Review of: “The 1538 eruption at Campi Flegrei resurgent caldera: implications for future unrest and eruptive scenarios”, by Rolandi et al.

Overview

This paper contains a great deal of useful information about historical unrest at Campi Flegrei. The information comes from the published literature and from new reconstructions of the volcano’s behavior. The large amount of information has led to a lack of focus in the main theme and the mixture of old and new data has obscured its novelty. In addition, the conclusions have not obviously been derived from the observations provided. Judicial editing and restructuring of the text would yield a paper in which the logical thread of the analysis and the advance in understanding are made clearer. I have made numerous suggestions below. The number of changes indicates major revision. Even so, the adjustments should be straightforward.

Common themes are:

(1) Reorder the text. One option is:

- Describe observations and current views on ground movement and seismicity before the Monte Nuovo eruption in 1538 and since the unrest that began in 1950. Describe the key features of the subsurface structure of the volcano and connect this to the processes (*g.*, transport of gas and magma) that may be contributing to unrest.
- Describe *new* reconstructions of events before 1538. There is no need to repeat in detail previous studies – these can be acknowledged by citations in the text. Give just the key points and show how they have been modified by the new studies here.
- Compare the pre-1538 and post-1950 behavior to investigate whether the same sequence of events can account for both sequences. The different roles of the transport of magma and magmatic fluids could be a key topic.
- Discuss future scenarios based only on the data presented.

Reply: We thank the reviewer for the useful suggestions, so we will re-organize the paper the way he suggests.

(2) Clarify which material comes from previous studies and which is new to this paper. Much of the historical data has appeared elsewhere and does not need to be described at length here. Previous work must be fully cited: this would highlight the **novelty** of this work.

Reply: Yes we can do that.

(3) Remove extraneous topics that do not contribute to the goals and conclusions of the paper (*e.g.*, the account of the Monte Nuovo eruption).

Reply: We aim to emphasize that the account of Mt. Nuovo eruption significantly contributes to the objectives and conclusions of thispaper. The study focuses on accurately describing the events occurred before, during and after the 1538 eruption, in order to provide insights into potential future scenarios. In fact, we highlight some observations that have been never reported in previous studies, which are crucial for assessing what could happen during a future eruption. As an example, it is generally thought that only ash fall from the 1538 arrived at Pozzuoli; but we unambiguously identified, in outcrops at intermediate distance, pyroclastic flows arriving there, and forming the about 2 m of deposits found on the Serapeum columns.

(4) Explain the significance of a resurgence occurring as a block. Repeated pressurization at a common depth (which may happen to be shallow) is expected to produce the observed patterns of ground movement without needing to invoke wholesale movement of bounding faults (see for example Acocella (2019)). Hence, it would be good to have further guidance on the importance of whether or not fault-bounded movement is essential and whether this can constrain the potential for eruption. For instance, might a block model be appropriate only after a critical amount of movement has been achieved (*e.g.*, movements before 3.700 years ago) but not significant otherwise (*e.g.*, movements before 1538 or since 1950)?

Reply: There is substantial evidence supporting the central block resurgence at Campi Flegrei, as reported in the cited references. The necessity of differential movement along the bounding faults has also been thoroughly assessed in numerous studies: not only to reproduce the observed ground deformation pattern, but also to explain the occurrence and distinctive characteristics of seismicity. In this paper we simply recall and reorganize this evidence to explore its implications for unrest evolution as well as potential pre-eruptive and eruptive patterns. Nevertheless we will further clarify this matter, by including additional references, in a revised version of the paper

(5) Remove the text that is peculiarly negative in tone. In some sections, new material is presented with the apparent aim to demonstrate where others are wrong, rather than to show how it can improve current understanding.

Reply: Our aim is solely to address and correct previous interpretations of the pre-eruptive, eruptive and post-eruptive patterns associated with the 1538 eruption, which are demonstrably inconsistent with some fundamental observations. This is not intended as criticism of the researchers themselves, but rather of their scientific interpretations. Such efforts reflect the natural progression of science. Nevertheless, we are willing to ensure that our objective — disproving weaker scientific interpretations to enhance the understanding of volcanic phenomena, in line with standard scientific practice — is made even more explicit.

Specific Comments

Abstract

This will need to be adjusted to accommodate the revised text.

Reply: We agree and are willing do that.

Introduction

Lines 32-34. These lines cite previous work on reconstructing historical movements at Campi Flegrei and then state that all their conclusions need to be modified. However, only the work by Parascandola (1943) and Di Vito et al. (2017) is later discussed in detail. Dvorak and Mastrolorenzo (1991) is not mentioned, while work published by Bellucci et al. (2006) is repeated but not cited. The referencing of earlier studies must be more inclusive throughout.

Reply: We can do that and enhance the referencing throughout the entire paper.

Eruptive history of Campi Flegrei.

Lines 68-100. Reduce text by 30% and focus attention on the movement of a central block. The conventional interpretation in the literature is that the caldera formed about 40,000 years ago, during the eruption of the Campanian Ignimbrite (CI) and that the collapse during the Neapolitan Yellow Tuff (NYT) eruption represents a subsidiary movement. To state that the caldera formed only 15,000 years ago may well be correct, but it is not yet generally accepted. The key point here is that evidence of a resurgent block is only available since the NYT eruption, regardless of whether the caldera formed then or during the CI eruption. To acknowledge the uncertainty, perhaps the text could be adjusted to something like:

"Campi Flegrei is an active caldera to the west of Naples in southern Italy. About 12-14 km across, its southern third is submerged beneath the Bay of Pozzuoli. Following the most recent – and perhaps only (Rolandi et al., 2020) - episode of caldera formation, some 70 eruptions have occurred across the caldera floor, ranging from the effusion of lava domes to explosive hydro-magmatic (?) eruptions (Di Vito et al., 1999; Smith et al., 2011; Isaia et al., 2015). The most recent eruption occurred in 1538, producing the cone of Monte Nuovo (Di Vito et al., 2016)…"

For completeness, add citations to previous work on resurgence at Campi Flegrei, including: Luongo et al. (1991), Orsi et al. (1996, 1999) and Acocella (2010).

Reply: We agree and are willing to do that.

Subsidence and uplift before the 1538 eruption

Lines 103-368. This key section presents an exhaustive evaluation of historical observations on ground movement at Campi Flegrei. However, it is not clear which observations are new and which are referring to previous studies. To highlight the new material, it would be helpful to have:

1. A paragraph identifing previous interpretations, by bringing forward Fig. 13 a and b (and related text) to provide a starting reference. I would add also the trends proposed by Dvorak & Mastrolorenzo (1991), Bellucci et al. (2006) and Morhange et al. (1999, 2006).
2. A clear statement at the end of the section that specifies the novel results from this study. For example, how new are the trends for the via Herculea (Fig. 3) and Serapeo (Figs 6 & 13c)?

Reply: We thank the reviewer for the useful suggestions and are willing to incorporate them.. However, the observations regarding via Herculea and the trends at Serapeo are entirely new and well supported by historical sources.

Lines 109-110. I would avoid claiming that this paper is "correcting misrepresentations or erroneous reconstructions". This is a value judgement and may be viewed by readers as hostile. Why not say something like "the new data allow more detailed (?) reconstructions than have previously been possible and, as a result, provide tighter constraints on the mechanisms driving unrest." Such an approach conveys the positive idea that this paper in *building on* previous work, rather than *correcting* it.

Reply: Ok, we will make the changes accordingly. However, science progresses through the gradual correction of incorrect hypotheses, based on new observations. As non-native English speakers it is possible that the formal language we used to express this evident concept may have sounded somewhat unclear. We will adjust our statements in line with the phrasing suggested by the reviewer.

Lines 128-171. Only a single source (Parascandola, 1943) has been cited for the descriptions here. If this is correct, then what advances have been made by the current paper? If this is not correct, can the authors identify the sources of their new data?

Reply: Most of the historical sources are actually included in the Appendix, which provides the references for each numbered point shown in figure. However, we agree that including them in the main text as well would improve readability. We will make this adjustment.

In Fig. 3, the different trends in ground position shown by the continuous and dashed lines need to explained in the caption; similarly, what do the numbers 1-10 indicate? Which measurements have been published before (*e.g.* by Parascandola (1943)?) and which are new to this paper? If the original data have been moved to Supplementary Material, why not add a table here that gives short descriptions?

Reply: As previously explained, the sources are provided in the Appendix. However, we can include a table, in the main text, with brief descriptions and corresponding references.

Line 197 (and Line 116). It may help to include a subheading (not necessarily numbered) that the following text has switched observations of ground movement from Via Herculea to the Serapeo. If so, an equivalent subheading for Via Herculea could be inserted around Line 116.

Reply: Ok, we can do that.

Lines 238-265. These lines describe Fig. 7. The text is virtually identical to that in Bellucci et al. (2006, pages 149-50), for which Rolandi is co-author. There is no need to repeat the text at the same level of detail: it can be shortened with reference to Bellucci et al. (2006), which ought to have been cited in the first place.

Reply: Ok, we agree and will do that.

The numbered sources in Fig. 6 need to be identified and not relegated to Supplementary Material. Why not add these to the table already recommended to accompany Fig. 3? What is new about this plot? How does is differ from previous measurements? How are the dates on the right of the graph related to the trends? In particular, nine dates are given from 1430 to 1538, yet only three points are shown defining the uplift between c. 1430 and 1538: why have more dates been shown than the number of points?

Reply: The dates on the right, in black, indicate the occurrence of the main eartquakes. We made an error with the first date, which is listed as 1430, but should be corrected to 1446. We will certainly correct this mistake and clarify the caption regarding these dates. Additionally, we agree to include two tables in the main text to provide the sources for the data points shown in figures 3 and 6.

An important omission is the interpretation of ground movement by Bellucci et al. (2006) and which the present authors have used in subsequent publications (*e.g.*, Troise et al. (2007, 2019)). It includes a possible uplift around 700-800 AD, following archaeological evidence presented by Morhange et al. (1999). Although Morhange et al. (2006) later removed this uplift without explanation, no clear evidence has been presented to resolve whether or not the uplift occurred. Any new data in Fig. 6 are thus *especially significant* because they offer an opportunity to clarify this point. Indeed, Fig. 6 shows horizontal arrows to highlight two measurements that happen to show continued subsidence across the relevant time interval. Is this deliberate or a coincidence? An explanation is necessary here.

Reply: The two arrows mark the limits of submersion, as indicated by the range altered by lithodomes. Historical chronicles clearly show that the submersion progressed during the time interval between the 6^{th}-7^{th} centuries and the 9^{th}-10^{th} centuries.in the 6^{th}-7^{th} centuries, the citizens of Pozzuoli were forced to abandon the lower part of the city, which had been invaded by water. By the 9^{th}-10^{th} centuries, subsidence had become even more pronounced, leading to the formation of Lake Agnano in the Plain of the same name.

Lines 295-312. Please add references to previous studies in the main text: don't hide them in figure captions. Can the authors specify where they have added their "own reworking" and so highlight the advances in this paper?

Reply: In fig.8, only panel A reproduces the original figure by Soricelli (2007). The panel B, redrawn by us, focuses on a specific part of the area shown in A (as already mentioned in the caption), which was partially submerged in 1430 as depicted in panel C. Panel C was reconstructed and drawn by us, with the primary source for this reconstruction being De Jorio (1820), an observation also cited by Dvorak and Mastrolorenzo (1991). We agree to include the main references in the main text for clarity.

Lines 318-328. The authors present evidence why they believe some inferences about ground movement made by Di Vito et al. (2016) need to be revised. Can they suggest which observations led Di Vito et al. (2016) to estimate different values? As for their comments on Claims (2) and (3), it is notable that the trend shown in Fig. 6 from the 12th Century to 1538 supports that presented by Bellucci et al. (2006, Fig. 7), which should be referenced to avoid giving the incorrect impression that all the data are new to this paper.

Reply: No, we were unable to identify, either in the main text of the Di Vito et al. (2016) paper, or in its supplementary material, the observations that led these authors to propose the three mentioned interpretations. However, we certainly agree to reference the paper by Bellucci et al. (2006), which has already been cited multiple times in our paper including in this specific context, at line 328.

Lines 336-338. The inference of a maximum subsidence in the 1400s is not new to this paper. Dvorak & Mastrolorenzo (1991) and Bellucci et al. (2006) reached the same conclusions: at best, this paper confirms their results (although the similarity with Bellucci et al. (2006) may also indicate that some results are being repeated, especially since the lead author here was a co-author on the earlier paper).

Reply: Certainly, this paper, in some sections (though not all) supports the findings of Bellucci et al. (2006) and, to some extent,, by Dvorak and Mastrolorenzo (1991). However, in this study we provide additional and more detailed historical references. This approach aims to eliminate any ambiguity that may have led subsequent authors to propose differing interpretations (e.g. Di Vito et al., 2006).

Lines 341-346. The occurrence of seismicity is out of place. I would move the observation to a general discussion after the sections that describe the seismicity more fully. The new paragraph could then begin directly with "Our findings..." (Line 346).

Reply: Ok, we can do that anyway.

Line 358-9. As above, the comment on seismicity can be moved to a later discussion.

Reply: Ok, we can do that.

Lines 366-8. The comment about the “anomalous” pre-eruptive uplift at the site of the Monte Nuovo eruption appears without context. I suggest mentioning this behaviour at the start of the reconstruction and then explain why it can be neglected when investigating ground movement across the caldera.

Reply: Ok, we agree to do that.

Ground movements after the 1538 eruption

Lines 370-428. This section describes subsidence between 1538 and c. 1950. To emphasize the relevance, it may help to compare rates of subsidence with those before 1538 and, perhaps, draw conclusions about whether conditions in the crust before the 1430-1538 uplift might have been similar to those before 1950, hence further supporting the idea that the two sequences may have been driven by similar subsurface processes.

Reply: We thank the reviewer for the suggestion. Indeed, we observe similar rates of subsidence before and after the uplift phase associated with the 1538 eruption. Additionally the uplift rate observed after 1950 is of a similar order of magnitude, slightly lower thus far, compared to the average uplift rate prior to the 1538 eruption.

Schematic model for the preparatory phases of the 1538 eruption

Lines 448-455. I would adjust the text to argue that the resurgent-block model is *consistent* with observed ground movement and seismicity, but is not the only possible interpretation. Numerous models have accounted for the observations without involving block movement (among others, Berrino et al., 1984; Bianchi et al., 1987; Amoruso et al., 2008, 2014; Woo & Kilburn, 2010). The authors could then explain the significance of whether or not block movement is significant to determining uplift before 138 and since 1950 (bringing forward text from Lines 472-478).

Reply: As previously explained in response to a similar comment by the reviewer we believe that the resurgent block model is well constrained by the ground deformation and seismicity patterns recorded over several decades in recent times, as well as by reported observations from ancient times. Regarding the models that attempt to explain ground deformation and seismicity without invoking subsidiary movement along the ring faults bordering the resurgent block, the models proposed by Berrino et al. (1984) and Bianchi et al. (1987) presented significant drawbacks, as demonstrated since the 1980s (see De Natale et al., 1991). These limitations formed the basis for the first studies suggesting the role of ring fault movements (see De Natale et al., 1993; 1997). The paper by Amoruso et al. (2008; 2014) aimed only to explain the ground deformation patterns, not seismicity, and relied on an 'ad hoc' layered model to simulate the observed shape of ground deformation. The issue with such models is that they are generally designed to explain single ground deformation events, but it is far more challenging to account for the remarkably consistent ground deformation and seismicity patterns observed over decades (or potentially centuries). This difficulty is compounded when considering likely changes in source and/or medium properties over such extended periods. We can clarify this this point. Importantly, the concept of resurgent block model has been widely used since the earliest studies of caldera unrest. In this paper, our aim was simply to refine the identification of the moving block by integrating the most constraining data. However, this is not the primary focus of the paper.

Lines 455-471. I would move the description of subsurface structure to the start of the paper, as part of the background context.

Reply: This section has been included here because the involvement of a resurgent block naturally emerges from the description of both secular and recent ground deformation, based on comparisons bwtween historical sources and recent observations. The detailed description of all the observations and data supporting the existence of the resurgent block is logically linked to this purpose. However, depending on the editor's decision, we could re-arrange the paper by presenting this evidence in the introductory section, and then referencing it here. That said, we do not believe the current structure is unclear.

Lines 475-483. Could the authors clarify the contradiction in proposed substructures? The authors argue that tuff occupies the upper 1.5-2 km of the crust (Lines 474, 482 and 499). How does this contradict the "layer of loose pyroclastics from recent eruptions" (Lines 476-7)? How "loose" will these deposits really be 1.5 km below the surface? Will they behave differently from a tuff in any significant way? Presumably the upper levels **do** contain deposits from recent eruptions too.

Reply: The tuff within the deposits inside the resurgent block is lithoid, with a compression strength of approximately 30-50 kg/cm^2), whereas the loose pyroclastics, even when compacted are not lithoid, having a compression strength below 10 kg/cm^2). Therefore, they are clearly distinguishable. While it is true thatthe upper levels contain deposits from more recent eruptions, the upper level of the yellow tuff is a clear indicator of the block resurgence.

Lines 491-507. The text would be easier to follow by starting with field data that support the presence of a thermometamorphic horizon (*e.g.*, borehole and gravity data (Rosi & Sbrana, 1987)) and fluid filled rock at shallow depth (doesn't this correspond to Campi Flegrei's hydrothermal system? If so, just say this and cite suitable papers) and *then* speculate on its origin. I don't see that the discussion on Lines 491-498 adds anything to the argument. It could be omitted, perhaps moving the citation to Vinciguerra et al. (2006) elsewhere.

Reply: This section aims to clarify that, with the 3-4 km depth range, there exists a thermo-metamorphic low permeability layer, that confines the upper, high permeability zone. This zone can accommodate gas overpressure from below, which occasionally ruptures when pressures become too high , causing the injection of supercritical gas into the upper layers. The tests conducted by Vinciguerra et al. (2006) are particularly significant as they suggest that earthquakes are likely confined to the lithoid tuff zone. Nonetheless, we are willing to clarify this point further in a revised version.

Lines 519-522. Why a mush? The authors have argued that renewed uplift may have occurred during c. 1400-1538 and since 1950. If driven by magma intrusions, then presumably the maximum volume of magma intruded corresponds to the amounts associated with these uplifts. Would not the amount involved have fully solidified by now?

For example, the slowest rate of heat loss is expected to be by conduction. The thickness of a solidified layer is on the order of $(4kt)^{1/2}$, where the thermal diffusivity k is ~ 4×10^{-7} m^2 s^{-1}, and time t is in seconds. For a single sheet-like body, the thickness will thus be about 7 m in 1 year, 22 m in 10 years, 35 m in 100 years and 80 m in 500 years. Since cooling occurs across the upper and lower boundaries, the total thickness solidified will be about twice these values. If the surface area of the sheet is ~10 km^2 (*e.g.*, a circular sheet c. 2 km in radius), then the corresponding minimum volumes of magma involved are 0.15, 0.45, 0.7 and 1.6 km^3. In others words, an intruded volume of 1.6 km^3 before 1538 may have completely solidified before the return to uplift in 1950. At the other extreme, an intrusion of as much as 0.6 km^3 in 1984 would have solidified before the return to uplift in 2004.

Although these calculations are approximate, they suggest that it may be difficult to preserve a mush layer at depths of 2-4 km over the required time intervals; indeed, the authors suggest as much when discussing the uplift since 1950 in Lines 594-631. If this is correct, can the authors address the apparent contradiction between mush and full solidification and, if necessary, propose an alternative scenario that does not depend on the existence of mush at such depths?

Reply: According to Bachmann and Bergantz (2006), as cited in our paper, crystallized magma (which could equivalently be referred to as hot crystalline mush, since solidification is a continuous process), can be mobilized by the inflow of exsolved fluids coming from mafic magmas, a process known as 'gas sparging'). Therefore, the mechanisms described are sufficiently general to be independent of the degree of cooling of the original magma. However, we can further clarify this point to make it easier for readers to understand.

Lines 529-534. Subsidence has been occurring between 3,700 years ago and 1538. Can the authors offer some independent checks for consistency? For example, if the rate has been similar to that in historical time (1.5-2 m per century), then ground level 3,700 years ago must have been some 50-60 m higher than in 1538. Is this consistent with the reconstruction of prior resurgence?

Reply: There is no data available to constrain ground subsidence before the 2nd century AD. However, if the rate of subsidence had been of 1.5-2 m per century, the ground level

3700 years ago would have been approximately 30-40 m above sea level. We do not see the relevance of this point.

Lines 537-546. Is the level of detail necessary here? None of the specified processes can be demonstrated to have operated at Campi Flegrei. I would simply postulate that a mush zone exists – perhaps referencing more direct evidence from the seismic-tomographic analysis by Zollo et al. (2008), who argued that the crust at these levels contain dispersed patches of molten rock. Additional papers can be left as citations.

Reply: Ok, we can simplify the discussion here.

Lines 546-551. I found the text confusing. Do the authors mean that the rapid uplift between 1430 and 1538 could have been caused by the release of gas? If so, why not just state this in a single sentence?

Reply: The uplift between 1430 and-1538 was likely caused by a combination of gas inflow and magma inflow. The mechanism described here can support both processes.

It may be of interest that Mormone et al. (2011) have presented evidence from melt inclusions that for deeper magma entering the "8-km" reservoir (mush zone?).

Reply: Yes, this suggests new magma inflow into the 8 km reservoir, which could be the mechanism driving the present unrest.

Lines 552-583. The description jumps back and forth between different levels in the crust. I would restructure and shorten the text – perhaps following progress upwards from the "8-km" reservoir.

Reply: Ok, we will rearrange the text here to be clearer and concise.

Lines 552-554 could form part of a revised introduction to the Campi Flegrei's subsurface structure. Links to Yellowstone can be removed, or moved to a later discussion that compares *in a single section* the authors' model of Campi Flegrei with other volcanoes.

Reply: OK, we can move this part to the discussion

Lines 557-561. Can the authors connect their description of saturated rock to the hydrothermal system that (according to the literature) exists at similar depths?

Reply: Yes, we are obviously inside the hydrothermal system, here.

Lines 567-8. These lines need to be adjusted. Kilburn et al. (2023) proposed common depths for magma intrusion and gas accumulation *below* the thermo-metamorphic horizon at depths of c. 3 km (and not beneath a shallower cap-rock). I would also suggest changing how the comment is presented. For example: "We consider that magmatic gases may not necessarily be restricted to below the thermo-metamorphic horizon (Kilburn et al., 2023), but may instead accumulate at shallower levels beneath the "summit" lava at a depth of c. 2.5 km." In general, I would avoid statements that previous work is "wrong" or "incorrect". After all, the authors have **not** demonstrated this: they have instead proposed an alternative view for consideration (which may or may not turn out to be an improvement).

Reply: Ok, we can make this change in a revised version.

Another constraint to consider is whether the proposed depth of 2.5 km for gas (and pressure?) accumulation is consistent with depths closer to 3-3.5 km estimated by geodetic modelling of uplifts in 1982-84 and since 2004 (*e.g.*, Amoruso et al., 2008, 2014; Amoruso & Crescentini, 2022).

Reply: These depths are probably not distinguishable within the errors due to lack of detailed knowledge about the properties of the medium and the secondary movements of the ring faults. However, most of the papers cited in this study suggest a source depth within the range of 2.5-3.0 km.

Line 567. The term "summit" lava is distracting. Why not refer to the "lava level at 1.5 km" (or something similar) to connect with the lava mentioned in Line 560-1. Moreover, could this unit be an intrusion rather than a "lava", which implies that it flowed over the surface?

Reply: Yes, we can change the term, to be more general.

Lines 572-645. The ideas described are interesting, but have jumped to processes operating today, rather than before 1538, which is the title of the subsection. I suggest reorganising the text to appear as a separate section focussing on processes operating today, followed a separate discussion comparing the pre-1538 and modern behaviour (see potential connection to Lines 838-856 below)..

Reply: Ok, we can follow this suggestion.

Lines 594-635. The authors describe the intrusion of magmatic sills (presumably at the depths of 2.5-4 km (Line 602)?) and solidification of these sills within 20 years (Line 610). This conclusion is based on previous work. It appears to contradict Lines 546-551, which argue that gas transfer is the mechanism driving unrest. The connection between gas release and magmatic intrusion needs to be clarified – and succinctly: to be honest, I think the description could easily be reduced by 40% (which would force the authors to focus on essential features). Fig. 16 is attractive, but essentially shows collections of arrows indicating an upward transfer of pressure. Which arrows refer to gas transfer and which to magma movement? I would redraw simplified figures that specify the features described in the text. There is no need here to rely on speculative generic models, such as that proposed by Bachmann & Bergantz (2006): the figure can be based solely on the evidence presented in this paper.

Reply: As previously mentioned, we believe the uplift before 1538 was caused by both gas and magma intrusion. Therefore, there is not contradiction with our earlier statements. We can make these parts clearer and more concise.

Line 585. See comment on Lines 567-8: Kilburn et al. (2023) placed the zone of pressurization *below* the thermo-metamorphic horizon.

Reply: Ok, we will make the correcions accordingly.

Lines 587-593 can be omitted*. All they do is introduce what is about to written next. Go straight to the next paragraph. (*Or perhaps a shorter version can be moved to the conclusions?).

Reply: Ok, we can omit this part here and move a shorter version in the conclusions.

The eruption of 1538

Lines 647-719. Why is this section included? The eruption of Monte Nuovo doesn't contribute to the rest of the paper? It is really a review of previous work and does not present new information relevant to the main arguments of this paper. (N.B. In Fig. 19, Wohletz's figure of magma-water interaction and eruptive explosivity has previously been applied to Campi Flegrei. I can't remember the authors or journal, but believe it was published in the late 1980s or 1990s.).

Reply: As mentioned earlier in response to a previous question, we want to emphasize that the account of Mt. Nuovo eruption directly contributes to the goals and conclusions of this paper, which aims to accurately describe the events before, during and after the 1538 eruption, in order to provide insights intowhat could occur in the future. In facy we highlight some observations that have never been reported in previous studies, which are also important for assessing what might happen during a future eruption.

The seismicity before and after the 1538 eruption

Lines 731-822. The connection between earthquake magnitude and intensity is extremely interesting. Again, though, I think too much detail has been added. It breaks the structure of the paper and loses sight of the key objectives. Figures 18 and 19 are the key results and the text could be shortened so that they become more prominent. Thus I would (1) move the methodology to supplementary material; and (2) tabulate the sequence of events, because they have already been described comprehensively by Guidoboni & Ciuccarelli (2011). I expect the whole section could be reduced to two figures, one table and half a page of text.

Reply: Ok, we can follow this suggestion.

Post-eruption seismicity

Lines 825-835. How relevant is this section? It hasn't clearly been connected to understanding the processes operating beneath Campi Flegrei.

Reply: This brief sectionis important because it shows that seismicity does not necessarily cease after an eruption and can continue for several decades. This constitutes a crucial scenario for understanding the present unrest, with clear implications for civil protection..

Comparison of precursory phase of the 1538 eruption with current unrest

Lines 838-856. If the authors could present a graph showing the seismic behaviour during unrest since 1950, this paragraph could then be moved to the end of the revised description of pre-1538 seismicity (Figs 18 & 19).

Reply: Ok, we can take into consideration this change.

Lines 858-918. The alternative scenarios are a good idea, but not clearly connected with the picture of gas and magma transport the authors have been developing. This is a missed opportunity.

Reply: we can make the connection clearer, but the goal is to highlight the alternative scenarios.

The discussion of future vent location and final rapid ascent of magma (Lines 871-894) is interesting but does not naturally follow from the previous text. It thus seems arbitrary and can be removed.

Reply: We do not agree with the reviewer on this point. We can clarify further, but the key point is that the most likely location for a future vent is along the ring faults surrounding the resurgent block. Among these, the part of the ring fault exhibiting the maximum shear stress, i.e. the Solfatara-Agnano area. This area also has important implications for civil protection.

The same holds for Lines 897-918. This section passes comment on previous work – especially Kilburn et al. (2023) – without evidence or serious analysis. If the authors wish to propose alternative views, they should do so in a separate paper that presents an in-depth analysis; in any case, these views have nothing to do with the present paper and are not supported by the data in the preceding text. [For the record, the statements in Lines 904-914 are simply wrong: (1) the seismic time series *did* change in 2015-17 and, as we know now, evolved towards a full seismic crisis; (2) the *corrected* uplift in 2015 *had* reached its 1984 level; and (3) major rupturing *did* begin in August-September 2023.] .

Reply: We understand the issue, as the reviewer is also the author of the paper. However, what is stated here is almost self-evident and supported by the evidence: there has been no eruption nearly 0 years after the supposed start of the 'critical phase' (which in other volcanoes, as demonstrated by the author in several papers, typically anticipates an eruption within a very short time). The reason we referenced the 2023 paper is that his model of critical stress is very important, and we could not cite the 2017 paper without also mentioning the 2023 follow-up. To address the points raised:

1) Seismicity at Campi Flegrei has increased since 2006, both in magnitude and in frequency, following the stress increase model (though this model does not account for the strong dependence of seismicity on the rate of deformation, which is very evident at Campi Flegrei). However, an eruption did not occur, even 10 years after the supposed entry into the 'inelastic state', whereas, for instance, at Rabaul the same authors found the eruption occurred only 2-3 years after entering the inelastic state, and after much less time at the other studied volcanoes (Robertson, R. M. & Kilburn, C. R. J. Deformation regime and long-term precursors to eruption at large calderas: Rabaul, Papua New Guinea. *Earth Planet. Sci. Lett,*. **438**, 86–94 (2016)).
2) Correcting the uplift compensating it for the average secular subsidence rate implies that secular, slow subsidence is caused by the same source mechanism of the rapid uplift during unrest periods. However, the mechanism behind the present unrest is likely gas inflow, or possibly magma; secular subsidence, whose mechanism remains unclear, should be due to different factors, like the progressive cooling and volume reduction of the main magma chamber.
3) The same considerations as in point 1.

However, we do not believe that quoting the 2023 paper and its likelihood is essential for the goal of this study. Therefore, we can remove this part.

It would be more interesting to see how the revised analysis of events before 1538 might better constrain what is happening today. As I understand it, the authors' pre-1538 reconstruction suggests early uplift driven by gas transfer, followed by magmatic intrusion and eruption (comparison with the model of Bodnar et al. (2007) and Lima et al. (2009) may be valuable here). If this is correct, can the authors identify comparable stages today – or is the modern sequence significantly different in some way? Given the large amount of work in preparing the reconstruction, it seems a pity not to take full advantage of the new results.

Reply: We thank reviewer for the suggestion, and we can do that.

Figures

Most of the figures are relevant and of high quality.

Figures 3 & 6. The time axis is labelled as “Centuries”. This can cause confusion, because the century may be taken to be the preceding hundred years (for instance, in standard English usage, the fourth century refers to 1300-1399). I would replace the times with actual years (as done in Fig. 13).

Fig. 15. Consider changing the colours to make the shapes outlining zones clearer on the map (especially the red of the caldera outline).

Please check language. Some terms remain in Italian: *e.g.*, in Figs 3, 6 and 13, time "AC" should be "AD" (use either the combinations BC and AD, or BCE and CE ("Before Common Era" and "Common Era"), and in Fig. 13 "duttile" should be "ductile".

Language

The English is generally good, but contains grammatical oddities that could be spotted with help from a native English speaker. I would avoid phrases that describe interpretations as "likely". Their use may be mistaken as attempts to support uncertain interpretations without sufficient evidence. They are not necessary here.

References

Please check that all references are in alphabetical order (*e.g.*, Lines 1013-1024 need to to be brought forward), and also some of the dates that have a number missing (*e.g.*, Line 1010). Also, check that citations in text are presented in *chronological* order.

Recommended Additional References

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Citation: https://doi.org/10.5194/egusphere-2024-2035-RC1

Reply : We can improve the figures, language and references as suggested.