Dear Editor,

First and foremost, thank you for your helpful feedback and that of the reviewers. We made a concerted attempt to respond to the majority of the comments clearly and concisely. Specifically, we clarified that the simple 2D analytical method proposed in the Appendix is used to validate the boundary conditions for the 2D Finite-element method. Results from both 2D analytical and FE share the same geometry and thus, can be compared. Although more sophisticated, Kuvshinov's analytical approach was not used because it does not reproduce exactly the geometry and assumption of the 2D FE modelling (due to the averaging of the radial strain over the outer diameter of the cable). Boundary conditions determined in 2D were then adapted to 3D FE following symmetry and geometry considerations. In addition, we clarified why only P-waves were considered in both analytical and FE modeling. This has to do with the DAS-VSP data acquired at New Afton which only focused on P-wave reflections. As suggested, we moved the section introducing the New Afton data earlier in the manuscript. This helped clarify the context of our work and certain simplification/assumption made in our approach (i.e. P-waves only). We also clarified that DAS is by no means limited to P-wave exploration.

In the attached files, we provide a point-by-point response to comments raised by the reviewers and the editor. We hope that you will find changes applied to the manuscript and responses provided acceptable.

Thank you for considering our manuscript for publication in Solid Earth.

Sincerely,

On behalf of all the authors

Comments on the manuscript of SE

Investigation of the Effects of surrounding media on the distributed acoustic sensing of a helically-wound fibre optic cable with application to the New Afton deposit, British Columbia. By Hendi Sepidehalsadat et al.

General comments

Solid Earth is bunded to excellency of manuscripts before acceptance and the review process ensures the highest quality of the published papers. Checking the scientific quality of the final manuscript is the job of the editor. This manuscript

received 2 reviews (referees 1 and 2) and a revised version was uploaded. The revised version was sent to referee 1 and an additional referee 3. The latest version of the manuscript has then been reviewed by myself (guest editor, see below details).

Following this latest review and following the referee's comments, the manuscript still cannot be accepted in its present form, as there are still many unclarities and inconsistencies in the manuscript, making your modelling comparison not reproducible. In addition, answers given in the real manuscript partially do not consider some referees comments, without clear enough justifications. There are also format problems in the presentations, e.g., in the abstract there should not be with paragraphs (as required by a referee), yet they are still present in the latest manuscript. One referee finds the abstract too long. In addition, minor elements such as double punctuation, unanswered response to the third reviewer, makes it as if, as suggested by the referee, corrections and preparation to the final version were too quickly implemented. Those formal issues could indeed be sorted out after the latest version has been presented and accepted, but they do not help convince us editors to accept the manuscript, especially a, and more importantly, the scientific content is still lacking consistency and information. This is a pity as your work is very valuable for getting insights in the understanding of data recorded with helically wound fibre optic cable as compared to straight cables. It would indeed be great to provide to the community a simple method to understand helically wound fibre optic cable signals. Therefore, I suggest you address again and carefully previous remarks of the 3 referees, and implement them or not with clear justifications, and follow up with the concerns and the detailed issues indicated below, before the manuscript can be accepted. To do so, I would suggest you implement your answers within the referees' text and within this document, so it would be easier to follow up with the next steps, instead to have to jump from questions and answers, which does not help make the work easier. In addition, it is not sufficient to answer the referees and editor comments in such a file. The manuscript needs to change reflecting the comments, so that readers would not wonder with similar questions. This would help making your paper a cited paper.

If I understand correctly, the objective of your paper would be to make clear enough to readers that your simplified 2D solutions are close enough to 3D complex modelling solutions and with which uncertainty. This simplified method, certainly easier to implement, would make indeed a valuable approach for understanding

observations performed with a helicoidal cable (as mentioned by a referee). If this is not the objective of the paper, it indicates that it is not clear at all!

(It is explicitly stated in different locations what are the objectives of this paper, for example "we first present a simple adaptation of the 2-D analytical solution of Folds and Loggins (1977) to model HWC's dynamic strain due to acoustic waves. The analytical solution estimates the HWC response using a relatively simple model that does not require specialized software. In the context of this work, the most significant value of this analytical solution is that it provides a means to validate the choice of boundary conditions of a 2-D numerical model developed using the commercial software COMSOL Multiphysics. Then, having established the effectiveness of the 2-D numerical model, a 3-D model was developed using the same methods and tools, by adapting 2D boundary conditions to 3D simulation based on geometrical and symmetry considerations. Material properties and HWC geometry used for the modeling were based on the field conditions and data previously acquired during a survey at the New Afton deposit in Canada. Several scenarios are modelled and compared to help identify near borehole conditions explaining the difference between data acquired with straight and HWC at New Afton, specifically the weak-amplitude data obtained with HWC (Figure 2). The HWC modeling results and approach used here can provide insights for optimizing field deployments. Service companies could benefit by using this workflow prior to installation of HWC fiber optic cable in the field")

The first concern is that the results do not demonstrate clearly enough that you have made a valid 2D approach to replace or approximate a 3D computation. Let's consider an approach with 3 points below:

1. Using Comsol Multiphysics which is a 3D finite element computation tool, a 3D modelling can be performed really in 3D with both an helicoidal fibre and a straight fibre showing the "real" difference between them.

2. With the 3D modelling tool, you are in an excellent position to justify and demonstrate the difference between 3D computations and simplified results obtained with the 2D approach you propose. I am not a specialist of modelling. However, I guess implementing the comparison only by claiming boundary conditions are similar is not enough, is it? If yes, then you have to indicate clearly this in the text "as usually done in fem modelling, etc…" giving references. Not all readers are specialists of fem modelling method

and its modelling tricks (Please see our response to the prior remark, which is marked in yellow.)

3. The referee 1 complains about your analytic method"), although Kushinov (2016) proposed an approximation method which seems to be more powerful and accurate that the one you propose. If you disagree and want to show that the simplification you bring is still valid, then you need to show this much better. Why not implement Kushinov method (which seems simpler) and then compare it with your results and the 3D modelling with fem?

This approach would make your manuscript much more convincing.

The 2D analytical method presented in the Appendix is used to validate the boundary conditions for 2D FE modeling. The 2D geometry of both analytical and FE simulation are identical, meaning that results can be compared. This is not the case with Kuvshinov's approach which includes averaging of the radial strain over the outter diameter of the cable, casing, etc. Thus, this is why Kuvshinov's method, although more sophisticated is not used. Once properly established in 2D, boundary conditions were then adapted to the 3D FE simulation based on geometry and symmetry relationships. The three-dimensional FE modeling approach used in our paper is state-of-the-art and provides all the accuracy required to model HWC embedded in complex and realistic geological situations. FE modeling has no geometry restrictions (i.e., planar or cylindrical) and further allows analyzing the strain around the cable, something not easy to achieve with analytical methods. Again, we would like to re-emphasize that the main results of our paper are those obtained with 3D FE modeling and that the analytical method is used to confirm the choice of boundary conditions for the FE modeling.

The second concern is about the order of your presentation. The abstract does not even mention New Afton Deposit (It did and still does mention New Afton). As suggested by a reviewer, I would start by presenting the data with the issue you address by modelling (Applied). Something like "We do not understand why we do not measure the same thing as in the data, so this makes us willing to make 3D modelling. As those are complex, we propose a simplified method and show that this method is valid within ??%, and compares by ??% with Kushinov method." Instead, your discussion introduces data, which is clearly not the place to present them (That part was moved to the introduction). The discussion should be more perspective on the method, what it would bring etc… and enlarge to other locations. I would therefore start from the data, indicating that you have issues to understand why the helically-wound fibre gives different results that straight fibres, and that you want to address this by modelling. This would make the story clearer. The third concern is related to the presentation of your method, in particular in the appendix A. Notations are not consistent (Fixed) throughout the presentation, as the reference system is changing along the demonstration making results very confusing. In addition, equations disregard σxz at some place, but then it is introduced for the boundary conditions in which σzz does not exist, without explanation (Applied). The key equation A47 is wrong (We think notation was confusing, the equation is still valid) and should be corrected. Many symbols are not explained, making the reading hard to follow, unclear and may lead the reader to confusion and be doubtful on your work. In addition, hypothesis of the validity of the method are not exposed clearly enough. For example, what are the wave length of the seismic waves you use in the modelling (Added)? Any limitations? All 2 those issues should be indicated to make the paper as clearly and self-consistent as possible. This is at present not the case. The forth concern is that some assertion are simply wrong, for example, on the inability of straight fibre to measure correctly certain seismic waves, and on imaging capability of Rayleigh waves for exploration. In both cases, this seems to be a lack of understanding of the methods. There are many references in the scientific literature that demonstrate the use of surface waves to image the subsurface (Fixed)

Finally, the paper does not show clearly what the benefit of your study. How should we use the code to obtain which accuracy? Some reviewer asked about the size of the model (Applied). No answer on this appears in the manuscript.

(The RMSE is in the range of 0.003-0.004ε, which is a negligible error when comparing the 2d numerical modelling with the analytical model. The results of this paper can be used as a guideline for analyzing the impact of surrounding media and incident angle on the response of helically wound cable, optimizing the installation of helically wound cable in various conditions, and to validate boundary conditions of 3-D numerical model built for analyzing complex scenarios)

Additional details.

Line 47. For completeness, you should add seismology.

The references you use are quite old, and new applications have appeared, showing that the sentence line 52 and 56 is simply not true anymore (see for example Jousset, P., Reinsch, T., Ryberg, T., Blanck, H., Clarke, A., Aghayev, R., Hersir, G. P., Henninges, J., Weber, M., Krawczyk, C. Dynamic strain determination using fibre-optic cables allows imaging of seismological and structural features. *Nature Communications* **9**, 2509. DOI:

doi.org/10.1038/s41467-018-04860-y (2018). (Added)

Lindsey, N.J., Dawe, C. T. and Ajo-Franklin, J.B. Illuminating seafloor faults and ocean dynamics with dark fiber distributed acoustic sensing. *Science* **366**, 6469, 1103-1107, DOI: 10.1126/science.aay5881 (2019). (Added)

Sladen, A., Rivet, D., Ampuero, J-P., De Barros, L., Hello, Y., Calbris, G. &

Lamare, P. Distributed sensing of earthquakes and ocean-solid Earth interactions on seafloor telecom cables. *Nature Communications* **18**, DOI: 10.1038/s41467- 019-13793-z (2019). (Added)

Walter, F., Gräff, D., Kindner, F., Paitz, P., Köpfli, M., Chmiel, M. and Fichtner, A. Distributed acoustic sensing of microseismic sources and wave propagation in glaciated terrain. *Nature Communication* **11**, 2436. Doi:10.1038/s41467-020-15824-6 (2020). (Added)

At line 56, thanks to wave conversion, seismic strain can also be detected for hydraulic fracturing. In real Earth, not only P wave are generated, but also shear waves and Rayleigh waves, which all can be recorded with DAS. This paragraph is very restrictive and omit recent progresses (Fixed). This does not withdraw the very important advantages that the helically wound fibre optic cable brings. But as it is said in the text now, one has the wrong impression that only helically cable would save DAS measurements. When looking at the figure 16 in the last part of the current manuscript, it is not obvious that this is really the case 3 (We clarified why only P-waves were considered in the modeling work. This is simply because the reflected wavefield at New Afton is dominated by P-waves. S-wave reflections and mode-converted waves (P-S and S-P) were not identified on that data. Surface waves were not observed on the data measured deep at the New Afton mine).

Line 70. I do not understand why you do not start from the observations from Figure 16, which is not new, so you could start from this (Fixed).

In the figure 1, the scale is missing. How thick is the cable? (this info is in Table 2)

ezz is certainly a very bad choice for expressing what by the way?? In Kushinov, this figure report *ez* for this term. Then in the appendix A, one is confused with the strain component. The issue, also in Kushinov to be fair, is that the reference you are using is not given. It would be nice to define all the axis x,y,z in a reference system that is consistent all along the demonstration. I would call this term *el*, as along the fibre, which then would allow you to call *e*.V. . as *ezz*, along the vertical axis, much more conventional and which also can be used for the straight fibre. (Applied)

Line 96. I start right away to be confused, as the reference given computes the transmission of waves in a medium made of parallel geological layers. It is not clear if you use the technique, but there is a flaw, as the cable "media" surrounding the fibre are not planes but cylinders, or is you use a geologically layered model in which the cable is drilling. How much error do you do when approximating a cylinder with a 2D model?

(Please refer to page 2, the text highlighted in gray)

Line 104. It appears that your manuscript is based on the simple idea that "As boundary conditions give similar results with two methods (one simple 2D and one complex 3D), then I can use the 2D method in all cases". If you find a method which is simpler and more accurate, this is very good. However, it is not clear if the determination of the similarity in the boundary conditions in different methods is sufficient to validate the simplest approach in all case and inside the domain. If this is the case, then it must be made much clearer to the reader why the argumentation of saying "I can use a simpler 2D method instead of the more

complex 3D method" is valid within this uncertainly level. However, at present due to the confusion introduced by the reading of the appendix a, then, your whole argumentation is very weak. May be elements of answers can be found in https://journals.sagepub.com/doi/full/10.1177/1094428116641191?

(Please check the yellow highlighted answer above and also "The modeling work presented in this paper is used to help understand the performance of a helically-wound fiber from a VSP field study at the New Afton mine (Bellefleur et al., 2020). Straight and helically-wound fibre optic cables were deployed in a single borehole to assess the efficiency of DAS to detect geological interfaces and structures associated with mineralization. The New Afton deposit is a porphyry deposit comprising primarily disseminated Cu-Au mineralization. The fibre-optic cables were deployed in a steeply-dipping (70° from horizontal) deviated borehole starting in a work bay located 650 m below the surface and ending at a depth of approximately 1300 m. Both cables were placed inside steel drill rods (used as casing) and cemented in place with grout. The grout was circulated to the bottom of the borehole via a grout tube located inside the casing until grout eventually reached surface from both inside and outside of the casing (drill rods). The grout cured for one month prior to the VSP survey. Based on the afore-noted grout returns both within and outside the casing, it was assumed at the time that grout had filled both the casing and the casing-formation annulus; however, data collected during the survey (discussed below) suggests this may not have been the case. The data were acquired with 1 kg of explosives fired in a 20 m deep shot hole at surface".)

Line 110. Need to know more about this lucky P-wave which is working well with a simple model using a complex modelling tool… (velocity, source location, …). How big is the model? I note here that a referee required that information, but they are missing in the current manuscript. (Applied)

Line 111-115. Those are very strong limitations of your models, if they were true. I do not get the point of minimizing S waves and Rayleigh waves as if they would not be useful for exploration, just because you limit your study to P-waves. In addition, it is completely wrong to claim that "Rayleigh waves are nor suitable to explore the subsurface". There are a huge amount of publications using Rayleigh waves (e.g., multichannel analysis of surface waves) to image the structure of the subsurface, at all scales, including DAS as well.

(This has been re-written to clarify the context of our work)

Lines 199-120. Unclear. What are the "range of scenarios"? range of frequencies? What is typical for seismic frequencies? (It was in frequency domain, with dominant frequency of 100Hz)

Line 123. Missing space between in and Table. (Applied)

Line 133. The sentence has no verb. (Fixed)

In table 1, check the text of all lines and correct where inaccurate (line 5 5). It is a pity not to consider a case scenario where the cable would be located behind casing. (Scenarios were consistent with the field tests Because the cable was positioned inside the casing in the New Afton mine, a response of fibre while cable is located behind the casing is not considered.)

Line 140. Bottom page note. I am not sure this is allowed in Solid Earth. Check in the format requirement and comply to them. (We saw bottom page notes in papers published in Solid Earth)

Line 150-151. The paragraph is "numerical modelling" with full 3D capability. You refer here to the analytical solution of Appendix A to compute separately radial and axial strain. I miss why you need to compute separately with two approximate methods, although you have a capability to compute in 3D. This actually, as

suggested by one referee: did you actually made one full 3D computation? Why not show a 3D plot of the total strain around the 3d fibre? This is what we would like to see. This would be the reference for all 2D methods (yours and Kushinov, 2016) and plot the differences between such strains.

(The purpose of using a 2D analytical method is to validate the choice of boundary conditions in numerical simulations. In this simulation, it is aimed to investigate the strain of cable in radial and axial directions and relate these strains to the strain of fiber, using equation #1 by assuming that the fibre is wrapped around the cable at an angle of 30 degrees. This wrapping angle is derived from the HWC cable used in the New Afton mine. As mentioned previously, the impact of the surrounding media is best observed on the radial strain. Thus, modelling strains separately allows to show details of radial strain and assess the effect of surrounding media more effectively).

§2.3. boundary conditions. It seems this is the core of the demonstration. This point should be made very clear: how a 3D cylindrical model is equivalent to a 2D flat model? Is it true that if I apply similar boundary conditions to the 2D as a simplified 3D or to a 3D model, the results will be the same?

(Because real geometry cannot be modelled in a 2d model, the results will vary based on the geometry, and if it is modelled in 2d, it will result in significant errors.)

In Figure 4, how were computed the numerical solutions? 2D or 3D? it is not clear what was the wrapping angle of the fibre you used in this model. It seems the solution will depend on it no?

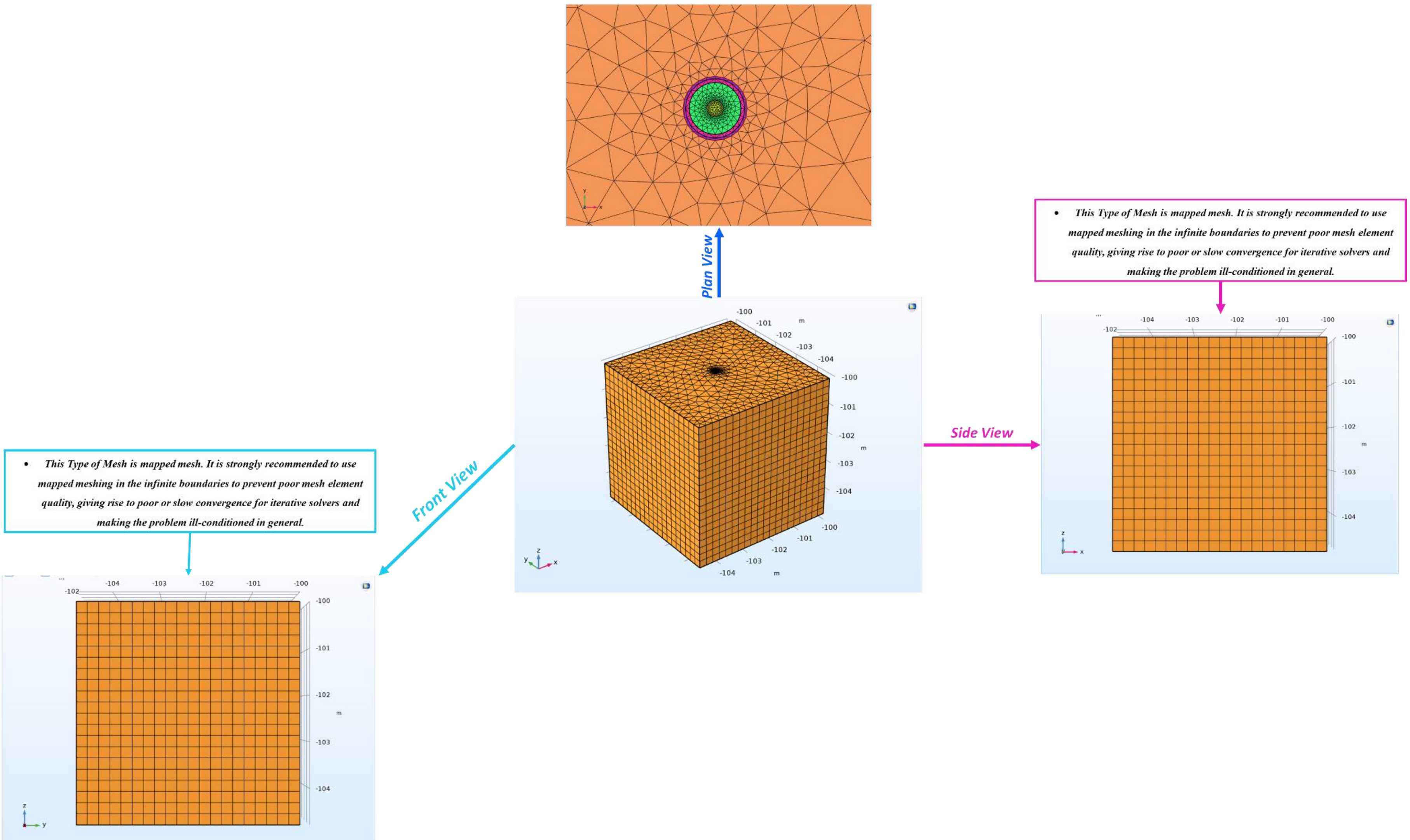
(clarified in the text. The fibre is wrapped around the cable at an angle of 30 degrees. This wrapping angle is derived from the HWC cable used in the New Afton mine. Indeed, the response depends on the wrapping angle.)

Line 192-194. There is a logical issue. Comsol is able to compute 3D solutions directly for the full strain tensor. Why do you compute separately axial and radial? In addition, why do you need call to the 2D analytical solution? This is where we completely lose track of what you are really doing. In addition, before comparing several scenarios, we would like to see the differences between 2d and 3D for a similar configuration.

(The purpose of using a 2D analytical method is to validate the numerical simulations. In this simulation, it is aimed to investigate the strain of cable in radial and axial directions and relate these strains to the strain of fiber, using equation A.49 by assuming that the fibre is wrapped around the cable at an angle of 30 degrees. This wrapping angle is derived from the HWC cable used in the New Afton mine)

Line 196. It is not possible to judge for anyone if those are really rendering issues of the results. How to trust that the results are indeed valid in the cable? What makes you so confident? From the graphs (that I find too small once again), the mesh used seems very lose inside the cable. This is to me a critical issue, as we measure data in the cable. If modelling is not able to render what is happening in the cable, how can we envisage getting further in the use of your method for DAS? Getting those issue for the "rendering" rises also questions on the validity of the boundary conditions used, and your story collapses. In contrary, I would strongly suggest to give every effort to increase the resolution of the mesh to be able to compute properly the strain within the cable. I trust the mesh cell could be much smaller to image properly the fibre and all the layers surrounding it.

The picture below should clarify that there are no meshing issues with COMSOL. Inside the domain, a tetrahedral mesh was employed, while lower reflecting surfaces were covered with a swept mesh (see figure 4).


Plan View
Side View
Front View
• This Type of Mesh is mapped mesh. It is strongly recommended to use mapped meshing in the infinite boundaries to prevent poor mesh element quality, giving rise to poor or slow convergence for iterative solvers and making the problem ill-conditioned in general.
• This Type of Mesh is mapped mesh. It is strongly recommended to use mapped meshing in the infinite boundaries to prevent poor mesh element quality, giving rise to poor or slow convergence for iterative solvers and making the problem ill-conditioned in general.

Line 203. Not clear if figure 6 is for hard? And fig 7 for soft? If yes, add “respectively”. (applied)

Line 206. This is a real pity. And by the way, if this is computed correctly as you indicate at line 196, why can’t you compare? Arguments on why it is not possible should be given, and I believe there is a way to compute with much higher resolution.

(Applied)

Line 218. I guess you mean figure 12 not 13…. In addition, which strain is shown? *ε xx* or *εrr*?

(Applied)

Line 225-230. Those lines reflect results that are not surprising. How could it be differently?

(Because real geometry cannot be modelled in a 2d model, the results would vary based on the geometry)

Line 235. Which frequency? (applied)

Line 236. Is attenuation considered? I understood that everything was elastic, actually acoustic, without S waves. So why mention attenuation? Or do you mean amplitude decrease due to geometrical spreading?

Clarifed. Attenuation is not considered in our modeling. In addition, there is no geometrical spreading for plane waves. This is the definition for a point source: As the wavefront moves out from the source, the initial energy released in the seismic wave is spread over an increasing area and therefore the intensity of the wave decreases with distance (the case of geometric spreading).

Line 242. Low incident angles… which values? where?

Line 248. “highly attenuating”. This is not modelled, so irrelevant (Applied)

Line 250. Those waves are not surface waves, but interface waves (Applied)

Line 251. I do not understand this argument. It seems wrong. In #6, the maximum amplitude is higher than in #5 indeed, but lower than in other scenarios without water. Can you explain (in the manuscript as well)?

(not sure I understand this point. the presence of water has an impact on radial strain, simply because part of the incident P-wave is converted to a tube wave (surface wave). This mean lower strain at the fibre. This conversion does not happen for scenarios 1-4. As a result, they (scenarios 1-4) have a larger response)

Figures 6-11 should display similar color scale and range so we can compare results.

(Unfortunately, we do not have access to the COMSOL anymore and our license has expired)

Fig. 12.15 could be shown all together for easier comparison. I do not get the benefice of comparing 2 by 2.

(If they were all on the same graph, there would be too much information for readers to handle.)

Figure 7 to 11. “Radial”. I really do not follow how the radial strain around the cable has this shape. Where is the wave coming from? Has the wave a waveform? The strain is changing with time, at which time is it represented? The scale between the different figure should be the same. The mesh seems better defined in Fig 8 and 9 compared to the figure 7-8 in the most central part. Why is this? Would it be possible to zoom closer to the cable to see the shape of the strain inside the cable? Figure 12. and 13. Indicate on the figure where is soft and hard. Reporting scenario # is nice, but not very clear enough. The caption could get the list of scenarios main characteristics this comment is not mandatory, but it would make the reading much simplified). I see on figure much less than 10% difference between the curves, but at line 221, there is another value indicated, make it consistent.

The seismic pressure equation is used to construct prescribed displacement boundary conditions to ensure that the displacement is properly in a plane based on wave propagation. As a result, the wave does not have a specified waveform.

As the source operates at a dominant frequency, the simulation took place in the frequency domain (100 Hz). This type of simulation aids in the analysis of outcomes at the required frequency. As a result, there is no time limit.

Figure 14. line 285. Rephrase: “strains” are not “angles”. (Applied)

I still do not understand why it is not possible to draw a figure with few loops of the 3d helix and show us how strain components vary along the fibre, from the 3D computation.

In this simulation, it is aimed to investigate the strain of cable in radial and axial directions and relate these strains to the strain of fiber, using equation A.49. So fiber strain for HWC is not a direct output from 3d modeling. However, radial and axial strains were the direct outputs from 3d models which were imported into Matlab to derive fiber strain.

Discussion.

As presented here this is not a discussion. A discussion would make the possible limitation of your study, and how to improve them. It would also be the location to discuss how to improve the deployment. Do you have suggestions, etc…

(Discussion was modified accordingly)

Line 292. For completeness, I would also indicate Reinsch et al, 2017. Reinsch T., Thurley T., Jousset P., 2017. On the mechanical coupling of a fiber optic cable used for distributed acoustic/vibration sensing applications — a theoretical consideration. - Measurement Science and Technology, 28, 12. http://doi.org/10.1088/1361-6501/aa8ba4 (Applied)

Line 295. This is not shown at all. You never made the link between your theoretical study and the new Afton experiment that we hear about now only. In addition, this is not the place in the discussion. Why not starting the paper from this observation and justify all efforts. The introduction would be much easier to set up, and this would give an objective clear for your study. (Applied)

Line 301. Space missing between 10 m and for. (Applied)

Line 310. Was the straight cable a Constellation fibre? And what about the helically one?

(all fibre-optic cables results shown in this paper were obtained with Constellation fibre (this is clarified in the text in the section on New Afton data.)

Line 370 – figure 16. What is shown? Longitudinal strain? Radial strain? What is the color map used?)

(seismic data (ie strain rate) are shown in this figure. This is clarified in the text. The color bar description is added in the figure caption)

The reference list is to be checked. For example, Innanen et al. 2019 is not cited in (Fixed)

the manuscript.

Appendix A.

Line 441. You cannot start the appendix by “this”. Which one? (Applied)

Line 443. The potential function letter (phi) is missing. (Applied)

Line 447. What is ∇? (Applied)

Line 453. You mean A.1? (Applied)

Line 460. What is a “perfect” plane wave propagation?

(Because the source is far away, the spherical head wave is treated as a plane wave. As a result, displacement curve is in the plane)

Equations A5 and A6. Where is the shear stress component? Do you develop the method only for P-wave? This is very limiting… How would it be to expand to S-wave as well?

(For both analytical and numerical modeling: "S-wave could also be modeled for isotropic cases by considering that particle displacement is orthogonal to the wave propagation direction and adjusting the incidence angle accordingly)

Line 467-469. This approximation is really strong. A better quantification on the

error made should be properly given.

(This is simply describing a 2D simulation scenario which obviously has its limitations. This choice (2D modeling) has been explained in the section on numerical modeling. In Appendix A, we are providing the development for this 2D analytical response)

Line 470. You did not indicate what are the layers. Otherwise it is a bit more criptic…

Line 475-477. The shear stresses do exist! But where is σ_{zz}? (Applied)

Line 477. What is this reference to “After 13”?? It seems you copied your text from

somewhere without checking properly the meaning of what you write… (Fixed)

Line 488. Again σ_{zz} is absent.

(Because continuity equations between layers satisfied)

Line 490, why do you need the infinitesimal factor?

(Epsilon is used to satisfy the boundary condition within the range adjacent to the layers)

Line 491. Define d1, d2, etc… from fig A.2? (Applied)

Equation A42. not clear what F11, F12 etc… are. (Applied)

Line 506. A1 is the amplitude of the wave generated by the seismic source. It is known in the case of an active experiment. However, one may want to use helically fibre with microseismic data, where the source is unknown… Are we then lost?

(This research is intended to investigate the factors that influence HWC response, which necessitates the use of well-known parameters. The amplitude of the source can be determined for microseismic events by performing some back analysis)

The term ε_{xz} is missing, why? (Applied)

Line 513. This approximation is really the strong assumption of the approximation which allows you to migrate from 3D to 2D. This is exactly this difference we want to see between the two and validated by the 3D true numerical computation. As indicated by several referees, this equation is wrong sand should be corrected. As you see, there are still many unclarities, questions, and I guess my reading was not exhaustive. I would suggest you to read the manuscript again very carefully and bring all attention to make the manuscript readable, clearer and fully justified following the indications given by the referee reports and this review. Once those details will have been addressed, we can reconsider your manuscript.

(The formulation was not wrong, just notation was confusing, we changed the notation)

(The purpose of using a 2D analytical method is to validate the numerical simulations. In this simulation, it is aimed to investigate the strain of cable in radial and axial directions and relate these strains to the strain of fiber, using equation A.49 by assuming that the fibre is wrapped around the cable at an angle of 30 degrees. This wrapping angle is derived from the HWC cable used in the New Afton mine)

Referee 2

Minor comments:
Abstract: Typically an abstract does not contain paragraphs. please confirm with the journal's style guide. The first paragraph of the abstract reads more like an introduction. An abstract is supposed to be concise.

Authors action: The abstract has been shrunk in size to 232 words.

In section 2.2 you should clarify the source location (Source location is in the infinity, mentioned in the text) or how you get to retrieve the different incidence angles in Figures 4ff (The displacement boundary condition was established as a function of incidence angles ranging from 0 to 90 degrees) . Your workflow is not clearly described and thus not reproduceable (The formulas, boundary conditions (2d versus 3d), inputs, and other details were all given in detail). This is currently the major flaw of this manuscript (I hope this is no longer the case). What is your input amplitude of the wave? (1m) The model dimensions are missing (Size of the model for hard and soft formations are 4.78^3 and 1.63^3 m^3), as well as a description how values were extracted from the model to obtain the figures. It is good scientific practice to explicitly write the steps required to reproduce your work. A homogeneous (cement) block would be great as baseline model.

Your models and figures only show HWCs never straight cable. in the data examples in the end you then make conclusions about comparison. I may have missed some arguments here?
(The presenting sequence has been adjusted to make it more understandable to the readers).

Line 22: Two dots before "Results" (Applied)
Line 22: "modified" -> you never mentioned the unmodified model, so this seems out of place... (Applied)

Line 56: DAS can very well detect (dynamic) strain from hydro-fracs in vertical wells. It all depends on relative geometry and incidence angle (Applied)

Line 79: awkward grammar with 5 verbs in succession (Applied).

Line 96: be consistent in upper or lower case writing of "Appendix" (see line 102) (Applied)

Line 114: Dot missing after "processing" (Applied)

Line 130ff: This feels like a repetition from earlier. Maybe track-changes left-over fragment? (Applied)

Table 1: Scenario 5: What does "they are cable" mean???? (Deleted)

Table 2: your eta is physically identical to the wavelength, which is in seismology typically denoted with lambda (Eta was replaced by lambda)
Why did you chose 1/12 of the wave length? (Be able to Compare at a certain wave position)

Why are some parameters given in absolute diameters, others relative to wave length? (Be able to Compare at a certain wave position)
Which (dominant) frequency is assumed? (100 Hz)

Line 153: Dot missing at the end of sentence (Applied)

Figure 3: You never mention the (absolute) dimensions of you model (Size of the model for hard and soft formations are 4.78^3 and 1.63^3 m^3)

Line 170: Change to "Figure 4 and 5" (Applied)

Line 172: What units are the RMS errors (I suppose in "strains", typically denoted by epsilon)? Or is it in percent? (Unit is strain)

Figure 4: please clarify your angle definition. I suppose 90deg is cable parallel?(Based on $e_{||(\text{Fiber})} = e_{zz(\text{Cable})} \cos^2 \alpha + e_{rr(\text{Cable})} \sin^2 \alpha$, parallel to the cable means alfa 0)
is your "fibre strain" the ezz of Kuvshinov, or the strain in the surrounding material (ezz is the fibre strain).

Figure 5: Here are smaller max values than in Figure 4. Comment on this (Applied, Scenario 4 differs from the others in that the soft formation is adjacent to the casing in this case. Because of the huge difference in material properties, the reflection index on this surface is larger, and a large quantity of energy is reflected to the soft formation)
It may be instructive to have a simple model of a cable in a homogenous cement block as a baseline model....

Line 203: Fig 6 and 7 "compare" it is difficult to compare if the two have different color scales...
Line 207: Repeat WHY they cannot be compared (Applied)

Line 218: Check figure numbering, Fig 13 seems wrong here (Applied)

Line 223 "lesser" -> "smaller" (Applied)

Line 240f: The sentences starting with Scenario #5 /#6 can surely be simplified by mentioneing similarities and differences (text re-written with the following: Figure 15 shows a comparison of modeled fiber strains for scenarios #5 and #6. These are both 5-layer scenarios similar to scenario #1, except for the presence of water either between the casing-formation annulus (scenario #5) or between the cable-casing annulus (scenario #6). Hard cement replaces water when not present for both scenarios.)

Fig 6-11: If I understand your paper correct you are modelling a plane wave traveling through your model. Here, you show a static strain "snap shot" of the wave? at what time step (It was in frequency domain)? How does that strain translate into DAS amplitudes (Amplitude is strain, isn't it?) How are these figures related to fig 4&5?

Line 241: This first paragraph of the discussion is in fact part of an introduction. I am sure the experience of the co-authors will help here to reformulate and restructure. (Applied)

CONCLUSION:
Explicitly write about benefits of HWC. At the moment it sounds very skeptical, but there seems to be features in the data that can only be explained when using HWCs (added one sentence highlighting the benefits of HWC at New Afton – mainly the identification of poorly cemented areas between the casing and rock formation)