Comment on se-2021-141 by Grosset et al.

Anonymous Referee #1

General comments

This study uses the thin elastic plate model software gFlex to calculate crustal deformation and corresponding stress changes due to ice loading in the Western Alps. Deformations are compared to GNSS data. Stress changes are then used to assess Coulomb failure stresses (CFS) and fault motion parameters at the locations of some major faults in the area. The authors find that observed vertical deformation can be explained by ongoing rebound after ice melt while observed horizontal motion can be much larger than the modelled result. Modelled fault motion does not fit observed parameters, thus the authors conclude that corresponding stresses due to ice unloading cannot explain the current seismicity pattern and rather inhibit fault motion.

I was looking forward reading this study. However, I must say that I am very disappointed now with this manuscript and its overall presentation. As far as I understand from a quick web search, this manuscript is part of a PhD study of the first author. While it looks, at first glance, well written with interesting figures, the reader easily spots a lot of irritating sloppiness when reading the text and looking more closely at the figures. Additionally, a more thorough literature search would have helped the authors putting their study and results in much better context. I will discuss my major concerns in more detail below.

As much as I would like to support and not disappoint a young student here, I am unfortunately left with a decision to reject this manuscript. You will see that my comments address the modelling performed here. The authors note occasionally that it is "simple", but from my perspective it is not appropriate for this type of research at all. At least, from what I can gather from the text. As it is presented, it is largely a black box which makes it impossible for the reader to repeat and confirm the calculations. Therefore, the conclusions must be questioned. Revising the manuscript thus goes beyond of major polishing of some parts.

Specific comments

L15ff - This statement is wrong. The references here and many related works by these authors and others explain seismicity at the end of the last glaciation with corresponding stresses due to glacial isostatic adjustment (GIA), not the recent seismicity. As far as I am aware of, only Brandes et al. (2015, 2019) suggest a potential link with historic and recent seismicity in northern Central Europe, but that it is due to stress build-up after previous stress release (Brandes et al. 2015) or deeper crustal stress changes (Brandes et al. 2019) in an area outside the formerly glaciated area. Please also have a look at Ojala et al. (2018) for phases of enlarged seismic activity in Fennoscandia and see further discussion of this subject in Olesen et al. (2021).

L19 - I do not understand these rapid decay times of 103-105 years and to what they relate. Please explain!

L20 - Where do these numbers come from? Please add reference, but also have a look at Arvidsson (1996) and Ojala et al. (2018).

L21 - This statement neglects the importance of stress migration due to the viscoelastic nature of the mantle, see e.g. Steffen et al. (2021). Even an icecap like the one previously covering the Alps has sensed the mantle - thus a thin plate model misses this contribution to the overall stress change.

L36 - The important studies by Sella et al. (2007), Peltier et al. (2015), Simon et al. (2016) and Robin et al. (2020) for North America seem much more appropriate for being cited here than the PhD thesis by Tarayoun (which is mainly written in French). You may also consider substituting Johansson et al. (2002) with very recent studies for Fennoscandia by Kierulf et al. (2021) or Lahtinen et al. (2022).

L47 - Please mark these earthquakes with a special symbol in Figure 2a.

L52 - Please mark these faults with names in Figure 2b.

L67 - Unclear to me why 150-200 km is appropriate. Please provide good reasoning for the reader. This comes out of nowhere. Looking at Fig. A2, 120 km Gaussian make it even smoother, so perhaps 100 or 105 km Gaussian radius would perhaps give a better fit to the model result.

L83ff - A thin plate method that uses elastic plate thickness T_e is not appropriate for GIA investigations. T_e describes a physical property (material parameter) of the lithosphere with the unit in km. T_e characterizes the strength of the plate to long-term loading in terms of millions of years. GIA is a much shorter, though not a short-term loading process, and leads to a "dynamic" change in T_e - so T_e cannot be longer used as constant.

L88ff - Your model description is insufficient. All input parameters should be explained and a table should be added with all values.

L90 - Unclear how this ice load model is implemented: What is the resolution of the model in time and space? How do you account for the 3D ice thickness variation in your 2D planar model?

L91 - Unclear why you remove the ice instantaneously. Do you use the ice load model from Mey et al. (2016) or do you just use the LGM limits and put 2 km of ice in there, which you remove at once at 15 kyr BP? That would have nothing to do with the glaciation history and give you misleading results. Unclear why LGM is at 15 kyr BP? Mey et al. (2016) clearly state that maximum extent was at 21 kyr BP with rapid thinning thereafter. Please explain carefully what you did here, especially which ice load model was implemented in which form (time and space)!

L94 - This is not a GIA model. It is a thin plate model with a loading function. The important stress migration contribution from the mantle to the lithosphere is missing.

L102 - Please show the results of all parameter combinations, e.g. in the Appendix.

L113 - Stresses should be investigated at the depth of main seismicity, not at the plate top (surface) as GIA stresses are not constant with depth.

L118 - These stresses appear to be too big given the ice thicknesses indicated in Mey et al. (2016), which are in large parts much less than 1000 meters, and given your preferred decay rates. Based on them not much depression should be left and thus impounded flexural stresses must have almost ceased. Doesn't seem logical. Please provide a map of the horizontal and Coulomb failure stresses and selected graphs for some locations of interest (faults). Also guide the reader what these numbers represent so that they cannot be misunderstood. If the stress values you provide would be related to the Laurentian or Scandinavian ice sheets, their formerly glaciated areas would be scattered with faults - but they aren't.

L128 - Suggest rephrasing to 'would account'.

L130ff - Strongly suggest to rephrase to a very careful tone as all these conclusions are based on a very, very simple model not reflecting GIA.

L141f - Sentence does not make sense.

L146 - Equation is wrong. Either $\Delta CFS = \Delta\tau - \mu' \Delta\sigma_n$ or $CFS = \tau - \mu' \sigma_n$, but not mixed.

L152 - Wrong definition here. Switch < and > symbols.

L162 - near-vertical fault dips mean ca. 90° dip, which you did not test. Please rephrase.

L167 - rake is defined from -180° to +180°, so a rake of 180°±20° doesn't make sense. Please rephrase.

L183ff - You are discussing results based on a very, very simple model that actually does not reflect the process you aimed at investigating. In my view, there is no paradox at all. For a thorough discussion, you should show ΔCFS over time from before glaciation to today. Then you should be aware of the fact that when ΔCFS reaches instability, a pre-existing fault is likely to move if the fault orientations favor it (see e.g. Steffen & Steffen 2021). This will lead to a stress drop. I suppose this very likely happened to many faults in the Western Alps during and/or soon after deglaciation. Since then, most, if not all, fault activity is likely related to other processes than GIA. This is what is discussed in e.g. Olesen et al. (2021). Given the size, thickness and history of the Alpine Icecap it is very unlikely that such GIA-induced stress build-up as discussed in Brandes et al. (2015) happened here. This should be investigated in future, of course, but definitely not with a thin plate model. I also miss a discussion in view of the findings of Keiding et al. (2015), who compare strain rates and seismicity in Fennoscandia.

Figure 1 - this is only a very simple sketch. Stress is not constant with depth, among others. Please revise carefully, see e.g. Steffen et al. (2021).

Figures 4 & 5 - Please draw ice extent at LGM and of the last glaciation time step in 4a and 5a. Although I understand that the modeling results do not provide a large range of rakes, it'd be helpful if you show 4b and 5b for different μ. Please also show a plot where the ranges of different models are mapped.

I could not access the model code. The given link led to 404 error.

Technical corrections

- L2 - comma missing after Fennoscandia
- L34 - Network should be System
- L59 - Strange unit style. Use mm/yr, mm*yr^{-1} or mm yr^{-1}.
- L63 and throughout ms - diacritical mark should by en-dash
- L85 - use correct font for wm
- L90 - LGM abbreviation not introduced yet
- L110 - 15 km (space missing)
- L156 - please explain BDFA
- L165ff - degree symbol (°) missing
- All figures - enlarge to page width
- Figure 3 - center scale for 3a (zero should be white, not 0.5).

References

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