nature portfolio

Peer Review File

Accelerating upper crustal deformation and seismicity of Campi Flegrei caldera (Italy), during the 2000-2023 unrest

Corresponding Author: Dr Augusto Neri

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

Dear Dr Neri,

Your manuscript titled "Investigating deformation and seismic data and their relationships during the ongoing (2000-2023) unrest of Campi Flegrei caldera (Italy)" has now been seen by 2 reviewers, whose comments are appended below. In the light of their advice we regret to inform you that we cannot publish your manuscript in Communications Earth & Environment.

You will see that the reviewers raise substantive concerns. Taking these points together with our editorial considerations, we are unable to conclude that your manuscript represents a sufficiently novel and compelling advance over the body of related work in the literature. Unfortunately, these reservations are sufficiently important to preclude publication of this study in Communications Earth & Environment.

Specifically, both reviewers pointed out the overlap between a similar paper published last year. The adding of two-year of new data did not change the conclusion or make significant contribution to our understanding of this volcanic system.

Although we cannot offer to publish your manuscript, I suggest that you consider Scientific Reports as a suitable venue for this work. To transfer your manuscript, please use our manuscript transfer portal. You will not have to re-supply manuscript metadata and files, unless you wish to make modifications. For more information, please see our manuscript transfer FAQ page.

We are sorry that we cannot be more positive on this occasion and thank you for the opportunity to consider your work.

Best regards,

Teng Wang, PhD
Editorial Board Member
Communications Earth & Environment
0000-0003-3729-0139

Carolina Ortiz Guerrero, PhD
Associate Editor
Communications Earth & Environment

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

Review of 'Investigating deformation and seismic data and their relationship during the ongoing (2000-2023) unrest of Campi Flegrei caldera (Italy) by Bevilacqua et al, INGV-Pisa.

* What are the major claims of the paper?
The paper analyses seismic and geodetic data from the ongoing unrest episode at Campi Flegrei. The major claims are: 1) There is a parabolic increase in vertical acceleration and super-exponential increase in the number of earthquakes and

seismic energy release and 2) There is an exponential relationship between deformation and seismicity consistent with quasi-elastic behaviour.

* Are these claims novel, and will they be of interest to others in the community? If the conclusions are not original, it would be helpful if you could provide relevant references.

This is an important topic and is rightly receiving high profile attention, including a large number of recent papers covering similar themes. In particular, I identified these two papers:

• Kilburn, C. R., Carlino, S., Danesi, S., & Pino, N. A. (2023). Potential for rupture before eruption at Campi Flegrei caldera, Southern Italy. Communications Earth & Environment, 4(1), 190.
• Bevilacqua, A., De Martino, P., Giudicepietro, F., Ricciolino, P., Patra, A., Pitman, E. B., ... & Neri, A. (2022). Data analysis of the unsteadily accelerating GPS and seismic records at Campi Flegrei caldera from 2000 to 2020. Scientific reports, 12(1), 19175.
Bevilacqua et al, 2022 in Scientific Reports has a very similar author list and presents much of the same material, but with a slightly shorter timespan (2000-2023 vs 2000-2021). Kilburn et al, 2023 comes to the same conclusion as major claim 2.

Figures:
Figure 1 is identical to that of Bevilacqua et al, 2022.
Figures 2 and 3 in this manuscript are subplots of Figure 3 in Bevilacqua et al, 2022. Panels 2a, 3a, 3c, 3d are identical, and the others are very similar but with minor differences (e.g horizontal/vertical or declustered/raw).
Figure 4 and Figure 5 of this manuscript are subpanels of Figure 4 and 5 from Bevilacqua et al, 2022.
Figure 6 of this manuscript is new as far as I can see,
Figure 7 presents Fourier analysis of the GPS data are RITE and is very similar to Fig 8 of Bevilacqua et al, 2022, but covers slightly different time periods. It is alarming to note that the results are very different, even when the time periods overlap.
Figure 8 presents earthquake count versus vertical uplift, which has previously been shown in Fig 4 of Kilburn et al, 2023.
Figure 9 presents similar data for the 1982-84 crisis and similar patterns are seen in Fig 6 of Kilburn et al, 2023, which shows a longer time series.

The text of the manuscript also seems to cover similar topics as these previous papers and I struggled to find any novelty. The parameterisation of the trends as parabolic (uplift) and super-exponential (seismic count) is potentially interesting, but there is no attempt to link it to a physical model or interpret the significance.
* Is the work convincing, and if not, what further evidence would be required to strengthen the conclusions?

The description of the methods and results lacks technical detail and were not presented systematically (just subplots from earlier papers), making the results somewhat unconvincing.

* On a more subjective note, do you feel that the paper will influence thinking in the field in terms of either conceptual understanding or technological capability? Please feel free to raise any further questions and concerns about the paper.

No, any novelty in this manuscript is deeply hidden in the current version and a complete rewrite would be required to justify publishing this here. What do the extra 2 years of data tell us that we did not already know?

Reviewer #2 (Remarks to the Author):

The authors carry out a statistical analysis of ground deformation and seismic data and conclude that the crust is behaving quasi-elastically and progressively weakening. They also propose a scenario for future seismic activity, if current trends were to continue. Much of the analysis (the frequency analysis of the oscillations and the FFM) is similar to that of a previous paper with the same first author (Bevilacqua et al, 2022), albeit with 3 years more data. The other significant analysis (number of earthquakes vs uplift) is similar to that of Kilburn et al (2023) with an extra 1 year of data. This extra year of data shows that the relationship has not become linear, as Kilburn et al (2023) concluded, implying the crust is still behaving behaving quasi-elastically (as previously concluded from earlier data by Kilburn et al (2017)). As such, this study can be a useful, incremental, update to previous studies, if some restructuring is carried out and more discussion of the results is added, as suggested below.

Major comments:

The analysis shows that uplift (u) is proportional to t^2 and a near-exponential relationship between number of earthquakes (N) and u between 2010 and 2023. This would imply an exponential relationship between N and t^2 for the same period (rather than the more generic description of super-exponential). This could be tested by plotting t^2 on the x-axis instead of t in Fig 3b.

But a bigger issue is that the meaning of the results of the analysis of the independent signals is not discussed at all. The discussion essentially dives straight into another analysis, where the signals are plotted directly against each other. These are the results that are then discussed further.

I suggest much of the independent analysis is moved to supplementary and the joint analysis is moved into the main methods/results, where it better belongs. There then needs to be a proper discussion of the implications of all the results, including the parabolic uplift and the dominant frequencies in both data sets.

In the studies of Kilburn et al, the uplift is corrected for the very long-term subsidence. This correction is not made for this study, which might be fine, but it should be mentioned that it is sometimes done, with a discussion on why it is not applied in this study.

The stated time lag of 30-45 days does not seem justified. For instance, the lag looks close to zero for Aug/Oct 2023 in Fig 5. Can you explain how the time range was reached?

Minor comments:

Fig 2: Y label axis incorrect

Fig 4: Use shaded vertical bars to indicate the temporal extent of the uplift episodes

While the meaning is generally clear there are errors in the english language throughout that need correcting.

References:

Bevilacqua, A. et al. Data analysis of the unsteadily accelerating GPS and seismic records at Campi Flegrei caldera from 2000 to 2020, Sci. Rep. 12:19175 (2022).

Kilburn, C.R.J., De Natale, G. & Carlino, S. Progressive approach to eruption at Campi Flegrei caldera in southern Italy. Nat. Commun. 8, 15312 (2017).

Kilburn, C.R.J., Carlino, S., Danesi, S. & Pino, N.A. (2023). Potential for rupture before eruption at Campi Flegrei caldera, Southern Italy. Nat. Commun. 4:190 (2023).

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Neri,

Your appeal for the manuscript titled "Investigating deformation and seismic data and their relationships during the ongoing unrest of Campi Flegrei caldera (Italy)" has now been seen by one of the original reviewers (Rev#2) and a new reviewer #3, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file. You may stated it in rebuttal letter, but in the manuscript, please compellingly justify your claims of novelty in your observations of Campi Flegrei, given previous observations of the quasi-elastic nature of the crust as listed by Reviewer #2.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred.

Important: The response to reviewers must not include any figures, tables or graphs. If you wish to respond to the reviewer reports with additional data in one of these formats, please add them to the main article or Supplementary Information, and refer to them in the rebuttal. Due to current technical limitations, any figures, tables, or graphs embedded in your rebuttal will not be included in the peer review file, if published.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Teng Wang, PhD
Editorial Board Member
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Carolina Ortiz Guerrero, PhD
Associate Editor
Communications Earth & Environment

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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

The structure is much improved and it is clearer now what the novelty is, but the study still comes across as an incremental update to previous studies. The conclusion of the quasi-elastic nature of the crust has been reached before (e.g. Kilburn et al. 2017). I checked again and the Kilburn et al. (2023) paper presents data until April 2022 (it's true that Fig 4 refers to June 2021, but other figures go into 2022, and April 2022 is stated in the text), which represents 1.5 years of new data. And the fact that the majority of seismicity has occurred in the last 3 years is a function of the increasing rate in time and I suspect the same could have been said for the previous 3 years.

I note that the FFM analysis conclusions are very couched in the paper, but that the authors state in the rebuttal that it is being used to correctly forecast the trends, and I would suggest adding more details on this interesting aspect to the paper.

I would also still like to see the idea of an exponential relationship between N and t^2 (which is implied) being both explicitly stated and tested. Why use more generic term super-exponential if there is a more specific empirical relationship? If the relationship between N and t^2 is not exponential, it should be explained why not, given the empirical conclusions for the relationships between N and t, and between N and u.

Reviewer #3 (Remarks to the Author):

* What are the major claims of the paper?
The paper studies temporal evolution of seismicity and deformation during the 2005-present uplift episode at Campi Flegrei. Deformation is found to increase following a parabolic trend, while seismicity (quantified mostly as earthquake counts) follows a super-exponential trend - i.e. exponential growth with the exponent increasing with time. Furthermore, seismicity as a function of deformation is observed to follow a exponential trend, which contrasts a previous study. These empirical observations are used to forecast time of failure (eruption) and probabilities of larger earthquakes than observed previously. Periods of oscillations and time-lag between oscillations in deformation and seismic data are also reported, although not used for further interpretation.

* Are these claims novel, and will they be of interest to others in the community? If the conclusions are not original, it would be helpful if you could provide relevant references.

As the inflation at CF is evolving it is important to scrutinize deformation and seismicity results as done in this paper. Therefore the the study can claim novelty; the finding that seismicity as a function of deformation follows an exponential trend is also novel, in comparison with a recent study (ref. 36 in paper) and thus important to be published. The main approaches applied in the paper for data analysis and interpretation are pretty standard. The particular forecasts of failure times and occurrence propabilities of larger earthquakes presented in the paper are useful for the scientific community, as well as the monitoring and civil protection in the area of CF. In a more general context, the study makes a contribution towards understanding better the relations of seismicity and deformation.

* Major comments:
While I think the data analysis and results/conclusions drawn in the paper are in relatively good standing, the paper can be significantly improved in:
(a) description of data and methods. I found the data and methods sections to be incomplete and confusing. It is for example lacking a more thorough explanation of what lies behind the seismic data set; e.g. are the earthquake counts manual or automatic; how are earthquake swarms affecting counts (e.g. saturation of count); what about station outages? The curve fitting method is also not described adequately.
(b) tabulating of results is missing: such as exponents from fit-functions, parabolic fit parameters, RMS of misfit. This is important for future reference and validation;
(c) quantitative arguments (misfit; f-test) for using a double exponential function instead of a single exponential functon(or

exponential function times a parabolic function). It should be easy for the authors to use misfit statistics to do a better job in convincing the reader that their proposed fit functions are preferred over others.

I also found the sentence structure often to be confusing, and I think the paper could improve much by edits from a native English speaker. This is a major issue because it is at times difficult to understand meaning of sentences, requiring long time to read and leaving some question marks regarding meaning of some sections.

The main claims and relations in the paper are drawn from single-station earthquake counts; either in number per time unit, or cumulative number. Earthquake count can be useful as an indicator of seismicity, but it is also tricky because sensitivity diminishes with distance from the seismic station, depth, weather, etc. Therefore, identical swarms at different distances would be represented differently. In case of intense swarms the earthquake counting will saturate. The counting method may also confuse small earthquakes with geothermal-related seismicity or anthropogenic activities. In general the strain release, deduced from network-wide locations is a more robust measure of seismicity. I think it would improve the physical meaning of the paper significantly to present strain release as function of deformation, e.g. as an additional panel in Fig. 8, and discuss accordingly.

* Minor comments:
Abstract:
" the caldera shows a slow but progressive inflation " Ambiguous wording. Some might think the deformation at CF is quite fast, although many faster deforming volcanoes exist. Suggest you just state here "shows an accelerating inflation".

" an accelerating parabolic increase " Delete "accelerating here", it is sufficient to say parabolic; the acceleration appears in the second part of the sentence

Introduction:
" mostly affected by a dominant sinking " Confusing wording; suggest simplify to "dominantly subsiding"

" Caldera deformation has been characterized by a well-defined bell-shaped pattern of vertical uplift " Here I suggest you add "and outward motion from center of uplift", otherwise the reader may think there is only uplift

" centered in the town of Pozzuoli, " add a reference to Fig S1

" seismicity is mostly located in correspondence of two main depth levels " Confusing wording; suggest "...located at two main..."

" The datasets used .... provided by the study." This paragraph is completely unneccessary - suggest deleting

Datasets:
"consistently recorded the most significant uplift". Confusing wording - significant can have several meanings. Suggest "recorded the fastest uplift" or "highest uplift rates"

" about 500 m into the sea, southwest of the Rione Terra, " Here it is most important how far away from RITE the uplift center is, not how far into the sea. Even better if you can provide uncertainties for that estimate; should be noted in the references.

" corresponding superficial pressurized source at a depth of about 3-4 km " some may find this confusing as 3-4 km may be considered shallow by some, but deep by others. Suggest just deleting "superficial"

" of final daily position time series with the associated uncertainty (estimated to be 1 cm on the vertical component and 0.3 cm in the two horizontal components16). " This description is not satisfying: (a) a reference from 2021 cannot include results up to 11/2023; (b) it is an oversimplification to say that the uncertainties are estimated 1 and 0.3 cm because ref. 16 states that the mean uncertainty is of these values. Please be specific in which uncertainty values you used, and if uncertainties are not used in the time series analysis, then state so explicitly.

Fig 1: "data merge leveling data, collected at benchmark 25A, and the RITE GNSS station." Refer the reader to Fig S1

Fig 1: "Overview of the earthquakes recorded at CFc in" This description is not detailed enough. Be specific; Number of earthquakes per month, as recorded at ....
" The permanent seismic network managed by INGV-OV presently comprises 25 permanent digital stations and 3 permanent analog seismic stations15" Delete the two latter occurrences of permanent

" The catalog we used consists of the earthquakes recorded, between 2000 and 12/2021, at the STH reference station situated near the Solfatara-Pisciarelli area (Fig. 1-SI), whereas, in the period 1/2022 and 11/2023, it relies on the database SERENADE. In total the catalog includes 16,250 earthquakes. " Since accessible references for how this is done are not given, the authors must describe in more detail how the earthquake counts are made. E.g., are they manual or automatic? A reference for SERENADE is missing; it is also missing to state whether the different approaches are compatible or not - with some references or extra material in supp.mat. please.

" Within this dataset, we can assume the completeness magnitude (Mc) equal to about 0.219. " Some information is missing before that magnitude is estimated for each event. How is the magnitude estimated for a single-station detection? This is

important for the basis of the strain-release calculations.

" in the 2000-2023 considered period." Example of overloaded wording; simplify to "during 2000-2023".

" month over the last about four decades" Overloaded wording; simplify to "month since 1983"

" seismic activity affected a radius of " Confusing wording - what does affected mean, is it where earthquakes were felt or waveforms recorded? --> suggest "occurred in a radius of ..." or "were located"

Methods:
" The deformation and seismic datasets have been closely analyzed by investigating their temporal evolution (Fig. 2 and 3) " Confusing wording: one can understand this text as if the data have already been analyzed by another study. Rephrase "We analyze ...."

" with specific reference to the displacement rate " What does this mean? Is there some reference rate used? Clarify.

" see [39] for more details and a comparison with trends computed over the period 2000-2020)" Improve two things here: (a) if you are following the methods of [39] then just state so implicitly. (b) The latter part of the sentence, for the comparison, is not a methods section material, but results

" at these stations the vertical time series are scaled down according to the observed bell-shaped deformation pattern " Confusing wording: can be understood as if you literally scale down the observed data. Rephrase.

" It is worth mentioning here that just one GNSS station, located in the area of Mt. Olibano-Academia (named ACAE), has shown, since about 2021, an uplift deficit in comparison to other areas at the same distance from the maximum uplift of the caldera. The analysis and interpretation of this anomaly is the object of another study47 and will not be further considered in the following." Suggest deleting this, at least from here. If you want to mention this observation, then do so in the discussion section, and include ACAE on a map/refer reader to Fig S1.

" for the background subsidence of the caldera36" Give some numbers for this background rate so that the reader can be convinced that it is irrelevant. I also find the arguments for a background subsidence to be limited: it does not show up on the EGMS InSAR data server (https://egms.land.copernicus.eu); If you want to keep this notion then it may be good to mention the physical cause (transport of material from depth?).

" mostly refer to all earthquakes recorded. " - at station STH?

" We base the estimate of the annual rate on left-side first-order finite differences so that the value at time t is not anticipating future information39. " This is a good description; I understand it as you just difference the first and last value of a sliding window. Please be specific about the timestamp for each calculation, as that will very much affect your time lag calculations; i.e. would it make more sense to use the central time of the sliding window rather than its right side? I miss seeing something more about the time steps: I suggest describing your method as a "sliding window" of some width that is moved by (I hope) daily increments.

Your methods section is missing to explain how the curve fitting is done. There is some mention caption for Fig 2, but that does belong in the methods section.

In the methods section, explain/define explicitly how you calculate the Benioff strain.

Results:
"Fig. 2 refers to the vertical" Confusing wording: why "refers to"? Implies that something different is shown. Suggest "shows"

" that the behavior since " ambiguous; refer to explicitly which behavior you mean

" Such a parabolic regression fits very well also the linear and log-scale plots as shown in Fig. 2a,b," This sentence makes no sense because you are plotting the same data in all parts of Fig 2! Of course it will well on all plots. Delete.

"R-squared = 99%, in" I suggest you consider better the statistical measure you present for your curve fitting. Include also an RMS value; and a table of all these values and parameters. Even better if you can provide uncertainty estimates for your curve parameter estimates.

"Very similar features of the deformation trend are also observed in the vertical and horizontal displacements at the other stations, as shown in the SI material (Fig. 2-SI and 3-SI), making the above considerations valid for all the points investigated" This text implies that the same kind of analysis was done on the other data, however, no such information is found in the supplemental material. It is preferable to use quantitative estimates --> also estimate curve fitting parameters for the stations shown in Fig SI-2-3.

Fig 2: " plots the blue points represent the daily average measure " This sentence describes a gross oversimplification of GNSS data processing. Sugguest "represent the computed daily position (16)"

"(Fig. 3a also shows labels marking the size of the 7 greater swarms recorded)." Avoid () as possible. This sentence stands perfectly fine without the parenthesis.

"shows a super-exponential behavior of the" Explain what is meant by "super-exponential", as not all may be familiar with this term

" in this case corresponding to a super-exponential trend also in absolute values as shown in Fig. 3a" I don't understand what you mean here. The same data will have the same trends.

" of the most significant events " unclear. Largest magnitude events?

Fig 3 caption " analyzed in the paper " It is utterly no need to state that these data are analyzed in the paper. Delete.

" display the annual rate of deformation and seismic " this is a bit confusing, since one may interpret this as the sliding window length is 1 year (6 mo in Fig 4). Suggest rather "in units of mm/yr" or "display the rate of defor...."

" computed by adopting a time step of 6 months to better " No this is not the time step, but the length of the sliding window. Isn't the time step still 1 day?

" The rate value computed " Simplify to the rate computed

"Aug-Oct 2023 shows the existence of a time lag of up to 45 days between the peaks of the two time rates." Here it is quite important to remind the reader of the time stamp for each of the seismic and deformation data; to me the time stamp seems to be made in the same way, good if you can highlight that.

" with a time step of 6 months, and Fig. 7c highlights a main period of about 2.9 years computed from 2011 to 2023 with a time step of 2 years. It is also interesting to note that the durations of these main periods are about 10-15% shorter that those estimated by [39] at the same RITE station over the period 2000-2020. A similar effect has been observed at all other GNSS stations and also in the seismic rate record." Interesting; suggest you mention this briefly and explanations in the discussion.

" Figure 7. Examples of Fourier " Why "examples"? Confusing. Where can the reader then find more complete Fourier analysis? Rephrase.

" curve. No clear trend was instead observed on the period 2000-2010, part of which has been characterized by subsidence and very weak seismic activity." I think it is important to mention stress reversal: if a medium has already been stressed in one direction, and the deformation reverses, then the first part of the processes is unstressing, where little or no seismicity is expected to happen. This happens both following inflation and deflation and is very clearly shown in your data. Consider relating to the "Kaiser" effect here (e.g. Heimisson, E. R., Einarsson, P., Sigmundsson, F., & Brandsdóttir, B. (2015). Kilometer-scale Kaiser effect identified in Krafla volcano, Iceland. Geophysical Research Letters, 42(19), 7958-7965.)

"exponential function already represents a good approximation of the observed relationship, i.e., R-squared > 99%. However, given the more accelerating behavior of the data with respect to a single exponential function, we have adopted here a fit with two exponential functions and optimized the “connecting time” between them". So, if you already have a R-sq>99%, how can you justify a fit including more parameters? Seems like you are just fitting noise... Suggest using misfit statistics here for argumentation. In your parameter tabulation, also include the connecting time.

"relationship was obtained, with different fitting exponents, by us" How different? Be quantitative; use tables of fitted parameters.

Fig 8: " The light and dark blue curves indicate " I see no difference in the blue curve colors. Suggest larger data symbols, and plot the curve on top as a thicker line(s)

Discussion:
Consider adding subsections for clarity

"). The reasons for the quasi-parabolic increase of the uplift are beyond the objectives of this manuscript and the analyses here developed. However, the understanding of the cause of this accelerating process is an interesting future goal and questions on the possibility that the driver of the uplift is itself an accelerating cumulative process, such as, for instance the injection of gas or magma in the upper crust from a deeper source. " Conflicting statement; first you say you are not going to talk about something, but then you talk about it. Rephrase.

" effect with time clearly visible since 2021 and even more since the beginning of 2023. " refer to Fig 5 here

" Patterns of fracturing of brittle rocks in laboratory clearly show that at low stresses fracturing activity is null or very low and the behavior is close to elastic, i.e., there is a quasi-linear relation between stress and strain. At stresses of about half of the values at bulk failure the fracturing activity begins to increase in an exponential way until the strain has typically reached about 90-95% of the failure value. Under such conditions the stress increases in a non-linear way with respect to strain, producing a curve strain-stress that is downward convex. This condition of the material is also characterized by dilatancy, i.e., an increase of the volume due to a quasi-homogeneous formation of small cracks within the rocks. At larger increasing

stresses, the fracturing activity typically accelerates very quickly producing the coalescence of fractures in a major fault and eventually leading to bulk failure of the brittle rock. For specific properties of the rock a variety of strain-hardening or strain-softening type behaviors of the material can also be observed before failure. Eventually, when the amount of stress has reached its maximum value, most of the supplied energy is spent in fracture and fault movements and the behavior of the rock becomes inelastic, i.e., the deformation continues under a constant stress. This regime is typically characterized by a proportional increase of seismic events with deformation." Many references missing from this paragraph; please improve.

" The same relation was obtained by considering different thresholds of magnitude. This is also different from the results of [35,36] who estimated an exponential trend over the period 1982-84 by apparently using a lower number of data points." OK, but suggest discussing what would be the effect of undercounting of eq numbers, as suggested in Figure 1. The undercounting would happen while the seismicity rate is greatest, and likely result in a constant maximum rate.

" Such a different relationship between deformation and seismic activity appears as further evidence of the different forcing source and/or mechanism associated to the 1982-84 crisis with respect to the present one." Well, as the seismicity rate is also strain rate dependent, could it not just be different inflow rates? Is the deformation source at the same location and depth? Suggest you add some notes in your discussion about strain rate dependency of seismicity.

" few years due to the continuous, although oscillating, increase of the caldera uplift. " refer to Figure

" time computed since 12/2023) from 0.5 to about 2.5 years with 95%ile values up to about 8 years (see Figure 9-SI for more details). It " I suggest you include Fig 9-SI in the main text, since it is important for your conclusions.

" these estimates cannot be considered reliable forecasts of the occurrence of specific volcanic events given the large number of false alarms typically experienced in the application of FFM to real crises. " References missing for this statement.

" n of the crisis (up to few years as median values) in case of continuation of the observed trends." What was the forecast for the 1980s unrest? Would be curious for comparison.

Conclusions:
Conclusions can be shortened by 50%, keeping points 1-4.

" trend, similarly to the number of earthquakes". I dont understand what you mean

" vertical uplift results to be well-represented, since " Confusing wording; rephrase

Supplementari Information:
". Plot (c) shows the quality of the fit, represented by the mean square distance between data and fitting curves, with the varying of the connecting time between the two exponential functions." This is important, show for other curve fittings as well.

Fig 9:" analysis of waiting time from " Explain further; waiting time until what? This is the SI material; you have space to explain !

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Version 2:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Neri,

Your revised appeal manuscript titled "Investigating deformation and seismic data and their relationships during the ongoing unrest of Campi Flegrei caldera (Italy)" has now been seen by our original reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor
Communications Earth & Environment

Teng Wang, PhD
Editorial Board Member
Communications Earth & Environment
0000-0003-3729-0139

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

Again, significant changes have been made that improve the clarity of the paper. I still have one concern, however, about the way that the relationship between seismicity and time is treated.

While a figure testing the exponential relationship between seismicity and t^2 has been added (though only in SI), the figure is not referred to in the text, and mention of the relationship only comes up in the discussion. I find this baffling – surely it makes sense to discuss this in the Results Section when the relationship between seismicity and time is first discussed? Instead, the relationship is described there still as super-exponential. I suggest remedying this and changing Fig 3 to demonstrate the relationship. Panels b and d can be replaced by their t^2 equivalents and the fit based on a model can be added to a and c (I suggest t is retained on the x-axis for these).

Reviewer #3 (Remarks to the Author):

After reading the revised article and replies to previous comments I have nothing more to add.

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Reviewers' comments (in red authors' responses):

Reviewer #1 (Remarks to the Author):

Review of 'Investigating deformation and seismic data and their relationship during the ongoing (2000-2023) unrest of Campi Flegrei caldera (Italy) by Bevilacqua et al, INGV-Pisa.

* What are the major claims of the paper?
The paper analyses seismic and geodetic data from the ongoing unrest episode at Campi Flegrei. The major claims are: 1) There is a parabolic increase in vertical acceleration and super-exponential increase in the number of earthquakes and seismic energy release and 2) There is an exponential relationship between deformation and seismicity consistent with quasi-elastic behaviour.

We believe that the manuscript contains several more innovative results, as described in several sections of the text. In addition to identify the novel parabolic increase of the uplift and the associate super-exponential increase of the number of earthquakes, the results include a new comparison with the 1982-84 unrest crisis of the Campi Flegrei caldera and, above all, an innovative predictive analysis of the seismic activity which is completely original. The predictive analysis is being proved to correctly forecast the trends observed in the last very few months and just in these months is receiving much attention from the Italian scientific authorities involved in the management of the crisis.

Last but not least, the manuscript includes about 3 more years of data with respect to recent papers dealing with the same subject (in particular about 3 more years with respect to Bevilacqua et al. 2022 and about 2.5 years with respect to Kilburn et al 2023, see their Fig. 4). This is a major added value considering the escalating nature of the Campi Flegrei crisis and the major evolution occurred just in the last 3 years. In the revised manuscript we have improved the description of the novelty of the manuscript emphasizing the most innovative results.

* Are these claims novel, and will they be of interest to others in the community? If the conclusions are not original, it would be helpful if you could provide relevant references.

This is an important topic and is rightly receiving high profile attention, including a large number of recent papers covering similar themes. In particular, I identified these two papers:

• Kilburn, C. R., Carlino, S., Danesi, S., & Pino, N. A. (2023). Potential for rupture before eruption at Campi Flegrei caldera, Southern Italy. Communications Earth & Environment, 4(1), 190.
• Bevilacqua, A., De Martino, P., Giudicepietro, F., Ricciolino, P., Patra, A., Pitman, E. B., ... & Neri, A. (2022). Data analysis of the unsteadily accelerating GPS and seismic records at Campi Flegrei caldera from 2000 to 2020. Scientific reports, 12(1), 19175.

Bevilacqua et al, 2022 in Scientific Reports has a very similar author list and presents much of the same material, but with a slightly shorter timespan (2000-2023 vs 2000-2021).

The manuscript adopts an approach and methods just partially common to those used by Bevilacqua et al (2022). In fact, the new manuscript extends the previous work with a detailed investigation of the relationships between deformation and seismic activity and, equally important, applies the analyses to 3 years longer datasets. The new analysis has been also applied to the 1982-84 crisis for comparison gaining new insights on the different forcing processes of the two unrest episodes. The outcomes of our analysis are indeed different from that of Kilburn et al. (2023).
It is worth to underline again that adding 3 more years in a progressive accelerating crisis of a high-risk volcano such as Campi Flegrei caldera is not a marginal neither incremental development but represents, in our view, a new timely contribution on an evolving crisis of great concern at the regional, national and international levels.

Kilburn et al, 2023 comes to the same conclusion as major claim 2.

This statement is incorrect. Despite we agree with Kilburn et al (2023) that the ongoing process is certainly making the shallow crust of CFc weaker due to increasing fracturing, Kilburn et al claim that the relationship between deformation and seismicity has become linear since 2020, implying a fully inelastic behavior of the crust. This is different from our results which instead still indicate an exponential type correlation, with exponent increasing with time, so suggesting the continuation of a quasi-elastic behavior of the system. As mentioned in the appeal letter, data collected in the last few months confirm the correctness of our prediction.
It is worth underlining that the new correlation between deformation and seismicity is independent from the interpretations in terms of rheological behavior of the shallow crust and gives evidence of a stronger sensitivity of the seismic activity on the uplift of the caldera.
Moreover, it is important to acknowledge that the analysis of Kilburn et al (2023) ends at June 2021 (see caption of Fig. 4 of their paper), i.e., about 2.5 years before our datasets.

Figures:
Figure 1 is identical to that of Bevilacqua et al, 2022.
Figures 2 and 3 in this manuscript are subplots of Figure 3 in Bevilacqua et al, 2022. Panels 2a, 3a, 3c, 3d are identical, and the others are very similar but with minor differences (e.g horizontal/vertical or declustered/raw).
Figure 4 and Figure 5 of this manuscript are subpanels of Figure 4 and 5 from Bevilacqua et al, 2022.
Figure 6 of this manuscript is new as far as I can see,
Figure 7 presents Fourier analysis of the GPS data are RITE and is very similar to Fig 8 of Bevilacqua et al, 2022, but covers slightly different time periods. It is alarming to note that the results are very different, even when the time periods overlap.
Figure 8 presents earthquake count versus vertical uplift, which has previously been

shown in Fig 4 of Kilburn et al, 2023.
Figure 9 presents similar data for the 1982-84 crisis and similar patterns are seen in Fig 6 of Kilburn et al, 2023, which shows a longer time series.

As mentioned above, we have investigated timeseries of the CFc geophysical data about 3 years longer that those investigated in Bevilacqua et al (2022). Of course, it could not be differently, the datasets used in the two papers partially overlap and therefore few figures have some similarities. But we want to stress again that we are dealing with an accelerating crisis which has generated major changes in the datasets just in the last 3 years. Indeed, several figures of the manuscript are just focused on the period 2019-2023. The analysis of the evolution of the crisis through an update of the previous work and mostly through new joint analyses of the two geophysical datasets are the key objectives of this manuscript as clearly discussed in text.

The text of the manuscript also seems to cover similar topics as these previous papers and I struggled to find any novelty. The parameterisation of the trends as parabolic (uplift) and super-exponential (seismic count) is potentially interesting, but there is no attempt to link it to a physical model or interpret the significance.

We definitely cover the same topic of the key papers mentioned but with several substantial differences and extensions. The main ones are already reported in our appeal letter and in the response to the Editors.
Here we just reply that it is not true that there is no attempt to link the analysis to a physical model, since the interpretation of the results are widely discussed, including its important limitations, in the context of the brittle-fragile behavior of the shallow crust subject to the action of a progressive stress and with respect to previous analyses (by highlighting the main differences) and available interpretations of the current unrest.
Nevertheless, we want to emphasize that, in our opinion, a further important merit of the manuscript consists in its aim to objectively quantify, on the basis of a rigorous mathematical analysis, the behavior of the volcanic system in terms of relationships between the geophysical data investigated with no or minimal modelling assumptions in their derivation.

* Is the work convincing, and if not, what further evidence would be required to strengthen the conclusions?

The description of the methods and results lacks technical detail and were not presented systematically (just subplots from earlier papers), making the results somewhat unconvincing.

All the technical details adopted in the analysis are reported in the method section or by referring to the relevant papers in the literature. All the datasets used are also provided as supplementary material for transparency and to allow full reproducibility of the analysis.

* On a more subjective note, do you feel that the paper will influence thinking in the field in terms of either conceptual understanding or technological capability? Please

feel free to raise any further questions and concerns about the paper.

No, any novelty in this manuscript is deeply hidden in the current version and a complete rewrite would be required to justify publishing this here. What do the extra 2 years of data tell us that we did not already know?

We have already provided above our responses to these points. However, in the new revised version, we have tried to improve the description of the novel aspects of the study as well as the differences with previous analyses in order to account the referee's comments. We also revised three figures (1, 1-SI and 9-SI) and added 4 recent papers in the reference list.

Reviewer #2 (Remarks to the Author):

The authors carry out a statistical analysis of ground deformation and seismic data and conclude that the crust is behaving quasi-elastically and progressively weakening. They also propose a scenario for future seismic activity, if current trends were to continue. Much of the analysis (the frequency analysis of the oscillations and the FFM) is similar to that of a previous paper with the same first author (Bevilacqua et al, 2022), albeit with 3 years more data. The other significant analysis (number of earthquakes vs uplift) is similar to that of Kilburn et al (2023) with an extra 1 year of data. This extra year of data shows that the relationship has not become linear, as Kilburn et al (2023) concluded, implying the crust is still behaving behaving quasi-elastically (as previously concluded from earlier data by Kilburn et al (2017)). As such, this study can be a useful, incremental, update to previous studies, if some restructuring is carried out and more discussion of the results is added, as suggested below.

We are pleased to note that the Referee appreciated the differences and developments with respect to the previous papers and noted that the study can be useful with some revisions that we have mostly implemented in the revised version.

Here we just want to highlight again here that the addition to the datasets of about 3 more years with respect to Bevilacqua et al (2022), and 2.5 years (not one year as mentioned by the Referee) with respect to Kilburn et al (2023. See their Fig. 4), for such a progressive accelerating crisis is a substantial new element of the manuscript which has also allowed to make comparisons, and indicate remarkable differences, with respect to the same published papers.

Major comments:

The analysis shows that uplift (u) is proportional to t^2 and a near-exponential relationship between number of earthquakes (N) and u between 2010 and 2023. This would imply an exponential relationship between N and t^2 for the same period (rather than the more generic description of super-exponential). This could be tested by plotting t^2 on the x-axis instead of t in Fig 3b.

This is correct. Nevertheless, in the revised version we have preferred to keep just the time on the x-axis of all plots to favor the direct and simple interpretation of the timeseries analyzed.
Moreover, as widely explained in the Discussion section, the robust forecast of seismic activity at the short (i.e., weeks/few months) timescale is prevented by the various oscillations affecting the timeseries analyzed as determined in this study. This is the reason why we provided estimates of potential seismic activity for a given uplift of the caldera (20 cm in the case discussed) instead of time.

But a bigger issue is that the meaning of the results of the analysis of the independent signals is not discussed at all. The discussion essentially dives straight into another analysis, where the signals are plotted directly against each other. These are the results that are then discussed further.

I suggest much of the independent analysis is moved to supplementary and the joint analysis is moved into the main methods/results, where it better belongs. There then needs to be a proper discussion of the implications of all the results, including the parabolic uplift and the dominant frequencies in both data sets.

We followed the recommendations of the referee by moving the presentation of the relationships between deformation and seismic activity to the Results section and by adding some more discussion about the trends of the independent variables and their relationships and implications in the Discussion section. However, we preferred to keep the plots of the geophysical variables in the main text to properly introduce the reader to the following analysis.

In the studies of Kilburn et al, the uplift is corrected for the very long-term subsidence. This correction is not made for this study, which might be fine, but it should be mentioned that it is sometimes done, with a discussion on why it is not applied in this study.

We added a paragraph in the Methods section about this point. In that we simply note that adding or subtracting a linear function to the vertical uplift does not affect at all the nature of the relationships derived and therefore the conclusions of the manuscript. Therefore, we preferred to avoid to add further assumptions and potential source of errors.

The stated time lag of 30-45 days does not seem justified. For instance, the lag looks close to zero for Aug/Oct 2023 in Fig 5. Can you explain how the time range was reached?

We corrected this sentence stating that the time lags can vary up to about 45 days. As acknowledged in the text, a detailed and robust quantification of this time lag is particularly challenging to the very noisy nature of the GNSS data. Such an analysis would require an ad-hoc investigation that is beyond the objective of this manuscript. Nevertheless, we wanted to mention such a strong temporal correlation between the two variables and give a first order-of-magnitude estimate of the time lag.

Minor comments:

Fig 2: Y label axis incorrect

Corrected.

Fig 4: Use shaded vertical bars to indicate the temporal extent of the uplift episodes

Just for the reason describe above, i.e., the noisy nature of the deformation data which prevent a precise definition the duration of the uplifts, we preferred to avoid to report shaded vertical bars on the figure. More information on these uplifts, including their duration can be found in De Martino et al. 2014 and Bevilacqua et al. 2020, both referenced in the text.

While the meaning is generally clear there are errors in the english language throughout that need correcting.

We have corrected a few English errors and improved the language throughout the manuscript.

Finally, in the new revised version, we have tried to improve the description of the novel aspects of the study as well as the differences with previous analyses. We also revised three figures (1, 1-SI and 9-SI) and added 4 recent papers in the reference list.

References:

Bevilacqua, A. et al. Data analysis of the unsteadily accelerating GPS and seismic records at Campi Flegrei caldera from 2000 to 2020, Sci. Rep. 12:19175 (2022).

Kilburn, C.R.J., De Natale, G. & Carlino, S. Progressive approach to eruption at Campi Flegrei caldera in southern Italy. Nat. Commun. 8, 15312 (2017).

Kilburn, C.R.J., Carlino, S., Danesi, S. & Pino, N.A. (2023). Potential for rupture before eruption at Campi Flegrei caldera, Southern Italy. Nat. Commun. 4:190 (2023).

ANSWER TO REVIEWER #2

We appreciated your careful review of our manuscript. We hope you will find the detailed and thorough revisions we have made, following the many valuable recommendations provided by you, the other reviewer, and the editors, to be satisfactory.
In summary, we have significantly extended both the analysis and interpretation of the data presented, and have better highlighted the novelty of our findings in several sections of the manuscript. Specifically, we have:
- Improved the description of the significant differences between our outcomes and those reported in the reference Kilburn et al (2023) as suggested by you.
- Expanded the discussion with a new paragraph on the stress memory (Kaiser) effect as suggested by another reviewer.
- Revised the paragraph on the FFM approach, including the addition of the new Fig. 10 (ex Fig. SI-9) as suggested by both reviewers.

Additionally, we have:
- Included 4 new figures in the Supplementary Information (SI) (Figs. SI-8, SI-9, SI-10, SI-11) to show the effect of additional parameters and fitting procedures.
- Extended 3 existing figures in the SI (Figs. SI-4, SI-5 and SI-6) to illustrate the effect of the type of catalog and magnitude completeness of the seismic data used.
- Added 4 new tables in the SI (Tables SI-1, SI-2, SI-3 and SI-4) to fully describe the regression fitting procedure and the associated parameters.
- Incorporated 11 additional publications into the reference list to strengthen and better justify our findings.
- Thoroughly revised the manuscript to improve English readability and effectiveness.

REVIEWER #2 (REMARKS TO THE AUTHOR):

The structure is much improved and it is clearer now what the novelty is, but the study still comes across as an incremental update to previous studies. The conclusion of the quasi-elastic nature of the crust has been reached before (e.g. Kilburn et al. 2017).

We appreciated that you found the novelty of the manuscript clearer. Kilburn et al (2017) proposed the interpretation of the shallow crust as a quasi-elastic medium based on data until 2016. Conversely, Kilburn et al (2023) suggested a transition from a quasi-elastic to inelastic behavior starting in May 2020, based on the observed shift from an exponential to a linear relationship between seismic count and uplift. In this manuscript, we demonstrate that the exponential-type relationship between seismic count remains valid up to November 2023, albeit with an inflection in the period 2020-2022, which we interpret as a stress memory (Kaiser) effect. This represents a significant difference in the data analysis results that we believe it is important to highlight. A key implication of this is the increased sensitivity of seismic activity to any further potential inflation of the caldera (see new Fig. SI-10). We have also revised several paragraphs in the abstract,

introduction, discussion and conclusions sections to better emphasize the novelty and timeliness of this study. All changes are shown in the marked-up version.

I checked again and the Kilburn et al. (2023) paper presents data until April 2022 (it's true that Fig 4 refers to June 2021, but other figures go into 2022, and April 2022 is stated in the text), which represents 1.5 years of new data.

This is correct. However, as you also noted, their primary claim of a shift from an exponential to a linear relationship (i.e., from a quasi-elastic to an inelastic behavior) is based on their Fig. 4, which include data up to June 2021. This is approximately 2.5 years earlier than our analysis of the same relationship.

And the fact that the majority of seismicity has occurred in the last 3 years is a function of the increasing rate in time and I suspect the same could have been said for the previous 3 years.

We agree, this is a consequence of the exponential relationship between seismic count and uplift, as well as the accelerating trend of uplift highlighted in the manuscript. However, the level of seismic activity, as the strain release, recorded in the last three years is significantly higher than that of the previous 3 years. This difference is what underscores the severity of the current crisis.

I note that the FFM analysis conclusions are very couched in the paper, but that the authors state in the rebuttal that it is being used to correctly forecast the trends, and I would suggest adding more details on this interesting aspect to the paper.

We have revised the description of the FFM approach as applied in our study. We have also moved to the main text Fig. 9-SI (now Fig. 10 in the revised version) as requested by another Reviewer. Regarding the trend forecasting, our rebuttal was referring to the plot illustrating the relation between uplift and cumulative seismic count (Fig. 8) which is fully discussed in the last subsection of the Discussion.

I would also still like to see the idea of an exponential relationship between N and t^2 (which is implied) being both explicitly stated and tested. Why use more generic term super-exponential if there is a more specific empirical relationship? If the relationship between N and t^2 is not exponential, it should be explained why not, given the empirical conclusions for the relationships between N and t, and between N and u.

As requested, we have added the plot of the relationship between t^2 and N in the SI (Fig. SI-9) to demonstrate that the relationship is exponential, as expected given the other relationships described in the text. We have also commented on this plot in the discussion section and introduced the more accurate term "squared-exponential" as suggested.

ANSWER TO REVIEWER #3

We appreciated your careful review of our manuscript. We hope you will find the detailed and thorough revisions we have made, following the many valuable recommendations provided by you, the other reviewer, and the editors, to be satisfactory.
In summary, we have significantly extended both the analysis and interpretation of the data presented, and have better highlighted the novelty of our findings in several sections of the manuscript. Specifically, we have:
- Improved the description of the significant differences between our outcomes and those reported in the reference Kilburn et al (2023) as suggested by the other reviewer.
- Expanded the discussion with a new paragraph on the stress memory (Kaiser) effect as suggested by you.
- Revised the paragraph on the FFM approach, including the addition of the new Fig. 10 (ex Fig. SI-9) as suggested by both reviewers.

Additionally, we have:
- Included 4 new figures in the Supplementary Information (SI) (Figs. SI-8, SI-9, SI-10, SI-11) to show the effect of additional parameters and fitting procedures.
- Extended 3 existing figures in the SI (Figs. SI-4, SI-5 and SI-6) to illustrate the effect of the type of catalog and magnitude completeness of the seismic data used.
- Added 4 new tables in the SI (Tables SI-1, SI-2, SI-3 and SI-4) to fully describe the regression fitting procedure and the associated parameters.
- Incorporated 11 additional publications into the reference list to strengthen and better justify our findings.
- Thoroughly revised the manuscript to improve English readability and effectiveness.

REVIEWER #3 (REMARKS TO THE AUTHOR):

** What are the major claims of the paper?*
The paper studies temporal evolution of seismicity and deformation during the 2005-present uplift episode at Campi Flegrei. Deformation is found to increase following a parabolic trend, while seismicity (quantified mostly as earthquake counts) follows a super-exponential trend - i.e. exponential growth with the exponent increasing with time. Furthermore, seismicity as a function of deformation is observed to follow an exponential trend, which contrasts a previous study. These empirical observations are used to forecast time of failure (eruption) and probabilities of larger earthquakes than observed previously. Periods of oscillations and time-lag between oscillations in deformation and seismic data are also reported, although not used for further interpretation.

We would like to add that, as clarified in the revised text, the main focus of our analysis is the investigation of the trends and relationships between geophysical variables over the last few years. This period has not been previously studied, and all parameters have significantly increased over this time. Additionally, we conducted a comparison with the

1982-84 unrest to highlight the significant differences between the two crises.

** Are these claims novel, and will they be of interest to others in the community? If the conclusions are not original, it would be helpful if you could provide relevant references.*
As the inflation at CF is evolving it is important to scrutinize deformation and seismicity results as done in this paper. Therefore the the study can claim novelty; the finding that seismicity as a function of deformation follows an exponential trend is also novel, in comparison with a recent study (ref. 36 in paper) and thus important to be published. The main approaches applied in the paper for data analysis and interpretation are pretty standard. The particular forecasts of failure times and occurrence probabilities of larger earthquakes presented in the paper are useful for the scientific community, as well as the monitoring and civil protection in the area of CF. In a more general context, the study makes a contribution towards understanding better the relations of seismicity and deformation.

Thank you, we appreciate that the reviewer has clearly understood the novelty and importance of this study, particularly in the context of the major ongoing crisis at the high-risk volcano of CF.

*** MAJOR COMMENTS:**
While I think the data analysis and results/conclusions drawn in the paper are in relatively good standing, the paper can be significantly improved in:
(a) description of data and methods. I found the data and methods sections to be incomplete and confusing. It is for example lacking a more thorough explanation of what lies behind the seismic data set; e.g. are the earthquake counts manual or automatic; how are earthquake swarms affecting counts (e.g. saturation of count); what about station outages? The curve fitting method is also not described adequately.

We have improved the Data section by adding several more information about the seismic datasets, including acquisition and localization procedures, associated errors and completeness magnitude of the catalogs. We have also shown the distribution of the seismic network in Figure SI-1. In the Methods section we have improved the description of the regression method used and the presentation of the fitting parameters as illustrated in 4 new comprehensive tables included in the SI.

(b) tabulating of results is missing: such as exponents from fit-functions, parabolic fit parameters, RMS of misfit. This is important for future reference and validation;

We have added 4 new tables containing the main regression parameters and associated errors in tabular form as suggested (see new Tables SI-1, SI-2, SI-3 and SI-4).

(c) quantitative arguments (misfit; f-test) for using a double exponential function instead of a single exponential functon (or exponential function times a parabolic function). It should be easy for the authors to use misfit statistics to do a better job in convincing the

reader that their proposed fit functions are preferred over others.

In the 4 tables added in the SI (Tables SI-1, SI-2, SI-3 and SI-4), we reported the main parameters and associated errors of the various exponential fittings shown in the manuscript. In the same tables we also reported a comparison with linear fitting functions adopted in previous papers (see new Fig. SI-10).

I also found the sentence structure often to be confusing, and I think the paper could improve much by edits from a native English speaker. This is a major issue because it is at times difficult to understand meaning of sentences, requiring long time to read and leaving some question marks regarding meaning of some sections.

We thoroughly revised the entire manuscript text to make the English clearer and more readable.

The main claims and relations in the paper are drawn from single-station earthquake counts; either in number per time unit, or cumulative number. Earthquake count can be useful as an indicator of seismicity, but it is also tricky because sensitivity diminishes with distance from the seismic station, depth, weather, etc. Therefore, identical swarms at different distances would be represented differently. In case of intense swarms the earthquake counting will saturate. The counting method may also confuse small earthquakes with geothermal-related seismicity or anthropogenic activities. In general the strain release, deduced from network-wide locations is a more robust measure of seismicity. I think it would improve the physical meaning of the paper significantly to present strain release as function of deformation, e.g. as an additional panel in Fig. 8, and discuss accordingly.

In the analysis, as described in the Dataset and Results sections, in addition to the single-station earthquake catalog mostly based on station STH, we have also used the catalog of located events to test the effect of the dataset completeness. We added a few sentences in the text to clarify the sensitivity of results to the seismic dataset used and to the completeness magnitude adopted. To this aim we also extended Figs. SI-4, SI-5 and SI-6 (in addition to Fig. SI-7) to demonstrate that the results are robust with respect to the seismic catalog used and to a completeness magnitude M_d=0.5 adopted. Finally, we also added the relationship between uplift at the RITE GNSS station and the cumulative strain release as a new figure in the SI (see Fig. SI-8).

*** MINOR COMMENTS:**
Abstract:
" the caldera shows a slow but progressive inflation "

Ambiguous wording. Some might think the deformation at CF is quite fast, although many faster deforming volcanoes exist. Suggest you just state here "shows an accelerating inflation".

We have improved the text according to the suggestion.

" an accelerating parabolic increase "

Delete "accelerating here", it is sufficient to say parabolic; the acceleration appears in the second part of the sentence

We have changed the text according to the suggestion.

Introduction:

" mostly affected by a dominant sinking "

Confusing wording; suggest simplify to "dominantly subsiding"

We have improved the text according to the suggestion.

" *Caldera deformation has been characterized by a well-defined bell-shaped pattern of vertical uplift " Here I suggest you add "and outward motion from center of uplift*",

otherwise the reader may think there is only uplift

We have improved the text according to the suggestion.

" *centered in the town of Pozzuoli*, "

add a reference to Fig S1

We have added the reference to Fig. SI-1.

" *seismicity is mostly located in correspondence of two main depth levels* "

Confusing wording; suggest "...located at two main..."

We have changed the text according to the suggestion.

" *The datasets used .... provided by the study."*

This paragraph is completely unneccessary - suggest deleting

OK, we have deleted it.

Datasets:

"*consistently recorded the most significant uplift*".

Confusing wording - significant can have several meanings. Suggest "recorded the fastest uplift" or "highest uplift rates"

We have improved the text according to the suggestion.

" *about 500 m into the sea, southwest of the Rione Terra,* "

Here it is most important how far away from RITE the uplift center is, not how far into the sea. Even better if you can provide uncertainties for that estimate; should be noted in the references.

We have revised the sentence and added the approximate distance from the RITE GNSS station. Uncertainty ranges of the modelled center locations are reported in the references provided.

" *corresponding superficial pressurized source at a depth of about 3-4 km* "

some may find this confusing as 3-4 km may be considered shallow by some, but deep by others. Suggest just deleting "superficial"

We have changed the text according to the suggestion.

" *of final daily position time series with the associated uncertainty (estimated to be 1 cm on the vertical component and 0.3 cm in the two horizontal components16).* "

This description is not satisfying: (a) a reference from 2021 cannot include results up to 11/2023; (b) it is an oversimplification to say that the uncertainties are estimated 1 and 0.3 cm because ref. 16 states that the mean uncertainty is of these values. Please be specific in which uncertainty values you used, and if uncertainties are not used in the time series analysis, then state so explicitly.

We have revised the sentence reporting the correct description of the uncertainty used in the analysis. The effect of the mean uncertainty on the fitting parameters of the uplift was also discussed in the text and illustrated in Table SI-3. The citation of reference De Martino et al (2021) was corrected.

Fig 1: "*data merge leveling data, collected at benchmark 25A, and the RITE GNSS station.*"

Refer the reader to Fig S1

We have added the reference to Fig. SI-1.

Fig 1: "Overview of the earthquakes recorded at CFc in" This description is not detailed enough. Be specific; Number of earthquakes per month, as recorded at ....
We have revised the caption of Fig. SI-1 as suggested and we added in the figure the distribution of the monitoring seismic network.

" The permanent seismic network managed by INGV-OV presently comprises 25 permanent digital stations and 3 permanent analog seismic stations15" Delete the two latter occurrences of permanent

We have deleted the two words.

" *The catalog we used consists of the earthquakes recorded, between 2000 and 12/2021, at the STH reference station situated near the Solfatara-Pisciarelli area (Fig. 1-SI), whereas, in the period 1/2022 and 11/2023, it relies on the database SERENADE. In total the catalog includes 16,250 earthquakes*. "

Since accessible references for how this is done are not given, the authors must describe in more detail how the earthquake counts are made. E.g., are they manual or automatic? A reference for SERENADE is missing; it is also missing to state whether the different approaches are compatible or not - with some references or extra material in supp.mat. please.

We have added a few more information on the seismic catalogs used and the methods and procedures for counting seismic events they adopt. We have also cited a few references where the catalogs are described in more detail.

" *Within this dataset, we can assume the completeness magnitude (Mc) equal to about 0.219.* "

Some information is missing before that magnitude is estimated for each event. How is the magnitude estimated for a single-station detection? This is important for the basis of the strain-release calculations.

We have added a brief description on how the magnitude of each event was computed in the catalogs used. More information can be found in the references cited.

" in the 2000-2023 considered period."

Example of overloaded wording; simplify to "during 2000-2023".

We have simplified the text as suggested.

" month over the last about four decades"

Overloaded wording; simplify to "month since 1983"

We have simplified the text as suggested.

" *seismic activity affected a radius of* "

Confusing wording - what does affected mean, is it where earthquakes were felt or waveforms recorded? --> suggest "occurred in a radius of ..." or "were located"

We have revised the text as suggested.

Methods:

" *The deformation and seismic datasets have been closely analyzed by investigating their temporal evolution (Fig. 2 and 3)* "

Confusing wording: one can understand this text as if the data have already been analyzed by another study. Rephrase "We analyze ...."

We have rephrased the sentence as suggested.

" *with specific reference to the displacement rate* "

What does this mean? Is there some reference rate used? Clarify.

We have clarified the sentence as suggested.

" *see [39] for more details and a comparison with trends computed over the period 2000-2020)*"

Improve two things here: (a) if you are following the methods of [39] then just state so implicitly. (b) The latter part of the sentence, for the comparison, is not a methods section material, but results

We have revised the sentence and the citation to Ref. [39].

" *at these stations the vertical time series are scaled down according to the observed bell-shaped deformation pattern* "

Confusing wording: can be understood as if you literally scale down the observed data. Rephrase.

We have revised the sentence to make it clearer.

" *It is worth mentioning here that just one GNSS station, located in the area of Mt. Olibano-Academia (named ACAE), has shown, since about 2021, an uplift deficit in comparison to other areas at the same distance from the maximum uplift of the caldera. The analysis and interpretation of this anomaly is the object of another study47 and will not be further considered in the following."*

Suggest deleting this, at least from here. If you want to mention this observation, then do so in the discussion section, and include ACAE on a map/refer reader to Fig S1.

We have moved the sentence from this section to the Results section as suggested. We also referred to Fig. SI-1 to show the location of ACAE.

" *for the background subsidence of the caldera36*"

Give some numbers for this background rate so that the reader can be convinced that it is irrelevant. I also find the arguments for a background subsidence to be limited: it does not show up on the EGMS InSAR data server (https://egms.land.copernicus.eu); If you want to keep this notion then it may be good to mention the physical cause (transport of material from depth?).

We added this paragraph during the first revision of the manuscript at the request of another Reviewer. We have slightly revised the text and included an estimate of the subsidence. However, we want to emphasize again that none of our results are affected by this assumption.

" *mostly refer to all earthquakes recorded.* "

- at station STH?

The sentence referred to both catalogs presented in the Data section. We have revised it to make the point clearer.

" *We base the estimate of the annual rate on left-side first-order finite differences so that the value at time t is not anticipating future information39.* "

This is a good description; I understand it as you just difference the first and last value of a sliding window. Please be specific about the timestamp for each calculation, as that will very much affect your time lag calculations; i.e. would it make more sense to use the central time of the sliding window rather than its right side? I miss seeing something more about the time steps: I suggest describing your method as a "sliding window" of some width that is moved by (I hope) daily increments.

This is correct and that is exactly what we did. We carefully revised the description of the method section to compute rates, following the suggestion and introducing the term “sliding window”.

Your methods section is missing to explain how the curve fitting is done. There is some mention caption for Fig 2, but that does belong in the methods section.

We have added a paragraph in the methods section to describe the fitting procedure and its main features.

In the methods section, explain/define explicitly how you calculate the Benioff strain.

We have added a sentence to describe how we calculated the Benioff strain.

Results:

"Fig. 2 refers to the vertical"

Confusing wording: why "refers to"? Implies that something different is shown. Suggest "shows"

We have changed the wording as suggested.

" that the behavior since "

ambiguous; refer to explicitly which behavior you mean

We have clarified the meaning of the sentence.

" Such a parabolic regression fits very well also the linear and log-scale plots as shown in Fig. 2a,b,"

This sentence makes no sense because you are plotting the same data in all parts of Fig 2! Of course it will well on all plots. Delete.

We agree and we have deleted this part of the sentence.

"R-squared = 99%, in"

I suggest you consider better the statistical measure you present for your curve fitting. Include also an RMS value; and a table of all these values and parameters. Even better if you can provide uncertainty estimates for your curve parameter estimates.

We have revised the sentence and described the regression in terms of RMS value, as suggested. We have also added a table with the main fitting parameters, including the distribution of the RMS value (see Table SI-2).

"Very similar features of the deformation trend are also observed in the vertical and horizontal displacements at the other stations, as shown in the SI material (Fig. 2-SI and 3-SI), making the above considerations valid for all the points investigated"

This text implies that the same kind of analysis was done on the other data, however, no such information is found in the supplemental material. It is preferable to use quantitative estimates --> also estimate curve fitting parameters for the stations shown in Fig SI-2-3.

We have added the reference to Tables SI-1, SI-2 and SI-3 to show the fitting parameters and associated errors of all the data illustrated in Figs. 2-SI and 3-SI.

Fig 2: *" plots the blue points represent the daily average measure "*

This sentence describes a gross oversimplification of GNSS data processing. Suggest "represent the computed daily position (16)"

We have corrected the sentence as suggested.

"(Fig. 3a also shows labels marking the size of the 7 greater swarms recorded)."

Avoid () as possible. This sentence stands perfectly fine without the parenthesis.

We have removed the parentheses as suggested.

"shows a super-exponential behavior of the"

Explain what is meant by "super-exponential", as not all may be familiar with this term

We agree and we have explained the meaning of the term super-exponential the first time we introduced it.

" in this case corresponding to a super-exponential trend also in absolute values as shown in Fig. 3a"

I don't understand what you mean here. The same data will have the same trends.

We have deleted this part of the sentence.

" of the most significant events "

unclear. Largest magnitude events?

We have modified the wording as suggested.

Fig 3 caption *" analyzed in the paper "*

It is utterly no need to state that these data are analyzed in the paper. Delete.

We have cut the words as suggested.

" display the annual rate of deformation and seismic "

this is a bit confusing, since one may interpret this as the sliding window length is 1 year (6 mo in Fig 4). Suggest rather "in units of mm/yr" or "display the rate of defor...."

We have deleted the word “annual” from the sentence.

" computed by adopting a time step of 6 months to better "

No this is not the time step, but the length of the sliding window. Isn't the time step still 1 day?

Yes, we agree and we have revised this one and similar sentences to clarify the meaning of time step and sliding window.

" The rate value computed "

Simplify to the rate computed

We have corrected the text as suggested.

"*Aug-Oct 2023 shows the existence of a time lag of up to 45 days between the peaks of the two time rates.*"

Here it is quite important to remind the reader of the time stamp for each of the seismic and deformation data; to me the time stamp seems to be made in the same way, good if you can highlight that.

We agree and we have improved the sentence accordingly.

" *with a time step of 6 months, and Fig. 7c highlights a main period of about 2.9 years computed from 2011 to 2023 with a time step of 2 years. It is also interesting to note that the durations of these main periods are about 10-15% shorter that those estimated by [39] at the same RITE station over the period 2000-2020. A similar effect has been observed at all other GNSS stations and also in the seismic rate record*."

Interesting; suggest you mention this briefly and explanations in the discussion.

We agree and we have briefly mentioned this point in the discussion section.

" *Figure 7. Examples of Fourier " Why "examples*"?

Confusing. Where can the reader then find more complete Fourier analysis? Rephrase.

We have rephrased the sentence as suggested.

" *curve. No clear trend was instead observed on the period 2000-2010, part of which has been characterized by subsidence and very weak seismic activity.*"

I think it is important to mention stress reversal: if a medium has already been stressed in one direction, and the deformation reverses, then the first part of the processes is unstressing, where little or no seismicity is expected to happen. This happens both following inflation and deflation and is very clearly shown in your data. Consider relating to the "Kaiser" effect here (e.g. Heimisson, E. R., Einarsson, P., Sigmundsson, F., & Brandsdóttir, B. (2015). Kilometer-scale Kaiser effect identified in Krafla volcano, Iceland. Geophysical Research Letters, 42(19), 7958-7965.)

Thank you for this very useful suggestion and reference that we have discussed in two new paragraphs added in the discussion section. We have also referred to the stress memory (Kaiser) effect in the abstract and conclusions.

"*exponential function already represents a good approximation of the observed relationship, i.e., R-squared > 99%. However, given the more accelerating behavior of the data with respect to a single exponential function, we have adopted here a fit with two exponential functions and optimized the “connecting time” between them*".

So, if you already have a R-sq>99%, how can you justify a fit including more parameters? Seems like you are just fitting noise... Suggest using misfit statistics here for argumentation. In your parameter tabulation, also include the connecting time.

As we have already described above, we revised the entire text making reference to the RMS values and added 4 new tables (Tables SI-1, SI-2, SI-3, and SI-4) containing all fitting parameters derived in the analysis.

"*relationship was obtained, with different fitting exponents, by us*"

How different? Be quantitative; use tables of fitted parameters.

We have revised the sentence as suggested and added the reference to the new tables of fitting parameters (Tables SI-1, SI-2, SI-3, and SI-4).

Fig 8: " *The light and dark blue curves indicate* "
I see no difference in the blue curve colors. Suggest larger data symbols, and plot the curve on top as a thicker line(s)

We have enlarged the two plots (Fig. 8) and changed the colors of the curves as suggested to make the plots clearer.

Discussion:
Consider adding subsections for clarity

We have added 3 subsections to make the text clearer as suggested.

"). The reasons for the quasi-parabolic increase of the uplift are beyond the objectives of this manuscript and the analyses here developed. However, the understanding of the cause of this accelerating process is an interesting future goal and questions on the possibility that the driver of the uplift is itself an accelerating cumulative process, such as, for instance the injection of gas or magma in the upper crust from a deeper source. "

Conflicting statement; first you say you are not going to talk about something, but then you talk about it. Rephrase.

We have revised the text to make it more consistent.

" effect with time clearly visible since 2021 and even more since the beginning of 2023."

refer to Fig 5 here

We have added the reference to Fig. 5 here.

" *Patterns of fracturing of brittle rocks in laboratory clearly show that at low stresses fracturing activity is null or very low and the behavior is close to elastic, i.e., there is a quasi-linear relation between stress and strain. At stresses of about half of the values at bulk failure the fracturing activity begins to increase in an exponential way until the strain has typically reached about 90-95% of the failure value. Under such conditions the stress increases in a non-linear way with respect to strain, producing a curve strain-stress that is downward convex. This condition of the material is also characterized by dilatancy, i.e., an increase of the volume due to a quasi-homogeneous formation of small cracks within the rocks. At larger increasing stresses, the fracturing activity*

typically accelerates very quickly producing the coalescence of fractures in a major fault and eventually leading to bulk failure of the brittle rock. For specific properties of the rock a variety of strain-hardening or strain-softening type behaviors of the material can also be observed before failure. Eventually, when the amount of stress has reached its maximum value, most of the supplied energy is spent in fracture and fault movements and the behavior of the rock becomes inelastic, i.e., the deformation continues under a constant stress. This regime is typically characterized by a proportional increase of seismic events with deformation."

Many references missing from this paragraph; please improve.

We have added several references some of which already cited in the previous sentences of the same paragraph.

" *The same relation was obtained by considering different thresholds of magnitude. This is also different from the results of [35,36] who estimated an exponential trend over the period 1982-84 by apparently using a lower number of data points.*"

OK, but suggest discussing what would be the effect of undercounting of eq numbers, as suggested in Figure 1. The undercounting would happen while the seismicity rate is greatest, and likely result in a constant maximum rate.

We have revised the text of this paragraph for clarity. However, the possible undercounting effect shown in Fig. 1 does not affect the crisis 1982-84 but just the period 1986-1998. We have clarified that the linear trend was obtained by assuming different uplift data types and completeness magnitudes of seismic data. We have also added a new figure in the SI (Fig. SI-11) to demonstrate the poor fit of the data with an exponential curve. All fitting parameters were also described in Tables SI-1, SI-2 and SI-4.

" *Such a different relationship between deformation and seismic activity appears as further evidence of the different forcing source and/or mechanism associated to the 1982-84 crisis with respect to the present one."*

Well, as the seismicity rate is also strain rate dependent, could it not just be different inflow rates? Is the deformation source at the same location and depth? Suggest you add some notes in your discussion about strain rate dependency of seismicity.

This is an interesting point. We have revised the paragraph to include a brief discussion, along with a few references, on the influence of the strain rate on the mechanical properties and fracturing of rocks, and consequently, on the investigated relationship.

" *few years due to the continuous, although oscillating, increase of the caldera uplift*. "

refer to Figure

We have added the reference to Figs. 4 and 5.

" *time computed since 12/2023) from 0.5 to about 2.5 years with 95%ile values up to about 8 years (see Figure 9-SI for more details). It* "

I suggest you include Fig 9-SI in the main text, since it is important for your conclusions.

We agree and we have moved this figure to the main text and revised its description.

" these estimates cannot be considered reliable forecasts of the occurrence of specific volcanic events given the large number of false alarms typically experienced in the application of FFM to real crises. "

References missing for this statement.

We have revised this sentence to avoid referring to previous applications of FFM to real crises, as those studies used a deterministic approach which differs from the probabilistic method we developed and applied here (see reference 39).

" *n of the crisis (up to few years as median values) in case of continuation of the observed trends.*"

What was the forecast for the 1980s unrest? Would be curious for comparison.

During the 1982-84 crisis, there was no decadal-scale increasing trend in deformation or significant seismicity preceding the crisis. Instead, the increase in deformation and seismicity was abrupt and not comparable to what has been observed in the current crisis. For that crisis, the FFM would have needed to be applied over a much shorter regression period, likely resulting in waiting times (i.e. the time to reach the critical point) of just few months. In that episode, deformation ceased in 12/1984, and the seismic crisis ended almost simultaneously, with no eruption.

Conclusions:

Conclusions can be shortened by 50%, keeping points 1-4.

We have carefully revised and shortened the conclusions. We believe that further reduction would result in the loss of important messages.

" *trend, similarly to the number of earthquakes*".

I dont understand what you mean

We have revised the sentence; we were referring to the cumulative number of earthquakes.

" *vertical uplift results to be well-represented, since* "

Confusing wording; rephrase

We have revised the sentence to make it clearer.

Supplementary Information:

". Plot (c) shows the quality of the fit, represented by the mean square distance between data and fitting curves, with the varying of the connecting time between the two exponential functions."

This is important, show for other curve fittings as well.

We have added a similar plot for Fig. 8a (see Fig. SI-6). Additionally, we have included the same plots for the correlation between t^2 and cumulative number of earthquakes, as requested by another reviewer (see Fig. SI-9).

Fig 9:" *analysis of waiting time from* "

Explain further; waiting time until what? This is the SI material; you have space to explain !

We have revised the description of the waiting time and moved the figure to the main text, as suggested above.

ANSWERS TO REVIEWERS' COMMENTS (in red)

Reviewer #2 (Remarks to the Author):

Again, significant changes have been made that improve the clarity of the paper. I still have one concern, however, about the way that the relationship between seismicity and time is treated.

While a figure testing the exponential relationship between seismicity and t^2 has been added (though only in SI), the figure is not referred to in the text, and mention of the relationship only comes up in the discussion. I find this baffling – surely it makes sense to discuss this in the Results Section when the relationship between seismicity and time is first discussed? Instead, the relationship is described there still as super-exponential. I suggest remedying this and changing Fig 3 to demonstrate the relationship. Panels b and d can be replaced by their t^2 equivalents and the fit based on a model can be added to a and c (I suggest t is retained on the x-axis for these).

We have modified Fig. 3 by adding 2 more panels (3c,d) to show the relationship between t^2 and cumulative seismic count. In particular, plot 3c represent the cumulative seismic count as a function of t^2, whereas plot 3d the log scale (10 base) of the same cumulative seismic count as a function of t^2. At this stage we did not add the model curves in the plots since the exponential relationship between the RITE uplift and the seismic count is first introduced in Fig. 8. However, we added in the text the reference to a later discussion of such dependency as well as to Fig. 9-SI. We decided to keep in Fig. 3 also the original panels 3b and 3d (now 3e and 3f) since they were explicitly requested by the other reviewer. We believe this is the best solution to properly consider the suggestions of both referees.

Reviewer #3 (Remarks to the Author):

After reading the revised article and replies to previous comments I have nothing more to add.

Thank you.