

# ***Interactive comment on “Subsidence associated with oil extraction, measured from time-series analysis of Sentinel-1 data: case study of the Patos-Marinza oil field, Albania” by Marianne Métois et al.***

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We thank reviewer 2/ (Simon McClusky) for his comment on our paper "Subsidence associated with oil extraction, measured from time-series analysis of Sentinel-1 data : case study of the Patos-Marinza oil field, Albania".

The main concern expressed in this review, i.e. the fact that more detailed analysis on the seismological data are required to conclude on whether induced seismicity is occurring in the oil field, is in accordance with the one pointed out by reviewer 1. As

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already stated in our reply to reviewer 1, we agree that some sentences of the submitted manuscript on induced seismicity may appear too affirmative and should be toned down. We also propose a more careful analysis of the local seismicity. Nonetheless, we reiterate that local seismicity remains poorly known due to sparse local and regional seismic networks (see figures 4, 5 and 6 of our reply that we wish to include in the revised version of the manuscript). Our main purpose on this paper is to bring new observations that clearly show relationship between surface deformation and oil extraction, the revised version of the manuscript will insist more on this point. The link between oil extraction and seismicity is only discussed in the paper as one hypothesis that remains to be tested but is out of the scope of this study.

That being said, we note that Reviewer 2 says "the analysis and modelling of the InSAR LOS deformation the Patos-Marinza oil field provides good quantitative evidence that extraction of hydrocarbons is leading to deformation", i.e. that the core of our paper is convincing and not subject to caution. We would like to stress the fact that our aim is to present original evidences of the subsidence of the whole Myzeqeja plain, and of a local high-rate subsidence that is spatially correlated to the center of the Patos-Marinza oil field. In the submitted version of the manuscript, we are not claiming that the observed seismicity in the area until 2004 has been induced but that such localized and strong subsidence rate as the one seen by InSAR may be associated to significant pressure changes at depth. Since this point has been questioned by both reviewers, we reckon we should rephrase parts of the paper and add new supplementary figures.

"It is not clear how the radii for computing moment release was chosen or how this chosen region is related to the deformation model derived. Even some simple analysis of what the background tectonic rate of stress/strain accumulation is, and how this might be manifest in seismic moment release. Is this sort of seismicity clustering occurring elsewhere in Albania ?"

To answer this question, we kindly refer to figures 5 and 6 of our previous answer to reviewer 1. We propose to add a figure showing the evolution of the moment release

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since 1939 when the production started. We also modify current figure 6: it is now broadened in time (1990-2019) and the cumulative moment curve is now calculated over 15 km radial distance from the center of the oil field. Over the course of the past century, no obvious correlation can be found between seismically released moment and the oil production when looking both at seismicity over a 50 km radius representative of the whole Myzeqeja alluvial plain, and over a 15 km radius more representative of the oil field potential influence (Figure 5). However, looking at the seismic moment release since 1990 in this restricted 15 km radius area shows an apparent increase in the moment rate starting in 2009 (Figure 6). Although we recognize that the statistical significance of this slight change is uncertain given the moderate seismicity background and the poor catalogue, and in any case impossible (given our current state of knowledge) to relate to the resurgence of oil extraction activity around 2005, we suggest that this relative increase in seismicity may explain the local population concern.

"As it currently stands in my opinion this paper is far too speculative in section 6 "Conclusions & implication for local seismic hazard" there are statements made about stress changes that have no basis, because the stress change calculations were not presented in the paper? There is simply not enough direct evidence presented in the paper to support the conclusions drawn, all be they vague."

We did not conduct stress calculations because we have very few information on the reservoir structure, initial pressure, wells locations etc. The first order modeling we do propose in section 5 shows that we can explain the big picture of the observed LOS motion with standard simplified compaction elastic or poro-elastic models, but we are well aware of the fact that much more refined models should be conducted to model pressure changes in the reservoir. This is what we state at the end of section 5 "Finally, to properly assess the net volumetric change of the reservoirs, one would need to build a 3D finite elements model of the media taking into account its physical properties, geometry, pressure history, etc., i.e. data we do not have access to at the moment."

Finally, Reviewer 2's feeling that our conclusions are not supported by enough ev-

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idences may stem from the fact that most of the focus was deflected in the online discussion of our paper toward the debate of whether seismicity was induced by oil extraction or not, whereas surprisingly little attention was given to the main contribution of our work. We recall that our paper is focused on new observations of intense and undocumented subsidence over the largest Albanian oil and gas field. Our work opens new issues about the nature of the seismicity observed in the area that we think should not be eluded, even if we do agree caution is required.

We start section 6 saying that "Unfortunately, local seismological arrays are still too sparse to provide accurate locations and focal mechanisms of the small magnitude earthquakes occurring in the vicinity of the Patos-Marinza oil field, a necessary prerequisite to conclude on the induced nature of the recent increase in local seismicity", that seems to be in agreement with the reviewer's point of view.

We conclude section 6 saying that "Whether the stress changes associated with the oil extraction in the Patos-Marinza field may be sufficient to trigger earthquakes on underlying faults is therefore an open question that challenges the seismic-hazard assessment in the area. Denser and more precise seismic catalogues, together with longer InSAR or other geodetic time-series to monitor the spatio-temporal evolution of the deformation, as well as detailed knowledge of the wells injection history and reservoir properties, will be essential to explore this issue further on." that is also a claim for caution. In order to answer both reviewer's concern, we will rephrase section 6 in order to insist more on our robust and original observations of land motion and tone down some discussion about potentially induced stress changes.

As a conclusive remark, we would like to insist on the fact that the seismic catalogue is simply not complete enough to support or reject the induced seismicity hypothesis. This situation will remain the same for a long time, if not forever. Our paper provides compelling observations of the impact of oil extraction on the vertical displacement of the surface, suggesting that underground effects (in terms of pressure changes) are substantial. Instead of restraining our analysis to the geodetic data processing

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and modeling, we enriched the paper by putting together a thorough review of recent changes in seismicity and oil extraction rates in Albania. We believe that this effort should be recognized as an attempt to elevate the level of the debate and point toward a research direction of obvious interest for seismic hazard assessment in the area. For this reason, we made our best effort to provide as much information as possible on our current state of understanding of the situation, including the main observations and the main questions pending on the subject. We claim that this effort will contribute to increasing the interest of readers on our paper and on the Solid Earth journal. Following both reviewer's suggestions, we think that the careful rephrasing of our discussion and conclusion sections will ensure that nothing is scientifically wrong in our assertions, while being sufficiently supported by data and prior knowledge of processes of induced seismicity to avoid excessive vagueness.

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