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Isterre,
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Topical Editor, Solid Earth
The Australian National University
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Manuscript ID: se-2020-77

Dear Editor,

We are pleased to resubmit for publication the revised version of se-2020-77 "*Monitoring surface deformation of deep salt mining in Vauvert (France), combining InSAR and levelling data for multi-sources inversion*". We have carefully followed the comments and have revised the manuscript accordingly. Our responses are given in a point-by-point manner below (in blue). Following reviewer's advice, the structure of the manuscript has been revised. Changes to the manuscript are shown in blue.

We would like to express our sincere appreciation to the two reviewers for their constructive comments, advice and questions. We would also like to thank you for allowing us to resubmit a revised version of the manuscript.

We hope the revised version is now suitable for publication in *Solid Earth* and we look forward to hearing from you.

Sincerely yours,

Séverine Furst
On behalf of the authors

Reviewer 1

This paper is interesting, and certainly worth publishing. In the past, only vertical displacements were measured above brine caverns fields. Satellites provide much more information, opening the way for a more comprehensive analysis of subsidence data.

> *We have changed the manuscript to include most of your advice. However, as Reviewer 2 suggested new formulations and a modified structure, some of the corrections were no longer relevant. Hereafter, we have addressed specific answers to your comments.*

It is suggested to add (at the end of the paper) a vertical cross sections along a selected profile (similar to Figure 3) in which both horizontal and vertical displacements are represented. A few changes are suggested below.

> *We think that it would be more difficult and redundant with Figure 7 to represent the vertical and horizontal displacement along a profile. Besides, considering that the combined dataset provides a unique and dense spatial distribution of the velocity rate, the representation of the horizontal displacements along a profile would be a loss of information. Instead, we added arrows corresponding to horizontal velocities on Figure 7.*

L.34: No, when cavern pressure is kept constant and smaller than geostatic, the stress distribution reaches a steady-state distribution which is not geostatic. “until the cavern volume vanishes”

> *We have changed accordingly.*

L.48: this sentence is unclear. To my knowledge, this model (SALT_SUBSID) takes into account the difference between geostatic pressure and cavern (fluid) pressure, which is not assumed to be zero. Old versions of Van Sambeek’s software were used to predict subsidence above salt mines, in which the pressure is zero.

> *We agree with you and modified the manuscript.*

L.131: Âń until lithostatic Âż: no, brine pressure is released before lithostatic (geostatic) pressure is reached.

> *In the case of Vauvert, wells are closed when salt saturation is insufficient. Hence, the brine pressure increases at well heads. Nowadays, the company is asked to release the pressure of abandoned wells.*

L.320. In the reviewer’s opinion, the mathematical functions should be provided in the paper (in an Appendix?)

> *The mathematical functions are already known, published and used in the literature. We think that they are not essential to the understanding of the procedure and would add too much to an already long manuscript. They can be found in Okada (1992).*

L.324: explain which parameters are concerned.

> *We have specified the concerned parameters.*

L.437: these sentences are confusing. Salt extraction does not generate subsidence per se. Creep closure (volume loss) does. A relation exists between extraction rate and subsidence rate; however, creep closure rate must be taken into account, and a large part of the effect of salt extraction is deferred.

L.448: This issue is difficult (see above). In fact, there are two aspects here: (1) is the subsidence a deferred (not instantaneous) effect of salt extraction? (it is) (2) is the stress distribution similar to the linear elastic distribution at some distance from the caverns? (maybe).

> *We agree that creep closure should be taken into account in the subsidence process. However, it implies visco-elastic processes and time-dependent deformation. Levelling survey have been performed since the beginning of the exploitation, and data show instantaneous response to the salt extraction. We assume that most of the short-period (1-2 years) signals is produced by the instantaneous (i.e elastic) response, but we admit that some of the subsidence may come from a deferred signal. Unfortunately, we do not have the stress distribution out of the wells.*

Reviewer 2

Dear Editor,

The paper " Monitoring surface deformation of deep salt mining in Vauvert (France), combining InSAR and leveling data for multi-sources inversion " by Furst S, Doucet S, Vernant P, Champollion C, and Carme J-L , presents a study of the surface deformation above the salt mines in Vauvert, including a modeling part.

The paper and the approach are interesting but the paper suffers from many weaknesses in its curent form. First, the writing has to be improved throughout the whole ms, and many English mistakes could have been avoided (I started to list all of them but I stopped, see below, I highlighted most of the small corrections in the pdf version). Second, I suggest to improve the structure of the paper, and particularly within each sections, where many redundancies and confusions are spoiling the study. Third, the paper suffers from an unappropriated vocabulary, which is regrettable for a scientific paper. For instance, the word deformation is used over the whole text, whereas it should be displacement or velocity. Before publication, I would suggest the authors to add several major aspects on their study, according to the journal expectations. My suggestions/comments are listed below (in the order of the presentation of the paper); I am convinced that they will strengthen the paper, and help to emphase the conclusions.

> *We have make a significant number of changes to the manuscript with respect to your comments and suggestions. A careful reading and correction of the English mistakes has been done. As suggested by the reviewer, changes in the structure of the paper has been done also as in the introduction for example.*

Introduction

The introduction should be rewritten. In its current form, it does not address clearly the problematics and the related methodological challenges. It is very hard to follow, since the structure is weak, with a section on the models, then on the data and data combination, then models again.

> *We reorganized and partly rewrote the introduction. We hope that it improves the understanding of the work presented in the manuscript.*

The authors forget to introduce the origin of the horizontal displacements. Are they induced by the slat flow? In this case, they should explain the process. Are they related to the centripetal displacements associated with the subsidence? This point is very important and should be better introduced in order to present the main objectives of the paper (L. 55-56).

> *We added in the introduction the presentation of the horizontal motions giving suppositions for their origins (L.42-49).*

Too many details regarding the models are given, whereas the main questions are not addressed yet. Therefor, it seems that more explanations are needed on important points (see L. 65-70, by the way this sentence should be with the following paragraph). Being in the introduction, some parts are lacking from clarity and important details (L. 50-52: this part is quite fast; the authors should be more clear about the models including a source with pressure or volume change (Mogi, penny-shape sill etc…))

> *As we have reorganized the introduction, we shortened and collected the description of the models (L.83-91).*

Context and Data (section 2)

I would recommend to separate the context and the data.

We have separated the geological context and the data.

Regarding the geological context, it is quite surprising that only one reference is cited.

The geological context and evolution of the Gulf of Lion in southern France was indeed presented by Séranne et al., (1995). We added the reference (L.103). However, the geological study of the surrounding area of the salt mine was conducted and published by Valette and Benedicto (1995) only.

Regarding the datasets, many questions and comments are rising. Surprisingly, it seems that the word technique and data are mixed several times. I would recommend to add a section on the accuracies of the measurements deduced from each technique, together with a discussion on the geometry of the displacements/velocity (LOS vs. vertical and horizontal).

The accuracy of the measurement was already described for levelling surveys (L.148), so we added the mean accuracy for InSAR (L.165-167) and GNSS (L. 194-195) measurements. However, the accuracies of each measurements being not the subject of this study, we did not dedicated a section. Besides, we are not sure to understand the request about the discussion on the geometry of the displacement.

It is important to give the order of magnitude of expected surface H and V displacements/velocities and compare them with the accuracies of the geodetic techniques used in this study.

We are not sure to understand the request of the reviewer. As a matter of fact we do not expect anything, we measure surface displacements and then try to understand the processes explaining these displacements.

We wonder why the ALOS SAR images are not included in the study.

We choose to only use Sentinel images because of its general features. We have specified our choice in the manuscript (L.140-142).

Regarding the InSAR, the displacement data are the InSAR measurements and not the SAR images. Also, the authors use the word « develop a PS-InSAR processing chain » (L. 160), however I am quite sure that the chain has not been developed by them.

We used the existing algorithms to develop the chain used in this study.

L163 : Master image are chosen so that the time and the perpendicular baseline are optimized.

Perpendicular baseline in SAR interferometry is the distance perpendicular to the Satellite flight direction between two repeat orbits.

Some sentences should be clarified (L. 169).

We rewrote the sentence.

The incidence angles and directions should be given in the data section and in the Figure 4.

We have specified the look and incidence angles in the data section and in Figure 4.

How are the InSAR-velocity uncertainties calculated?

InSAR-velocity uncertainties are estimated using the standard deviation of PS. We have modified the manuscript.

The authors use the term « bowl » without clarify its cover on surface (see L. 195).

We have given the cover of the subsidence bowl in the Introduction (L.43-44).

A major step is missing in the data/methodology for the InSAR data. How are the velocities calculated from InSAR displacements? Are the displacements stationary over time etc?

We detailed the step leading to the computation of velocities using InSAR data.

Methodology

In the section 3.1, it should be more clear that the adjustment concerns the InSAR data to the other data.

We have specified it.

Some more details should be given regarding the horizontal shifts of the registration of the InSAR results, and how the manual detection is made (L. 212)?

We detailed the horizontal shifts for the InSAR results in a few words, crossing roads are used to compute a mean horizontal shift.

What do you mean by « reference system of InSAR data » (L. 213)?

InSAR results are "Datum free" as they are measured relatively in time and space. By choosing 26 GNSS reference points collocated with PS-InSAR, we constrain the InSAR results in a Terrestrial Reference System (TRS, here ITRS).

What are the quantitative arguments to consider that the PS density is suffisant to make the kriging?

The relevance of any geostatistical method can be questioned by data density, but kriging has shown one of the best method to address irregular data coverage. It is not possible to have quantitative arguments due to the PS density variation over the area of interest. Moreover, not only PS density has to be checked but also spatial disposition/geometry as regular as possible. One can note that the density of both leveling and PS is sufficient to cover the spatial variations of deformation (from the center of the subsidence to the edge where the deformation is not significant).

The mathematical relations in the section 3.1.2 could be better written.

We are not sure about the meaning of this comment, but we kept the useful equations that were used in this study, to lighten the understanding of the approach.

The coordinate system should be clarified (N,E, Up or E,N,Up as in fig. 7?).

We have changed accordingly.

How is the centre of the subsidence bowl chosen?

The center of the subsidence is chosen such as it corresponds to the (1) maximum subsidence measured in near-up component velocities (so considering no effect of the North component) and (2) on the iso-contour where East-West component is equal to 0. We have specified it (L.275-276).

What about the squint angle?

Sentinel-1a/b TOPSAR data are sensitive to orbit errors (because of (1) high Doppler centroid variation, (2) non-continuous acquisition - bursts). It implies a possible phase ramp on interferometry and possible phase jumps in bursts transition. However, the small order of magnitude of these errors (Gonzalez et al., 2015), and the calibration of the PS-InSAR results with regional permanent GNSS data, make this effect negligible.

González, P. J., M. Bagnardi, A. J. Hooper, Y. Larsen, P. Marinkovic, S. V. Samsonov, and T. J. Wright (2015), The 2014–2015 eruption of Fogo volcano: Geodetic modeling *of Sentinel-1 TOPS interferometry, Geophys. Res. Lett., 42, 9239–9246, doi:10.1002/2015GL066003*

The part related to the projection of the velocity along the profile is not clear.

We rewrote this part of the methodology.

The section 3.1.3 (kriging) is very unclear, whereas it is very important for the paper. By refining the vertical velocities, it becomes possible for the authors to redefine the horizontal components of the velocities, or check that your initial assumptions are correct.

As the horizontal velocities are used to estimate vertical ones, re-estimate horizontal components after regression kriging will only allow to come back to firstly estimated horizontal velocities, and will not be a rigorous and independent check.

For the figure 7, I would recommend to plot the horizontal velocities with arrows and vertical with color.

We have superimposed a coarse sampling of horizontal velocities using arrows. But we think that the color scale is essential to capture the information about horizontal velocities as well.

Amplitudes reaching 20 mm/yr for the horizontal velocities are quite surprising, and are not consistent with the GNSS velocities. This kind of discussion should be included in the paper.

We have included a discussion about the comparison of the combined velocity field with GNSS data. It was previously in the discussion, but we agree that it should appear in the presentation of the combination results. Besides, we have seen this order of magnitude in other similar subsidence bowls in various regions (Middle East [Oman], Central Asia [Kazakhstan]).

Inversion

The authors claim « To infer the deep deformation using this combined dataset, the single dislocation plane is not sufficient to explain the horizontal motion. » (L. 302-303), but they should show it first. They should give more arguments, using the deep geometry of the mine.

Looking at the results, it seems important to add a fourth row at depth. It is not serious in a scientific paper to just affirm in the discussion that it would not change the residuals.

Less patches going deeper could be interesting.

What about a simple Mogi source to explain subsidence and radial horizontal displacements?

We have completely revised this section to present the comparison between 4 different models with different configurations including, a point source, a single plane of dislocation, and two collections of 21 and 28 planes of dislocations. We did not consider a model with less patches going deeper, because we aimed at modelling the salt layer which doesn't go deeper than 3500 m. We hope that it improves the integrity of the discussion.

L. 310: I have been lost. Why only 2 years, when you are using the whole dataset?

In the data section, we precise that we only consider 2 years of the dataset corresponding to Sentinel-1 time coverage.

I remain doubtful regarding the inversion with 21 patches, and 63 free parameters to retrieve.

We hope that it is clearer with the new comparison of the models.

It should be mentioned that the depth is fixed.

We have changed accordingly.

One more time, such a sentence « The optimal number of planes was found out by trial and error. », cannot be only mentioned, and not in the discussion part.

See previous answers.

« The dislocations parameters are set free to vary between -1 and 1 m » : most of the parameters are angles?

Dislocation parameters are displacements of one block over the other, along the defined plane of dislocation. Angles are defined and fixed to geological model as prior information.

The propagation of the uncertainties remains obscure.

We modified the description of the uncertainties. We hope it is more understandable. More details can be found in Mohammadi et al. (2016)

What is the smoothing conditions between patches?

We do not consider any smoothing conditions between planes.

The series of tests of other models should not be in the discussion section (L. 443).

See previous answers.

Discussion

I suggest to make this part more structured (with subtitles for the several points to address). The comparison of velocity amplitudes from the distinct datasets should mainly be in the result section, to validate or invalidate the approach.

We have added subtitles to structure the discussion and removed some points that are now discussed in other sections of the manuscript.

What are the constraints within the mine for validate the model?
L. 453: how do you constrain the stress tensor from your model?
L. 455: I am pretty sure that stress measurements have been done in situ from many years.

During the pumping stage, the fluid pressure is monitored. Logging while drilling measurements are commonly performed and some data were available. However, the stress can only be access by analyzing and interpreting the borehole breakouts as we suggested (L.520-525). So, we would need the microseismicity performed by Baker Hughes since 2019, hoping that there is an event, significant enough to be recorded by several seismic station, to identify the nature of the deep structure.

I would remove the discussion with the Teil event, which seems out of the subject of the paper. We do know what is the purpose of the study! Reading at the discussion, we get the feeling that the problematics of the study is still unclear for the authors.

We have removed the discussion about Le Teil event. We aimed at giving another opportunity to justify the interest of modelling the salt layer.

Acknowledgments:

The RESIF products should be references (DOI des données : 10.15778/resif.rg; https://www.resif.fr/donnees-et-produits/donnees-gnss/)

We have included the RESIF products in the "data availability" section.

Figures:

Figure 1:

Caption of Figure 1: Change « concerned » by « area under study ». Is that a «Geological structural scheme » or a cross-section?

> *We have changed the caption accordingly. We present a geological structural scheme and not a cross-section.*

Figure 2:

What is the meaning of KemOne properties?

Caption of Figure 2: change techniques into data. Indicate the meaning of the purple areas.

> *We have included the modifications on the figure.*

Figure 3:

The axes need a title (distance along profile (km) vs. vertical displacement (mm).

A color legend for the points is needed.

Caption of Figure 3: b) Time evolution of the vertical velocity of the marker... How is the velocity estimated?

> *We have modified the figure accordingly and explained in the text (L. 155-159) how the velocity is estimated.*

Figure 4:

Specify that the covered area is the same than in Fig. 2b.

> *We have specified it.*

Figure 5:

Scale for velocity uncertainties are missing. The titles of the axes are « longitude (m) » and « latitude (m) ».

> *We added the scale for velocity uncertainties in the figure, as well as longitude and latitude titles on the axes for all figures.*

Figure 8: What is the decollement (detachment)? It should be mentioned in the text.

Caption: the purpose of the figure is only mentioned at the last sentence. Reverse the order

> *Decollements are thrust fault D1 and D2 presented in Figure 1. We modified the text to include it (L.112-113). We have also reversed the order of the caption.*

Minor but important comments

> *Most of the modifications and comments have been considered. Only two are discussed hereafter.*

L. 335: do you mean assumptions?

> *No, in our case "guesses" refers to the initial value of the parameters. Ivorra and Mohammadi (2007) and Mohammadi and Pironneau (2009) use this term.*

L. 346: scenarii

> *The plural of scenario in English is scenarios.*