

Review of the first revision of "Near surface structure of Sodankylä area in Finland, obtained by passive seismic interferometry" by Afonin et al.

During this first round of revision, the authors partially replied to the comments/suggestions made by the reviewers, and improved the quality of their manuscript. However, one of my main concerns remains, namely the clarity and the relevance of the numerical simulation section.

Following my previous comments, the authors explained that they simulated an incoming plane-wave with an azimuthal angle of 40 degrees relative to the profile. Having an off-angle source is indeed crucial to prove that the surface-wave reconstruction could in theory work in their real-case scenario. However, looking at the geometry of the simulation model, namely the absence of heterogeneity along the Y-axis, it is puzzling to me to understand how a surface-wave with correct velocity could be extracted from this configuration.

Another puzzling observation is the presence of Love waves (polarized along the Y-axis) generated by the explosive source located along the profile/X-axis. Again, for reasons of symmetry of the model, I do not understand how this is possible. Is it a misunderstanding about the model configuration, maybe unmentioned heterogeneity? I once again suggest to clarify the figure regarding the simulation model and source configurations.

A third point of concern on the simulation topic relates to an addition/clarification that the authors have made in this new version and in their response to reviewer 1. They claim that "diffuse wavefield consisting of low-frequency (5-20 Hz) surface waves (Rayleigh) can be produced by scattering of a high-frequency (50 Hz in our case) plane wave at velocity heterogeneities." They stated that "the main goal, except of supporting the claim that analyzed wavefield is diffuse, was also to understand how relatively high-frequency wave (dozens of Hz) may produce low-frequency (about 5-20 Hz) wavefield during scattering on heterogeneities". If I am not mistaken, solving the standard elastic-wave equation does not allow such non-linear conversions to occur (did the authors simulate non-linearities?). The only possibility is that non-dominant frequencies already present in the source spectrum can be "selected" by scattering. Because there is virtually no noise in simulations, any non-zero frequency content of the source could be potentially revealed by appropriate scatterers.

While I acknowledge that the study presents a strong interest if absent of artifacts, It is still difficult for me to recommend the paper for publication in its current state because of the mentioned interrogations. I would suggest to ask for the additional opinion of someone with expertise in such numerical simulations and in non-standard Green's function extraction from ambient noise.

Best regards