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Earth Surface Dynamics Discussions

Interactive comment on "Systematic Identification of External Influences in Multi-Year Micro-Seismic Recordings Using Convolutional Neural Networks" *by* Matthias Meyer et al.

Anonymous Referee #1

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This paper addresses the issue of accurate attribution of seismic events to the correct source in long-term/large (micro-)seismic datasets. This paper has the potential to form a helpful methodological contribution to the geomorphic literature, and the overall result is promising. However, I do not believe the paper is ready for publication in its current format. Whilst there is some interesting information presented here, the focus, clarity and structure of the paper require further work.

General points

The language is often vague, with loose use of specific terminology. For example, in the abstract, the authors mention that '. . .Successful analysis depends strongly on the

capability to cope with such external influences'. What do they mean by 'successful analysis' and 'coping' with these influences? Similarly, the authors mention 'correct slope characterisation' in the next sentence. What does this mean? It suggests consideration of the structural/strength/geometric properties and/or damage condition of the slopes. It is not clear which the authors are addressing, and why. Linked to this, Fig. 5 suggests that the focus of the paper is on rockfalls, which again is different to 'slope characterisation'. In short, what is the geomorphic nature of the seismic activity the authors are considering? The final sentence in the abstract is also rather obvious and can be made without the detailed assessment presented in the paper. Indeed, this type of source characterisation is commonly done (and done well) by geomorphologists (see e.g. the work of Adam Young on coastal microseismic monitoring). The most interesting part here is the ability to distinguish between sources of microseismic activity in large/long-term monitoring datasets, and this needs to be more clearly presented.

The Introduction repeats the same points multiple times in subtly different ways – this section could be condensed considerably. Links to geomorphic processes are implicit at best, and largely absent. For example, what exactly are you trying to monitor? Rockfall occurrence? Ground cracking and associated micro-seismic signal? This isn't clear. There is also a stark lack of reference to appropriate literature (e.g. page 2, lines 10 – 16). The aim of the paper is not clear and the authors present instead a bullet-point list of study conclusions. What is the focus here and what is novel?

Section 2 again repeats much of what we have already been told in the introduction. The methods section is again repetitive, justifying the need for, and broad benefits of, the approach, rather than stating concisely how it works. Much of the information here is not clear. For example, Page 6, Lines 16 -21 - there is no specific detail about how tasks are undertaken and how 'a good set of classifiers' is objectively specified.

Much of the methods section lacks detail and feels very descriptive and subjective; many of the choices made are not fully/objectively demonstrated. For example, Fig. 6

does not clearly demonstrate the wind speed threshold required for a 'visible influence' on tremor amplitude. Important definitions do not appear in logical places (e.g. tremor amplitude is defined after it has been used in the text). The key aspect of the event trigger threshold by STA/LTA is not appropriately addressed; I would like to see more critique of the application of the method in this setting. Is it too sensitive and/or appropriate given the plots in Fig. 5? How is the accuracy of event attribution assessed, other than by ruling out mountaineers etc. and process of elimination (page 9, lines 8 – 10 suggests this is the case)? Sections 2.4.2 –3.4 contain some ostensibly important methodological steps, but again much of these sections feels descriptive, lacks an appropriate justification and a logical structure to follow the workflow and the choices made. The level of assumed knowledge about neural network is also rather high. I am not convinced by the 'statistical analysis' presented in Table 1 – this seems rather weak and limited in terms of the depth of data analysis.

The results section draws out the key argument that the authors wish to make, but I would like to see more assessment of the data presented in Fig. 5, even at the basic level, including the duration and frequency range/spectral density of different seismic sources. Can this information be used in a simpler manner to draw the same conclusions? How sensitive are the patterns shown by the graphs to the colour scale of the spectral density information?

The discussion section is underdeveloped, lacks grounding and critique in the context of related literature and does not address the geomorphic significance of the approach addressed. How does the constrained uncertainty of the approach considered compare to other sources of uncertainty, such as seismometer tilt (indeed, which component of the seismometer is being used, and why? – again see the work of Adam Young) and rock slope resonance and site effects (see e.g. Burjanek et al, 2012, 2017 GJI)? Some of the claims made about trade-off between time and accuracy feel poorly considered and require a more robust demonstration. There is also no discussion of the representivity of the case studies provided and how changes in the nature of the rock

mass may affect the accuracy/source attribution of the seismic readings through time (e.g. resonance effects on duration and frequency as a rock mass degrades).

Specific comments (not exhaustive)

There are many uses of e.g. in the manuscript – remove these and replace with 'such as' or 'for example' as appropriate.

Fig. 5 - what are the red/purple circles? Are these triggered microseismic events? This isn't clear in the figure or the caption.

Brackets for citations are not always used correctly. Please check and amend.

Page 1

Line 19 – check terminology. Rockfalls are a type of landslides (see e.g. Varnes, 1978, and subsequent iterations of this work).

Page 2

Line 1 – what is the difference between acoustic emission and micro-seismic emission? Clarify.

Line 3 – . . .HAS been demonstrated. . .

Line 5 – micro-seismic RECORDS?

Line 6 – biased assessments of what?

Lines 3 – 4 and 5 – 9 – very repetitive.

Line 9 – expand on scaling issues.

Lines 18 – 19 – Do you mean the accurate attribution of seismic events?

Line 21 – what is the significance of footsteps?

Line 22 – its (not it's)

Page 3

Line 16 – F1 is not defined at this point. Indeed, much of the terminology in this section (e.g. ensemble classifier) is not clearly defined.

Page 6

Line 28 - . . .has monitored. . .? Tense is not correct.

Line 19 – triaxial or three-axis?

Page 7

Lines 6/7 – clarify 'sampling rate' – how was the sampling done? Or are you referring to the data transmission interval?

Line 11 – The microseismic records considered in the case study were affected. . .?

Line 8 – expand on 'scaling issues' – this is unclear.

Line 24 – is 'sounds' the correct word here?

Lines 26 – 29 – the distinction between acoustic events and seismic events is confusing, seems a little arbitrary and lacks reference to the literature; some of the terms do not follow some conventions in e.g. laboratory monitoring of acoustic emissions; this is important for a contribution to the geophysical literature. These definitions and distinctions also come too late in the manuscript, since these terms are used earlier.

Line 32 – sentence beginning 'Additionally. . .' is not clear.

Page 9

Line 9 – Figure 5(e) does not show an example of a rockfall. It shows an example of the seismic signature of a rockfall event.

Page 10

Line 5 – do not use comma splices (re: therefore)

Table 1 – Reword the caption. It is not the case that none of the other categories 'apply'. Rather, it is where you have not been able to classify the signal as one of the three categories discussed.

Table 2 – this needs a lot more detail – what is this showing?

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