This review follows my first comments sent during the discussion process.
I still have two questions for which the answers given by the authors do not satisfy me.

1)
The strain-rate to velocity processing includes a time integration that requires a highpass filtering to get rid of low frequency noise, and that is ended by a –w/k multiplication that I understand as being simultaneous time derivation and wavenumber integration.
Why is it necessary to first integrate w/r to time and then differentiate w/r to time? Could this time integration/derivation be avoided?

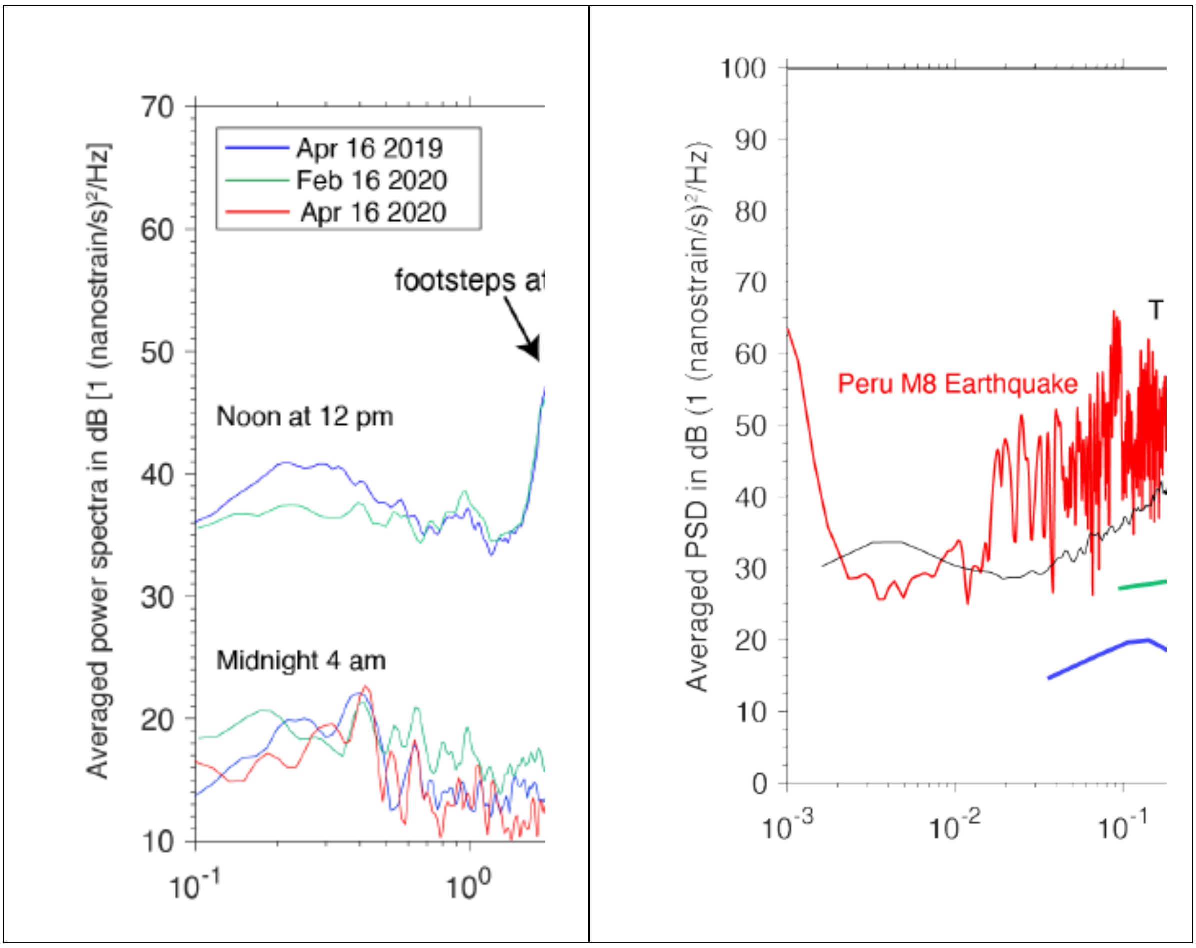
The answer given by the authors to my same remark was to refer to Daley et al., 2016; Wang et al., 2018 for different conversion methods.

Daley et al. 2016 use borehole data and assume body wave propagating along the vertical profile at a constant, non dispersive speed of 3500m/s.
In that case, it makes sense to transform the w/k multiplication by a simple scaling w/k=c=3500m/s. The whole process requires only one time integration and additional scaling.
In the FORESEE experiment, waves can be various, dispersive or non dispersive, and I don't see any way to infer characteristic c values.

Wang et al, 2018 use a similar approach to the present paper, first integrating w/r to time, then derivating w/r to time.

These two papers don't explain me why this time integration/derivation step is needed. The issues are i) in signal processing, integration steps are avoided when possible ii) The authors choose to detail the data processing, this is a good idea but it requires making things clear to the reader.
To conclude:
- Do I miss something in the w/k scaling step that is not explained (apparent velocities are estimated first)?
- Is this processing just applied because you are used to it, but there are no real justifications for it except that strain rate integration is performed automatically, or it comes as a standard toolbox? This would be an acceptable reason with the proper explanation.

2) I still disagree with the authors' statement concerning the frequency range specified on line 206. I try to illustrate it from snapshots taken from the paper.
   There is no PSD relative to the noise level of the instrument or comparison with the earth noise level at very low frequency. This would have been interesting though. Figure 11 give us some numbers for this noise down to 0.1 Hz



It varies from 15-20db at night to 35-40 at noon. On figure 8, left part of the Psd, Peru M8 earthquake, we reach this noise level somewhere in-between 0.02 and 0.01Hz. According to the text, no filtering has been applied.
If we refer to the relationship between strain-rate and velocity, that is roughly: strain-rate ~ w/c*velocity, we should observe a spectrum shape that is twice the derivative of the displacement spectrum for a Mag8 quake whose corner frequency is between 1e-3Hz and 1e-2 Hz.

This is to say that I don't see any reason why the source spectrum should increase below 0.002Hz. It should exhibit a bell shape with a maximum between 1mHz and 10 mHz
I think that the instrument/earth noise level is reached at ~ 1.e-2Hz, and below the spectrum has nothing to do with the EQ. The DAS records quite well this earthquake down to 1.e-2Hz, that is already remarkable.