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Solid Earth Discussions

Interactive comment on "New zircon data supporting models of short-lived igneous activity at 1.89 Ga in the western Skellefte District, central Fennoscandian Shield" *by* P. Skyttä et al.

Anonymous Referee #1

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The manuscript by Skyttä et al. is an interesting and important contribution to the research of the metal-rich Skellefte District in Sweden. Dating of the plutonic rocks gives new age constraints also to the VMS deposits in the area. The manuscript is well-written and of good scientific quality. One thing that was not entirely clear is the original motive for the study. As the authors stated, the Viterliden intrusion had previously been dated by Bergström et al. (1999), who acquired a relatively old and rather imprecise age of 1907+/-13 Ma for the intrusion. Was this age somehow suspected to be incorrect, or why did the authors choose to re-date the intrusion? The new, more precise age data presented in the manuscript did confirm a younger and much more constrained age for the Viterliden intrusion. One other point is the three missing

samples i.e. the ones that did not produce enough zircon for analysis. It would be interesting to know what other phases of the intrusion complex the authors would have liked to analyze. They need not be marked in the map (Fig. 2), but it would be good to know what is missing from the present dataset. Also, I started wondering why the authors did not date the "mine porphyry proper" and collected a sample from a more coarse-grained variant more that 5 km distant. Was this one of the "missing samples"? And what about the granite phase of the intrusion? I would also like to see some rewriting of the Chapter 2.3 (The Viterliden intrusion and the Kristineberg hanging-wall rhyolite). It would make the whole paper more reader-friendly if the age samples were described in this chapter along with the more general description of the rock types present in the intrusion complex. As for the regional geology, some things need to be clarified for a reader not so familiar with the area. In Fig. 1 the Jörn-type intrusions are divided into GI-type and GII-IV types. In the text there seems to be some inconsistency as in Chapter 2 (Lithology) the younger Jörn-types are marked as GII-III (page 360, row 29) and in Chapter 4 (Discussion) the authors write about Jörn-types GIII-IV (page 369, row 2). What is the reason for this? And in Chapter 2 the Björkdal intrusion is included in the Jörn-type but in Fig. 1 it is supposedly marked as an unclassified intrusion? The analytical data in the paper is of good quality and the interpretations are solid. But what is the explanation for the reverse discordance of zircons in sample IV (Kristineberg rhyolite)? May it be attributed only to the position of the sample in the holder and if this is the case, are there any reported occurrences of the same phenomenon? A final small thing, is there a reason for choosing the term "Svecokarelian orogeny" instead of "Svecofennian orogeny" in the Introduction and Geological overview? Some small details of mainly technical nature may be found, these are listed below. Page 358, row 24: Bothinan replace with Bothnian Page 360, row 22: Siksträsk replace with Sikträsk Page 361, row 16: reference to Fig. 3 is before reference to Fig. 2, rearrange! Fig. 1, legend: can the late to post-orogenic gabbro – diorite be assigned to Revsund-type, too? Text in page 360 suggests so. Björkdal intrusion is not marked, only an ore deposit with the same name? And one thing that would be interesting to know is how to

separate the Skellefte and Arvidsjaur volcanic rocks from each other, when they occur in the same area? But this is probably out of scope of this paper.

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