Interactive comment on "Review of some significant claimed irregularities in Scandinavian postglacial uplift in time scales from tens to thousands of years: earthquakes?" by S. Gregersen

H. Bungum (Referee)

hilmar.bungum@norsar.no

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The Fennoscandian region is known for its significant rebound since the last glaciation and for its spectacular cases of reverse faulting in the Lappland region immediately following the glaciation, referred to also by the author. For about a century now a discussion has been going on about to what extent the uplift may be responsible for contemporary earthquakes in Fennoscandia (e.g., Bungum et al., 2010), and there has also appeared a large number of claims on neotectonic activity. In Norway, more than 80 such claims have been systematically reviewed recently (Olesen et al., 2004; 2012), leaving only three as grade A (almost certainly neotectonic), with a few more as grade B (probably neotectonics) and grade C (possible neotectonics). The declassified claims have been mostly ascribed to gravitational effects. The classification scheme is due to Muir Wood (1993), including also a grade D (probably not neotectonics) and a grade E (very unlikely to be neotectonics). The present paper aims to do a similar assessment for Denmark, which is highly commendable.

General comments

The paper is essentially divided in two parts, where the first part (sections 1-3) is addressing more general issues related to total and present uplift and to postglacial and contemporary earthquakes, while the second part is discussing four particular neotectonic claims, concluding negatively on all, which generally is in line with what I just referred to from Norway.

(1) It is claimed that the postglacial uplift in Scandinavia is regular, which needs to be qualified through a consideration of wavelengths. For the longest wavelengths, such as those depicted through the BIFROST network, the strong implicit smoothing clearly provides 'regularity'. For shorter wavelengths, however, say of the order of 50-100 km, significant irregularities were indicated already in the NEONOR project results, assessed from tide gauges, precise leveling and GPS measurements (Dehls et al., 2000a,b), substantiated even more lately as more and better data have become available (Vestøl, 2006; Bungum et al., 2010). In fact, a "NEONOR 2" project is now starting (conducted by the Geological and Geodetic Surveys of Norway, NORSAR and the University of Bergen) aimed in part on monitoring a region in northern Norway by means of collocated broadband seismometers and GPS stations. In contrast to the Norwegian continental margin, where earthquakes are deeper and generally with reverse faulting, this is a region with shallow normal-faulting earthquakes where both erosional processes and glacio-isostatic adjustments (GIA) have been suggested as possible driving mechanisms (Bungum et al., 2005; 2010; Olesen et al., 2012).

(2) The geologically-based absolute uplift curves in Fig. 1 need to better assessed in terms of quality and reliability, not the least since there are fully drawn lines also in offshore areas. In the present paper (and in Gregersen and Voss, 2010) the author quotes Mörner (2003), but when I go there I find a reference to Mörner (1979), where in turn there is a reference to Mörner (1979a), which appears

to be a proceeding book that I do not have access to. In any case this falls on the author to pursue, including checking to which extent the original source of these uplift contours have been subjected to peer review. It could be noted here that Mörner (2003; 1979) did use dashed lines for offshore areas.

(3) Section 4 reviews four different neotectonic claims in the Danish region and, as stated earlier, concludes negatively on all. It would have been useful here if the 5-grade reliability classification referred to above could have been used. Also, the discussion on most of the claims is long and not particularly well focused.

(4) I note also that there is not a clear separation between geodetic and seismic activity, which are not necessarily connected. The first question to ask is if a neotectonic movement can be documented, and if so, if there has been seismic activity that could be tied to this. In particular cases, however, such as when concentrated earthquake zones (swarms) are documented, seismicity by itself can also be used as a neotectonic indicator (Olesen et al., 2004).

Specific comments

(5) Significant parts of both text and figures in the first sections are very similar to what has been published already by Gregersen and Voss (2010), which is not commendable even if it is the same first author. Also, are all of the references to earlier papers by the author really needed? Along the same line I note also that the English language in this paper does have some improvement potentials.

(6) The second sentence in Section 2 is quite unclear since it is hard to understand what it means that "the uplift stresses [are] bulging upward and pressing out". Instead I note that the old and simple Coulomb-Mohr-based model of Johnston (1987) is still viable, stating that the reduced vertical ice loads reduces the vertical stress and thereby allows the accumulated horizontal tectonic stress to reach a level of instability.

(7) Section 4 is entitled "Uplift patterns and claimed irregularities" but is exclusively a discussion about seismicity, not uplift patterns; see comment (4).

(8) The second sentence in Section 4 claims that the present-day seismicity in Scandinavia is caused by a combination of plate-related stress and uplift stresses. This ignores a number of other important sources of stress of a more regional and local nature, as discussed extensively in Olesen et al. (2012) and in numerous earlier papers over more than 20 years.

(9) Claim 1; Carlsberg Fault: Almost nothing of structural relevance is said about the Carlsberg Fault; physical dimensions, orientation, bedrock manifestation, offset, etc. On the other hand I am not sure if the quality of the Carlsberg beer is interesting in this context, unless it reflects on the hydrological properties of a fault under compression (Muir Wood and King, 1993). I would like to note also that large differences between time-separated conventional geodetic leveling surveys are quite common, especially when not tied to bedrock.

(10) Claim 2; Læsø: Presumably the 4 m displacement is vertical, and if so, which way? Also, what is the spatial extent of the claimed movements? Point 5, line 4: If a feature is related to erosion and/or gravity effects, it should not be termed faulting.

(11) Claim 3; Line across southern Denmark: The paper does not identify the nature of this feature, but in Hansen et al. (2011) I find that it is defined as the position of a buried basement high along the southern margin of the Ringkøbing-Fyn Basement High, separating the Danish and North German Mesozoic sedimentary basins, in turn connected to a sudden change of GIA by ~0.6 mm a^{-1}. Hansen et al. (2011) do not, however, aim to explain this spectacular 'jump', as they call it. The author's conclusion that it is not sufficiently justified to maintain this as a neotectonic feature seems reasonable.

The last paragraph under this claim is a discussion of present-day seismicity and as such is of a different nature; see comment (4).

(12) Claim 4; West coast of Sweden: This claim comes from Mörner (2003; 2009) and the author's conclusion seems reasonable also in this case. By the way, is there any report available now from the planned field trip to Hallandsåsen?

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