Solid Earth Discussions

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Reply on RC1

Thierry Camelbeeck et al.

Author comment on "The damaging character of shallow 20th century earthquakes in the Hainaut coal area (Belgium)" by Thierry Camelbeeck et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-74-AC2, 2021

Dear Reviewer 1,

Thank you for your valuable remarks on our paper on the damage impact of the Hainaut seismicity. Here below, we give a detailed reply to your constructive comments and show how we changed the text of the paper. We hope this meets your expectations.

Kind regards
Thierry Camelbeeck for the authors

Section 4
R1: I feel the discussion of individual cases in the middle of the paper, i.e., all of section 4 might be better suited for the Supplementary Material as opposed to within the paper. I understand that the authors wish to draw attention to each of these earthquakes. However, it does disrupt the flow of the paper – there are 11 pages in total. A suggested workaround might be to move a substantial number of these events into the Supplementary Material, retaining only a small handful within the text of the paper that stood out for some reason.
We indeed want to draw the attention to these widely felt or damaging earthquakes as these were never reviewed in detail in the past. Yet, as the reviewer indicates, it disrupted the flow of the paper. We followed the reviewer's advice and moved both the description of the events and the corresponding figures to Appendix B. We prefer the Appendix over Supplementary material so that the examples remain in the paper and can be used as reference material while reading the paper.

Section 4 now became Appendix B and we kept a short descriptive paragraph in *section 3.4 Intensity database* to explain the appendix and the figures. We did leave one map in the paper (Figure 3 now) to show how the macroseismic maps look like and how intensity data points fit the modelled attenuation for one event.

Supplementary Material
R1: I am happy to see tabulated lists of intensity assessments for each event. This involves a lot of work, not to mention the hours/days/months in archives battling dust and maybe spiders…… These lists (and future ones) will benefit from the inclusion of

summarized effects for each location with a reference (or references). This will seem like a big ask to incorporate at this stage in the project and does not impact the outcome of this article! It is a recommendation only; one I hope that future work of this nature will incorporate.

Thank you for the appreciation of our work on the supplementary data. You are right that adding the effects of each location with a reference would even more clarify the process from observation to intensity evaluation. However, that's indeed a big task at this moment but we will take this comment with us to the future when we evaluate the macroseismic database of Belgium (work in progress). Following your comment we highlighted the importance of the intensity books that are provided in the Supplement a bit more in the paper.

Section 7.1

It might be useful to consider the decay of intensity from other induced events outside the Hainault area. Perhaps put this in context with any similar work that might exist from Groningen gathered by the Netherlands version of the DYFI? Koen should have access to these if this is an avenue you decide to pursue. Alternatively, I think there is some work done for induced events in the central US and Canada where there is a distinct intensity signal between natural events and induced earthquakes. You might wish to consider papers by Gail Atkinson and Susan Hough who have looked at these more closely, and then put your work from Hainault in context from observation. How does it compare? Or does it not compare?

Thank you for this suggestion! To discuss the difference between the Hainaut attenuation and other worldwide shallow events, we followed a similar approach as Atkinson (2020), who discussed the damaging character of Oklahoma triggered events using the USGS DYFI? database. In our comparison, we gathered the intensity data of all M3.5±0.1 (n=3) and M4.0±0.1 (n=4) Hainaut events and computed the mean ± std intensity decay using distance bins of ~3 km through our intensity data. Then, we compare this decay with the median distance classes from Atkinson 2020 and our modelled Hainaut attenuation. For M3.5 events, we also added the intensity data of the Huizinge2012 (Z=3 km) event in the Groningen gas field (NL) that caused local damage. The figure that will be added to the paper, is available in the Supplement here below.

The comparison of the Hainaut attenuation with Huizinge and Oklahoma events shows that the computed Hainaut attenuation (see green line in the figure) is very fast. This observation indicates that the shallow depth mainly explains the damaging impact in the Hainaut region. Oklahoma events are usually deeper than events in Hainaut (Z ~5 km, Atkinson 2020), showing e.g. that a M4.0 in Oklahoma will have more impact in terms of damage than in Hainaut.

For M5 events, we compared the Hainaut attenuation with intensity data of the Völkerhausen 1989 (DE) potassium and salt mining triggered event (0.8 km) and the 2019 Le Teil (FR; 1 km) event (Sira et al. 2019). Here, the observation is similar and the Hainaut attenuation is again much faster than in these areas.

From this comparison we can conclude that for a shallow Mw3.5 event (see figure in the supplement to this reply) negligible to slight damage (starting from I=V) for M3.5 at the epicenter until a mean distance of 3 km or 4 km for the 84 percentile. In Oklahoma, this observation is relatively similar.

For shallow Mw4.0 events (see figure in the supplement to this reply), substantial damage (I = 7) will occur at the epicenter until a median distance of 2 km and 4 km for the 84 percentile, moderate damage for magnitude M4.0 (starting from I = VI) until a median distance of 3 km (7 km 84 percentile) and negligible to slight damage until a median distance of 5 km (8 km 84 percentile).

The Hainaut coal deposits are located in the frontal zone of the Variscan belt where the subsurface is strongly fractured due to Variscan compressional tectonics. The Hainaut attenuation is fast because of the combined effect of the high fracturation degree and a low Q-factor associated to the slower propagation velocity of coal deposits. This results in a faster attenuation compared to the neighbouring regions such as the Brabant Massif or Ardennes where bedrock with high Q-factoris present near the surface.

Section 7.2
The authors should begin this section by noting whether or not the spatial distribution of intensity observations is adequate in number and is spatially unbiased. I draw attention to Meltzner & Wald (1999) and Hough & Martin (2018) who comment/show how the number of points/observations and their azimuthal distribution can produce biased results. Admittedly, you do not use the modelling approaches that were later used by both those studies, but I feel this is an important point to make. I believe, Van Noten et al. (2017) shows how intensities can be biased by subsurface geology and Hough & Martin (2021) show how intensities, particularly, historical intensities can be biased by social factors. Can you add a few lines to reassure the reader that your intensity data, and by that measure, the attenuation relations are not biased by geology, social factors, and spatial distribution of observations? How reliable are the early reports of shaking effects in the mining areas? Is there the possibility that local mining companies might have wished to keep reports of shaking and or damage hidden from public view?
You are right in making this comment but the spatial and azimuthal distribution is constant within the Hainaut area. Within the Hainaut coal mining area, the population density is quite constant and dense in the cities (see inlet in the macroseismic maps in the Atlas, density ranging often between 500 and 2500 inh/km^2). The official forms that returned to the Royal Observatory of Belgium are statistically valid as they contain a summary of e.g. police, firemen and communal reports. So the data gathered by the communities is adequate in number. Outside the Hainaut area (within the border of the Brabant Massif and Ardennes), population density is lower and areas are more rural. This geographical difference explains why a lower amount of responses was returned to the ROB from these areas.

To examine the possibility of an azimuthal underrepresentation of the data, we composed an azimuthal intensity analysis (see the azimuthal figure in the Supplement to this reply) for each of the 12 events that are used for attenuation modelling. Within each azimuthal bin (20° bins), we have intensity data, hence the azimuthal coverage was sufficient to construct our attenuation law. For smaller events, this was not always the case, but this data was not used in the attenuation modelling. The large difference between the North-South maximal perceptibility in the figure in the Supplement to this reply is dominantly linked to the local geology, rather than to changes in azimuthal distribution. The 1967 Carnières event was wider felt in the Brabant Massif to the north than in the Ardennes in the south. So yes, intensity distribution is biased by subsurface geology (as indicated by Van Noten et al. 2017), but within the small Hainaut coal mining region we have evidence that using a homogeneous intensity distribution for modelling is justified.

Within our intensity evaluation we indeed also used reports from the mining companies. However, these are just a minority of observations. The "Felt in the mines" observations (see double red stars in the inlet in various macroseismic maps) is sourced from newspaper reports and often do not come from mining reports directly. The surface damage during the largest events was large and can be deduced from the community reports and hence the mining reports were not

needed for estimating the impact. So there is only a small risk that mining companies would have hidden shaking observations for themselves.

Section 7.3
Here the authors draw the conclusion that the shallowest seismicity was triggered by mining in the region and ended with the cessation of mining. However, nowhere in the article it is discussed when this seismicity began AND what was the local seismicity prior to the start of mining for the period in question. Can these two questions be answered without returning to the archives? If you do this, I feel it would add further weight to your assumption (Lines 592 – 593) that the very shallow seismicity was indeed triggered.
Thank you for this valuable comment. However, neither in our archives, nor in scientific and press reports there is a mention of seismicity that could have occurred prior to the start of the seismicity in our catalogue. This does not mean that low-magnitude seismicity never occurred (see also our reply to reviewer 2), but it seems very unlikely that damaging or widely-felt events would have been overlooked in the press reports and in the existing scientific work. In Belgium and surrounding regions, historical events are well catalogued and described in newspapers (e.g. Alexandre et al. 2007, Knuts et al. 2016, Camelbeeck et al. 2021), hence, this makes us confident to state that local widely-felt or damaging seismicity started after 1887.

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2021-74/se-2021-74-AC2-supplement.pdf