

Interactive comment on “Strength constraints of shallow crustal strata from analyses of mining induced seismicity” by M. Alber et al.

Anonymous Referee #1

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Referee Comment. The work of M. Alber and coauthors is dedicated to the discussion on coal mining induced seismicity following the exploitation of different mines in Germany. The work specifically focuses on rupture processes taking place in correspondence to different mining activity, consider background and induced stresses and strength information, and then discuss whether the observed signals can be correlated to fault reactivation or mass failure processes.

The overall topic is of interest and the scientific motivation clear. I feel the way the paper is note very well presented (at least for me, repeatead reading was needed to follow the whole reasoning). In particular the paper would gain in clarity, if authors improve the presentation of the dataset, give more information on the modeling approach, on the used focal mechanisms dataset, and improve figures layout. Moreover,

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it seems that some questions posed in the paper presentation and abstract are not fully discussed. The discussion should be improved with respect of the source type discrimination problem.

My main concerns are the following.

1. The abstract indicates that different failure processes are delineated from seismic records. Apart that not a single record is shown or discussed in the paper (a figure should be worked out to show signals from different processes), I think the author interest concerns here a kind of discrimination among both processes. Although both processes are considered in the paper, in terms of required stresses with respect to lab/on-site measurements, no clear conclusion is drawn with respect to the possibility of discrimination among fault reactivation and intact rock failure.
2. Linked to this question, the authors also claim at the end of the introduction that goals are the identification of failure mechanisms, induced stresses and strength constraints. I think all these results are not achieved in the paper, but perhaps the intention here was to discuss broader goals, form a wider project, and results discussed in the paper are only part of these.
3. Focal mechanisms should be of interest to address/discuss the different failure processes. However, they are shown only for few cases. No discussion at all is provided on the way they were obtained, nor on their uncertainties. Fig. 4 illustrate focal mechanisms while tunneling (by the way, the left plot only consider a part of the mine and other events at the western edge are not discussed). Here pure DC model are shown, and no information given on moment tensors or tensile component, which should be different in both cases. Moreover the distribution of focal mechanisms is quite broad, and not so easy to simplify as suggested (bottom part of pag. 740). Normal faulting is a feature, as well as a steep dip angle. Orientations seem quite variable: it seems rather that, if some of them could fit the believed fault orientation, others are more or less parallel to the tunnel. May this indicate a superposition of both proposed rupture

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processes? I miss similar figures for the other cases, at least for the same mine during the longwall processes, and a discussion whether the distribution of focal mechanisms change. If I well understand during tunneling, authors would expect mostly reactivation (which is not seen by the focal mechanisms distribution), while more mass failure processes would be expected during longwall (and here the analogous figure is missing, limiting the discussion).

4. An interesting case is discussed in Par. 3.2, through the seismicity linked to pillar undermining. It is however unclear when and how this is performed.

5. A clear naming convention and mine overview should be given for the Ruhr dataset. Longwalls should be named consistently, and some details given on when they were exploited. From Figure 2, longwall I is North of longwall II. From Figure 3, panels have different names. Figure 4 is again consistent with Figure 2. Figure 8 should be Longwall I, and has a similar shape. But again, in Figure 10, Longwall II is used for the Northern longwall. I am confused!

6. Figure 9 shows results of stress modeling, but a detailed information is missing on how this is performed. It is also not clear which focal mechanisms is this computed. The conclusion here seems that during pillar undermining, seismic activity could include rock failure (but also there is no reason to exclude fault reactivation, or I missed something?). If this is true, focal mechanisms could be differently distributed than the previous case, where only fault reactivation was plausible. However, a discussion on focal mechanism is missing. Moreover, even if full moment tensors are not available, the authors should discuss whether tensile or non-DC component could be expected here.

7. Figure 10 right remains obscure to me, and perhaps it is not needed, if no further information/discussion are given. The text only says that maximal velocities are shown, but it is not clear at which station they have been recorded, how they link to the events magnitude, and what is the purpose of such analysis.

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8. The discussion section starts with some ambiguous statements. From the work I would understand that also here both fault reactivation and mass failure are expected. So, this would not be in contrast with Fritschen (2010). I find the discussion section a bit weak. This could include some comments on the possibility of discrimination; a list of contributions of possible interest where recently published on a special issue of Journal of Seismology. Although dealing with the discrimination among natural and induced seismicity, some cases e.g. based on focal mechanisms, stresses or seismicity rates (see Dahm et al. 2013, Cesca et al. 2013, Passarelli et al. 2013) may be of interest for the discussion.

Interactive comment on Solid Earth Discuss., 5, 737, 2013.

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