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Reply

"Successive ~1.94 Ga plutonism and ~1.92 Ga deformation and metamorphism south of the Skellefte district": A reply to H&L

Torbjörn Skiöld[a,*], R.W. Roye Rutland[b,1]

[a] *Laboratory for Isotope Geology (retired), Swedish Museum of Natural History, Box 50 007, S-104 05 Stockholm, Sweden*
[b] *Research School of Earth Sciences, Australian National University, Canberra, ACT 0200, Australia*

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1. Introduction

We regret having to make a public rebuttal of the "discussion" by Högdahl and Lundqvist (2008; H&L below), to which we were not given the opportunity to respond before it was made public. However, we feel obliged to demonstrate that their sweeping introductory and concluding judgments are wholly unjustified by the intervening scientific arguments. We must, therefore, take this opportunity to clarify the principal issues of scientific interpretation and judgement involved.

H&L (p. 1) observe that our interpretation "has profound implications for the model of the basin evolution" and that they are not opposed "to the idea of a two-stage evolution as such, but that has to be demonstrated in a rigorous fashion, including field evidence and well-defined geochronological data". We note, however, that H&L do not challenge, nor provide any pertinent discussion of, the foundation of geophysical and field evidence, which has been provided on the regional and outcrop scales for the distinction between older 'Svionian' metamorphic complexes and younger 'Bothnian' sequences, and at the outcrop scale for all our metasediment and granitoid samples (Rutland et al., 2001a,b, 2003; Skiöld and Rutland (2006); S&R (2006) below). Instead, they follow their tendentious and muddled discussion of our geochronological data and interpretation with a more general Discussion (H&L, pp. 4–5) made up of a disjointed assortment of opinions, misrepresentations and assertions that are unsubstantiated by geological or geochronological evidence comparable to our own.

They are quite wrong to assert (H&L, p. 4) that the *"conclusions drawn by S&R (2006) rely wholly on their interpretation of the zircon geochronology"* (although we are happy to defend that interpretation below). In fact there is a solid geological foundation for two critical aspects of our interpretation, viz. (1) that the D_1 deformation is older than ~1.9 Ga, and (2) that the sequence containing the Rob-1 sample was deposited before the intrusion of our dated ~1.94 Ga granitoid. H&L also make the simplistic and absurd assertion that *"all available maps show conformable relations between the complexes of the two sequences"*, without offering any example. We must first attempt to clarify these basic geological issues, which bear on the interpretation of the geochronological data.

2. The nature of the geological evidence for the age of D1: conformable deposition or unconformity

There can be no dispute that a major tectonic discontinuity is present between the Skellefte group (our Bothnian) and the Robertsfors group (our Svionian), whatever interpretation of that discontinuity may be adopted. On the regional scale (Rutland et al.,

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* Corresponding author.
E-mail addresses: torbjorn.skiold@nrm.se (T. Skiöld), roye.rutland@anu.edu.au (R.W. Roye Rutland).

[1] Fax: +61 2 6125 0738.

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2001a,b) the boundary zone between the two groups is intruded by ~1.89 Ga granitoids, and subsequently deformed at ~1.86 Ga by ductile D_2 deformation, which is parallel to the zone, and to the D_2 folding in the Skellefte group. South-side up faults and shear zones mark the final stage of D_2 deformation. This deformation is also present in the Robertsfors group as shear zones, and is superimposed on earlier D_1 structures, which trend at a large angle to, and are truncated by, the boundary zone. These D_1 structures are not present in the Skellefte group, the base of which is not exposed.

We have used these relationships and others to infer that the D_1 structures pre-date deposition of the Skellefte group and are therefore older than ~1.9 Ga; and to infer a primary unconformable relationship (Rutland et al., 2001a,b; S&R, 2006, p. 184 and p. 201). Similar tectonic relations occur between the Svionian and Bothnian sequences in the other areas that we have studied (Rutland et al., 2001a,b, 2004; Williams et al., 2008), and it is well recognized that there is a deformation episode preceding intrusion of ~1.89 Ga granitoids in the Svionian sequences.

Thus there is not, and cannot be, any positive stratigraphic evidence of continuous conformable deposition between the Skellefte group and the sequences in the Robertsfors group to the south. If H&L wish to sustain their view (H&L, p. 4, section 3 Discussion, first sentence) that deposition in their Bothnian Basin started before 1.95 Ga and *continued conformably* until after 1.87 Ga as argued by Lundqvist et al. (1998, pp. 361–362), they must do so *in spite of* the observed tectonic relationships between D_1 and D_2. This must include evidence that not only D_2, but also the D_1 deformation, is post-1.87 Ga, in spite of the absence of D_1 from the Skellefte group. In fact, H&L offer no new evidence. Their view that no orogenic deformation took place before 1.87 Ga is essentially an assumption, based on a claimed lack of evidence to the contrary: hence their concern with our paper, which provides such evidence. However, their own view is not only incompatible with our marginal basin accretion hypothesis but also, apparently, with recent elaborations of the arc accretion/collisional hypotheses (e.g. Talbot, 2005; Korja et al., 2006).

It therefore seems highly unlikely that primary relations between the two sequences could ever have been conformable anywhere, and we know of no map that purports to display such relations. As a small example, Fig. 3 of S&R (2006) shows two small outliers of low-grade cleaved sediments, N and NW of the Rob-1 locality, overlying the higher grade and highly deformed Robertsfors group. We consider that these are probably Bothnian sediments cleaved during D_2, which were deposited unconformably on the already deeply eroded Robertsfors group (deformed during D_1). Unfortunately we have not had the resources to make comparative studies of the samples that are available to elucidate these relationships; and no other structural studies have been made of similar relationships elsewhere.

The evidence of conformable deposition is no stronger on the southern boundary of the Robertsfors group near Örnsköldsvik (S&R, 2006, Fig. 1), south of the area of our published studies. Again faults are shown in the boundary zone, and the limited outcrop inhibits comprehensive structural analysis even in the type area of the Härnö group to the south of the boundary (e.g. Lundqvist, 1987; our Bothnian, deformed only by D_2, Rutland et al., 2001b, Fig. 6), and none has been attempted. Thus estimates of stratigraphic thicknesses are not well founded. In a revealing comment, relevant to their proposed conformable relations between the two sequences on all available maps, H&L (p. 4) also note that "*the locations for the stratigraphically lower and higher levels are often not known*".

3. The geological and previous geochronological evidence for minimum age of sedimentary deposition

It is our view that the geological evidence strongly favours deposition of the sequence containing the Rob-1 sample before it was intruded by our dated ~1.94 Ga granitoid. As part of their argument against this view, H&L (p.4) distort our comments on earlier work (S&R, p.184), and mislead the reader about previously published work, by arguing that the data from one of the previously dated granitoids (the Husum granodiorite, Lundqvist et al., 1998), *"cannot be used to limit the assumed pre-1.91 Ga sequence"* In fact, this is only one of the granitoids, including the nearby Seltjärn granodiorite, used by Lundqvist et al. (1998) themselves to delimit the sequence.

Lundqvist et al. (1998), in discussing the Husum granodiorite, refer to a working hypothesis that the rock was originally emplaced at about 1930 Ma but that it experienced "a rather hard reactivation later on". The authors note that an age estimate from one zircon fraction of 1933 ± 3 (MSWD 0.97) "compares well with the age of 1930 ± 11 Ma" (sic; actually 1931 ± 11 Ma) from Seltjärn; but they go on to note that it "is uncertain whether the actual intrusion age is 1930 or ca. 1885 Ma, especially as the younger zircon population exhibits typical magmatic features." However, this does not deflect Lundqvist et al. (1998, p. 361) from their general conclusion, "that granitoids ... have intruded the Swedish part of the Bothnian Basin ... at least from 1.95 Ga, up to c. 1.85–1.84. This means that the deposition of the greywackes/argillites in the Basin *started before 1.95 Ga*", and their Fig. 6 indicates the possibility that it could have begun much earlier. It should also be noted that Lundqvist et al. (1998) provides the basis for showing the Husum and Seltjärn granodiorites in an age category of 1.92–1.96 Ga on the Mid-Norden map (Lundqvist et al., 1996) and of 1.91–1.96 Ga on the Geological Map of the Fennoscandian Shield (Koistinen et al., 2001). Moreover, in the text accompanying the former (Lundqvist and Autio, 2000, p. 50), it is stated without qualification that "gneissic granodiorites at Husum and Seltjärn ... have yielded ages of 1931 ± 11 Ma and c. 1930 Ma" (sic: the two dates should be transposed). In view of the censorious tone of the discussion by H&L, one may ask whether this constitutes a reasonable summary of the conclusions of Lundqvist et al. (1998), as we have previously supposed, or whether the omission of the possible final intrusion age of 1.885 Ga in all these publications constitutes a questionable use of U–Pb geochronological data.

In any case it should be noted that *none* of the previously dated pre-1.92 Ga rocks in Sweden, including the Knaften rocks of Wasström (1993, 1996), have been placed in a well-defined structural context or checked by complementary SIMS data, *as we have done for our granitoid samples*. Thus, we would have preferred that H&L had devoted their considerable efforts to answering their own call for rigorous field evidence and well-defined geochronological data by attempting to resolve the doubts concerning the TIMS analyses of the Husum and Seltjärn granodiorites (Lundqvist et al., 1998), e.g. by undertaking complementary SIMS analyses and placing the samples in a proper structural context. Even better would be studies of the host metasediments of the granodiorites, or the rafts of foliated metasediments that occur within them, that seem to indicate the possibility that orogenic deformation occurred before intrusion. But H&L offer no new geological or geochronological evidence. In the meantime, we have to make do with the best data available. Whatever their precise age or relation to their host rocks, the Husum and Seltjärn granodiorites appear to lie in a southern extension of our Svionian Robertsfors group where our granitoid ages of ~1.94 Ga are not disputed by H&L. (p. 4: for Rönn-3, *"A magmatic age can reasonably be discerned at ca. 1.95 Ga*"; for Rönn-2, "*the magmatic age is reasonably well defined*"). Our inferred maximum depositional age of at least ~1950 Ma for Rob-1, pre-dating granitoid emplacement, is fully consistent with the depositional history favoured by Lundqvist et al. (1998). But a key geological question is whether the granitoid intrusion provides a minimum age for the deposition of Rob-1.

H&L (p. 4) attempt to discredit our geological interpretation of the relationship of Rob-1 to Rönn-3 by the specious double negative argument that *"there is no evidence in S&R (2006) showing that Rob-1 and Rönn-3, located more than 50 km apart, do not represent different tectonostratigraphic levels"*. Equally, there is no evidence that they do represent significantly different stratigraphic levels: but wishful thinking is no substitute for evidence. What we do show, and state in our paper (p. 199) is that *"The D_1 structures in the Bjuröklubb area have similar character to those at the Robertsfors-1 locality and their NE to N trend can be traced on the aeromagnetic maps between the two localities. We consider therefore, that the D_1 metamorphic episode that affected Robertsfors-1 also affected Rönn-2 and-3"*. Rob-1 was carefully selected to avoid the influence of the superposed D_2 deformation, present at Rönn-2 and-3, and which, as we also observed (p. 188) *"prejudices the identification of the earlier D_1 episode in the zircon populations"*. We did not, as H&L falsely state more than once (p. 5), interpret a metamorphic age of 1936 ± 4 Ma for D_1 at Rönn-2, but we did compare that single analysis with the range of analyses in Rob-1, which gave a pooled age of ~1916 ± 5 Ma ($n = 14$) for our interpreted metamorphic event.

Thus Rob-1 lies in a domain with distinctive D_1 structures and aeromagnetic characteristics and it lies between our own dated granitoids at ~1940 Ma and those dated by Lundqvist et al. (1998) at ~1930 Ma. It is difficult to escape our geological conclusion, from all the evidence available, that the whole sequence bearing these characteristics was deposited before the intrusion of the various pre-1.92 Ga granitoids, in which case the event dated at ~1916 ± 5 Ma in Rob-1 *must* be post-depositional. However, the interpreted age of the D_1 deformation, obtained for Rob-1 from both the geological and the geochronological evidence is an important part of our hypothesis, and we now turn to H&L's specific criticisms of the geochronological interpretation.

4. The geochronological interpretation of Robertsfors-1 (Rob-1)

The data on Rob-1 represents the only study yet made of the zircon populations of metasediment in the proposed older Svionian domain in Sweden, but it can be compared with several similar samples that we have studied in Finland. We acknowledge that we would have preferred to have more than our 40 analyses, although this is more than in previous studies, which were all of samples from the younger post-~1.92 Ga sequences. And of course it is possible to argue about the detail of our interpretations. Our study was not designed to permit a detailed analysis of the multiple factors controlling zircon crystallization in metamorphic rocks, *"such as metamorphic temperature and pressure, the nature, number and timing of the thermal pulses, fluid activity and host lithology"* (Williams, 2001, for a study in a similar geological environment) but we have considered these factors in making our interpretations. We cannot discuss every contentious point, but we observe that we have clearly separated our interpretations from the base data, so that the validity of data exclusions and interpretations can readily be judged by readers disposed to consider them objectively. We will focus on the main issue, our interpretation of the 1916 ± 5 Ma population as post-depositional.

As H&L acknowledge, in our interpretations, we have taken account, not only of the 207Pb/206Pb ages and their confidence limits, but also of the Th/U ratios, the U and Th concentrations and the varying morphologies and CL patterns involved. The following key paragraph in the H&L discussion (pp. 1–2) indicates their own cavalier approach to our data, with our highlighting: "*Even though two plateaus can be discerned, the data display a trend without any real gap between 1995 ± 8 and 1895 ± 17 Ma (Fig. 1A)* [*note 1* below]. *There is nothing in the data that excludes the supposedly metamorphic zircon from being magmatic* [*note 2*]. *Therefore the timing of D_1 cannot be extracted from the data* [*note 3*], *and the minimum age for deposition is indicated by the youngest dated zircon (i.e. 1895 ± 17 Ma), not > 1.95 Ga as suggested by S&R (2006)* [*note 4*].

Note 1. "Even though two plateaus can be discerned, the data display a trend *without any real gap* between 1995 ± 8 and 1895 ± 17 Ma (Fig. 1A).

H&L have taken considerable trouble to represent our data in a different format. This would have been more useful if they had been consistent in their presentation and comments. It will be evident that there is a trend without any discernable gap in the measured ages in their Fig. 1B for the granitoid Rönn-3. However, in this case they have represented our two distinct populations differently in the figure, and the result is quite informative in illustrating our interpretation, which they accept. In the case of Fig. 1A they have chosen *not to show* the analogous distinction that we have made between two populations in Rob-1. Had they done so, the result would have been even more informative in illustrating a clear distinction between the two populations in both parts of the figure. The omission appears to indicate a very subjective approach to the data. We are aware of the dangers of pooling disparate analyses but we have given good reasons for treating the younger group as a single population and we stand by our interpretation of the age of the younger group (S&R, 2006, p. 196) at 1916 ± 5 Ma. The key question is whether it should be interpreted as a post-depositional event, or a pre-depositional event as preferred by H&L.

Note 2. "*There is nothing in the data that excludes the supposedly metamorphic zircon from being magmatic*."

In the case of the small number of older overgrowths dated at ~1980 Ma, and in their anxiety to argue that the overgrowths need not be "metamorphic", H&L apparently miss our point. We have ourselves emphasized that the measured ages overlap within error, but we take it as axiomatic that, if a distinctive group of overgrowths can be identified, it is geologically younger than the host grains, even if it may also be interpreted as "magmatic' (cf. Corfu et al., 2003, cited by H&L). Our point was that some of the oscillatory zoned host grains, giving overlapping, but apparently younger ages than the overgrowths, may well have had their isotopic systems significantly disturbed. In other samples, it happens that we have found that similar overgrowths are distinctly younger than their host grains, and this influenced our description of the overgrowth event as "metamorphic", though, as we have discussed elsewhere (Rutland et al., 2004), we do consider that this event took place in the source igneous complexes before erosion and deposition. This population may well represent a late thermal event at ~1980 Ma closely associated with the earlier magmatic history.

Note 3. "*Therefore* the timing of D_1 cannot be extracted from the data" (H&L, pp. 1–2).

This is a non-sequitur. H&L (p. 3) apparently believe that if the younger overgrowths *'could easily be interpreted as being magmatic overgrowths'* this 'magmatism' must necessarily be related to a magmatic environment like that represented by the older ~1980 Ma igneous population.

We strongly disagree. We point out that *both* our D_1 and D_2 deformation episodes are associated with metamorphism and, at higher grades, with migmatisation. It is therefore not surprising that zircon overgrowths formed during these 'metamorphic' episodes should sometimes have more 'magmatic' compositions. In the Rönn-3 granitoid, H&L basically accept our interpretation of the overgrowths as being related to the D_2 event, although, even there they perversely argue that the responsible fluids are not 'magmatic', and associated with the discrete D_2 granitic veins that we figured (S&R, 2006, Fig. 4a, b and c), but "*are most probably metamorphic and associated with migmatite formation*" (H&L, p. 5).

For us, this is basically a distinction without a difference: in an earlier paper we point out that in these rocks, where the D_1 migmatites are overprinted by D_2 shear zones, *"A new leucosome producing migmatites in a new D_2 foliation is associated with these shear zones in more pelitic lithologies (Fig. 6b and c), while discrete pegmatites occur in shear zones in less ductile quartzo-feldspathic lithologies* (Rutland et al., 2001a, p. 227).

In the case of the younger group of overgrowths in Rob-1, as we said in our paper (S&R, 2006, p. 198), "the variable Th/U ratios may reflect the absence of coeval monazite to scavenge the Thorium, *or it may imply a more migmatitic/anatectic environment* for Rob-1 at the time of the early [D1] metamorphism." Thus characteristics that could be interpreted as magmatic can more plausibly be related to fluids associated with the D_1 migmatisation. Curiously, H&L (2008, p. 3) quote only the first clause of our suggested explanation. The failure to quote or acknowledge the second clause might well be construed as a wilful misuse of the normal conventions of accuracy in discussion, particularly as they note in their introduction that "many magmatic textures resemble metamorphic ones and vice versa". Although it is inappropriate to compare rocks of different bulk composition, they also purport to discredit the first clause by comparing the metasediment Rob-1 with the granitoid Rönn-3.

We therefore stand by our correlation of the 1916 ± 5 Ma age of the younger group (S&R, 2006, p. 196) with the D_1 deformation, metamorphism and migmatisation. We have given several reasons in support of this post-depositional interpretation, in addition to the obvious application of Occam's razor (if the correlation is not made we are left with a major observed tectonothermal event without any expression in the zircon population). Not least of these reasons, is the strong geological evidence discussed above that the sequence was intruded by the Rönn-3 granitoid at ~1.94 Ga, together with the fact that this age corresponds with the gap between the two main zircon populations in Rob-1. We also regard it as significant that the age obtained is virtually identical to that obtained (by different analysts) on analogous rocks in Finland, where the post-depositional interpretation was also supported by a metamorphic monazite age Rutland et al. (2004).

H&L are not impressed by our suggestion that the post-depositional interpretation is strongly favoured by the fact that these overgrowths can also occur on detrital Archaean grains. We can add however that we have learnt to analyse both overgrowth and host wherever possible. We have also found these ~1.92 Ga overgrowths on Archaean grains in several samples that we have studied in comparable rocks in Finland, but we have never found Archaean grains as hosts to the pre-depositional ~1.98 Ga overgrowths, which may be closely related to the preceding magmatism. This clearly militates against the alternative explanation offered by H&L.

We also note the weakness of that alternative revealed by H&L's surprising comment (p. 4) that "the ~1.91 zircons... do not show any mechanical wear, implying that they may have been derived from a local magmatic source of this age". This point seems to imply that, after all, they recognise our distinction between the 1916 ± 5 Ma population and the earlier overgrowth population dated at ~1.98 Ga. But it would be a strange and implausible "local magmatic source of this age" that provided *no* typical igneous grains, but only "magmatic overgrowths on older xenocrysts" (H&L, p. 3). Surely, in the light of all the other evidence, the lack of mechanical wear more plausibly indicates that the overgrowths are post-depositional. We suggest that H&L cannot see the wood for the trees; and if anybody has made forced interpretations of our U–Pb geochronological data, it is they.

Note 4. "the *minimum* age for deposition is indicated by the youngest dated zircon (i.e. 1895 ± 17 Ma), not > 1.95 Ga as suggested by S&R (2006)." (H&L, p. 2).

Presumably, H&L meant to say the *maximum* age for deposition: we did indeed suggest a maximum age of deposition of at least ~1950 Ma and possibly as high as ~1980 Ma (S&R, p. 198). We also used the age of the Rönn-3 granitoid as evidence that "sedimentation had already occurred by ~1950 Ma." In any case, the alternative maximum age suggested by H&L would only be valid if the grain were demonstrably pre-depositional, in opposition to our discussion above. However, even then it would be unwise to hang the argument on a single zircon analysis and if one did, current geochronological wisdom would require the use of 2-sigma error.

5. Conclusion

In our 2006 paper, we have used the geochronological data as a test of our hypothesis of marginal basin accretion (Rutland et al., 2001a,b). While one would always wish to have more data, we believe that the geochronological data we have presented is the best available. We also believe that we have shown that our interpretations are sound, and fully withstand the criticisms by H&L. Our hypothesis is consistent with all available geological and geophysical information in the area concerned, and also in two comparable areas that we have studied in Finland (Rutland et al., 2004; Williams et al., 2008). Of course our hypothesis, like any other, needs to be tested as comprehensively as possible, but it cannot be dismissed on the basis that it is "unconfirmed". As noted above, and in an earlier discussion (Rutland et al., 2003, p. 152), we would welcome any further work to test or elaborate our hypothesis.

Nonetheless, H&L have the right to be skeptical, and we would have welcomed well-founded constructive discussion. Unfortunately this has not been offered. Not only is their discussion scientifically weak and ill directed, but, as we have shown, H&L have breached the normal standards of scientific discussion; and they have been permitted to publish pejorative unsubstantiated opinions as "conclusions". We trust that in the future, authors will be given the right of reply before such a discussion is made public.

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