

Interactive comment on “An anticlockwise metamorphic P-T path and nappe stacking in the Reisa Nappe Complex in the Scandinavian Caledonides, northern Norway: evidence for weakening of lower continental crust before and during continental collision” by Carly Faber et al.

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Dear Editor, Dear Authors,

here come my comments to the article “An anticlockwise metamorphic P-T path and nappe stacking in the Reisa Nappe Complex in the Scandinavian Caledonides, northern Norway: evidence for weakening of lower continental crust before and during continental collision” by Carly Faber and others. I enjoyed reading the article and apologize

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for the late response. The manuscript is well and clearly written, it presents an enormous amount of data, and all figures are carefully done. Also in view of the special publication format (discussion article), I recommend publication at the present state - it is already out and I do not see why that status should be questioned. I do have a few comments and suggestions, but nothing that should be considered as a mandatory change before publication. I congratulate the authors to this nice work.

The manuscript presents field data leading to a convincing nappe correlation, structural data, petrological data combined with detailed thermodynamic modeling, and abundant U-Pb dating of zircon and titanite addressing magmatism and metamorphism. The fundamental conclusions that the Reisa Nappe Complex did not experience deep subduction prior to the Scandian event (like the Seve nappe in the same structural position further South) and was at high-temperature-low-pressure conditions at the onset of nappe stacking is in my view convincing. It represents an important and probably controversial contribution to unraveling the Caledonian orogen in northern Norway.

I have not done a review for this kind of article before. I do it in a common way and list some general remarks before going through the manuscript.

General comments

Consider “Paths” in title.

The abstract confronts the reader with a flood of data and I found it somewhat confusing. I would consider, not to squeeze in as much data as possible but to rather summarize the story.

The petrological data (especially microprobe data) is a little thin compared to the extended modeling exercise. For some samples, one would like to see element maps or, where there is a map, one would like to see more elements - at least in the supplement. Often, the text is phrased in a general way as if several samples were analyzed, but all that is presented is a couple of point analyses (e.g. sample AR71).

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Data acquisition and modeling effort are quite asymmetric, and it is not clear to me why some samples were treated with so much more care than others. Sample UL248 is handled in a way accounting for garnet fractionation – why only this sample and not the likewise important sample R153? A lot of work is put into showing the anti-clockwise evolution in sample AR25b including the Raman probe analysis of sillimanite in garnet cores. Together with the diagram 10b, this results are maybe the most convincing case of an anti-clockwise PT path. And then, the authors discuss the results away: The inferred conditions might be wrong due to disequilibrium or even be pre-Caledonian. But why take the trouble of the Raman, if one has no explanation for a progressive evolution from sillimanite-stable to Ky + melt, anyway? One could think of explanations, why melting increased towards the Ky-field (e.g. flushing). On the other hand, the important sample A01 (in which titanite has been dated to capture metamorphism) is not treated petrologically at all.

A few comments on the thermodynamic modeling (fig. 7 and 10): I would extend the isopleths in figures 7 and 10 to all assemblages. One should be careful using Mn isopleths, especially in figure 7b. Garnet is the only Mn-containing phase (the only other considered phase, ilmenite, is not stable in discussed assemblages). That means, the amount of Mn in garnet reflects only the total amount of garnet. This becomes especially problematic, if garnet in a first assemblage is removed for subsequent modeling - except for an arbitrary amount (25%), which determines, how much Mn reenters the bulk. It would be easy to tune Mn-isopleths in figure 7b. Modern software actually allows calculating full or partial fractionation of minerals along defined PT paths. That way, one could also test if certain zoned minerals actually grow along a path. This would be required for interpreting zoning as growth zoning.

The authors interpret the upper part of the Kåfjord nappe (KN) as belonging to the Nordmannvik nappe (NN), the lower part as belonging to the Vaddas nappe (VN) (if I get it right). Why do they distinguish a KN at all? Also, the NN and the VN have completely different histories according to this study: While the VN was at the sur-

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face until lower Silurian times, the NN experienced high-temperature conditions maybe since Proterozoic times (if I get it right). So why are they put together in a RNC?

How did the Vaddas/Kåfjord nappe (or a part of it) get from the surface to the depth of intrusion (after the deposition of Silurian surface rocks)? If the nappe is seen to consist of different parts juxtaposed during nappe stacking, it should be stated more clearly.

How does one make a flat lying nappe pile of relatively thin nappes with large horizontal dimensions if the system was hot and shallow at the onset of thrusting? I would not expect a hot arc/backarc crust to deform in this style.

How was the temperature of metamorphism during intrusion of gabbro in the VD inferred? And what is it? There is no temperature in fig. 14A and no temperature mentioned in the text – but a thermal gradient (?). This could be the most convincing case of anti-clockwise PT-evolution. I would generally say a little more about the pressure determination from Getsinger et al. (2013).

Detailed comments

page 2/line 2 (2/2): Remove first sentence?!

2/16: “in Silurian to Devonian times”?

2/23: Maybe indicate, where the Magerøy nappe is located.

2/24: “units” instead of “rocks”.

2/26-27: I think, a structural position can not be “paleogeographically unique”. Maybe: “Its structural position corresponds to a distinct paleogeographic position.” (that has to be unraveled)

4/14-15: Defining “Scandian” as “the main Caledonian deformation” is probably not a good idea and not the definition proposed/discussed in Corfu et al. (2014; “final stage of Caledonian orogeny”, “after 430 Ma”)

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4/21: Should be made clear at the beginning of the sentence that this is about KNC (could be GNC).

4/23: There should be a few references at the end of this sentence.

4/30: "Outcrop" is not a verb. ("The RNC crops out...")

6/6" "axes of open folds"?

6/9" "black ones/ black lines".

12/18f: Reading this description I wondered why the KN is distinguished at all - the upper part seems to have affinity to NN, the lower to VN. If the defining characteristic is that the base cuts the gabbros, I would state this.

12/27f.: Does this not indicate that much of the deformation (S2) is post nappe stacking? Where is the nappe stacking event?

13/18-19: First, I thought that using the LOI for modeling the second stage would be a great idea, but if melt is part of the assemblage, it could have been considerably more H₂O-rich and degassed during solidification, or not?

14/8: I see one questionable rutile-inclusion in the grt ruin in fig. 4C.

18/18ff: See above: documentation for these observations is pretty poor. The text suggests that several grains have been studied including the rim all around the grain. Of these several grains "very few" show asymmetric rims. What we get to see is one grain and the rim in one direction. Where is the other data?

19/4ff: Exhumation during shearing? (VN)

22/13: One should say a little about the sample material that was used for determining the bulk composition. Was it a mixture of melanosome and leukosome? In the following one should maybe emphasize that both stages in fig. 10A assume melt-present condition and thus no fractionation because of melt extraction.

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22/16: "final"? (remove?)

24/8-9: This caveat hangs a little in the air – can stage 1 be anywhere in the stability field of the assemblage. Is there any way to constrain the range? From fig. 9A, I can not see evidence that the core was not affected by diffusion. Small changes in grt compositions make rather large PT variations in this assemblage, right?

25/3: Maybe remind reader of the structural position of sample (near boundary to underlying KN).

25/11: Remove "apparent"? I do not get the sense anyway – can sectioning create artificial bimodal grain-size distributions?

25/7-26: The text is hard to follow, both because of writing and convincing observations supporting the statements. The little data there is in fig.9 seem stretched to come to the generalizations. I cannot recognize two distinct generations and would like to see an element map. In Fig. 9C, I wonder how X_{mg} can vary from X_{py} so little with X_{alm}=0.7-0.75.

28/3: Fluid or melt?

28/12: What kind of microstructural relationships would that be? The ones mentioned before?

31/21-22: A pressure-increase is not visible to me!

35/6: Mention also volcanoclastic rocks from Magerøy nappe.

35/33: What is meant by "thickened continental crust"? Is the Vaddas nappe composed of rocks that were at the surface and at 35 km depth before deformation? Would you call this "a nappe"? Should be discussed that nappe-internal strain is so intense.

36/11: What is significance of sillimanite? If it is pre-caledonian, the NN might have well been subducted at 450 Ma (and could correspond to the Seve nappe).

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Figure 1 does not have a very good technical quality in my pdf. Can it not be reproduced with a higher resolution?

Figure 4H. Are chl primary inclusions? chl seems to be consistently along cracks.

Figure 6. Other maps in supplement!? Is there really no map for AR153?

Figure 7. Why are isopleths of grt composition not seen through the entire grt stability range? I think this should be the case in order to see how unique the compositions are.

Table 3: I would not calculate Fe³⁺ in grt based on cation balancing! The spreadsheet obviously makes some assumptions (e.g. filling the T-site with Tschermak-Al that then has to be accounted for by Fe³⁺ in the A-site.) that are questionable and actually "drown" in measurement error. The scatter of Si around 3 with several measurements > 3 makes it problematic to interpret Si < 3 as Tschermak component. Besides - only one grt ends up having Fe³⁺.

Table 4: Some pl analyses rather poor. Xan = Ca/(Ca+Na+K) can be questionable, especially in sample AR153, where Ca seems low compared to Si, Al, and Na. However, a higher Xan yield even a better fit with the predicted PT range.

I hope the authors find my comments useful. I leave it to them to revise the manuscript or not.

Best Regards, Thorsten Nagel

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