Peer Review File

Non-linear elasticity, earthquake triggering and seasonal hydrological forcing along the Irpinia fault, Southern Italy

Corresponding Author: Dr Stefania Tarantino

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Review for “Non-linear elasticity, earthquake triggering and seasonal hydrological forcing along the Irpinia fault, Southern Italy”

This manuscript describes the relationship between spring discharge, ground deformation, seismicity, and elastic nonlinearity in Southern Italy. I find the observations, modeling, and interpretations in this manuscript to be well prepared and presented and the evidence for the main conclusions to be compelling. There is sufficient detail in the methods that I think I could reproduce the work.

I indicated a few minor things in the annotated manuscript for correction.

There are two points that I think could be discussed further.

1. The authors describe this as a nearly quasi-static system since the cycle is annual and much longer than previous laboratory and earth studies. Looking at Figure 3e, it appears that when the earth is compressing, the relationship between strain and dv/v doesn’t have any slow relaxation processes (at least slower than the strain rate). That is, it has a nearly constant slope. However, during extension, it appears that the relaxation rate lags the strain rate, then “catches up” near peak extension. The last 5 data points during extension all have approximately the same strain but increasing softness (decreasing dv/v), indicating a slow relaxation process. Are the points evenly distributed in time? I'm assuming they are. It would be interesting to see the full time series rather than the average cycle. Are the cycles different depending upon the strain magnitude? In the laboratory and in the earth, with shorter duration cycles, it is typically opposite, where stiffening relaxation is slower than softening relaxation. With increasing strain magnitude laboratory materials get softer (cycle mean). It almost looks like with increasing strain magnitude, the earth would get stiffer (cycle mean) in this case, but I can’t verify that without seeing the full time series. If this is the case, maybe it is related to something like diffusion of fluids? The earth may not become fully relaxed during this cycle.

2. In your last few sentences of the discussion, you make a distinction between earthquake triggering via Coulomb failure and earthquake triggering via nonlinear weakening behavior. This would seem to indicate that the nonlinear weakening rate is faster than the stressing rate (due primarily to pore pressure). If the stressing rate was faster, then earthquakes would trigger by Coulomb failure. This interpretation sounds plausible. I think separating the two effects would be difficult. If there was a way to estimate the sensitivity of seismicity to (tectonic) strain rate outside of the karst regions then you could subtract the rate expected due to the hydraulically induced strain in the karst regions, and the rest (if any) would be due to nonlinear weakening. I don’t know if such an analysis is possible, it’s just an idea.

Overall, I feel like this is an exciting observation and analysis and will inspire future studies in nonlinear elasticity in the earth.

-Andrew Delorey

Reviewer #2

(Remarks to the Author)
This is a study using geophysical field observations (ambient seismic noise) to get a measurement of seismic velocity changes, which are then converted to strains. The strains correlate with hydrologic forcing and with seismicity rates, indicating a poroelastic relationship between the formation and seismic activity which is nonlinear. I found this to be an interesting study, however this is a bit outside of my expertise and I am not familiar with many aspects of the methods and calculations. Most of my comments have to do with clarity, writing precision and with some of the interpretations.

I found that there was maybe a bit too much focus on the technique, which is not new (e.g. Hillers et al., 2015 JGR) and that more emphasis could have/should have been placed on the novel aspects of the study, which I believe is the direct connection to seismicity rates (although I admit that I do not know the literature extensively). I also do not think that these should be called "experiments", since nothing is controlled by the "user" as a boundary condition. Rather this is a set of geophysical observations that has been interpreted.

I was confused or not entirely convinced by the interpretations in the discussion section. The main observation is that there are seasonal changes in dilatancy/compaction that correlate with seismicity, so why is a weakening/healing mechanism invoked? Is this meant to be a link between the nonlinear poroelastic effect in the rock volume, and plastic deformation (fault slip)? If dilatancy at the grain scale is the interpretation, can't this be induced by the poroelastic effect alone? I feel like this could be better explained. The poroelastic effect on the IFN itself should be felt as a change in the shear stress via changing effective stress conditions, frictional healing is highly dependent on the fault material and fault conditions and is not a straightforward link.

I did not entirely understand why aseismic slip was necessary to trigger the largest event. This is usually indicated by either direct measurements of the aseismic slip, or a migration in small-magnitude seismicity in a specific direction. Were either of these observed?

I think some critical information is missing (apologies if I simply missed it). What are the moment magnitudes of the seismicity? Are these mostly small events? The largest magnitude is given, but not the range of magnitudes. What is the slip sense of the fault zone itself? I assume because of the region that it is a normal fault – what is the dip and dip direction? The discussion mentions particles and gouge, is the fault core indeed composed of incohesive gouge rather than cataclasites?

The text on lines 155-156 (and possibly 191-192) suggests a component of anelastic behavior. I would like to see some discussion of anelastic behavior and whether the authors can distinguish between non-linear elasticity and anelastic behavior, with some potential implications on their interpretations.

Line 23-24: please explain exactly how the parameters are expected to affect the earthquake cycle and nucleation physics
Line 51-52: please include an equation here, it is very difficult to imagine this relationship without actually seeing it
Line 58: need references for these lab studies
Line 102-104: is this the output of some modeling that the authors did themselves? If so, this should be mentioned.
Line 230: I would say modulus reduction, not softening
Line 238-239: this assumption of critical state seems important. It is indicated by the sensitivity, i.e. the shear stress must be near the strength limit for small changes to have an effect, but this should be mentioned earlier and more prominently.

Reviewer #3

(Remarks to the Author)
The manuscript "Non-linear elasticity, earthquake triggerring and seasonal hydrological forcing along the Irpinia fault Southern Italy" documents seismic velocity changes (dv/v) near the Irpinia Fault system in Italy to evaluate its underlying mechanism of the temporal behavior by comparing hydrological (spring discharge), geodetic (horizontal GNSS data), and seismicity. Overall, I strongly think that the manuscript is written very well. A temporal correlation among observational data is convincing that the velocity change is controlled by hydrological strain changes. I do not have strong objections to what the authors describe in this manuscript. I think, however, that a more detailed method section would be crucial for transparency and reproducibility (although I do not have any doubts of authors' results here) as I described below.

1. CWI part: The authors use auto-correlations (ZZ, NN, and EE) for their analysis. Although I do not have any problems on this, my personal impression is that it may be worthwhile exploring correlations between different components i.e., ZN, ZE and NE where we can use a frequency normalization(whitening) to suppress any transient noise in correlation data. I was wondering if the authors considered this. Related to this frequency normalization, in general, it would be involved using temporal normalization. I did not see any approaches applied in this manuscript. Was there no temporal normalization? Also, I do not mean to ask for much additional work but why was 0.5-1.0 Hz selected? If one would like to explore the depth dependency of dv/v, at least it would be worth trying computing dv/v in several (e.g., another 2 or 3) frequency ranges? If no systematic dv/v is observed, then one can say no hydrological-induced dv/v on that depth? Finally I can see that there is always some ambiguity in the depth estimate of dv/v but how did the authors use Rayleigh and Love wave sensitives to determine the depth of dv/v? In the manuscript, they used sensitivity kernels, and I did not follow how to use two kernels together. Also, I think you will have one sensitivity kernel at each frequency band. Why do we have only one line at each wave? At least you need two lines: one from 0.5 Hz and the other from 1.0 Hz? Or the line comes from 0.75Hz (the middle of

the frequency band)? Additionally, to compute surface wave sensitivity, you need a S-wave velocity model? I only see a 1-D P-wave model.

2. Deformation modeling: I think the authors follow the previous work to use. i.e., assuming a 3.5 km-thick layer of elementary cuboid source. I feel that I did not follow this part well. First, why do you need modeling? I see in the end the authors need horizontal dilatation, which I sort of understand but why is a 3.5 km-thick layer of source required to evaluate dilatation? Would it be possible to get such from GNSS data directory? I guess my point is that when we need to model, our goal is to explore whether we can explain observation (GNSS data) by modeled data (synthetic GNSS data). But I think that this manuscript does not need any modeling? I feel that I missed something here. It would be great to explain why the modeling is required. Another question I have is modeled(?) Exx is at the surface? Or the depth corresponding to where dv/v change occurred?

3. Seismicity part. The authors did a declustering first, which is understandable. I think this part is very sensitive to parameters involved. The current manuscript states “its standard parameters values.”, but I think this is not sufficient enough for others to reproduce the same declustered catalog. I think more information would be required to show how this part was done.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I have no further comments. I support acceptance.

Reviewer #2

(Remarks to the Author)
In general, I think the authors have done a largely good job addressing the reviewer comments and making the manuscript clearer. There are some minor grammatical errors but these can be caught with thorough copyediting. I only have two remaining comments:

1. I think one of my comments was misunderstood; I did not mean INelasticity, but rather I was referring to ANelasticity, which is elastic behavior but with a time component such that in increase in stress (or strain) does not instantaneously change the strain (or stress). So I am wondering if the authors can comment on if they believe that these observations have a component of anelastic behavior and how this might be useful for interpretations and/or future predictions.

2. I am still not sure that the observations here can be interpreted as a weakening/healing process. Frictional healing, as discussed with reference to the Scuderi paper, is a plastic process (mainly, or at least partially), that is then disrupted or destroyed by fault sliding, which is also a plastic process. So the weakening/healing cycle is really two competing plastic processes, which then can’t be used to explain these observations if the authors want to consider them as an elastic response. I think it is perfectly acceptable to interpret the observations as a dilational/contractional cycle alone, which is hydromechanically elastic, without referring to frictional healing. Frictional healing is of course probably happening on the fault surfaces but I think it is separate from the effect the authors are focusing on.

Reviewer #3

(Remarks to the Author)
The authors address all concerns/questions raised by the reviewers and I am satisfied with the revision.

Version 2:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
The authors have addressed all my comments and I am happy to recommend publication of this article

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

Review for “Non-linear elasticity, earthquake triggering and seasonal hydrological forcing along the Irpinia fault, Southern Italy”

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1. The authors describe this as a nearly quasi-static system since the cycle is annual and much longer than previous laboratory and earth studies. Looking at Figure 3e, it appears that when the earth is compressing, the relationship between strain and dv/v doesn’t have any slow relaxation processes (at least slower than the strain rate). That is, it has a nearly constant slope. However, during extension, it appears that the relaxation rate lags the strain rate, then “catches up” near peak extension. The last 5 data points during extension all have approximately the same strain but increasing softness (decreasing dv/v), indicating a slow relaxation process. Are the points evenly distributed in time? I'm assuming they are. It would be interesting to see the full time series rather than the average cycle. Are the cycles different depending upon the strain magnitude? In the laboratory and in the earth, with shorter duration cycles, it is typically opposite, where stiffening relaxation is slower than softening relaxation. With increasing strain magnitude laboratory materials get softer (cycle mean). It almost looks like with increasing strain magnitude, the earth would get stiffer (cycle mean) in this case, but I can’t verify that without seeing the full time series. If this is the case, maybe it is related to something like diffusion of fluids? The earth may not become fully relaxed during this cycle.

>> Stimulated by the constructive comments and suggestions of Reviewer#1, we modified the section “Non-linear elasticity” to describe more carefully the dependency on strain of the velocity changes and the estimation of the non-linear parameters. Following the suggestion of Reviewer#1, we now show the full time serie of the velocity changes vs strain (Figure 3a) seeking for systematic patterns in the annual cycles (Figure 3a, 3b) beyond the (negative) velocity sensitivity on strain which appears to be the only reliable nonlinear parameter in our observations. In particular we included selected annual cycles (Figure 3c-h) to provide actual examples of the raw data and their variability. We modified lines 160-168 of the new version and we will refer to that also below.

2. In your last few sentences of the discussion, you make a distinction between earthquake triggering via Coulomb failure and earthquake triggering via nonlinear weakening behavior. This

would seem to indicate that the nonlinear weakening rate is faster than the stressing rate (due primarily to pore pressure). If the stressing rate was faster, then earthquakes would trigger by Coulomb failure. This interpretation sounds plausible. I think separating the two effects would be difficult. If there was a way to estimate the sensitivity of seismicity to (tectonic) strain rate outside of the karst regions then you could subtract the rate expected due to the hydraulically induced strain in the karst regions, and the rest (if any) would be due to nonlinear weakening. I don't know if such an analysis is possible, it's just an idea.

>> Thank you for the comment. It was not possible to investigate the sensitivity to strain rate outside the karst regions, but we further investigated the comparison with synthetic catalogues. We modified figure 4 adding two more examples (each one with 2 more subpanels) with synthetic catalogues resulting from the sum of a homogeneous Poisson process and a nonhomogeneous hydrological forcing, with respectively a linear or quadratic dependence. This allows us to quantify the sensitivity of the seismicity to the hydraulic forcing expressed in the strain or in the seismic velocity variations. We modified the main draft in lines 219-228 and the section of Methods in lines 336-353. We also found a typo mistake in the labels of previous fig 4, it is 10^-2, but we wrongly typed 10^-3. However, this is not changing the nature of our results and the discussion in our paper as we mainly focused on the behaviour of the curves and their trends with respect to the ones obtained randomising the catalogue with temporal permutations (that is the homogeneous Poisson process).

Overall, I feel like this is an exciting observation and analysis and will inspire future studies in nonlinear elasticity in the earth.

-Andrew Delorey

Reviewer #2 (Remarks to the Author):

This is a study using geophysical field observations (ambient seismic noise) to get a measurement of seismic velocity changes, which are then converted to strains. The strains correlate with hydrologic forcing and with seismicity rates, indicating a poroelastic relationship between the formation and seismic activity which is nonlinear. I found this to be an interesting study, however this is a bit outside of my expertise and I am not familiar with many aspects of the methods and calculations. Most of my comments have to do with clarity, writing precision and with some of the interpretations.

I found that there was maybe a bit too much focus on the technique, which is not new (e.g. Hillers et al., 2015 JGR) and that more emphasis could have/should have been placed on the novel aspects of the study, which I believe is the direct connection to seismicity rates (although I admit that I do not know the literature extensively).

>> Following the suggestions by the reviewer#3, actually we provided more details in lines 277-279 and 287. However, we agree with the reviewer#2, the novel aspects of the study are not methodological.

I also do not think that these should be called "experiments", since nothing is controlled by the "user" as a boundary condition. Rather this is a set of geophysical observations that has been interpreted.

>> We changed the text in 'analysis analogue to a pump-probe experiment'

I was confused or not entirely convinced by the interpretations in the discussion section. The main observation is that there are seasonal changes in dilatancy/compaction that correlate with seismicity, so why is a weakening/healing mechanism invoked? Is this meant to be a link between the nonlinear poroelastic effect in the rock volume, and plastic deformation (fault slip)? If dilatancy at the grain scale is the interpretation, can't this be induced by the poroelastic effect alone? I feel like this could be better explained. The poroelastic effect on the IFN itself should be felt as a change in the shear stress via changing effective stress conditions, frictional healing is highly dependent on the fault material and fault conditions and is not a straightforward link.

>> We invoked a weakening/healing mechanism since the process we observe seems to be reversible, with a restoration of the initial conditions (and of the elastic properties), similar to a fault relaxation process by which the crust retrieves its original characteristics prior to the damage episode. We added details in lines 236-240.

I did not entirely understand why aseismic slip was necessary to trigger the largest event. This is usually indicated by either direct measurements of the aseismic slip, or a migration in small-magnitude seismicity in a specific direction. Were either of these observed?

>> We clarified this point better in line 255-257. We suppose the reviewer may refer to 'No significant localization along the segments responsible for the 1980 Ms 6.9 earthquake is observed suggesting that a diffused triggering mechanism in the volume interested by hydrological forcing and resulting nonlinear reduction of elastic properties is more likely than an accelerated aseismic slip along major fault zones'. Here we wanted to point out that the recent background seismicity (since 2008) is not localised near the main faults responsible for the Irpinia earthquake (1980), but it is diffuse in the volume, probably because of a diffused triggering mechanism in the volume interested by hydrological forcing.

I think some critical information is missing (apologies if I simply missed it). What are the moment magnitudes of the seismicity? Are these mostly small events? The largest magnitude is given, but not the range of magnitudes.
>> We reported the magnitude range, which is $-0.4 \leq ML \leq 3.7$, in the seismicity section, but we added also in lines 83-85 in Results section.

What is the slip sense of the fault zone itself? I assume because of the region that it is a normal fault – what is the dip and dip direction?
>> The focal mechanisms of the three faults involved in the 1980 Irpinia earthquake are normal (Bernard & Zollo, 1989). The recent microseismicity occurring in the region is characterised by normal mechanisms (De Matteis et al., 2012), in agreement with the geodetic regional strain field (D'Agostino, 2014). We added this information in the lines 83-85.

The discussion mentions particles and gouge, is the fault core indeed composed of incohesive gouge rather than cataclasites?
>>Thank you for the question. The text was not clear, we therefore modified the sentence (lines 247-249).

The text on lines 155-156 (and possibly 191-192) suggests a component of anelastic behaviour. I would like to see some discussion of anelastic behaviour and whether the authors can distinguish between non-linear elasticity and anelastic behaviour, with some potential implications on their interpretations.

>> The reviewer may refer to the observation of hysteresis, which can be a marker of inelasticity (Holcomb, 1981; Agnew, 1981; Ostrovsky and Johnson, 2001; Hillers et al., 2015). The absence of systematic hysteretic patterns in our observations challenges the discussion of possible inelastic effects (new lines 160-168).

Line 23-24: please explain exactly how the parameters are expected to affect the earthquake cycle and nucleation physics
>> We added further details in the text in lines 24-31 which may help the reader to get the importance of these parameters in controlling the earthquake cycle.

Line 51-52: please include an equation here, it is very difficult to imagine this relationship without actually seeing it
>> Thank you, we inserted the formula [1] and further lines 50-55.

Line 58: need references for these lab studies

>>We inserted the references.

Line 102-104: is this the output of some modeling that the authors did themselves? If so, this should be mentioned.

>> We modelled as explained in the method section, we tried to empathise it adding “that we modeled”

Line 230: I would say modulus reduction, not softening

>> According to your suggestion we modified the text.

Line 238-239: this assumption of critical state seems important. It is indicated by the sensitivity, i.e. the shear stress must be near the strength limit for small changes to have an effect, but this should be mentioned earlier and more prominently.

>>Thank you, we introduced more thoughtfully in the lines 183-188 the evidence of critical state in the study area.

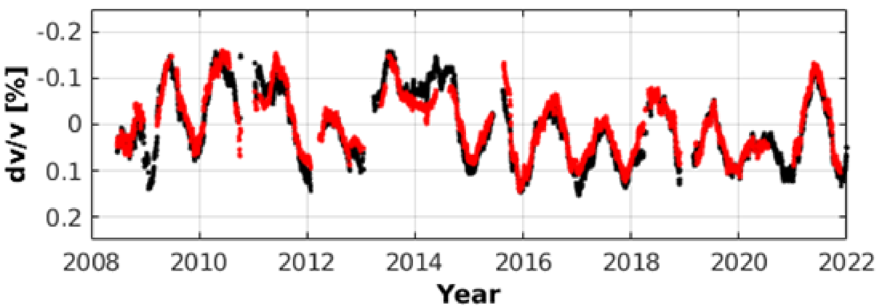
Reviewer #3 (Remarks to the Author):

The manuscript “Non-linear elasticity, earthquake triggerring and seasonal hydrological forcing along the Irpinia fault Southern Italy” documents seismic velocity changes (dv/v) near the Irpinia Fault system in Italy to evaluate its underlying mechanism of the temporal behavior by comparing hydrological (spring discharge), geodetic (horizontal GNSS data), and seismicity. Overall, I strongly think that the manuscript is written very well. A temporal correlation among observational data is convincing that the velocity change is controlled by hydrological strain changes. I do not have strong objections to what the authors describe in this manuscript. I think, however, that a more detailed method section would be crucial for transparency and reproducibility (although I do not have any doubts of authors’ results here) as I described below.

1. CWI part: The authors use auto-correlations (ZZ, NN, and EE) for their analysis. Although I do not have any problems on this, my personal impression is that it may be worthwhile exploring correlations between different components i.e., ZN, ZE and NE where we can use a frequency normalization(whitening) to suppress any transient noise in correlation data. I was wondering if the authors considered this. Related to this frequency normalization, in general, it would be involved using temporal normalization. I did not see any approaches applied in this manuscript. Was there no temporal normalization?

>> Thank you, we did not consider exploring the mixed component. We performed a test to compare pure autocorrelations (black dots) also with correlation of combined components (red dots) in the same frequency band and for the same coda time window (Fig. R1 of this document). The two time-series look very similar to each other. However, the time series of pure autocorrelations shows to be more continuous when looking at data above 85% of cross correlation coefficients, probably because it converges more rapidly, limiting the number of stacked days.

We added further details about our processing in lines 277-279 and 287, as in our analysis 1) we reject daily records with less than 20 hours and 2) we also applied one-bit normalisation. We select, also, velocity variations with a cross correlation coefficient above the reference value of 0.85.



Also, I do not mean to ask for much additional work but why was 0.5-1.0 Hz selected? If one would like to explore the depth dependency of dv/v, at least it would be worth trying computing dv/v in several (e.g., another 2 or 3) frequency ranges? If no systematic dv/v is observed, then one can say no hydrological-induced dv/v on that depth? Finally I can see that there is always some ambiguity in the depth estimate of dv/v but how did the authors use Rayleigh and Love wave sensitives to determine the depth of dv/v? In the manuscript, they used sensitivity kernels, and I did not follow

Figure R1. Comparison of the curve obtained with autocorrelation (black dots) and single station cross-correlation (mixed components and autocorrelations, red dots) for the same frequency band (0.5-1.0Hz) and the same coda time lapse window (10-50s), both stacked above 90 days.

how to use two kernels together.

>> We added more details in lines 295-300 to justify our choice, mainly dictated by the quality of the data. We additionally show results for a lower frequency band: 0.25-0.50 Hz (Fig. R2 of this document) obtained with the same processing and the same time window duration in later coda (20-60s). We retrieve increasing velocity during winter and decreasing during summer, in agreement with what we observe in the frequency band 0.5-1.0 Hz, but lower signal to noise ratio, resulting in a less continuous time series.
Regarding the sensitivity depth of the velocity variations, we added some text in lines 106-107. Our arguments are based on numerical simulations and theories (Obermann et al., 2013, 2014, Barajas et al., 2022), indicating that the sensitivity of coda waves is a combination of the sensitivities of body waves and surfaces waves. Since we have no insights into the relative contribution of body and surface waves, we illustrate the sensitivity kernels separately.

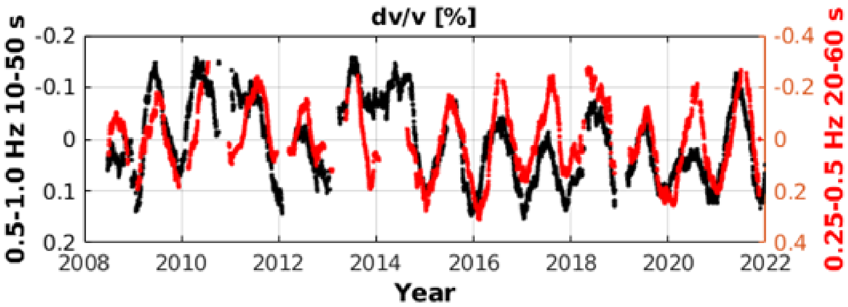


Figure R2. Comparison between 2 frequency bands, 0.25-0.5 Hz (left side y-axis, black dots) and 0.25-0.5 Hz (right side y-axis, red dots).

Also, I think you will have one sensitivity kernel at each frequency band. Why do we have only one line at each wave? At least you need two lines: one from 0.5 Hz and the other from 1.0 Hz? Or the line comes from 0.75Hz (the middle of the frequency band)?

>> We updated the figure S1 with 2 lines.

Additionally, to compute surface wave sensitivity, you need a S-wave velocity model? I only see a 1-D P-wave model.

>> Yes, we used the S-wave velocity model. We updated the figure S1 inserting also the S-wave velocity model (Matrullo et al., 2013).

2. Deformation modeling: I think the authors follow the previous work to use. i.e., assuming a 3.5 km-thick layer of elementary cuboid source. I feel that I did not follow this part well. First, why do you need modeling? I see in the end the authors need horizontal dilatation, which I sort of understand but why is a 3.5 km-thick layer of source required to evaluate dilatation? Would it be possible to get such from GNSS data directory? I guess my point is that when we need to model, our goal is to explore whether we can explain observation (GNSS data) by modeled data (synthetic GNSS data). But I think that this manuscript does not need any modeling? I feel that I missed something here. It would be great to explain why the modeling is required. Another question I have is modeled(?) Exx is at the surface? Or the depth corresponding to where dv/v change occurred?

>> Thank you for the question. Here we modelled the horizontal strain in the shallow crust (0–3.5 km), which is a high-permeability upper layer controlling the seasonal and multiannual modulation of seismicity along the Irpinia Fault Zone (D'Agostino et al., 2018). The use of this modelling approach to calculate the horizontal components of the "hydrological" strain rate field is required by the need to regularize the sparse density coverage of the GPS stations and incorporate the geometry of the karst aquifers. All components of strains (not only Exx) refer to the strain at the surface and it is obtained with homogeneous cuboids. We add further details in lines 310-315.

3. Seismicity part. The authors did a declustering first, which is understandable. I think this part is very sensitive to parameters involved. The current manuscript states "its standard parameters values.", but I think this is not sufficient enough for others to reproduce the same declustered catalog. I think more information would be required to show how this part was done.

>> We followed the approach by Gardner & Knopoff (1974). We added further details of this approach in lines 329-336. Below we show the cumulative number of events for the initial catalogue, the background seismicity and the clusterized events (Fig. R3 of this document).

We added in the supplementing material figure R4 of this document (new figure S2), which shows that seismicity rate is not changed significantly by varying the windows used to scan the catalogue. We modified accordingly the text in lines 336-338.

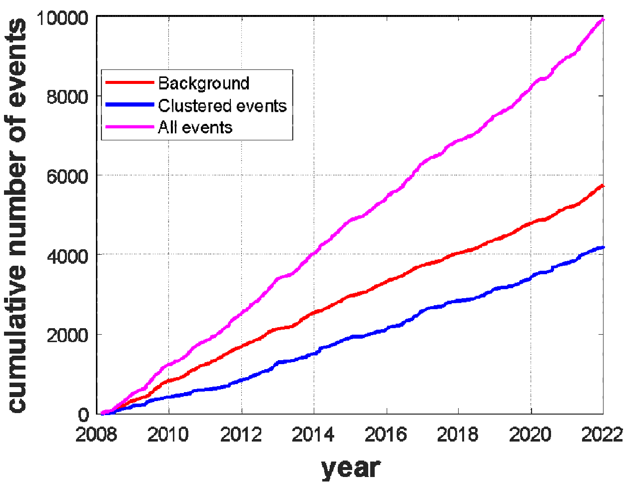


Figure R3. Declustering of the catalog: the different curves are described by the legend, the background seismicity is the seismicity we considered in our analysis.

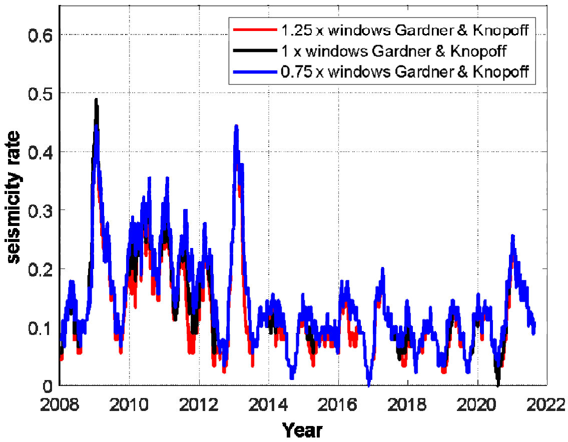


Figure R4. Seismicity rate for varying space-temporal windows from Gardner & Knopoff.

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

I have no further comments. I support acceptance.

Reviewer #2 (Remarks to the Author):

In general, I think the authors have done a largely good job addressing the reviewer comments and making the manuscript clearer. There are some minor grammatical errors but these can be caught with thorough copyediting. I only have two remaining comments:

1. I think one of my comments was misunderstood; I did not mean INelasticity, but rather I was referring to ANelasticity, which is elastic behavior but with a time component such that in increase in stress (or strain) does not instantaneously change the strain (or stress). So I am wondering if the authors can comment on if they believe that these observations have a component of anelastic behavior and how this might be useful for interpretations and/or future predictions.

This is certainly an important point. Our measurements shows that all observations (dv/v, strain, seismicity, hydrology) have simultaneous peaks which appear to rule out significant time-dependent response. In lines 161-171 we discuss the observed pattern of hysteresis concluding that we don't have yet convincing evidence for time-dependent processes.

2. I am still not sure that the observations here can be interpreted as a weakening/healing process. Frictional healing, as discussed with reference to the Scuderi paper, is a plastic process (mainly, or at least partially), that is then disrupted or destroyed by fault sliding, which is also a plastic process. So the weakening/healing cycle is really two competing plastic processes, which then can't be used to explain these observations if the authors want to consider them as an elastic response. **I think it is perfectly acceptable to interpret the observations as a dilational/contractional cycle alone, which is hydromechanically elastic, without referring to frictional healing**. Frictional healing is of course probably happening on the fault surfaces but I think it is separate from the effect the authors are focusing on.

We thank Reviewer #2 for actually raising this point. We agree that the process underlying modulus softening/hardening that we describe cannot be ascribed to frictional sliding (as in the Scuderi paper) but is a volumetric deformation induced by hydromechanical forcing that ultimately affects the frictional strength.
Although we never explicitly used the term "frictional healing", we agree that the manuscript will benefit for clarification on this issue.
For these reason we modified those sentences which could determine some misunderstanding (lines 68-69; 153-154, 250). We remove the first part of the sentence to line 241-242: **"In agreement with the precursory changes in seismic velocity discerned during the seismic cycle in the laboratory...".**

Reviewer #3 (Remarks to the Author):

The authors address all concerns/questions raised by the reviewers and I am satisfied with the revision.

3rd revision

REVIEWERS' COMMENTS

Reviewer #2 (Remarks to the Author):

The authors have addressed all my comments and I am happy to recommend publication of this article

Thank you. We appreciated your feedbacks and suggestions.