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!

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.
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 second concern is about the order of your presentation. The abstract does not even mention New Afton Deposit. As suggested by a reviewer, I would start by presenting the data with the issue you address by modelling. 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. 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 throughout the presentation, as the reference system is changing along the demonstration making results very confusing. In addition, equations disregard σ_{xz} at some place, but then it is introduced for the boundary conditions in which σ_{zz} does not exist, without explanation. The key equation A47 is wrong 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? Any limitations? All 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.

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. No answer on this appears in the manuscript.

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).

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).

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).

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).

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. 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…

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.

In the figure 1, the scale is missing. How thick is the cable? e_{zz} is certainly a very bad choice for expressing what by the way?? In Kushinov, this figure report e_z 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 e_l, as along the fibre, which then would allow you to call $e_{¿\vee¿¿}$ as e_{zz}, along the vertical axis, much more conventional and which also can be used for the straight fibre.

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?

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?

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.

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.

Lines 199-120. Unclear. What are the “range of scenarios”? range of frequencies? What is typical for seismic frequencies?

Line 123. Missing space between in and Table.

Line 133. The sentence has no verb.

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.

Line 140. Bottom page note. I am not sure this is allowed in Solid Earth. Check in the format requirement and comply to them.

Fig. 2. In all figures, texts are too small. If it remains so, the reader will not read your paper, as unclear. The caption is very enigmatic. What are #1 and #2 etc.?

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.

§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?

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?

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.

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.

Line 203. Not clear if figure 6 is for hard? And fig 7 for soft? If yes, add "respectively".

Line 205. How do you see this result? We want to see much closer zoom.

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.

Line 218. I guess you mean figure 12 not 13…. In addition, which strain is shown? ε_{xx} or ε_{rr}?

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

Line 235. Which frequency?

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?

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

Line 248. "highly attenuating". This is not modelled, so irrelevant.

Line 250. Those waves are not surface waves, but interface waves.

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)?

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

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

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.

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

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.

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…

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

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.

Line 301. Space missing between 10 m and for.

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

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

The reference list is to be checked. For example, Innanen et al. 2019 is not cited in the manuscript.

Appendix A.
Line 441. You cannot start the appendix by “this”. Which one?

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

Line 447. What is ∇?

Line 453. You mean A.1?

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

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?

Line 467-469. This approximation is really strong. A better quantification on the error made should be properly given.

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}?

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…

Line 488. Again σ_{zz} is absent.

Line 490, why do you need the infinitesimal factor ?

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

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

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?

The term $\varepsilon_{xz} = \frac{1}{2}\left(\frac{\partial u_{xx}^{n}}{\partial z} + \frac{\partial u_{zz}^{n}}{\partial x}\right)$ is missing, why?

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.