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**Object: A review of PhD Thesis typescript entitled " Tectonic evolution of the Central Part of the Tepla Barrandian Unit" by Jaroslava Hajna**

The typescript consists of five main chapters which are presented in form of five publications that are either: published in Precambrian Research (Chapter 1), Gondwana Research (Chapter 2), prepared for submission in Precambrian Research (Chapter 3) in review process in the International Journal of Earth Sciences (Chapter 4) or in press in International Journal of Earth Sciences. The team of co-workers is large and apart of supervisors it includes also specialists (like M. Chadima in rock magnetism, W Dorr and A; Gerdes in zircon geochronology, P. Kraft in sedimentology and stratigraphy). Therefore, the way how the PhD thesis is presented (collection of research papers) and the variety of methods used in collaboration with experts in different subject areas calls for explanation, what is really the work of Jaroslav Hajna and what is a contribution of collaborators. This is a typical problem of modern PhD theses policy which force students to present their work in form of publications, trend which becomes largely abandoned in west European countries.

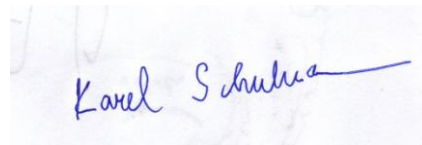
The content of PhD typescript can be formulated as follows: 1) A redefinition of lithostratigraphy of TBU in terms of lithological structure of large scale accretionary prism which, together with structural geology and AMS work brings a new insight into possible geodynamic Late Proterozoic evolution of this unit. The problem of definition of lithotectonic units and their structural geology is dealt in the first publication (Precambrian Research). The interpretation of lithotectonic units in terms of large scale accretionary prism is presented in the second publication presented in Gondwana Research. The interpretation of one lithological complex as a Franciscan type mélange is a subject of third publication prepared for Precambrian Research. The fourth paper attempts to interpret the Cambro-Ordovician basinal sequences using structure of basaltic and andesitic dykes as a precursor to opening of Rheic ocean and finally, the last paper deals with Variscan deformation reworking Proterozoic and Cambrian fabrics of the selected segment of TBU.

This is an impressive amount of work which is well presented, accompanied with excellent illustration and in generally very well written. The papers are characterized by good and coherent structure, the applied methods are well suited to solve individual problems and both regional and topical literature is well studied and appropriately referenced. Almost all papers gone through peer review process and therefore I have no major objection to their formal content.



There are problems but of scientific nature which I would like to discuss with the author during defence.

Therefore, from all these reasons I conclude that Jaroslava Hajna performed excellent work, which certainly merit requirements to obtain the title Doctor of Philosophy of Charles University. I am pleased to recommend this PhD thesis to be defended in front of Jury.



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### **Comments and questions:**

I have several scientific comments and questions which do not diminish the quality Thesis but should be considered as material for discussion during defence and for further scientific work.

I am of the opinion that the Proterozoic structure is not clearly and unambiguously defined in terms of polarity of a Platt's accretionary wedge.

1) First, the Mariánské Lázně Complex was subducted during Devonian at least to 60 - 80 km depth and later (still during Devonian) attached to the upper crustal part of the TBU. Therefore, it cannot be considered as a part of the wedge but rather as a part of distal passive margin of Saxothuringian block attached to hangingwall plate during Devonian. This is crucial moment in Hajna et al. model and perhaps needs to be further discussed.

2) The geophysical pattern, namely the RH9 profile reflects Devonian to Carboniferous subductional and collisional structure and certainly not Proterozoic fabric. I would like to hear more about the way how the geophysical Proterozoic pattern can be preserved in the deep structure of the TBU during defence.

3) The Franciscan mélangé is famous for HP metamorphic blocks preserved in serpentinite matrix (not only) but the key is preservation of HP rocks in the

subduction channel. The authors use the model of John Platt and J.M. Lardeaux modification to design the shape of wedge. The Platt's wedge model was indeed developed to explain the circulation of HP rocks within the wedge, but the units (Alpine) are generally coherent. To block and matrix model fits better to the Garry Ernts model of Franciscan mélangé. The latter has completely different geometry compared to that presented here.

4) I miss entirely the concept of ocean floor stratigraphy, even if several features are well presented in the TBU. This is of course a major shortcoming and shows quite clearly the corner flow model is not appropriate, but a model of (possibly accreted) ocean floor volcano-sedimentary sequence is significantly more suitable here. In your model the unit to the east (chert dominated) is the deepest one in the corner flow unit, which is difficult to defend regarding the model (should reveal highest metamorphic reworking). The structure of Nankai, Makran, Japanese island wedges are well illustrated in the literature.

5) Regarding sea mount interpretation of the mélangé. This is possible but: a) the chemistry of basalts does not correspond to OIB or yes?, there is no proof of sea mount toe mélangé, relationships to blocks of limestones and basalts to host rocks, and mixing of limestones, basalts and deep water sediments as typically reported from Japanese examples. These relationships are well described from recent examples of Japan but also from other excellent Proterozoic examples from the Gorny Altai for instance (Safonova et al., and many others). The existence of subducted sea mounts would imply Pacific scenario - does it indeed?

6) The main problem is the persistent reference to the Cadomian - Avalonian orogeny. This orogen is proposed to rim the northern Gondwana margin by some authors. Those colleagues who work in Africa or South America would have completely different view. I am interested to hear during defence more about Pan-African belt, existence of accretionary belt in Nubian shield and general structure of Trans-Amazonian belt. All that is essential if one deals with typical Pan-African rock assemblage and North African (possibly Morocco type) early Paleozoic sequences. I am simply not convinced that in the Variscan belt is possible to identify Proterozoic orogenic front orthogonal to major system related to accretion of Gondwana.

7) In order to bring some new light into Cambrian and Ordovician fragmentation of Gondwana, paleomagnetism is needed. Ordovician poles are quite well defined, what is the message?

8) In the last paper (IJES) the authors are dealing with AMS work of granitoids. Venera et al., (2000) done a lot of work in this field, and it is evident that the authors came to different interpretation. The existence of major detachment between Proterozoic part of the TBU and that part reworked during Devonian is the only plausible solution. In addition, the cooling ages (which are metamorphic as well) are older than those related to emplacement of the Cista pluton which preclude syntectonic emplacement. To me, the extensional collapse is a must but there is no trace in your work. It looks like that the Cista pluton induce limited thermal softening resulting in major reworking of Tis pluton and that, more importantly, your results are not coherent with geochronology. What is the modern opinion about the subject?

9) I have several technical question regarding interpretation of AMS and microstructure which I leave for a defence.