able) primary signal for the GSSP is the FAD of *Mesogondolella monstra* in the section.

I find the *Sweetognathus* lineage problematic until a thorough taxonomic revision of its species is undertaken and published.

Let me suggest two ways to improve the proposal:

1. Explicit statements about the secondary signals for correlation of the GSSP level (fusulind, chemostratigraphic, numerical) should be stressed. Some of the Permian stage GSSPs have been defined by conodont events with little explicit attention being paid to the other signals that can be used to correlate the GSSP levels. These secondary signals need to be stressed in the proposal, because they add to the correlateability of a GSSP level, and thereby will add to the quality of the proposal.
2. Also, do remember that the GSSP is simply a point in the section--the GSSP is not the conodont event. That event is the primary signal by which the GSSP point/level is correlated. I realize that may seem bit semantic, but I think that it is an important distinction, and the proposal should be clear on that. Thus, there is the GSSP point, its primary signal (here well founded as the FAD of *M. monstra*) and the secondary signals (fusulinid events, chemostratigraphic events, numerical ages). That is the way to present it, and so presented it makes for a conceptually clear and strong proposal.

29th October 2016, from Charles Henderson

I agree with Spencer that it is important to emphasize the other secondary signals - the more of them the better in order to correlate away from the GSSP. While it may be a bit semantic, I agree that we are defining a point in a section. We disagree in the sense that, in my view, the point has to have a definition (not just a signal), and in most cases that has been a conodont FAD. That does not preclude the importance of all the other markers - they are essential in the absence of the conodont species, and even if the conodont species is there they will help determine if we are close to the FAD or simply within the taxon range. It may be a bit semantic, but we define a point that we think is correlatable and correlate away from it.

We have at least two "votes" for *M. monstra*. I would really like to hear from Valery on this matter. Many of us (everyone that has replied) agree that Usolka is a good section (maybe not perfect, but no section is).

29th October 2016, from Spencer Lucas

Agreed--and a bit of semantics thrown in for good measure!

3rd November 2016, from Ausonio Ronchi

Dear all,

Sorry for my delayed contribution to the discussion. Like Joerg, I am not an expert on marine biostratigraphy but, since I had the possibility in 2015 to check in the field the Usolka type section for the Sakmarian–base, I would say that I was positively impressed. Apart from fundamental data that occur in the section, like conodont records, fusulinids and already completed chemostratigraphy and radioisotopic ages, the outcrop and logistic conditions also look good. The proposal in Permophiles 63, even though still a draft, shows a large amount of data, sufficient for a vote on the GSSP.

Therefore I agree with Charles: the Usolka is a good (condensed) section (maybe not perfect, but no section is) as the base Sakmarian GSSP.

10th November 2016, from Tamra Schiappa

All,

So very sorry that I am now getting into the game. Here are my comments concerning the proposal for the Sakmarian GSSP at the Usolka section.

I agree with all the comments that have been circulated up to this point. I have visited the Usolka section and also think that it is a viable candidate for the Sakmarian GSSP. The conodonts are well documented in this section, and at this point it does not matter whether the *Mesogondollela* or *Streptognathodus* lineage are used as long as correlation to other sections is fully documented in the full proposal. I would also like to see a more detailed stratigraphy/sedimentology section added to the proposal to document the exact occurrences. Using meters above base instead of bed numbers and positions within beds is easier to replicate. Use Mab instead of just meters in bed description.

Here are my comments regarding the specific questions asked by the SPS Chair.

1. Is the Usolka section good enough to be proposed as the GSSP section for the base of the Sakmarian Stage?
   Yes, I think the Usolka section is a viable section and should be considered for the Sakmarian Stage GSSP. I do, however, favor the Kondurovsky section because of its historical preference and more abundant ammonoids, but am satisfied that it is an auxiliary section. It also addresses the concerns that Usolka is a condensed section made by Spencer.
2. Are the palaeontological, geochronologic and geochemical data presented sufficient to be integrated as a formal proposal for SPS and ICS voting? If not, what other work or data need to be added?
   Yes, I think the data provided in the proposal is acceptable and quite detailed. I would, however, like to know if there are any ammonoids in the Sakmarian portion of the Usolka section. When I visited I only collected ammonoids from the Gzhelian and Asselian portion of the section due to time constraints. I understand that the Sakmarian portion of the section is extensive and would like to know where in the section, if any, the ammonoids are found. If they are not present, it would make for an interesting question to answer. I do have Sakmarian ammonoids from the Kondurovsky section and these are most likely good indicators of the species that were present during the time of deposition. Including a more detailed discussion on the ammonoids would improve this proposal (but probably not essential). You do mention in the proposal that “slowly accumulating sediments at the Usolka section are enriched in fossils,”--what type?
3. If you agree with the Usolka section as the candidate of GSSP, which lineage we should choose to define the GSSP?
   The *Mesogondolella* lineage seems like the best choice, and there are sufficient data for correlation to other sections.
4. Which index species should we choose to define the GSSP? Specifically, *Mesogondolella uralensis* or *M. monstra* (if we use the *Mesogondolella* lineage)?
   Either species would satisfy the definition for the GSSP, however I would like to see further discussion on the benefits of one over the other. (This may have already occurred and I have just missed it).
5. Other suggestions to improve the proposal?
   In addition to the comments I made above, I would like to see a more detailed discussion of the sedimentology and stratigraphy. This is a deeper water section, but there are still turbidites present. One of the arguments against Kondurovsky is that there are redistributed fossils found in the basal units of the turbidites. This may be true at the Usolka section as well, and, if not, then a brief note explaining this should be added.

11th November 2016, from Yichun Zhang

Dear all,

Very sorry for the late reply.

I think the Usolka section is suitable for a Sakmarian-base GSSP section.

I am not a conodont expert, but, I think the FAD of *Mesogondolella monstra* is better than the FAD of *Mesogondolella uralensis* because of the following two reasons: Firstly, *Mesogondolella* *monstra* can provide a good correlation between the Urals and North America whereas *M. uralensis* has not been recognized yet outside the Urals. Secondly, I have paid attention to the fusuline species *Sphaeroschwagerina sphaerica*. This species was widely reported in the Tethys region and conventionally considered to be an upper Asselian species. As outlined in the proposal, the FAD of *Mesogondolella monstra* is close to the last occurrence of *Sphaeroschwagerina sphaerica*.

8th December 2016, from Joerg Schneider

Dear All,
We have had a lot of answers to the first call of Shuzhong: Mike Stephenson, with his comments on palynomorphs; Mark Schmitz with his comments on Usolka ("…no other stratigraphic section and interval with the same intercalation of biostratigraphy and volcanism… first-rate in terms of this criterion for establishing a GSSP"); Spencer Lucas with his critical comments (The strengths of the Usolka section are well detailed in the proposal--extensive conodont records, good fusulinid record and already completed chemostratigraphy and radioisotopic dates. … secondary signals need to be stressed in the proposal, because they add to the correlateability of a GSSP level, and thereby will add to the quality of the proposal); Charles Henderson in his reply to Spencer and Spencer in his reply to Charles (I agree with Spencer that it is important to emphasize the other secondary signals - the more of them the better in order to correlate away from the GSSP). While it may be a bit semantic, I agree that we are defining a point in a section. We disagree in the sense that, in my view, the point has to have a definition (not just a signal), and in most cases that has been a conodont FAD. That does not preclude the importance of all the other markers - they are fundamental in the absence of the conodont species and even if the conodont species is there, they will help to determine if we are close to the FAD or simply within the taxon range. It may be a bit semantic, but we define a point that we think is correlatable and correlate away from it. Tamra Schiappa´s states "The *Mesogondolella* lineage seems like the best choice and there is sufficient data for correlation to other sections". All of this is well summarized by Charles: "Many of us (everyone that has replied) agree that Usolka is a good section (maybe not perfect, but no section is)."
I think, Shuzhong is right, to propose that he will work out a revised proposal with Valery and other members and then we will vote for the proposal.

13th December 2016, from Valery Chernykh

I agree to use *M. monstra* as the marker of the lower boundary of the Sakmarian.
I want to focus your attention on the fact that this form appears together with another
characteristic form – *Sweetognathus binodosus*. If one considers that the latter is found
in Canada, USA and, possibly, in China, then we can use this taxon as an additional marker
of the boundary.

Base-Sakmarian GSSP: additional points supporting a proposal to use the FAD of *Mesogondolella monstra*

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The GSSP for the base-Sakmarian Stage had been proposed again by Chernykh et al. (2016) in Permophiles 63. The proposed marker, *Mesogondolella uralensis,* is indicated, but *M. monstra* is offered as a potential alternative. *Sweetognathus merrilli* is no longer suggested as a marker and *S. binodosus* has the same FO as *Mesogondolella monstra* in the Urals and North America. *Mesogondolella monstra* and *Sweetognathus binodosus* are considered as immediate descendants of *Mesogondolella uralensis* and *Sweetognathus 'merrilli'* respectively (Chernykh, 2005, 2006). Therefore, there are now four species on the table related to the proposal.

Mesogondolella uralensis and *M. monstra* are "deep-water" conodont species and have a distribution that should be wider than *Sweetognathus merrilli* and *S. binodosus,* which are "shallow-water" conodont species. In fact, *S. merrilli* (or *S. 'merrilli'*) was reported from the Urals (Chernykh, 2005, 2006), North America (New Mexico, Kozur and LeMone, 1995; west Canada, Moore and Henderson, 1998; Kansas, Boardman II et al., 2009), Central Iran (Leven and Gorgij, 2011) and ?North China (Gao et al., 2005), and *S. binodosus* was reported from the Urals (Chernykh, 2005, 2006), North America (West Texas, Davydov et al., 2005; west Canada, Zubin-Stathopoulos et al., 2013; ?Nevada, Wardlaw et al., 2015) and ?Central Iran (Balini et al., 2015), but *Mesogondolella uralensis* has only been reported from the Urals (Chernykh et al., 2016). Fortunately, *M. monstra* has a relatively wide distribution, including in, at least, the Urals (Chernykh, 2005, 2006), North America (Nevada, Chernykh et al., 2016), Central Iran (Balini et al., 2015) and Thailand (as cf. *monstra*: Metcalfe et al., 2017), and is a deep-water species that can provide correlation between Paleotethys and western North America (Fig. 1).

The evolutionary lineage, *Mesogondolella arcuata-M. uralensis-M. monstra*, provides a foundation for the GSSP of the base-Sakmarian Stage. The major difference between *M. uralensis* and *M. arcuata* is the diagnostic characteristic of carinal denticles. However, *M. uralensis* is very similar to *M. arcuata* in some individuals (e.g., fig. 5.21 vs. fig. 5.16, fig. 5.19 vs. fig. 5.13 in Chernykh et al., 2016), because both of them have a robust cusp and a similar outline of platform. In addition, juvenile *Mesogondolella* specimens usually have more discrete denticles and gerontic specimens of *Mesogondolella* usually have more fused denticles. Thus, the gerontic forms of *M. arcuata* are very similar to *M. uralensis*, and the juvenile *M. uralensis* are very similar to *M. arcuata*, which could make it difficult to identify the boundary between *M. uralensis* and *M. arcuata*. However, *M. monstra* has a relatively smaller cusp, fewer, but larger denticles, and very different platform outline, which can be easily distinguished from *M. uralensis*. Therefore, the boundary between *M. uralensis* and *M. monstra* is a distinct and clearly recognizable point.

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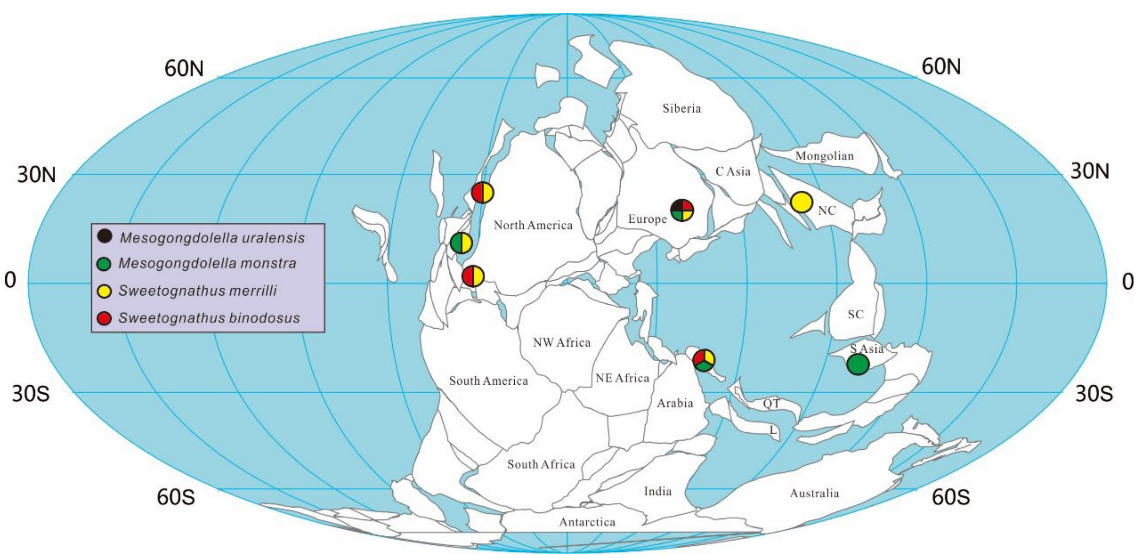

Fig. 1. Reconstruction showing the paleogeographic distribution of *Mesogondolella uralensis*, *M. monstra*, *Sweetognathus merrilli* (or *S. 'merrilli'*) and *S. binodosus*.

Henderson's Harangue #2

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Introduction

As an attempt to stimulate debate or perhaps simply because something smells fishy, I deliver my second harangue. In Italian, it would be "L' arringa di Henderson" (the double "r" is important).

Why do so many people take issue with *Hindeodus parvus* and the PTB?

The species and the Global Stratotype Section and Point (GSSP) at Meishan are often referred to as "a mistake" and "unfortunate" and yet the species and the boundary are standing the test of time under very intense scrutiny, given the prolific number of studies on the boundary. To use the vernacular of the day, we could think of *Hindeodus parvus* as the Donald Trump of the conodont world – seemingly not liked by many, but still the winner. I will discuss this topic from several perspectives. First, I will discuss different philosophies regarding defining a species. Second, I will look at different biostratigraphic methodologies utilized in correlating this species. Third, I will consider the GSSP decision of Meishan. In all of these, the underlying theme will be why do geologists and paleontologists appear to dislike or distrust definitions and decisions by others? It may be true that "science knows no authority", but good science should be authorized. Is there any truth to the following joke? How many geologists does it take to produce a viable GSSP? "One, because if there was a second geologist in the room there will be at least two possibilities". I hope to show that despite some imperfections that are normally expected in the rock and fossil record, the base-Triassic GSSP was worthy of the 2001 celebration at Meishan (see Fig. 1).

When is a species a species?

Easy to ask – not so easy to answer, with the many species concepts that are usually debated by biologists and not so much by paleontologists. The world today is like a single, thin bed in the geologic past – it is a snapshot of evolution in process. Today we have species, species with shades of grey given the possibilities of hybridization and horizontal gene transfer, and incipient species. In any given bed within the range of *Hindeodus parvus* or even before *H. parvus* we might have the same range of possibilities. When does *H. parvus* (Fig. 1A) become a species? Let's assume that the large anterior denticle (ventral, for new-age conodontologists; erroneously referred to as a cusp by some) is genetically controlled. When this mutation first occurs do we have a new species or just the possibility of a new species? When natural selection starts to select in favour of this new form we would have an incipient species –populations would have a few individuals with this large conspicuous denticle, but most specimens would not have it. It is worth considering that a given 10 cm thick sample from the upper Changxing Formation might actually preserve the growth series from more than a thousand conodont generations! From a "population" perspective, it is assumed that *H. parvus* has not yet become a species in this case – the population would be referred to *H. praeparvus* with rare specimens of the "parvus" morphotype. This is exactly what we see in the rock record. The FO (FAD?) of *H. parvus* (several specimens with the large denticle, fewer without) at the Meishan section D is in

Fig. 1. Clockwise from A. *Hindeodus parvus* from Dawen microbialite (JAES 2009, 36, 442-458), B. Charles Henderson and Aymon Baud at 2001 Ribbon Cutting, C. Circus-like atmosphere in 2001 at Meishan celebrating the fact geologists made a decision on the PTB, D. GSSP close-up with magnetostrat trace fossils for scale, E. Geopark at Meishan in November 2003.

bed 27c (and more in 27d and many more in bed 28). However, intense sampling lower at Meishan (beds 27a, b) and elsewhere have revealed a few rare "parvus" morphotypes before the species becomes the dominant form. This has been used as criticism that the FO at 27c is not the FAD and that therefore the GSSP at Meishan fails. I don't think so; to this point the base-Induan GSSP (PTB) at Meishan has served as a tremendous template to correlate the PTB boundary around the world. This is because the base-Induan is correlated with many stratigraphic tools, including major carbon isotopic shifts, magneto-polarity zones and geochronologic ages. Finding *H. parvus* in other regions simply says we are within the parvus Zone – not necessarily at its FAD. In fact, we shouldn't expect to find the FAD except where it evolved in an isolated population. Is it possible to know where this isolated population was located? Possibly? One place that *Hindeodus parvus* almost certainly did not evolve was the Sverdrup Basin in the Canadian Arctic. My oft-cited short paper with Aymon Baud (IGC30 proceedings 1997, 143-152) demonstrated the occurrence of *H. parvus* at Otto Fiord above the range of *Otoceras concavum* and within the range of *O. boreale*. I only have a few specimens, they lack morphologic variability and there are no indications of a preceding *Hindeodus* lineage – this is almost certainly a migration event of a new successful species, very early in the Triassic. The species likely evolved in an area where morphologic variability was high and where certain morphotypes became more common relatively quickly, as in South China, but it could have evolved elsewhere in the eastern Tethys. Rather than the taxonomic battles that currently dominate the literature, we could actually set about testing these concepts. The other key genus in this interval is *Clarkina*, but despite cladistic analysis (Palaeoworld 2007, 16, 190-201) that proved its validity, there are still paleontologists that insist on calling them *Neogondolella*, which is a Middle Triassic genus. We seem to love to disagree.

In most cases the temporal distribution of *Hindeodus parvus* has been assessed using partial range lineage Interval Zones (IZ). More recently, some paleontologists (ESR 2016, 155, 153-171) are utilizing Unitary Associations (UA), claiming that it is a superior quantitative technique. It is important to note that the working data of both IZ and UA are FOs and LOs (Permophiles 47, 2006, 8-9). The analysis of any part of the fossil record is fraught with pitfalls that cannot be smoothed over with quantitative analyses alone. UA analysis has many assumptions and adjustments as part of the process and it is as dependent on taxonomic philosophy as are IZ analyses. But, UA analyses do offer a test and provide levels of uncertainty that are of definite value. Resolving issues of species temporal ranges with the precision required by today's high-resolution questions, demands the inclusion of as many techniques as possible (isotope stratigraphy, magnetostratigraphy, sequence stratigraphy, and geochronology to name a few). Correlation in time becomes an activity of stratigraphy only when we utilize all of these techniques. Currently our approach seems to be decided by who speaks loudest at a convention session, but perhaps it would be better to constructively deliberate within teams of specialists.

The strongest critique of the Meishan base-Induan GSSP is that the interval in question is condensed. There is a discontinuity at the top of bed 27a that may either be a firmground or a dissolution surface (Palaeoworld 1998, 9, 147-152). However, there is no such surface at the base 27c bedding plane and the lithofacies throughout 'bed' 27 are similar. Finding a more expanded section of the interval would be very valuable as a secondary reference section, but so far the Meishan template has more-or-less stood the test of time. It is not perfect, but no section in the rock record is perfect. Years ago, my Mom taught me a valuable lesson when she said "who said life is always supposed to be fair". In the same vein, why do we constantly search for the perfect GSSP. GSSPs are decided through many years of work by many well-meaning professionals. They are voted on by working groups, subcommissions, the International Commission on Stratigraphy and IUGS and at each level the proposals are scrutinized. It is probably easier to pass a bill in Canada's House of Commons or the US Congress. GSSPs define stages, systems and erathems of the International Chronostratigraphic Chart or Geologic Time Scale (GTS), thereby providing us with a common language. Even though a lot of science is conducted in the process, a GSSP definition is not really science, but rather the prerequisite for further science. Accepting these sections and points provides the opportunity to look between the defined boundaries for other stratigraphic stories and events. Wouldn't it be nice to say that we finally finished the GTS that Sedgwick, Murchison and others began 200 years ago! When done, unlike the upcoming presidency south of the Canadian border, stratigraphic geologists and paleontologists will not have uncertainty in our work – instead we will know with a reasonable degree of certainty how our local units correspond to the boundaries of all systems and stages of the Phanerozoic and even a couple below.

Palynology in the establishment of GSSPs

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In a comment in Permophiles 59, June 2014, I mentioned the lack of attention paid to palynology in the development of GSSPs, particularly the Dal'ny Tulkas section for the Artinskian GSSP. The comment certainly was not a criticism of other workers in the field, nor of the process of establishing new GSSPs - which is exemplary. It was more of a comment that palynologists need to get more involved. I think the point is still important.

This year and the last I was involved in an extensive review of the literature of Permian palynostratigraphy. A paper has just been published for an upcoming Geological Society of London Special Publication that summarises the findings (Stephenson 2016; available open access at http://sp.lyellcollection.org/content/early/2016/12/07/SP450.2). The amount of palynological data for the Permian is staggering, probably numbering around a thousand scientific papers and innumerable company reports. In the Permian, palynostratigraphy has been used for decades to correlate coal- and hydrocarbon-bearing rocks within basins and between basins, sometimes at high resolution. Though these palynostratigraphic schemes related to resource extraction have been very successful; their main shortcoming has been a lack of correlation with schemes outside the basins, coalfields and hydro-

Fig. 1. Palynological sampling of the proposed GSSP for the base of Sakmarian Stage at Usolka. The author is indicating the proposed horizon at the base of bed 31.

carbon fields that they serve, and chiefly a lack of correlation with the international Permian scale. This leaves the detailed data derived from commercial palynological work, which may only be correlated at field or regional level, and sometimes codified in company in-house schemes, often inaccessible and not easy to integrate with data from academic studies.

One of the reasons that it is difficult to correlate palynological assemblages precisely to the international Permian scale is because Permian GSSPs are generally correlated by marine faunas (chiefly by conodonts) but also by fusulinaceans and ammonoids. Marine environments that favour preservation of conodonts and fusulinaceans often are not favourable to palynomorphs.

To my knowledge, none of the Permian GSSPs, either those established or those in development, have explicit palynological correlatives (in other words palynological ‘events’ that are recommended by authors that can be used to correlate a GSSP). The nearest we have are descriptions of the palynology of the Aidaralash Creek basal Permian GSSP by Dunn (2001) and that of the basal Triassic at Meishan by Ouyang and Utting (1990). Both papers are full and detailed accounts of the palynology in and around these two GSSPs but do not particularly concentrate on palynological correlatives – for example useful taxa of combinations of taxa that could be used to correlate the boundary. This of course could be because no suitable correlatives could be found at the time. Even if a palynological correlative could be found, further work in other sections would be needed to corroborate and support the ‘event’.

With the efforts that have been going in to correlate the new Cisuralian and other Permian GSSPs it would seem sensible to make a parallel concerted effort to bring palynology into correlation of GSSPs. Of course this is not always as easy as it sounds.

As I mention in the short comment in Permophiles 59, I collected samples from the Dal’ny Tulkas section which was at the time the candidate GSSP for the base of the Artinskian and reported on them in Permophiles 50 (Stephenson, 2007). The main purpose of the study was to investigate the possibility that the palynological succession may help to correlate the proposed GSSP. The palynological samples were found to be dominated by indeterminate non-taeniate and taeniate bisaccate pollen, *Cycadopites* spp. and *Vittatina* spp. Algal (?) forms such as *Azonaletes* cf. *compactus* and ‘Algal palynomorph sp. A’ were also locally common (see fig. 1, Stephenson, 2007). At the time, it was difficult to be sure whether palynology could be used to correlate the boundary partly because of the rather poor preservation, and partly because my knowledge of the whole Russian Cisuralian palynological sequence was not complete enough to determine where there was something palynologically unique about the section in Dal’ny Tulkas (particularly at the level of the proposed GSSP) to distinguish and correlate it. However the taxa that I called *Azonaletes* cf. *compactus* and ‘Algal palynomorph sp. A’ (see Stephenson, 2007) seemed to hold some promise. This potential needed investigation and corroboration at other alternative boundary sites and better understanding of the palynological succession throughout the Cisuralian.

Perhaps a concerted effort, between Russian, US and Chinese palynologists, and those interested in GSSP correlation needs to be established, perhaps with funding for PhD studentships. Key sections could be revisited , perhaps Meishan and Aidaralash Creek, for very high resolution palynological sampling, and for focus on candidate taxa to refine their taxonomy. The benefits of a few well-characterised, palynomorph taxa known to occur at GSSPs would allow palynologists to identify key chronostratigraphic levels in palyniferous successions, for example isolated non-marine cratonic basins.

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The Newwellian Substage of the Wolfcampian Stage in the southwestern United States

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Introduction

The Wolfcampian Series of Adams et al. (1939) is based on a stratigraphic section in the Glass Mountains of West Texas, USA, where the "type" Wolfcampian strata rest unconformably on underlying strata (Fig. 1). However, since at least the work of Thompson (1954), American fusulinid workers have included an interval older than the "type" Wolfcampian strata in a tripartite Wolfcampian Stage. This interval either is not present in the Glass Mountains or is incompletely represented there by a thin limestone interval underneath the unconformity at the Wolfcampian base (Ross and Ross, 2012). This lower interval of the Wolfcampian is well known by fusulinids from the Bursum Formation of New Mexico, and is also well recognized in the West Texas Permian basin (e.g., Zone PW-1 of Wilde, 1990). The base of the "type" Wolfcampian is thus correlated as the base of the middle Wolfcampian, which has also been called the Nealian (e.g., Ross and Ross, 2003). The Bursum Formation of central New Mexico, which yields *Thompsonites* ("*Schwagerina*") and other fusulinids long regarded as of early Wolfcampian age (e.g., Thompson, 1954; Lucas et al., 2000), was thus considered to be the oldest Permian unit in the western USA.

In 1998, the International Commission on Stratigraphy ratified the definition of the base of the Permian (= base of Asselian Stage) to lie at the level of the first appearance of the conodont species *Streptognathodus isolatus* at Aidaralash Creek in western Kazakhstan (Davydov et al., 1998). Prior to that time, North American workers placed the Permian base at the base of the Wolfcampian Series (or Stage in most usages), basing it on the first appearance of the fusulinid *Thompsonites*/"*Schwagerina*" (or "*Pseudofusulina*" in other usages). However, most workers concluded that the definition of the base of the Permian by the first appearance of the conodont *Streptognathodus isolatus* is younger than the Wolfcampian base, closer to the base of the middle Wolfcampian (e.g., Baars et al., 1992, 1994a, b; Wahlman, 1998; Sanderson et al., 2001; Wahlman and King, 2002; Lucas et al., 2013). (Note, however, that the LO of *S. isolatus* in the Bennett Shale of Kansas is associated with Bursum-age fusulinids [Boardman et al., 2009; Wahlman and West, 2010], leading to the conclusion that the conodont-based definition of the base of the Permian is close to the LO of "*Schwagerina*" *auctorum* in the North American section [Schmitz and Davydov, 2012; Davydov et al., 2013] or that the LO of *S. isolatus* is diachronous [Lucas, 2013]).

Attempts to resolve the perceived mismatch of the bases of the Wolfcampian and Permian have generally redefined the Wolfcampian so that its base is equivalent to the Permian base. The "early Wolfcampian" of previous usages has either been given a separate stage/substage name (Bursumian of Ross and Ross, 1994; Newwellian of Wilde, 2002) or simply has been considered the younger portion of an extended Virgilian (e.g., Baars et al., 1994a, b) (Fig. 1).

Bursumian

In the standard global chronostratigraphic scale, each system base conventionally corresponds to the base of a stage. Therefore, many workers believe that the secondary standard (*sensu* Cope, 1996) provided by the North American regional stages (in this case the Virgilian and Wolfcampian) should also have the Carboniferous (Pennsylvanian)-Permian System boundary correspond to the base of a stage, but this requires some modification or redefinition of the regional stages. Baars et al. (1992, 1994a, b), working in Kansas, proposed to solve this problem by redefining the Virgilian Stage to encompass strata previously included in the lower Wolfcampian (Fig. 1). A second solution, advocated by Ross and Ross (1994), was to recognize an uppermost Carboniferous Bursumian Stage equivalent to the lower Wolfcampian of earlier usage (Fig. 1).

The problematic nature of the Bursumian Stage was discussed at length by Lucas and Wilde (2000), Lucas et al. (2000, 2001), Davydov (2001) and Wilde (2002, 2006) and can be summarized as follows:

1. A Bursumian Stage will always lack an ideal stratotype in the Bursum outcrop belt in New Mexico. This is mostly because the Bursum Formation is everywhere overlain by nonmarine siliciclastic red beds of the lower Permian Abo Formation, making

definition of the top of the Bursumian impossible in the Bursum outcrop belt.

2. Bursumian is equal to only one or at most two fusulinid zones, so the concept of a "Bursumian" stage is no more than a stage name applied to one or two fusulinacean zones.

3. The Bursum Formation has a macroinvertebrate fauna of essentially Virgilian aspect, so on macroinvertebrates alone, its affinities are Virgilian. There is no distinctive "Bursumian" macrofauna.

4. If there is value in defining a new stage or other named chronostratigraphic unit (substage) between the Virgilian and Wolfcampian, it should be defined outside the Bursum outcrop belt.

5. The interval called "Bursumian" may best work as a substage of the Wolfcampian, comparable to the Nealian or Lenoxian. However, this does not realign the North American stage boundaries to match the conodont-based Carboniferous-Permian boundary.

6. From a global standpoint, Bursumian may be a synonym of the Orenburgian substage, which has been designated the latest Gzhelian (Davydov, 2001).

Definition of the Newwellian

Acting on points 4 and 5 above, Wilde (2002) defined a Newwellian substage of the Wolfcampian outside of the Bursum outcrop belt as a precisely-defined substage term intended to replace Bursumian. Newwellian takes its name from New Well Peak in the Big Hatchet Mountains of southwestern New Mexico, USA (Fig. 2). Wilde (2002, 2006) identified the Newwellian stratotype as an interval of the Horquilla Formation at New Well Peak that is 69 m thick. He stated that the Newwellian is "defined by 18 species of *Triticites*, two of *Leptotriticites*, three of *Pseudofusulina*, one of *Schwagerina* s. s., and a very interesting new species of what, at present, is being referred to as *Alpinoschwagerina*, which occurs in the same beds with the highest occurrence of *Triticites*" (Wilde, 2002, p. 60). Subsequently, Wilde (2006) provided extensive documentation and precise taxonomy of the Newwellian fusulinids he collected at New Well Peak.

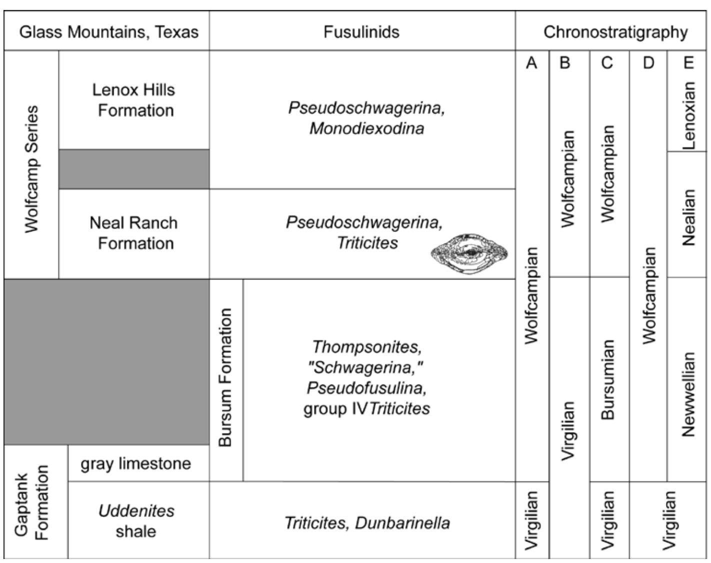

Fig. 1. Type Wolfcampian section in the Glass Mountains of Texas and proposed stage and substage nomenclature. Column A is the traditional stage usage. Columns B and C are attempts to resolve the perceived mismatch of the Wolfcampian base with the base of the Permian by redefining the base of the Wolfcampian and extending the Virgilian (B) or inserting a Bursumian Stage between the Virgilian and Wolfcampian (C). Columns D-E do not attempt to rectify the mismatch but show the subdivision of the Wolfcampian into three substages, Newwellian, Nealian and Lenoxian.

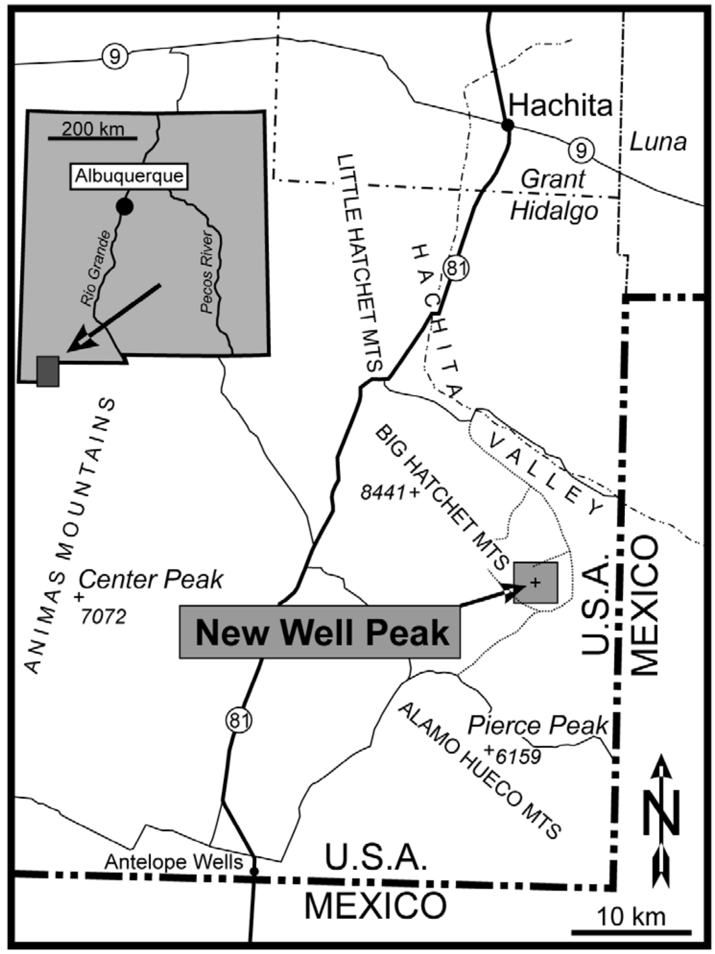

Fig. 2. Map showing the location of New Well Peak in the southeastern part of the Big Hatchet Mountains, New Mexico, USA.

The LOs (lowest occurrences) of "*Schwagerina*" and very advanced (ventricose) *Triticites* ("group IV" of Wilde 1975, which are large, fusiform and extremely inflated) mark the base of the Wolfcampian at New Well Peak according to Wilde (2006). He identified this base about 725 m above the base of the Horquilla Formation section. He divided the Wolfcampian strata at New Well Peak into Newwellian (69 m thick), Nealian (82 m thick) and Lenoxian (120 m, top faulted) intervals. According to Wilde (2006), at New Well Peak, the LO of *Pseudoschwagerina* marks the base of the Nealian, and the species *P. convexa* is characteristic of the Lenoxian. Wilde (2006, figs. 2, 5) considered the LO of "*Schwagerina*" to be the base of the Newwellian. However, his stratigraphic diagrams actually place the Newwellian base at the LO of *Triticites creekensis*, a few m below the LO of "*Schwagerina*."

Redefinition of the Newwellian

Here, we present a precise redefinition of the Newwellian substage based on recently collected data in press (Krainer et al., 2017; Lucas et al., 2017). We believe this redefinition honors the original concept of Newwellian of Wilde (2002), but makes its boundaries more precise. At New Well Peak, we identify the Wolfcampian base about 965 m above the base of the Horquilla Formation section (Fig. 3). Our sample NWP52 (segment A: Fig. 3) has the LO of *Thompsonites* (a name which currently replaces "*Schwagerina*" sensu Dunbar and Skinner and *auctorum* non sensu Möller; see below), so this is the traditional Wolfcampian base, which is the base of the Newwellian fusulinid substage of Wilde (2002). Sample NWP 22 (segment B: Fig. 3) has the lowest occurrence of *Pseudoschwagerina*, typically taken to mark the base of the Nealian substage of the Wolfcampian. Our data indicate a Newwellian thickness of ~ 108 m.

Note that Wilde (2002, 2006) used Zeller's (1965) lithostratigraphy of the Horquilla Formation. However, there are discrepancies between Zeller's (1965) measured thicknesses and ours. These are mostly due to differences in the dip angle applied to measurements in the field and to how a fault zone in the lower part of the Horquilla Formation was bridged. Nevertheless, our section of the Newwellian interval of the Horquilla Formation at New Well Peak (Fig. 3) was first shown to two of us (KK and SGL) by the late Garner Wilde in 2002, so it is the same section he studied.

The Newwellian here is precisely defined as the interval between the LOs of *Thompsonites*/"*Schwagerina"* and *Pseudoschwagerina* in the New Well Peak section (Fig. 3). *Thompsonites* Bensh, 1987 has taxonomically replaced *Schwagerina* sensu Dunbar and Skinner, 1936 and 1937, which differed from *Schwagerina* Möller, 1878 emend. Davydov, 1984. Independent of Davydov's (1984) emendation, this latter taxon was also called *Globifusulina* Alekseeva, Izotova and Polozova in Izotova, Polozova and Alekseeva (sic), 1983 (see Bensh, 1987; Loeblich and Tappan, 1987; and Rauzer-Chernousova et al., 1996). *Schwagerina* is phylogenetically related to *Daixina* (Davydov, 1988), whereas *Thompsonites* is related to *Triticites*, so the two genera correspond to two different lineages. Furthermore, *Schwagerina* is a Tethyan and Uralian taxon, whereas *Thompsonites* is endemic to North America.

The Newwellian is well characterized in this interval by abundant "group IV" *Triticites, Thompsonites (= Schwagerina)* and relatively uncommon *Leptotriticites*. The common fusulinids present are *Triticites cellamagnus* Thompson, *Tr.* ex gr. *cellamagnus*, *Tr. imperialis* Kauffman and Roth, *Tr. pinguis* Dunbar and Skinner, *Tr.* cf. *creekensis* Thompson, *Tr.* aff. *secalicus* (Say in James), accompanied by *Thompsonites longissimoidea* (Beede emend. Thompson), *Th. emaciata* (Beede), *Th.* cf. *emaciata*, *Th. campensis* (Thompson), and various other *Thompsonites.* Two other secondary fusulinid markers, *Pseudofusulina* sp. and *Biwaella americana* Skinner and Wilde, have their LO in the type Newwellian. Other fusulinids of the assemblage are *Staffella powwowensis* Thompson, *Nankinella* sp., *Schubertella* cf. *ciscoensis* Kauffman and Roth, *S. sphaerica* Suleimanov, and *S. kingi* Dunbar and Skinner.

Most of the smaller foraminifers in the Newwellian stratotype are of little biostratigraphic value: *Tuberitina bulbacea* Galloway and Harlton, *Eotuberitina reitlingerae* Miklukho-Maklay, *Diplosphaerina* sp., *Spireitlina conspecta* (Reitlinger), *Endothyra* ex gr. *similis* Rauser-Chernousova and Reitlinger, *E.* sp., *Planoendothyra* sp., *Bradyina* spp., *Palaeotextularia* sp., *Climacammina* sp., *Deckerella* sp., *Tetrataxis* ex gr. *conica* Ehrenberg, *T.* ex gr. *acuta* Durkina, *Globivalvulina* ex gr. *bulloides* (Brady), *G. scaphoidea* Reitlinger, G. spp., *Ammovertella* sp., *Calcivertella* sp., *Calcitornella* sp., *Palaeonubecularia* sp., and *Hedraites* sp. More biostratigraphically useful are *Raphconilia modificata* (Potievskaya), *Bradyina lucida* Morozova, *Climacammina*? *sphaerica* Potievskaya, *Syzranella* sp., *Nodosinelloides bella* (Lipina), *N. netschajewi* (Cherdyntsev), *N. potievskayae* Mamet and Pinard, *N. longissima* (Suleimanov), and *N. longa* (Lipina) (see discussion in Yarahmadzahi et al., 2016).

Dasycladales are diverse in the Newwellian stratotype, with *Epimastopora* cf. *likana* Kochansky-Devidé and Herak, *E.* sp., *Gyroporella clavata* Chuvashov, *G. dissecta* Chuvashov, *G.* sp., "*Atractyliopsis*" *carnica* Flügel and *Connexia* cf. *fragilis* Kochansky-Devidé. These taxa could have a relatively important biostratigraphic value, especially in comparison with the traditionally studied microflora of the Urals (Chuvashov, 1974), the Carnic Alps (Flügel, 1966; Vachard and Krainer, 2001a, b) and Croatia (Kochansky and Herak, 1960). The phylloid algae *Eugonophyllum* spp. and *Archaeolithophyllum lamellosum* Wray, as well as the algospongia *Ungdarella* ex gr. *uralica* Maslov and *Claracrusta* ex gr. *catenoides*, are other members of the Newellian microfloral assemblage, which need further biostratigraphic study.

Among other possible secondary biomarkers, the following taxa are noticeable: (1) the green alga *Permocalculus* sp., which appeared in the late Virgilian and remained rare in the Newwellian; (2) the foraminiferal-cyanobacterial consortia *Latitubiphytes* sp. 2, *L.* sp. 3, *Tubiphytes obscurus*, and *T.* ex gr. *obscurus* at the summit of the Newwellian; and (3) rare microproblematica palaeoaplysinids, the paleontological study of which could be also interesting.

In collaboration with J. Barrick and S. Ritter, we undertook extensive sampling of the Newwellian strata for conodonts with relatively disappointing results. Thus, Barrick and Ritter (in Lucas et al., 2017) report that sample B79 yielded *Streptognathodus minacutus* Barskov and Reimers, and in B85, *S. invaginatus* Reshetkova and Chernykh 1986 was recovered. Both species occur in lowest Permian strata (Asselian), and range from the *S. isolatus* through the *S. nevaensis* zones (Boardman et al., 2009). *Sweetognathus expansus* Perlmutter is much higher, in samplesB122 and B130. It ranges from the Asselian *S. isolatus* Zone into the Artinskian *S. florensis* Wardlaw, Zone. B135 produced *S. constrictus* Reshetkova and Chernykh, which ranges from the upper Asselian *S. fusus* Zone into the Sakmarian *S. postconstrictus* Zone (Boardman et al., 2009).

Cyclostratigraphy

The 108-m-thick Newwellian interval at New Well Peak (Fig. 3) begins in a 3.7-m-thick algal wackestone that contains the LO of *Thompsonites*/*Schwagerina*. Stratigraphically higher Newwellian

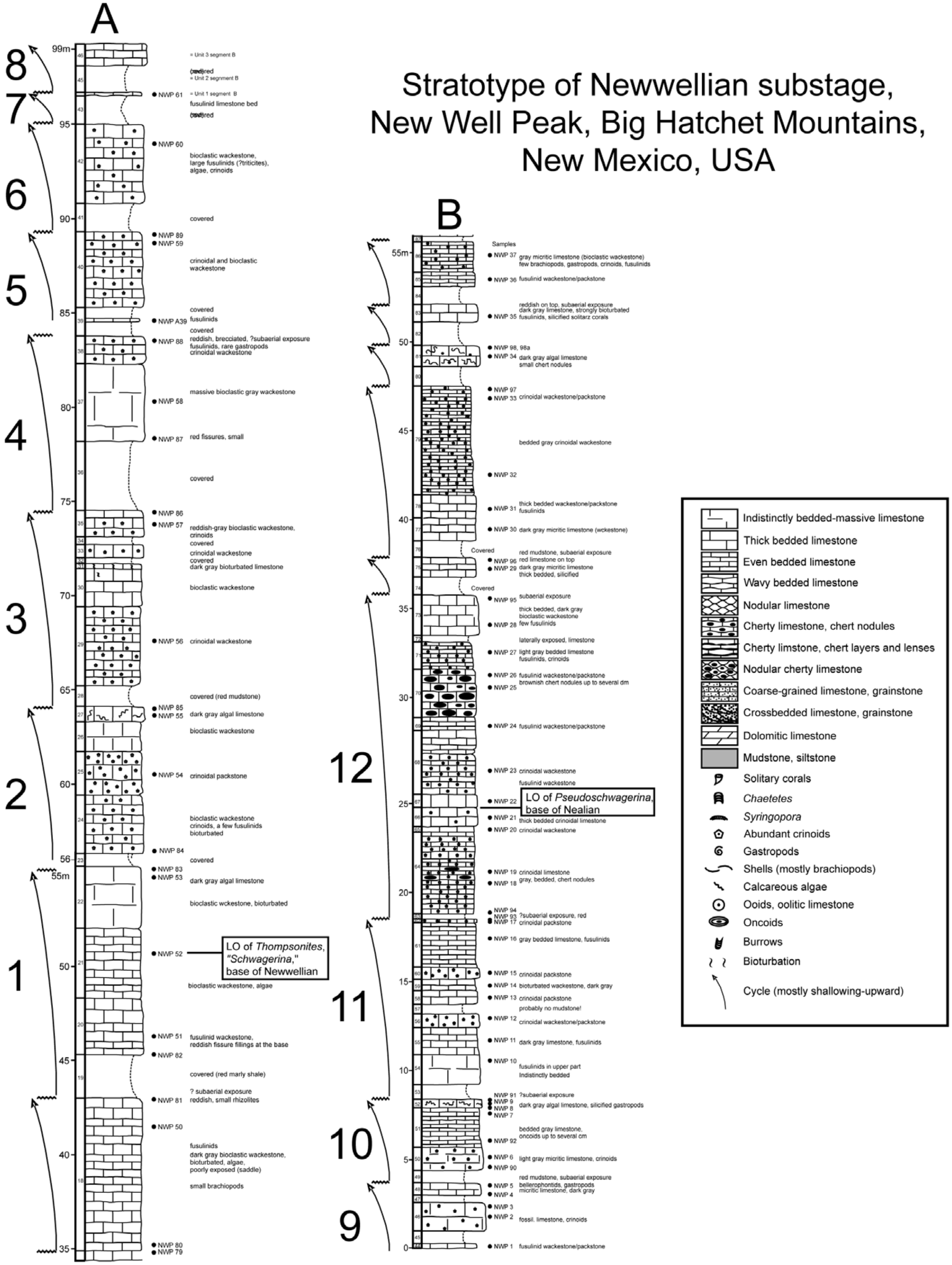

Fig. 3. Stratotype of the Newwellian Substage of the Wolfcampian Stage at New Well Peak. This is an interval of the upper part of the Horquilla Formation measured on the south side of New Well Peak in two overlapping segments, A and B. Sections were measured in the SW ¼ sec. 32, T31S, R14W and NW ¼ sec. 5, T32S, R14W.

limestones are mostly crinoidal, bioclastic or fusulinid wackestones. These limestones lack chert except for one bed near the top of the Newwellian. Covered slopes are few and locally are seen to be salmon-colored/pale red shale. From a lithostratigraphic point of view, the Newwellian strata are more similar to underlying Virgilian strata than they are to the thicker-bedded and more cherty, overlying Nealian strata.

The Newwellian stratotype can be organized into 12 shallowing-upward cycles (Fig. 3). Both the Newwellian base and the Nealian base at New Well Peak are within a cycle, so these chronostratigraphic boundaries do not correspond to obvious disconformities. The cycles are composed of the following lithologies (Fig. 3):

Red mudstone to siltstone (mostly covered), sharply overlain by:

Thin- to medium-bedded muddy limestone (bioclastic wackestone), grading into:

Medium- to thick-bedded, rarely crossbedded bioclastic packstone and grainstone containing abundant crinoidal debris, calcareous algae and locally abundant fusulinids.

On the top of such limestone intervals, which are overlain by red mudstone to siltstone of the next cycle, the limestone is often colored reddish, locally brecciated, displays mudcracks, microkarst features, alveolar structures (rhizoliths) and thin paleocaliche crusts.

These cycles are very similar to the PED 2 (silt-based) cycles described by Soreghan (1994) from the Horquilla Formation elsewhere in the Pedregosa basin, composed of calcareous siltstone grading abruptly into a progradational succession of subtidal wackestone, grading upward into packstone and grainstone, and locally into a peritidal facies. According to Soreghan (1994), cycle tops are characterized by emergence features such as brecciated crusts, calcrete and cryptokarst, rhizoliths and indicators of meteoric diagenesis. Soreghan interpreted these cycles as high-frequency cycles (~ 413 ky) resulting from glacioeustatic sea-level fluctuations. The mudstone-siltstone facies probably represents eolianites (Soreghan et al., 2007) that formed at the beginning of the transgressive systems tract (TST). Subtidal bioclastic wackestone facies also formed during relative sea-level highstand, representing the early highstand systems tract (HST). The packstone-grainstone facies indicates the beginning of sea-level fall (late highstand systems tract). Sea-level lowstand is represented by subaerial exposure surfaces developed on the cycle tops (lowstand systems tract, LST).

If we infer that the cycles in the Newwellian interval at New Well Peak are glacio-eustatic cycles forced by ~ 400 kyr eccentricity, this suggests a duration of the Newwellian of about 4.8 Ma. In the Kansas section, the LO of *Thompsonites/Schwagerina* is in the Glenrock Limestone and the LO of *Pseudoschwagerina* is in the Florence Limestone (e.g., Wahlman and West, 2012; Wahlman, 2013), an interval of 11 or 12 major cyclothems with an estimated duration of 289.6-295.7 Ma, or 6.1 Ma, according to Schmitz and Davydov (2012). One problem, though, with the Kansas section is that it lacks fusulinids in a substantial interval (Stearns Shale through Blue Springs Shale) below the LO of *Pseudoschwagerina*, so that LO may be delayed--stratigraphically too high. Nevertheless, the Newwellian interval corresponds to about 11 or 12 cycles in both the New Well Peak and the Kansas sections, which suggests a Newwellian duration of about 4.8 Ma.

Conclusions

Newwellian is a precisely defined chronostratigraphic concept best regarded as a substage of the Wolfcampian Stage. It is readily correlated by fusulinid biostratigraphy in the USA, especially in Arizona, New Mexico, Texas, Oklahama and Kansas. The Newwellian is approximately 4.8 Ma in duration based on cyclostratigraphic inferences. Whether or not it should be considered Carboniferous or Permian will require resolution of ongoing problems with correlation of the base Permian GSSP.

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Late Permian peat mire ecosystem developed under boreal conditions along the shores of the Mongol-Transbaikalian seaway, Central Mongolia

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The pan global Permian coal measures are unique in the evolution of the Earth, not matched in any period before or since (Carey, 2000). In southern and central Mongolia significant coal deposits accumulated during the Late Permian; the most famous is the Tavan Tolgoi deposit in the South Gobi Basin which may contain up to 10Bt of coking and high energy thermal coal (Michaelsen, 2014). The Permian system in Mongolia contains two separate marine basins which were located in different climatic settings (Manankov, 2004, 2012; Manankov et al., 2006).

This ongoing research project has initially focused on the coal-bearing middle part of a c. 2,600 m-thick Permian-Triassic succession developed along the southern shores of the Mongolian Transbaikalian boreal seaway (Fig. 1). The sedimentary sequences from this seaway are sporadically exposed over c. 900km x 150km in central and NE Mongolia. The study area is located in the Bayanjargalant district in central Mongolia (Fig. 1) and draws on a new integrated database encompassing 38 drillholes (with a composite length of 2.600m), almost 3km of shallow trenching, extensive field work, petrological analysis of sandstone samples, identification of macro-flora, fauna and trace fossils, as well as analytical results from 82 coal quality samples.

The entire Permian-Triassic sedimentary succession was mapped as Upper Permian by Erhembaatar et al. (1993), and was subdivided into five informal stratigraphic units by Erhembaatar et al. (1993). However, this study highlights that the two upper units are distinctly different from the three lower units and contain no organic material (Table 1). Given the striking similarity to Early Triassic alluvial sediments in the South Gobi Basin and the Bowen Basin, Australia (e.g. Michaelsen et al., 2001; Michaelsen, 2002, 2005, 2016) the two upper units are considered Lower Triassic in this study. It is noted that the exact location of the Permian-Triassic boundary within the sedimentary package in the study area is not yet firmly established.

The sedimentary record strongly indicates c. 420m thick Upper Permian coal measures developed along the shores of a relatively shallow boreal seaway during frequent sea-level changes. Eight transgressive–regressive cycles characterized by the inter-digitation of paralic–shallow marine and coal-bearing alluvial facies, are recognized from the stratigraphic record. These cyclothems might represent 405ky Milankovitch eccentricity cycles of the Permian Period.

The immature nature of the fluvial and shallow marine sandstones, coupled with the significant thickness of the Permian succession, suggest relatively rapid sediment aggradation. Petrological analysis showed abundant elongate quartz slivers, indicating a volcanic source.

An 8-12cm thick shellbed is preserved at the base of the coal measures. The shell bed is dominated by cold-resistant relatively robust archaeogastropods and taxodont bivalve taxa with subordinate brachiopods and rare fragmented conulariids. These taxa probably occupied a marginal shallow water habitat somewhat comparable to the recently documented Permian bivalve-dominated assemblage by Simões et al. (2016) from the Paraná Basin in Brazil.

The thickest coal seam (up to 12.8m) is developed above the shellbed at the base of the coal measures, probably during a major regressive event with sufficient time for peat-forming plants to colonize the exposed seaway deposits and aggrade significant tracts of peat (possibly +100m of un-compacted peat in places).

Paleoclimatic indicators within the study area include; (1) glendonites; (2) strongly developed annual growth rings in fragmented petrified tree trunks; and (3) rare ice-rafted debris. These paleoclimatic indicators suggest Late Permian coal formation under boreal conditions. The boreal setting is in agreement with the work by Manankov (2004), Manankov et al. (2006) and Manankov (2012) which focused on brachiopod-rich deposits to the southwest and northeast of the study area. The seaway was likely frozen during the dark cold winter months. During the summer months the cold resistant peat-forming plants (incl. *Taeniopteris* sp., *Rufloria* and *Koretrophyllites*) probably

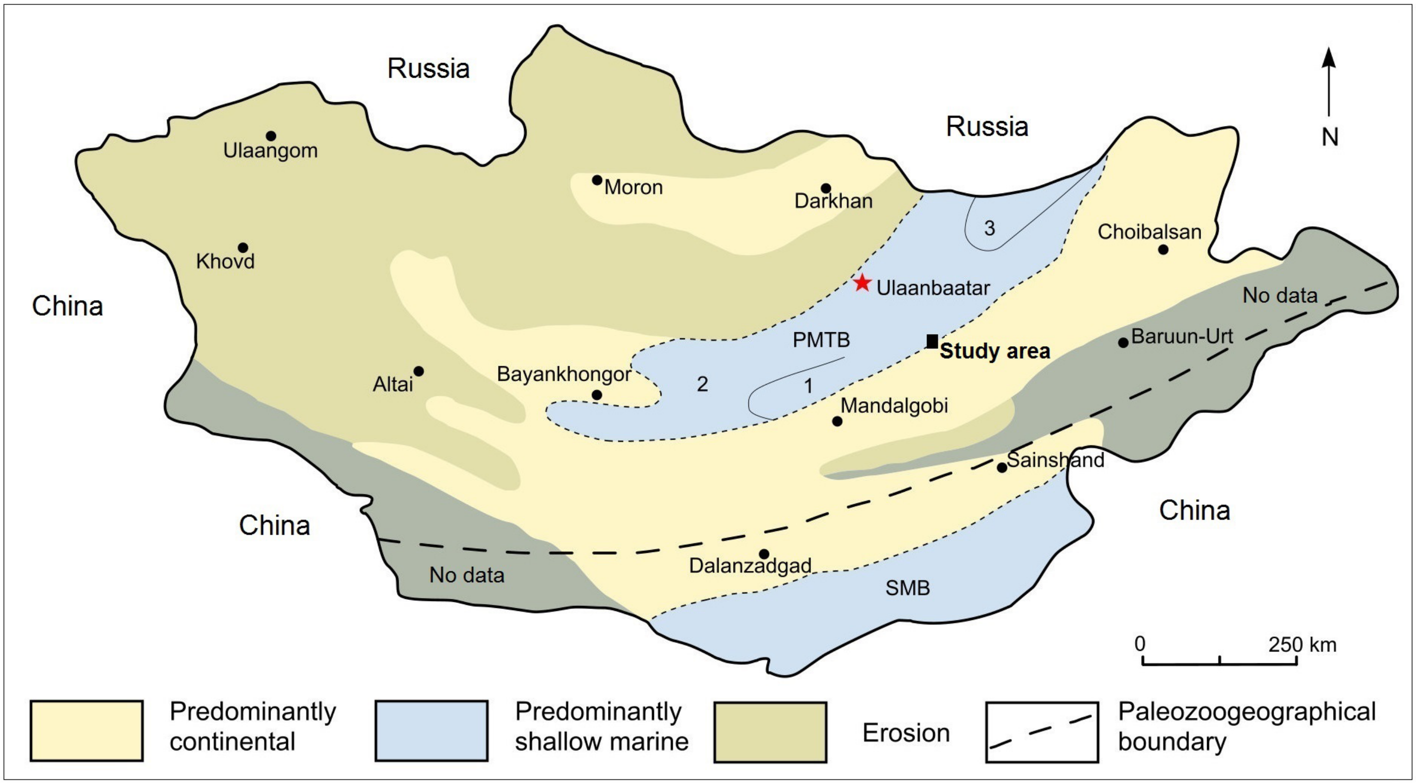

Fig. 1. Generalized spatial distribution map of Permian sedimentary strata in Mongolia showing location of the study area (modified from Manankov et al., 2006).

Chronology	Lithostratigraphy		Approximate Thickness (m)	Fossils	Paleoclimatic Indicators	Depositional Systems	Environmental Conditions
	Formation	Sub-Group					
Early Triassic		T^{1B}	240	None observed		Alluvial	
		T^{1A}	720	None observed		Predominantly fluvio-lacustrine	
Late Permian	Tsenkher Gol	P2 cn^{3B}	180	Abundant *Thalassinoides*, *Skolithos* and *Planolites* ichno fossils in places		Predominantly shallow marine	Common base-level changes
		P2 cn^{3A}	420	*Koretrophyllites* macroflora. Shellbed with cold resistant taxodont bivalves, brachiopods, archaeogastropods and Connulariid fragments. *Ophiomorpha cf. nodosa*. Petrified wood	Glendonites. Petrified wood with strongly developed annual growth rings	Cold resistant peat mire ecosystem developed on coastal plain, inter-fingering with near shore depositional system	Boreal with high-wind regime. High frequency base-level changes. Major sequence boundary developed at base
		P2 cn^{2}	360	*Taeniopteris* sp. macroflora Petrified wood	Petrified wood with strongly developed annual growth rings	Predominantly shallow marine	Boreal. Common base-level changes
		P2 cn^{1}	700	*Rufloria* macroflora. Abundant ichno fossils in places	Glacial erratics		

Table 1. General overview of the five informal Permian-Triassic stratigraphic units and their main attributes.

benefited from moist air currents along the seaway.

It is noted that *Koretrophyllites* occurs prolifically in the > 1,000,000 km^2 large Permian-Carboniferous Tungusska Coalfield of Siberia (Mironov, 1964). It probably grew in tufts similar to extant reeds and rushes around bogs, marshes and swamps. *Koretrophyllites* occupied the same niche as *Phyllotheca* did in the Permian of Australia (Rigby, pers. comm.).

The consistently high mineral matter content within the coal seams (average of 46.95% dry basis from 82 coal core samples) is unusually high for Mongolian Permian coals. It might be linked to the proximity of the shoreline coupled with a high wind regime which propelled fine-grained particles into the peat mire. The frequent thickness variations and unstable nature of the coal seams suggest a syn-tectonic influence on their emplacement (i.e. active growth faults). Overall the Late Permian peat mire ecosystem was influenced by a dynamic interplay of syn-tectonic growth faults, frequent sea-level changes and climatic controls.

None of the fossils documented here provide precise age control on the host rocks. However, a sample from one outcrop stratigraphically underlying the coal-bearing strata reveals a fertile spike of a taeniopterid. This morphology did not develop until earliest Late Permian and continued into the Triassic (Rigby, pers. comm.). In this context the coal measures are considered here to be time equivalent with the upper coal-bearing part of the Upper Permian Tavan Tolgoi Group in the South Gobi Basin (e.g. Michaelsen, 2014, Michaelsen, 2016), and the Rangal Coal Measures in the Bowen Basin (e.g. Michaelsen et al., 1999, Michaelsen, 2002).

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Abstract from "*Biomineralization and global change: A new perspective for understanding the end-Permian extinction*" by Garbelli C., Angiolini L., and Shen S., Geology, 45, 1, 19-22

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In this paper we presented and discussed the possible meaning of qualitative and quantitative data related to the shell microstructure of Upper Permian brachiopods. This data collection is basically the result of a five year study started with my Master Degree and ended with the PhD (both under the supervision of Lucia Angiolini), spaced out by a six month visit to the NIGPAS (advised by Shen Shuzhong). During these years, I screened about 500 brachiopods shells to study their microstructure, preservation and geochemistry. The specimens come from different localities of the Tethys (Fig. 1) and are Late Permian in age. The geochemical analyses of these shell materials have been in part published by Garbelli at al. (2012, 2014 and 2016).

In the recent years, the shell microstructure of Paleozoic brachiopods has been mainly studied for its value as a screening test, to assess the pristinity of the shell (Fig. 2) and the reliability of geochemical values (e.g. Samtleben et al., 2001; Brand et al., 2011). However, in its origins, the brachiopod shell microstructure has been studied more for its paleontological and paleobiological values, to understand the ontogenesis and the taxonomic relationships between fossils as exemplified by the quintessential work of

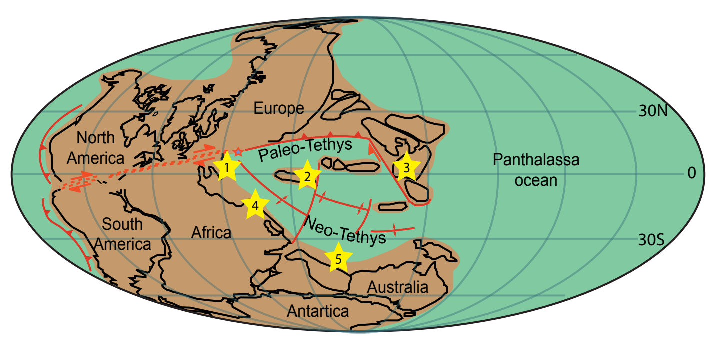

Figure 1. Paleogeographic reconstruction showing the location of the studied section; 1—Dolomites, 2—Iran, 3—South China, 4—Turkey, 5—Tibet.

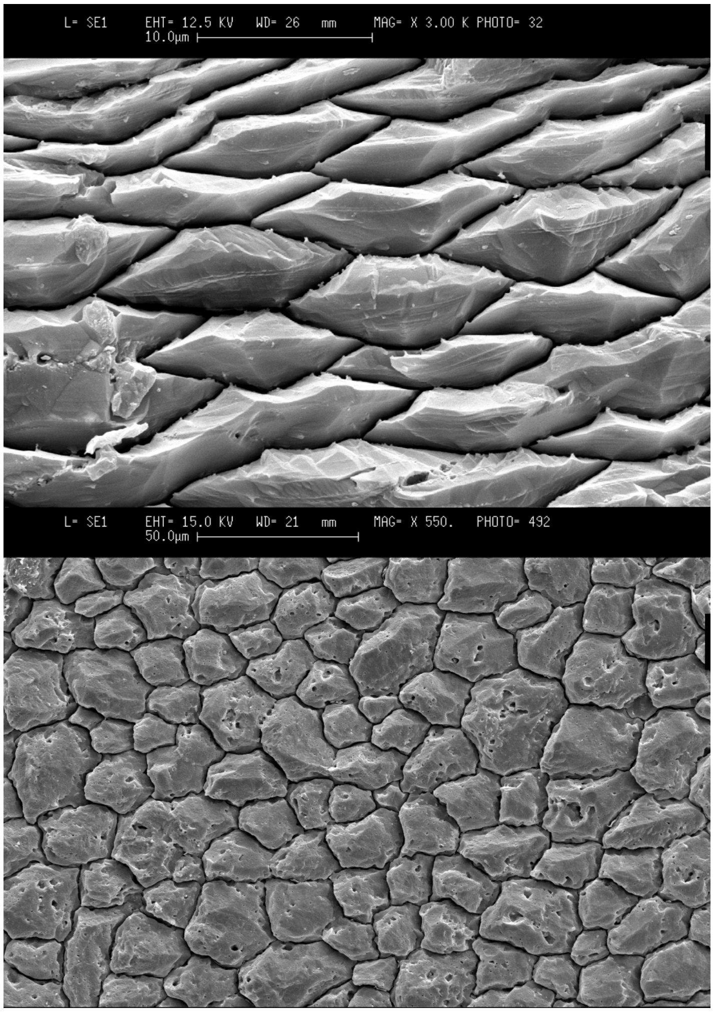

Figure 2. Comparison of shell fabric morphological preservation: preserved (A) and altered (B) fibrous fabrics.

Sir Alwyn Willliams (i.e. Williams 1968, 1970, 1997; Williams and Brunton, 1993; Williams and Cusack, 2007). In this perspective, shell microstructure analyses have received some attention for Mesozoic and Paleozoic brachiopods (i.e. Armstrong, 1968; Brunton, 1972; Angiolini, 1993; Dewing, 2004; Smirnova and Zeghallo, 2016a, 2016b), but they are understudied. For examples, in a recent phylogenetic revision of the Strophomenida, Congreve et al. (2015) did not include characters related to shell microstructure data because they were unavailable for the majority of the species analyzed, despite Dewing (2004) having suggested that differences in the shell fabric of plectambonitoids and strophomenoids are important to draw phylogenetic relationships. In fact the brachiopod low-magnesium calcite (bLMC), being very resistant to diagenesis, can be very informative from this perspective since it easily preserves the record of the biomineralization processes, enabling comparative studies (e.g. Smirnova and Zhegallo, 2016a).

Furthermore, with the growing interest about how climate changes, the increase of pCO2 and the seawater acidification affect marine calcifying organisms, the study of fossils microstructure earns an additional value.

Modern biologists are going to collect environmental data and to perform experiments to understand how biomineralization processes will be affected in the future (see for instance ICES Journal Marine of Science , vol. 73, 2016: Towards a Broader Perspective on Ocean Acidification Research), but in doing so they are limited by short-time scale which does not account for the long-term evolutionary processes (i.e. Cross et al., 2015, 2016).

The study of shell microstructure in fossil brachiopods has the potential to overcome the limits given by the short temporal scale on which biological and ecological studied are usually performed. This clearly requires a deep knowledge of the biomineralization processes in modern brachiopods, which is until now partially studied, and a detailed quali-quantitative analyses of shell micro-

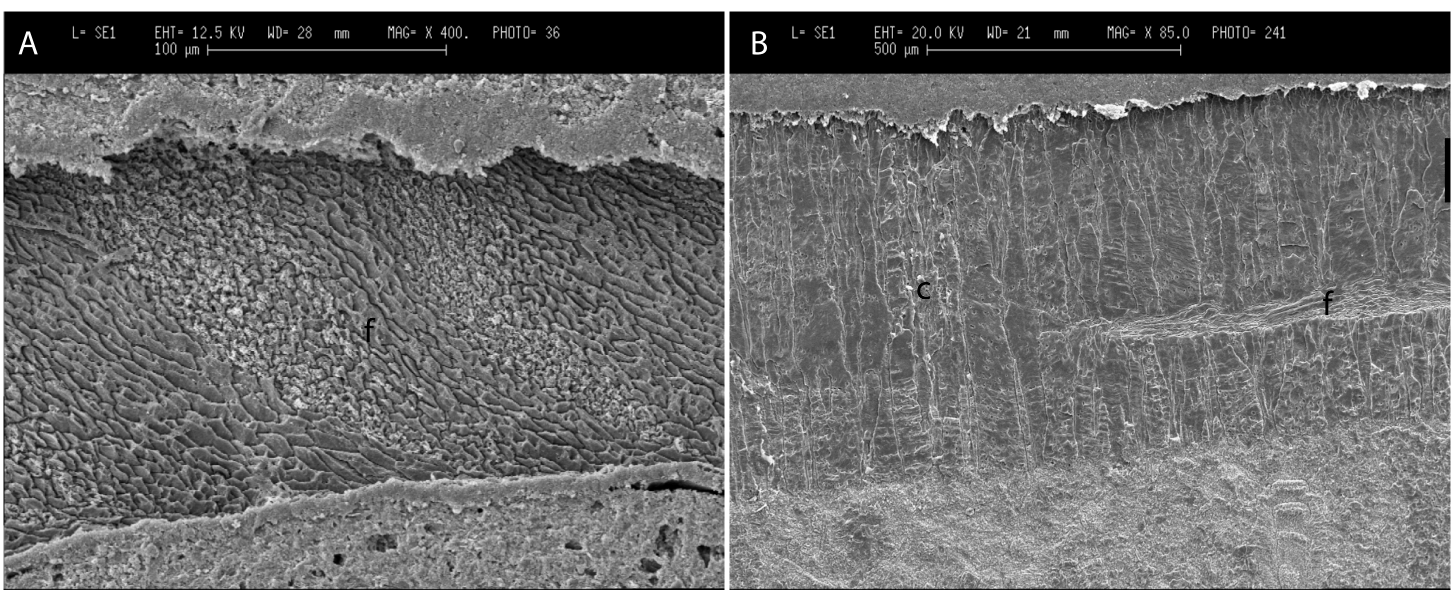

Figure 3. Comparison of the shell fabric in *Paracrurithyris* (A) and *Permophricodothyris* (B). The first is composed exclusively of secondary fibrous fabric and it occurs in the extinction interval (but also below it); the second one has a multilayered shell composed of fibrous(f) and columnar (c) layer and it occurs below the extinction interval, but it is not found in the extinction interval. To note that in B the outer fibrous shell is not preserved.

structure of the fossils.

In the Geology paper, the trends of variation of shell microstructures in the Late Permian are discussed and interpreted in the light of the knowledge of modern marine calcifiers. The scenario related to a decrease in pH seems plausible to explain the observed trends during the Late Permian and at the onset of the end Permian exctinction. In fact, competing hypotheses, such as nutrient collapse or anoxia, are less suitable to explain our data. Despite our finding is not definitive on the debate related to the extinction mechanisms at the end of the Permian, this topic is a provocative contribution of broad interest for several reasons:

First of all our data reveal that the analysis of patterns and trends in biomineralization in fossils can be important to understand climate changes in the Earth history and can be useful to unravel the kill-mechanism of drastic events, as the end-Permian mass extinction. This kind of approach strongly revitalize a more traditional paleontological approach;

In a broader perspective, our finding from the *ancient past* is consistent with recent analogues, showing once again that the geological past may be a lesson for today and the future; this also should encourage more studies on modern brachiopod biomineralization and their response to environmental factors. This is a key step, because a deep knowledge of the organisms biology and ecology is fundamental to interpret the fossil record;

Our data reveal a new pattern, that is, just before and during the end Permian extinction interval, brachiopods preferentially produced more organic-rich shells (Fig. 3). This could be consistent with the hypothesis of ocean acidification in the latest Changhsingian, already proposed (i.e. Knoll et al., 2007) as one of the kill mechanism of the end Permian mass extinction;

It is the first time that this kind of approach has been used to study the end Permian events; this may be also extended to other time intervals and biota and may be useful to understand the impact of acidification and its consequences in the next future. This open a broader opportunity for palaeontologists to re-evaluate old brachiopods collection and collect new material to study from this perspective.

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SUBMISSION GUIDELINES FOR ISSUE 65

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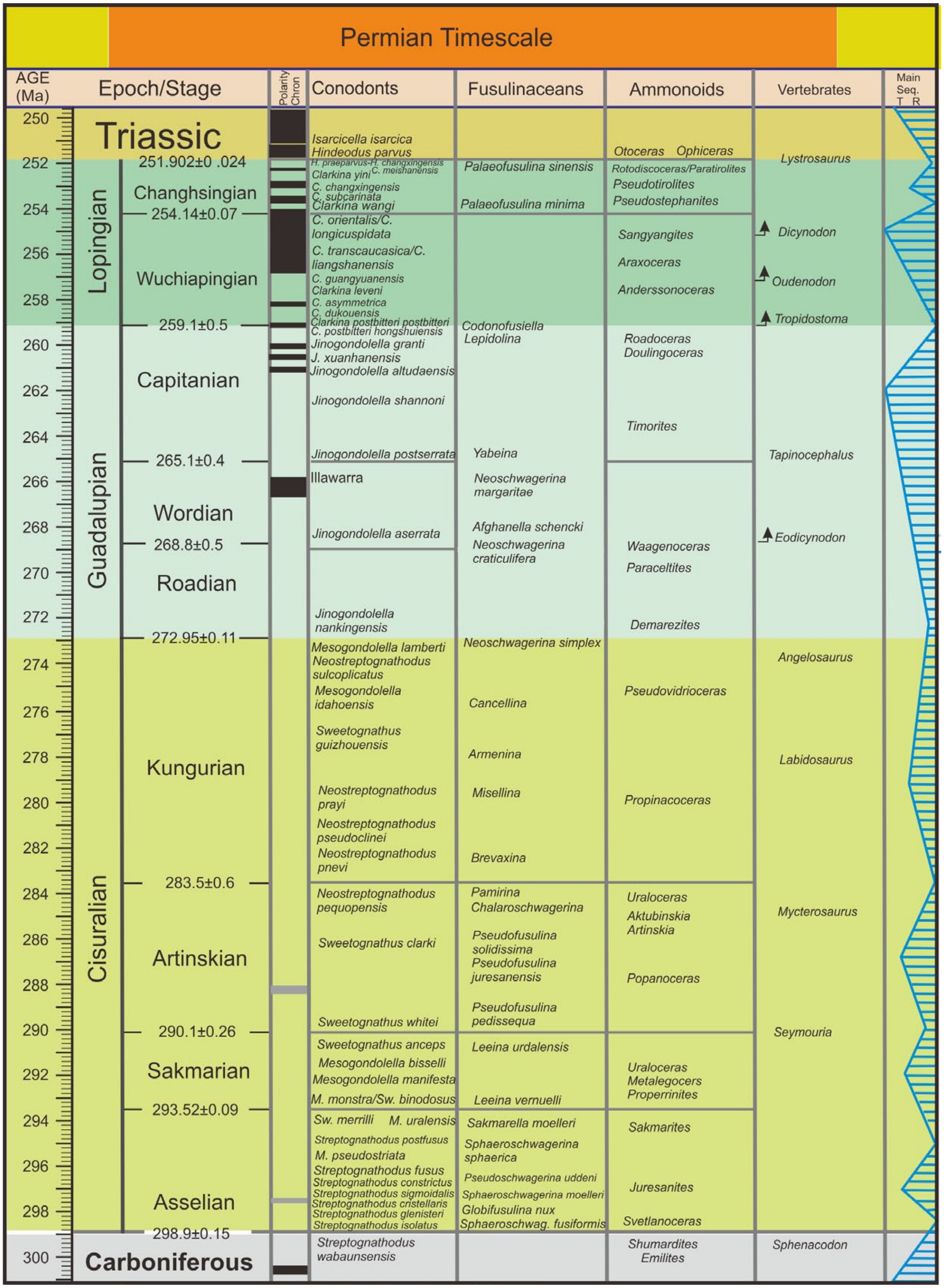
The deadline for submission to Issue 65 is 30th June, 2017.



Note: This is the latest version of the Permian timescale which SPS recommends (updated by Shuzhong Shen and Lucia Angiolini). We welcome any comments to improve it. All the information will be updated from time to time here. Geochronologic ages are combined from Burgess et al. (2014, PNAS 111, 9, p. 3316–3321); Shen et al. (2011, Science 334, p. 1367-1372) for the Lopingian; Zhong et al. (2014) for the Guadalupian-Lopingian boundary; Wu et al. (2017, Palaeo-3, 466, p. 361-372) for the base of the Guadalupian; Schmitz and Davydov, (2012, GSA Bulletin 124, p. 549-577.) for the Cisuralian. Tetrapod biochronology is after Lucas (2006, Geological Society London Special Publications 265, p. 65-93).