nature portfolio

Peer Review File

Gas buffering of magma chamber contraction during persistent explosive activity at Mt. Etna volcano

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

26th Apr 23

Dear Dr Carbone,

I hope you are well?

Your manuscript titled "Gas buffering of magma chamber contraction during persistent explosive activity at Mt. Etna volcano" has now been seen by 4 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. In particular, they commend highly the inter-disciplinary nature of your study. However, they have raised quite substantial concerns that must be addressed, many of which are techical in nature. In light of these comments, we cannot accept the manuscript for publication in its current form, but would be interested in considering a revised version that fully addresses these concerns. As you revise your manuscript, please also consider the following editorial thresholds:

- Provide compelling evidence that volatile exsolution can explain the observed dichotomy between erupted and geodetic volume and mass changes under physically realistic conditions, and discuss the implications of any assumptions made (e.g. the effect of magma compressibility).

- Verify the equations used in all calculations and provide justification for all input parameters and starting conditions used.

- Present a clear and logical narrative where necessary information is introduced at the relevant position in the text; this may require some restructuring.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will 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.

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

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter) and any completed checklist:

[Link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Emma Nicholson, PhD
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Joe Aslin
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Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements (Download the link to your computer as a PDF.)

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This study investigates gravity and deformation changes at Etna and discusses exsolution of volatiles as potential cause for the discrepancy between erupted volume, deformation, and gravity measurements. Overall, the idea of the study is relevant and brings a novel approach to discuss the discrepancy between erupted volume and deformation. Their conclusion is a reasonable explanation for the observed data; however, I think the paper would benefit from discussing if any other explanations are possible and what assumptions have been made during the course of the study. Furthermore, I believe the paper has a mistake in one of the equations and discusses several points only in the abstract and conclusion. I would recommend this study for publishing after some edits have been considered by the authors.

1) Several points (including deformation vs eruptive volume) are only mentioned in the abstract and the conclusion. I find it uncommon to mention discussion points only in the abstract and the conclusion section. I would shorten the conclusion to summarize the main conclusions and how this could lead to further research, and move some of the discussion points into the main section of the paper. However, if there are cultural differences between what can/should be written in a conclusion, please ignore my comment here.

2) Following comment 1, I think the paper would benefit from a discussion paragraph that helps tie everything together. You explain well how the discrepancy between the gravity measurements and the deformation data can be explained by exsolution of volatiles that buffer the deflation. Could you add here on how eruptive volume and deformation are related and their discrepancy instead of in the conclusion? Could you explain how eruptive mass and gravity measurements are related and if comparing them supports your hypothesis?

3) How do your inversion results compare to previous studies of the depth and volume of the magma reservoir? Have previous studies also estimated the reservoir to be spherical and at 2.7 km b.s.l.?

4) I believe equation 5 is not correct.

Let's use as simple example to show that the equation as written does not lead to reasonable results. Let's assume we mix 1 L of water with 1 L of water:
$\rho_1=1\ [g/cm^3],\rho_2=1\ [g/cm^3],n=0.5$ (where n is the weight fraction of the second liter of water in the mixture).

Plugging into your equation 5 would give:
$\rho_{mix}=n/\rho_2-(1-n)/\rho_1=0.5/1-0.5/1=0$ g/cm3 - the density of water mixed with water clearly does not have a density of 0.

I believe the right equation should be:
$1/\rho_{mix}=n/\rho_2+(1-n)/\rho_1$

Derivation of equation:
($\rho=m/V$ where m is the mass and V is the volume)

$1/\rho_{mix}=V_{mix}/m_{mix}=(V_1+V_2)/(m_1+m_2)$, replacing V with m/ρ gives $1/\rho_{mix}=(m_1/\rho_1+m_2/\rho_2)/(m_1+m_2)=(m_1\rho_2+m_2\rho_1)/(\rho_1\rho_2)\times 1/(m_1+m_2)=$

$(m_1\rho_2+m_2\rho_1)/(\rho_1\rho_2(m_1+m_2))=(m_1\rho_2)/(\rho_1\rho_2(m_1+m_2))+(m_2\rho_1)/(\rho_1\rho_2(m_1+m_2))=m_1/(\rho_1(m_1+m_2))+m_2/(\rho_2(m_1+m_2))$

Converting the mass of substance to into a mass fraction n: $n=m_2/((m_1+m_2))$ and $1-n=m_1/((m_1+m_2))$

Then we have $1/\rho_{mix}=(1-n)/\rho_1+n/\rho_2$ (substance 2 would be the gas phase in your example)

Please make sure to not only correct the equation in your paper but recalculate your results if necessary!

5) How would recharge or volatile fluxing from a deeper reservoir affect your results? If you are not considering these possibilities in your study, please state that you are assuming that no recharge or volatile fluxing is occurring, but that this might affect your results. Also, do other studies (for example the volatile studies you cite) support no volatile fluxing?

6) On page 6, you are mentioning several possibilities that could cause the discrepancy between deformation and gravity measurements. However, you are only discussing the possibility of exsolution of volatiles for the rest of the paper. How would any of the other possibilities affect your results? Is

there a reason why you are not discussing them further? If you are assuming that magma compressibility, filling/emptying of void spaces, mixing of magmas of different densities does not occur, how do you justify these assumptions (plus please mention in the paper that you are making these assumptions). Your main point is that there is an exsolved volatile phase that buffers the system. This makes magma “highly” compressible so it seems odd that you are not incorporating it after mentioning it. Your hypothesis that the discrepancy between deformation and gravity can be explained seems reasonable to me; however, I think the paper would benefit from a short discussion on how other factors (like compressibility or recharge) could affect your model and whether there are any other possibilities that could explain your observations.

7) Please explain on what data the initial concentration of H_2O and CO_2 are based. Based on your citations, the data is from melt inclusions. However, since these values are significant for your study, I would recommend to elaborate in a few words.

8) Table 1: I would recommend writing large numbers in scientific notation. Also, make sure all digits are significant.

9) Table 2: unit issues, significant digits, citation issues: These are not wt%. They might be weight fractions? Also, please double-check your citation. I believe you used Métrich et al. (2004) Table 2 sample 2a data here (not Moretti as written, also it is cited in text and table caption). Finally, TiO_2 values seems wrong. It should be either 0.018 (in weight fraction) or 1.8 wt%. Please make sure you have used the correct value in your calculations. It is common in petrologic papers to use only two digits after the decimal point. Please consider deleting extra digits (issue will be resolved once converted to wt.%). It might be worth stating that this composition was measured from a melt inclusion (and is not bulk composition or matrix glass).

10) Is the volume in line 211 converted to DRE (dense-rock equivalent)? Please state if so. How does that affect your conclusion otherwise?

Some further minor suggested edits:
-Line 33: Please define RF frequency.
-Figure 1: The axes labels (longitude/latitude) are not centered. Is this on purpose? The same is the case in some other figures.
-Line 37: Consider replacing the word “sketch”. “Map” or so might be more appropriate here.
-Line 42: Consider replacing “line” with “rectangle”.
-Figure 2: Why did you choose June 5th (last lava fountain of previous series) as starting date instead of June 15th (first lava fountain of the 38 fountains under investigation of the current study)? Especially in subfigure b, the described trend doesn’t seem to start on June 5th. Could you add a sentence detailing your reasoning for non-gravity experts?
-Line 58-59: “10 sec and 1 day averages are shown” is written twice here. Consider rephrasing this sentence to make it more concise. Also, I think “1” and “day” are hyphenated (1-day average)
-Line 60: The word “closer” implies a comparison. Did you mean “closest”?
-Line 73: Same as line 60. Did you mean “closest” instead of “closer”?
-Figure 3: It is almost impossible (especially in subfigure b) to distinguish between observed and calculated arrows on a black-white copy of the paper. Consider choosing different colors or change the line style to make it easier to distinguish. Subfigure c is very small.
-Line 94: Clarify the word “that”.
Equation 1: The left-hand side of the equation uses δ where the right-hand side uses Δ. I think they should be consistent on both side of the equation.

-Line 115 and 116: I think it is uncommon to use "*" to signify multiplication in science paper. I would recommend using "x" or so throughout the paper.
-Line 190: Is your variable D different from d in equation 1? (top vs middle of reservoir maybe)? Could you clarify or use the same variable name if they are equal?
-Line 199: Consider replacing "conclusive" with "concluding". There is a small difference in their meanings.

Reviewer #2 (Remarks to the Author):

Carbone et al., 2023, Nature Communications Earth & Environment - Review

"Gas buffering of magma chamber contraction during persistent explosive activity at Mt. Etna volcano"

In this paper, the authors present geodetic observations (gravity and ground deformation) during a period of eruptive activity at Mt. Etna in 2021. The study is focussed on a two-month period (June-July), over which the geodetic data is characterised by a decrease in the gravity field and subsidence. The authors invert the ground deformation and gravity data using an analytical model (Mogi) to calculate the volume changes associated with the observations. There is a discrepancy between the derived volumes (less deformation observed than suggested by mass loss), which the authors argue indicates that magma withdrawal is not the only mechanism occurring during eruption. The authors go on to explore an alternative mechanism, volumetric buffering through volatile exsolution and expansion, which forms the basis for the remainder of the study. Results from the geodetic inversions are used to derive density changes as a function of the pressure changes within the source. Notably, independent geochemical analyses (using silicate melt composition data) are used to constrain the pressure and volatile content (H_2O and CO_2) of the system. The authors finally comment on the validity of the pressure range obtained and the potential mechanical failure of the magmatic system.

The manuscript presents a novel investigation of geodetic observation (including short-term gravimetry) constrained by geochemical modelling. While at times the approaches are simplistic (e.g., assuming a Mogi deformation source, rather than a finite source geometry), this study provides a good basis for furthering integrated geochemical-geodetic modelling at both Mt. Etna and other volcanoes that are suitably instrumented.

The manuscript is generally well written. However, there are several points where the authors should loop their results/interpretations back to the observations and their primary argument. This would improve coherence, helping to lead the reader through the study. There are occasional spelling and grammatical errors within the manuscript that should be addressed, and some sentences could be simplified/clarified.

Within the "Interpretation" section, the authors comment on the volume change associated with the gravity inversion (Lines 124-126), but neglect to include the density (ρ) used in the calculation ($\Delta M/\rho$). Please include the density value used and show the resultant volume change ($\Delta M/\rho$) for the reader.

The data presented in Table 2 and used in the independent geochemical modelling are obtained from Moretti et al. (2018) and pertain to eruptive products from the 2001 eruption of Mt. Etna. This specific information is missing from the text and should be included.
Furthermore, Moretti et al (2018) provide 4 geochemical analyses for the 2001 eruption.
• What was the rationale for picking this single analysis?

• Do the other analyses produce any significant variations in the H_2O/CO_2 curves derived from SOLWCAD model?
• Are there any changes in the estimates of ΔP from the intersection with the results of the geophysical inversion?

The volume of erupted products over the period in question is first mentioned within the conclusions. Given that erupted volume vs geodetically-derived volume loss is a key point of the abstract, it should be first mentioned much earlier; preferably alongside the data inversion results.
• If the erupted volumes of the fountain events are approximately known (stated as $1\text{-}2\times10^6$ m^3 on line 211), it would be of interest to the reader to see cumulative eruptive volumes through time in an additional panel in Figure 2, even if just for the early June-late July period.
• How much of the difference between total erupted volume ($20\text{-}40\times10^6$ m^3?) vs geodetically-derived volume loss (2.23×10^6 m^3) could be due to vesiculation? I.e., what is the difference between the DRE and the geodetically-derived volume loss?

Based on the above evaluation, I recommend the manuscript for minor revisions prior to acceptance. Please find some line-by-line recommendations below.

Signed,
Matthew Head, University of Illinois at Urbana-Champaign

Abstract

Line 9-11: When did these eruptions occur? This sentence could be restructured for clarity – leading with the eruptive activity, followed by the observations.
"Mt. Etna erupted more than 20 times during a two-month period in 2021, and was accompanied by ground deflation and a decrease in the local gravity field."

Line 16-17: "… incompatible with a pure mechanism of magma withdrawal" – do you mean that the second point (ii) is incompatible with this mechanism (based on sentence structure, this is what is suggested), or that both (i) and (ii) together are incompatible? These sentences should be restructured for clarity.

Introduction

Line 26-27: "closely spaced lava fountain events" – Given the eruptions occurred from one summit crater (line 24), does this sentence refer to the timings? If so, please clarify.

Figure 1: Visual clarity of the figure could be improved.
• The box showing the region of the inset map is difficult to see, consider a different colour (e.g., red) or a thicker line.
• The markers/annotations of the gravity stations aren't very clear – perhaps bolder symbols/colours could be used for gravity, and paler for the GNSS?

Figure 2:
• Panels (a-c) could all be on one set of axes, like the displacement, making comparisons of the gravity change easier. Given the 10 sec/10 min averages are not used, do they need to be displayed in the main text?
• Horizonal gridlines will aid the reader in understanding the magnitude of changes in gravity/displacement with time.
• Text error in the GNSS annotation, should be 'EPDN' not 'EDPN'?

• I understand showing only a subset of GNSS stations in (d), however given that gravity station SLN is colocated with GNSS station ESLN, it would be helpful to see the deformation data for this station too.

Line 66: Striking, instead of streaking?

Line 73: "20 GNSS stations closer", closest?

Line 77: GNSS

Lines 77-79: This sentence can be simplified to aid the reader. Consider something like "the amplitude of the observed gravity signal decreases with increasing distance away from the summit."

Figure 3:
• What is the x-axis of panel c? Is this reverse distance from the summit crater? By ordering the data points by proximity to the crater, this will help illustrate the point in Lines 77-79 (see comment above).

Data Inversion Results

It should be noted that the inversions and calculations carried out are for the net change over the deflation phase indicated in Figure 2.

Line 107-112: These sentences are a bit confusing, I had to read them several times to get this meaning. Please clarify for the reader, and consider something along the lines of:
"It is important to note that the gravity data are not corrected for changes in elevation associated with the observed ground deformation. Instead, these corrections are carried out within the inversion scheme, using modelled vertical displacements."

Interpretation of the Inversion Results

Line 126-127: This would be a good place to assert that the differences in derived volumes are not consistent with a mechanism of pure magma withdrawal.

Line 131: "with magma of different density"

Line 132: "emptying"

Line 136: "Constraint"?

Line 153: "which computes"

Line 178-180: Part of the narrative of the paper is missing here, and needs to be expanded. Here the authors should explain that the exsolution/expansion of volatiles buffers the volume lost during eruption, and how it connects to the differences observed between the volumes derived from ground deformation vs gravity.

Conclusive Remarks

Line 218: Again, this is another good place to assert that the geodetic observations and derived volumes are "incompatible with a pure mechanism of magma withdrawal", and requires a component of volumetric buffering from volatile exsolution.

Reviewer #3 (Remarks to the Author):

Review of “Gas buffering of magma chamber contraction during persistent explosive activity at Mt. Etna volcano”

Summary:
The authors investigate a time series of continuous gravity and GNSS data from Mt. Etna during a 2-month period of volcanic activity characterized by ~20 short-lasting explosive eruptions. Gravity data show a decrease in the local gravity field coincident with ground deflation and contraction centered on the volcano summit. Gravity and deformation data are jointly inverted using a point source model, and it was found that the inferred change in volume (~2.3e6 m^3) is smaller than expected from the inferred change in mass (~ -1.5e11 kg) if the mass loss were assumed only to be a result of magma withdrawal (e.g. taking a magma density of 2600 kg would correspond to a volume loss of 56e6 m^3 for the given mass change). They propose that exsolution of magmatic volatiles during depressurization caused a density decrease in the magma reservoir that accounts for the observed gravity change. To test this idea, they construct some volatile solubility models in SOLWCAD for a magma with Etna composition. They compare the predicted density change from SOLWCAD to what they estimate they need from the observables (the dashed black line in Fig 4). The modeled density changes appear to intersect the required drho/dP over a pressure range of ~ -30 to -80 MPa. They conclude that these pressure changes are reasonable and that volatile exsolution can thus explain the observed gravity and deformation data.

Strengths:
A strength of this paper seems to lie in the acquisition of high-resolution continuous gravity data that seem not to need as much precise ground control as more traditional/older types of gravity instruments. In addition, the use of geodetic data to understand thermodynamic magma reservoir processes is commendable and of broad interest to the volcanology community.

Weaknesses:
Equation 3 states that the change in mass equals the volume times change in density. Why don’t volume changes contribute to dM? I would have thought that dM would approximately equal V*drho + rho*dV. If they assume constant volume, then doesn’t this violate equation 2 relating pressure change to volume change? The authors need to explain this or re-do the analysis considering volume change contributions.

A crucial piece of information to this analysis is the estimated volume of the erupted products. The abstract states that “the bulk volume reduction sensed by the ground deformation data is much lower than the volume of the erupted products,” but it isn’t until line 211 in the Conclusive Remarks that the volume of eruptions is provided. Line 211 states that each eruption was between 1-2 e6 m^3. For 20 eruptions, this corresponds to a total volume erupted of ~20-40e6 m^3. For a magma density of ~2600 kg/m^3, this corresponds to a mass change of ~5.2e10 - 1.0e11 kg. The inferred mass change from their inversion is ~1.5e11 kg, so there is actually only a discrepancy of ~0.5e11 kg in terms of explaining the mass change. Can density change during volatile exsolution account for this smaller fraction of the mass change?

It is well known that the volume of erupted products typically far exceeds the volume change of the reservoir. See Huppert and Woods, 2002; Rivalta & Segall, 2008; Kilbride et al., 2016; Le Mevel et al.

2016 JGR; Wasser et al. 2021 G-cubed; and Townsend, 2022 EPSL for various studies related to the discrepancy between geodetic signal and magma reservoir changes. Especially relevant is Rivalta and Segall (2008), which finds (equation 7) that the ratio of the erupted volume to volume change in the reservoir is equal to 1 + magma compressibility / reservoir compressibility. In the Etna study presented here, if we take the inferred volume change to be 2.3e6 m^3 from Table 1, this corresponds to a ratio of between ~8 and 18, which could be explained by having a compressible magma (perhaps 1e-9 1/Pa) within a relatively less compressible reservoir (perhaps 1e-10 1/Pa).

Given the possibility that magma compressibility (without the effects of pressure buffering by volatile exsolution included) can entirely explain the volume discrepancy, it is really the remaining mass discrepancy (~0.5e11 kg) that seems to warrant further study. I suspect it would be easy to come up with the remaining mass change as a density change by volatile exsolution.

Even if the analysis presented here to derive the drho_dP relationship were correct, Figure 4 does not sufficiently convince me that gas exsolution alone accounts for the volume/mass discrepancy. According to Figure 4, the minimum pressure change at which the solubility models intersect the black dashed line is 30 MPa. There needs to be more discussion about whether this pressure change is physically realistic. Although the authors provide a brief argument about the pressure change required for tensile failure in a reservoir, this discussion is hard to follow and is missing some key points, for example:

-What is the range of overpressure needed to initiate failure to produce an eruption? Given that this system is erupting over the study period, that failure threshold has already been reached. The eruption (if it is fed by an overpressured reservoir) will end once the reservoir returns to an equilibrium pressure (perhaps close to lithostatic). In that framework, the pressure change during an eruption cannot greatly surpass the overpressure that initiated the eruption. See Box 2 of Caricchi et al. 2021 (“The build-up and triggers of volcanic eruptions”) for a similar discussion.
-If the volume of an eruption is approximately equal to the reservoir volume times compressibility times pressure change (Huppert and Woods, 2002), how does a pressure change of 30 MPa correspond to a reservoir volume-compressibility product for the given erupted volume? (Again the authors would need to provide this erupted volume estimate up front to do this calculation). Is the volume-compressibility product reasonable/consistent with previous studies of the Etna reservoir?

Minor comments:
-It would be nice to include some more context for why this time series was chosen, and specifically how the start and end time were chosen. Given that eruptions precede this study period and gravity continued to decrease following the time period, it seems that the study period was chosen to be the time coinciding with both gravity decrease and surface deflation.

-More information about the inversion is needed (perhaps in a supplement). The authors use a Pattern Search Algorithm that searches for parameters that minimize the misfit, but there is mention (line 95) of a weighted mean of square residuals. How were the two datasets weighted, how was the weighting decided, and how does it affect the results? Are there tradeoffs in model parameters?

-Did the authors provide references for Etna volatile contents?

-At what temperature and pressure were the SOLWCAD models run? Are the solubility curves very sensitive to these choices? Are there references for the temperature and pressure of the Etna magma system?

-Figure 4: even if the authors want to keep the pressure range out to 100 MPa, why include such a huge density range? It makes it hard to see the more relevant part of the graph near the lower-pressure, lower-density range. I recommend cropping that to a more relevant range.

Best of luck,
Meredith Townsend, with additional input from Kathryn Scholz

Reviewer #4 (Remarks to the Author):

I congratulate the authors on a very interesting piece of multidisciplinary research – in my opinion, this kind of work is exactly what the field of volcanology needs. Although volcanoes monitored with both gravity and deformation networks are rare, the issue of a discrepancy between deformation and gravity sources, and between the magnitude of these sources and the volume of erupted products, is a recurring quandary. The holistic approach here of resolving that discrepancy definitely pushes the field forward. I also appreciate the authors' choice of well-documented tools and equations that would make reproducing this work and expanding it to other volcanic systems feasible.

My one serious misgiving with this paper is the value chosen for the shear modulus in the calculations. See below for more detail, but I am concerned that the value may be incorrect. As this value affects the calculated pressure change and thus also the cross-checking with the geochemical modeling upon which the conclusions of the paper rest, the validity of this value and the calculations would need to be addressed before I would be comfortable with this paper being published.

Overall comments

While you state in multiple places that the gravity meters used in this work are "nearly driftless", you never actually state what the drift of these meters is. Since the gravity change that your work is based on is essentially a linear change over a period of time, knowing the drift is very important to evaluating whether or not that gravity change is accurate. Being more precise about the drift is also important for drawing a clear distinction between the capabilities of superconducting and quantum gravimeters vs traditional spring meters that makes the kind of monitoring presented in this paper possible.

I'm concerned that the value you use for the shear modulus (30 GPa) in your calculations may be incorrect, or at the very least it does not match the reference cited. The paper you cite for that value, Heap et al. 2009, suggests a Young's modulus for Mt. Etna between 22-32 GPa. Assuming a Poisson's ratio of 0.25 (which is not in line with that paper, but I believe required by the equations you use), a Young's modulus between 22-32 GPa yields a shear modulus around 7-8 GPa. If you use 7-8 GPa for the shear modulus, that yields a delta P/delta rho curve that intersects the calculations from SOLWCAD at very different points (around -95 and -75 MPa, by my estimations). Can you please clarify this discrepancy?

To what extent are your conclusions dependent on a spherical source geometry? Could an ellipsoidal source, for example, also fit the data? Would this change your conclusions with regards to the pressure change required?

Line-by-line comments

Line 32 – Please specify whether the Earth tide corrections account for ocean loading effects – potentially significant given Mt. Etna's location.

Line 66 – typo, replace "streaking" with "striking"

Lines 77-79 – The sentence "The amplitude … craters decreases" is a little hard to follow - consider rephrasing for clarity

Line 79 – Replace "time" with "temporal" for clarity

Line 80 – Please explain how you calculated the uncertainties on the temporal gravity changes.

Line 92 – A few more details on the joint inversion method would be helpful here, since the paper cited explores a few different inversion approaches. In particular, it would be helpful to know 1) how the gravity data are weighted relative to the GNSS data and 2) how this paper handled setting the geometry (ie, was everything fit all at once, or was the geometry solved for first and then the density?)

Lines 98-107/Equation 1 – The way this section is written is gives the impression that only the gravity data are included in the inversion scheme. Are you only using equation 1 to link data to model, such that the deformation data is included implicitly, or are you using two separate equations for gravity and deformation data (similar to Chauhan et al 2020)?

Line 106 – Given that the gravity network and measurements at Mt. Etna are so extensive, is there a more site-specific value you could use for the free air gradient? Has a value of the free air gradient been established at Mt. Etna?

Lines 124-125 – When you write "even though the sources of deformation and gravity coincide", is the implication that the inversion scheme allows the deformation and gravity change sources to be in different locations? Or are you reiterating that the two sources are assumed to be the same location/geometry in the inversion scheme? Please make this clearer.

Line 126 – What density are you using to determine this estimate of the volume change from gravity variations?

Line 136 – Typo: replace "constrain" with "constraint"

Figure 4 – Given that you calculate uncertainties for delta M and delta V, and the conversion from those values to delta rho and delta P is relatively straightforward, it would be interesting and potentially bolster your conclusions to propagate those uncertainties and include them on the dashed lines in Figure 4.

Gas buffering of magma chamber contraction during persistent explosive activity at Mt. Etna volcano

Manuscript: COMMSENV-23-0308-T
By D. Carbone et al.

Point-by-Point Responses to Reviewers

We thank the reviewers for their valuable and insightful comments, which helped to substantially improve our manuscript.

Reviewer #1

General comments.
This study investigates gravity and deformation changes at Etna and discusses exsolution of volatiles as potential cause for the discrepancy between erupted volume, deformation, and gravity measurements. Overall, the idea of the study is relevant and brings a novel approach to discuss the discrepancy between erupted volume and deformation. Their conclusion is a reasonable explanation for the observed data; however, I think the paper would benefit from discussing if any other explanations are possible and what assumptions have been made during the course of the study. Furthermore, I believe the paper has a mistake in one of the equations and discusses several points only in the abstract and conclusion. I would recommend this study for publishing after some edits have been considered by the authors.

1) Several points (including deformation vs eruptive volume) are only mentioned in the abstract and the conclusion. I find it uncommon to mention discussion points only in the abstract and the conclusion section. I would shorten the conclusion to summarize the main conclusions and how this could lead to further research, and move some of the discussion points into the main section of the paper. However, if there are cultural differences between what can/should be written in a conclusion, please ignore my comment here.
We agree with the suggestion provided by the reviewed and, in the new version of the ms, we added a part to the section named "Interpretation of the inversion results" where we discuss the implications of the comparison between erupted volumes and bulk mass/volume changes deduced from the inversion of the geophysical data (lines 141 to 165 in the revised version of the ms).

2) Following comment 1, I think the paper would benefit from a discussion paragraph that helps tie everything together. You explain well how the discrepancy between the gravity measurements and the deformation data can be explained by exsolution of volatiles that buffer the deflation. Could you add here on how eruptive volume and deformation are related and their discrepancy instead of in the conclusion? Could you explain how eruptive mass and gravity measurements are related and if comparing them supports your hypothesis?
In the new part we added to the "Interpretation of the inversion results" section (see previous point) we addressed these issues.

3) How do your inversion results compare to previous studies of the depth and volume of the magma reservoir? Have previous studies also estimated the reservoir to be spherical and at 2.7 km b.s.l.?
The spherical symmetry was utilized many times as the forward model to invert deformation and gravity data from Mt. Etna, e.g., by Palano et al. (2008, *J. Volcanol. Geotherm. Res.*, 169, 99–120); Aloisi et al. (2018, *Front. Earth Sci.*, 6:109); Greco et al. (2022, *J. Geod.*, 96, 101).

Past studies (Bonforte et al., 2008, *J. Geophys. Res.*, 113, B05406; Bonaccorso and Aloisi, 2021, *Front. Earth Sci.*, 9:638742) showed that the shallow-intermediate plumbing system of Mt. Etna is rather complex, with storage zones at different depths. In particular, storage zones at depths of between 2500 and 3000 m were proposed by Puglisi et al. (2001, *Bull. Volcanol.*, 62, 371– 384); Bonaccorso et al. (2006, *J. Geophys. Res.*, 111, B12207) and Carbone et al. (2019, *J. Geophys. Res.*, 24, 4035–4050). In the revised version of the ms, we added citations to the papers of Aloisi et al. (2018) and Greco et al. (2022) (line 104).

4) I believe equation 5 is not correct…
Please make sure to not only correct the equation in your paper but recalculate your results if necessary!
The reviewer is true: there were typos in equation 5 and the correct form is that indicated by the reviewer (see Bower and Woods, 1997, *J. Geophys. Res.*, 102, B5):

$$\frac{1}{\rho} = \frac{n}{\rho_g} + \frac{1-n}{\rho_l}$$

With:
ρ = density of a magma-volatile mixture;
ρ_g = density of the exsolved volatiles;
ρ_l = density of the magmatic liquid;
n = weight fraction of the exsolved volatiles in the mixture.
However, the results shown in Fig. 4 were obtained through the SOLWCAD code, using the correct form of eq. 5. Indeed, the mistake we made concerned only the writing of the equation in the main text and did not affect the calculations we performed.

5) How would recharge or volatile fluxing from a deeper reservoir affect your results? If you are not considering these possibilities in your study, please state that you are assuming that no recharge or volatile fluxing is occurring, but that this might affect your results. Also, do other studies (for example the volatile studies you cite) support no volatile fluxing?
We indeed do not explicitly consider gas fluxing from deeper levels of the plumbing system of the volcano, also because we do not have data that could be used to set quantitative constraints on this process. Nevertheless, the melt inclusion data we consider reflect overall volatile contents in the reservoir at ~2.7 km b.s.l. and are likely to already include information on the gas fluxing. Obviously, large additions of gas during pressure decrease would result in $\Delta\rho/\Delta P$ curves different from those we obtained. However, in the absence of specific quantitative information on the possible amount of the gas fluxing, we must stick to the available data. In the new version of the text we added a sentence to clarify this (lines 237-240).

6) On page 6, you are mentioning several possibilities that could cause the discrepancy between deformation and gravity measurements. However, you are only discussing the possibility of exsolution of volatiles for the rest of the paper. How would any of the other possibilities affect your results? Is there a reason why you are not discussing them further? If you are assuming that magma compressibility, filling/emptying of void spaces, mixing of magmas of different densities does not occur, how do you justify these assumptions (plus please mention in the paper that you are making these assumptions). Your main point is that there is an exsolved volatile phase that buffers the system. This makes magma "highly" compressible so it seems odd that you are not incorporating it after mentioning it. Your hypothesis that the discrepancy between deformation and gravity can be explained seems reasonable to me; however, I think the paper would benefit from a short discussion on how

other factors (like compressibility or recharge) could affect your model and whether there are any other possibilities that could explain your observations.
In the new part we added at the beginning of the "Interpretation of the inversion results" section, we briefly discussed the reasons why, in our opinion, the other possible mechanisms that could explain the discrepancy between deformation and gravity data are less reliable (lines 160-165 in the revised version of the ms). However, it is important to note (as also stressed by the reviewer himself) that we indeed consider decompression-related density decrease as the most likely possibility behind the above discrepancy. We thus pick one of the mechanisms (magma compressibility) proposed through lines 129 to 132 in the original version of the manuscript.

7) Please explain on what data the initial concentration of H2O and CO2 are based. Based on your citations, the data is from melt inclusions. However, since these values are significant for your study, I would recommend to elaborate in a few words.
Following the reviewer's suggestion, we included a more detailed description of the MI datasets used for retrieving the volatile contents of Etnean magmas (lines 219-222 in the revised version of the ms).

8) Table 1: I would recommend writing large numbers in scientific notation. Also, make sure all digits are significant.
We changed the numbers reported in Table 1, as suggested.

9) Table 2: unit issues, significant digits, citation issues: These are not wt%. They might be weight fractions? Also, please double-check your citation. I believe you used Métrich et al. (2004) Table 2 sample 2a data here (not Moretti as written, also it is cited in text and table caption). Finally, TiO2 values seems wrong. It should be either 0.018 (in weight fraction) or 1.8 wt%. Please make sure you have used the correct value in your calculations. It is common in petrologic papers to use only two digits after the decimal point. Please consider deleting extra digits (issue will be resolved once converted to wt.%). It might be worth stating that this composition was measured from a melt inclusion (and is not bulk composition or matrix glass).
We thank the reviewer for reporting these mistakes. Indeed, in the original version of the ms, the units in Table 2 were in weight fractions. In the revised version of the ms, we addressed this issue by converting the values to wt% and solved the significant digits issue as well.
As noted by the reviewer, there was also a mistake in the TiO_2 values, which was corrected in the revised version of the ms. However, all the calculations were performed using the right value of the TiO_2 content.
As for the citation, the one we reported in the original version of the ms is correct. We indeed refer to sample LUSI-2b-iv from Table A1 (Supplementary Material) in Moretti et al. (2018, *Chem. Geol.* 482, 1-17). In the revised version of the ms, we added a sentence to the caption of Table 2, stating that the reported values were obtained from melt inclusions.

10) Is the volume in line 211 converted to DRE (dense-rock equivalent)? Please state if so. How does that affect your conclusion otherwise?
In the new version of the ms we addressed this point by saying that the reported volume of emitted tephra was already converted to DRE (line 142 in the revised version of the ms).

Some further minor suggested edits:
-Line 33: Please define RF frequency.
Changed to "radio frequency reference".

-Figure 1: The axes labels (longitude/latitude) are not centered. Is this on purpose? The same is the case in some other figures.
We centered the axes labels in all figures.

-Line 37: Consider replacing the word “sketch”. “Map” or so might be more appropriate here.
Changed as suggested.

-Line 42: Consider replacing “line” with “rectangle”.
Done.

-Figure 2: Why did you choose June 5th (last lava fountain of previous series) as starting date instead of June 15th (first lava fountain of the 38 fountains under investigation of the current study)? Especially in subfigure b, the described trend doesn’t seem to start on June 5th. Could you add a sentence detailing your reasoning for non-gravity experts?
As we also stated in the response to the first of the “minor comments” from reviewer 3, we chose the start and end dates of the period across which the deformation and gravity changes used for the joint inversion developed, taking into account different criteria, including the need of obtaining, from the information provided by each GNSS station, an as coherent as possible three-dimensional deformation field. In reality, the originally selected start date (5 June) is one day after the last fountain of the previous “cycle”, which occurred on 4 June and is thus not considered in our analyses. As appears in Fig. 2, the start date of the selected period can be moved a few days later, without significantly changing the characteristics of the data to be inverted. In the revised version of the ms, we thus moved the start date to 7 June, to make it clearer that the selected period does not include the lava fountain on 4 June. This change has no significant impact on the results of the joint inversion (see Table 1 in the revised version of the ms). We also added a sentence to the “Introduction” section, aimed at explaining the reasons why those start and end dates were selected (lines 72-75 in the revised version of the ms).

-Line 58-59: “10 sec and 1 day averages are shown” is written twice here. Consider rephrasing this sentence to make it more concise. Also, I think “1” and “day” are hyphenated (1-day average)
It is not the same sentence written twice; indeed, in the first case, it reads “10 min and…”, while, in the second case, it reads “10 sec and…”

-Line 60: The word “closer” implies a comparison. Did you mean “closest”?
Changed as suggested.

-Line 73: Same as line 60. Did you mean “closest” instead of “closer”?
Changed as suggested.

-Figure 3: It is almost impossible (especially in subfigure b) to distinguish between observed and calculated arrows on a black-white copy of the paper. Consider choosing different colors or change the line style to make it easier to distinguish. Subfigure c is very small.
We applied the requested changes.

-Line 94: Clarify the word “that”.
The sentence was changed to “The best parameters are estimated by minimizing the objective function…”

Equation 1: The left-hand side of the equation uses δ where the right-hand side uses Δ. I think they should be consistent on both side of the equation.
Changed as suggested.

-Line 115 and 116: I think it is uncommon to use "*" to signify multiplication in science paper. I would recommend using "x" or so throughout the paper.
Changed as suggested.

-Line 190: Is your variable D different from d in equation 1? (top vs middle of reservoir maybe)? Could you clarify or use the same variable name if they are equal?
Indeed, D is the depth of the reservoir roof, while *d* in equation 1 is the depth of the reservoir center. That was clarified in the revised version of the text.

-Line 199: Consider replacing "conclusive" with "concluding". There is a small difference in their meanings.
Changed as suggested.

Reviewer #2

The manuscript presents a novel investigation of geodetic observation (including short-term gravimetry) constrained by geochemical modelling. While at times the approaches are simplistic (e.g., assuming a Mogi deformation source, rather than a finite source geometry), this study provides a good basis for furthering integrated geochemical-geodetic modelling at both Mt. Etna and other volcanoes that are suitably instrumented.

The manuscript is generally well written. However, there are several points where the authors should loop their results/interpretations back to the observations and their primary argument. This would improve coherence, helping to lead the reader through the study. There are occasional spelling and grammatical errors within the manuscript that should be addressed, and some sentences could be simplified/clarified.

Within the "Interpretation" section, the authors comment on the volume change associated with the gravity inversion (Lines 124-126), but neglect to include the density (ρ) used in the calculation (ΔM/ρ). Please include the density value used and show the resultant volume change (ΔM/ρ) for the reader.
In the new version of the ms, we included the density value (2800 kg/m^3) used to determine the possible volume change inferred from the observed gravity variations (line 135).

The data presented in Table 2 and used in the independent geochemical modelling are obtained from Moretti et al. (2018) and pertain to eruptive products from the 2001 eruption of Mt. Etna. This specific information is missing from the text and should be included.
Furthermore, Moretti et al (2018) provide 4 geochemical analyses for the 2001 eruption.

- What was the rationale for picking this single analysis?
- Do the other analyses produce any significant variations in the H2O/CO2 curves derived from SOLWCAD model?
- Are there any changes in the estimates of ΔP from the intersection with the results of the geophysical inversion?

In the revised version of the ms, we added further information on the source of the data presented in Table 2 (lines 200-202). We also explained the reason for choosing one of the compositions reported by Moretti et al. (2018) and added a sentence to clarify that, given the small differences, consideration of the oxides compositions from other 2001 eruption samples in Moretti et al. (2018) would have negligible impact on the results shown in Fig. 4 (lines 202-203).

The volume of erupted products over the period in question is first mentioned within the conclusions. Given that erupted volume vs geodetically-derived volume loss is a key point of the abstract, it should be first mentioned much earlier; preferably alongside the data inversion results.
As said in the response to point (1) of reviewer 1, we added a new part to the section named "Interpretation of the inversion results" where we discuss the comparison between erupted volume and mass/volume changes retrieved from the inversion of the gravity and deformation data (lines 141 to 165 in the revised version of the ms).

• If the erupted volumes of the fountain events are approximately known (stated as 1-2x10^6 m^3 on line 211), it would be of interest to the reader to see cumulative eruptive volumes through time in an additional panel in Figure 2, even if just for the early June-late July period.
We prefer not to follow this suggestion from the reviewer. Indeed, there are strong assumptions behind the evaluation of the DRE volume of emitted tephra, implying that this estimate should be treated more as an order of magnitude than a precise value (lines 151-153 in the revised version of the ms). Furthermore, data are not available for all the lava flows during the >20 lava fountains during the study period, implying that a reliable event-by-event time series of cumulative emitted volumes could not be obtained.

• How much of the difference between total erupted volume (20-40x10^6 m^3?) vs geodetically-derived volume loss (2.23x10^6 m^3) could be due to vesiculation? I.e., what is the difference between the DRE and the geodetically-derived volume loss?
As stated in the new version of the ms, for the 22 eruptions during early-June to late-July, it results a total erupted volume which is more than an order of magnitude larger than the source volume reduction defined by the GNSS data (lines 157-159 in the revised version of the ms).

Abstract
Line 9-11: When did these eruptions occur? This sentence could be restructured for clarity – leading with the eruptive activity, followed by the observations.
"Mt. Etna erupted more than 20 times during a two-month period in 2021, and was accompanied by ground deflation and a decrease in the local gravity field."
We prefer to maintain the original structure of the sentence: the geophysical observations are indeed our main focus and we think it is more appropriate that they come first in the sentence. However, following the comment from the reviewer, we added "in 2021" after "…during a 2-month period".

Line 16-17: "… incompatible with a pure mechanism of magma withdrawal" – do you mean that the second point (ii) is incompatible with this mechanism (based on sentence structure, this is what is suggested), or that both (i) and (ii) together are incompatible? These sentences should be restructured for clarity.
We mean that the strong mass decrease is incompatible with a pure mechanism of magma withdrawal. We removed "that is" and the sentence now read like this: "the observed gravity changes point to a strong mass decrease, incompatible with a pure mechanism of magma withdrawal".

Introduction
Line 26-27: "closely spaced lava fountain events" – Given the eruptions occurred from one summit crater (line 24), does this sentence refer to the timings? If so, please clarify.

Changed as suggested.

Figure 1: Visual clarity of the figure could be improved.
• The box showing the region of the inset map is difficult to see, consider a different colour (e.g., red) or a thicker line.
We made the line thicker and changed its color.
• The markers/annotations of the gravity stations aren't very clear – perhaps bolder symbols/colours could be used for gravity, and paler for the GNSS?
We applied the required changes.

Figure 2:
• Panels (a-c) could all be on one set of axes, like the displacement, making comparisons of the gravity change easier. Given the 10 sec/10 min averages are not used, do they need to be displayed in the main text?
We prefer to leave panels a-c as they are, for the sake of clarity. The higher frequency averages are shown to provide a figure of how (differently) noisy the three time series are, which also explains the different size of the error bars in panel c of Fig. 3.
• Horizonal gridlines will aid the reader in understanding the magnitude of changes in gravity/displacement with time.
We added horizontal gridlines to the plots, as suggested.
• Text error in the GNSS annotation, should be 'EPDN' not 'EDPN'?
We corrected this error in the revised version of the figure.
• I understand showing only a subset of GNSS stations in (d), however given that gravity station SLN is colocated with GNSS station ESLN, it would be helpful to see the deformation data for this station too.
In panel (d) of figure 2, the time series of vertical ground displacements from the uppermost GNSS stations (i.e., those closest to the summit active craters) are presented. Since they were acquired at the stations closest to the main pressure source active during the considered period, these time series feature the most favorable signal-to-noise ratio. We prefer not to include in the plot the time series from ESLN, which is farther from the summit active area and thus, due to a poor signal-to-noise ratio, does not help to retrieve the general characteristics of the main change during the considered period.

Line 66: Striking, instead of streaking?
Corrected.

Line 73: "20 GNSS stations closer", closest?
Changed as suggested.

Line 77: GNSS
Corrected.

Lines 77-79: This sentence can be simplified to aid the reader. Consider something like "the amplitude of the observed gravity signal decreases with increasing distance away from the summit."
We simplified the sentence, as requested.

Figure 3:
• What is the x-axis of panel c? Is this reverse distance from the summit crater? By ordering the data points by proximity to the crater, this will help illustrate the point in Lines 77-79 (see comment above).

We changed the x-axis of panel c to horizontal distance between stations and projection of the modeled source

Data Inversion Results

It should be noted that the inversions and calculations carried out are for the net change over the deflation phase indicated in Figure 2.

We changed the first sentence in the "Data Inversion Results" section (line 97 in the revised version of the ms).

Line 107-112: These sentences are a bit confusing, I had to read them several times to get this meaning. Please clarify for the reader, and consider something along the lines of:

"It is important to note that the gravity data are not corrected for changes in elevation associated with the observed ground deformation. Instead, these corrections are carried out within the inversion scheme, using modelled vertical displacements."

Changed, as requested.

Interpretation of the Inversion Results

Line 126-127: This would be a good place to assert that the differences in derived volumes are not consistent with a mechanism of pure magma withdrawal.

We added a new part to the "Interpretation of the Inversion Results" section (lines 141-165 in the revised version of the ms), providing context to the proposed mechanism behind the observed geodetic changes (lines 166-171).

Line 131: "with magma of different density"

Changed as suggested.

Line 132: "emptying"

Corrected.

Line 136: "Constraint"?

Corrected.

Line 153: "which computes"

Changed.

Line 178-180: Part of the narrative of the paper is missing here, and needs to be expanded. Here the authors should explain that the exsolution/expansion of volatiles buffers the volume lost during eruption, and how it connects to the differences observed between the volumes derived from ground deformation vs gravity.

We expanded the sentence to better explain how pressure-driven exsolution and gas expansion may buffer the chamber contraction, thus leading to a volume reduction much smaller than the erupted volume, and induce the local mass decrease behind the observed gravity change.

Conclusive Remarks

Line 218: Again, this is another good place to assert that the geodetic observations and derived volumes are "incompatible with a pure mechanism of magma withdrawal", and requires a component of volumetric buffering from volatile exsolution.

We think that this concept is already thoroughly explained at the end of the concluding section, in particular, along lines 275 to 280 in the revised version of the ms. We thus don't think that further text would be needed here.

Reviewer #3

Strengths:
A strength of this paper seems to lie in the acquisition of high-resolution continuous gravity data that seem not to need as much precise ground control as more traditional/older types of gravity instruments. In addition, the use of geodetic data to understand thermodynamic magma reservoir processes is commendable and of broad interest to the volcanology community.

Weaknesses:
Equation 3 states that the change in mass equals the volume times change in density. Why don't volume changes contribute to dM? I would have thought that dM would approximately equal V*drho + rho*dV. If they assume constant volume, then doesn't this violate equation 2 relating pressure change to volume change? The authors need to explain this or re-do the analysis considering volume change contributions.
We agree with the reviewer that a complete consideration of the ΔM parameter should take into account the term related to the volume change in the reservoir:

$$\Delta M = (\rho' - \rho_{\mathrm{r}})\Delta V + \Delta\rho V$$

where $\rho' - \rho_{\mathrm{r}}$ is the difference between magma lost from the reservoir and host rock density. However, even if one assumes a very large density difference (500-1000 kg/m^3), the absolute value of the term $(\rho' - \rho_{\mathrm{r}})\Delta V$ remains on the order of 1-2 x 10^9 kg, thus, about two orders of magnitude smaller than the absolute value of ΔM resulting from the joint inversion (1.4 x 10^{11} kg). Hence, the term $(\rho' - \rho_{\mathrm{r}})\Delta V$ can be neglected in equation (3).
We added a new part in the revised version of the ms to clarify this point (lines 177-186).

A crucial piece of information to this analysis is the estimated volume of the erupted products. The abstract states that "the bulk volume reduction sensed by the ground deformation data is much lower than the volume of the erupted products," but it isn't until line 211 in the Conclusive Remarks that the volume of eruptions is provided.
We agree with the suggestion provided by the reviewed and, in the revised version of the ms, we added a part to the section named "Interpretation of the inversion results" where we discuss the implications of the comparison between erupted volumes and bulk mass/volume changes deduced by the inversion of the geophysical data (lines 156 to 165 in the revised version of the ms).

Line 211 states that each eruption was between 1-2 e6 m^3. For 20 eruptions, this corresponds to a total volume erupted of ~20-40e6 m^3. For a magma density of ~2600 kg/m^3, this corresponds to a mass change of ~5.2e10 - 1.0e11 kg. The inferred mass change from their inversion is ~1.5e11 kg, so there is actually only a discrepancy of ~0.5e11 kg in terms of explaining the mass change. Can density change during volatile exsolution account for this smaller fraction of the mass change?
Here the reviewer is disregarding the bulk volume change resulting from the joint inversion. The point is that the estimated erupted volume is much closer to the volume change deduced from gravity (i.e., from the calculated mass change), rather than to the volume change resulting from the inversion of the deformation data. That lends support to our hypothesis of gas buffering the magma chamber contraction (see lines 166-171 in the revised version of the ms).

It is well known that the volume of erupted products typically far exceeds the volume change of the reservoir. See Huppert and Woods, 2002; Rivalta & Segall, 2008; Kilbride et al., 2016; Le Mevel et al. 2016 JGR; Wasser et al. 2021 G-cubed; and Townsend, 2022 EPSL for various studies related to the discrepancy between geodetic signal and magma reservoir changes. Especially relevant is Rivalta and Segall (2008), which finds (equation 7) that the ratio of the erupted volume to volume change in the reservoir is equal to 1 + magma compressibility / reservoir compressibility. In the Etna study presented here, if we take the inferred volume change to be 2.3e6 m^3 from Table 1, this corresponds to a ratio of between ~8 and 18, which could be explained by having a compressible magma (perhaps 1e-9 1/Pa) within a relatively less compressible reservoir (perhaps 1e-10 1/Pa).
As we already stated in the response to comment (6) from reviewer 1, we indeed explain the observed discrepancy between mass and volume changes resulting from the joint inversion through magma compressibility, or, more precisely, through decompression-related gas exolution/expansion. In other words, rather than picking a random value of magma compressibility that would fit the observations, we explore the relationship between $\Delta\rho$ and ΔP using the SOLWCAD code, which provides more reliable results than, e.g., eq. 11 in Rivalta and Segall (2008, *Geoph. Res. Lett.*, 35, L04306), since it employs a non-ideal, non-Henrian model and thus avoids oversimplifications at pressures larger than some tens of MPa.

Given the possibility that magma compressibility (without the effects of pressure buffering by volatile exsolution included) can entirely explain the volume discrepancy, it is really the remaining mass discrepancy (~0.5e11 kg) that seems to warrant further study. I suspect it would be easy to come up with the remaining mass change as a density change by volatile exsolution.
We disagree with the view that magma compressibility and gas exsolution/expansion should be treated separately, since they are intimately related (i.e., magma compressibility mainly stems from its gas phase). The volume discrepancy resulting from the joint inversion of deformation and gravity data could not be explained through compressibility of gas-free magma. Indeed, according to Bonafede and Ferrari (2009, *Tectonophysics*, 471, 4-13) and to Greco et al. (2022, *J. Geod.*, 96, 101), the ratio between ΔV_m (volume change deduced from the mass change) and ΔV_{out} (volume change deduced from the deformation data) is equal to:

$$\frac{\Delta V_m}{\Delta V_{out}} = 1 + \frac{4\mu\beta_m}{3}$$

where, μ is the rigidity modulus of the host medium, while β_m is the compressibility of the magma. Our results point to a value of the ratio of about 25, implying $\beta_m \approx 6 \times 10^{-10}$ Pa^{-1}, while the compressibility of degassed basalts is much lower, e.g., 0.6-1 x 10^{-10} Pa^{-1}, according to Rivalta and Segall (2008, *Geoph. Res. Lett.*, 35, L04306), or 0.3 x 10^{-10} Pa^{-1}, according to Malfait et al. (2011, *Am. Mineral.*, 96 (8–9), 1402–1409). This result further proves that gas plays an essential role in the processes behind our observations.

Even if the analysis presented here to derive the drho_dP relationship were correct, Figure 4 does not sufficiently convince me that gas exsolution alone accounts for the volume/mass discrepancy. According to Figure 4, the minimum pressure change at which the solubility models intersect the black dashed line is 30 MPa. There needs to be more discussion about whether this pressure change is physically realistic. Although the authors provide a brief argument about the pressure change required for tensile failure in a reservoir, this discussion is hard to follow and is missing some key points, for example:

-What is the range of overpressure needed to initiate failure to produce an eruption? Given that this system is erupting over the study period, that failure threshold has already been reached. The eruption (if it is fed by an overpressured reservoir) will end once the reservoir returns to an equilibrium pressure (perhaps close to lithostatic). In that framework, the pressure change during an eruption cannot greatly surpass the overpressure that initiated the eruption. See Box 2 of Caricchi et al. 2021 ("The build-up and triggers of volcanic eruptions") for a similar discussion.

Also in this case we disagree with the content of the reviewer's point.

Firstly, the inversion of the geophysical data indicates decompression and the intersection of the curves in Fig. 4 is meant to define the possible amplitude of the inferred pressure decrease; given the negative sign of the change, we don't understand what the reviewer means by saying that it "*cannot greatly surpass the overpressure that initiated the eruption*".

Secondly, the framework proposed by the reviewer is misleading: during early-June to late-July 2021, there wasn't only one eruption, continuing throughout the entire period, but, rather, several short-lasting (few tens of minutes to few hours) eruptions, all occurring from the same summit crater. This eruptive behavior requires a conservative, rather than destructive trigger for the eruptions, implying that rock failure under overpressure is not feasible in this case.

Consideration of the time series shown in Fig. 2 suggests that the inferred pressure decrease developed almost continuously throughout the 2-month period, while the possible transient pressure changes linked to each lava fountain episode occurred over time-scales of the order of hours and they are indeed not reflected in the daily GNSS data. This observation further supports the view that checking the amplitude of the longer-term pressure decrease through comparison with the shorter-term pressure fluctuations during the eruptions would not make sense.

Conversely, we reckon that the short discussion in the last part of section "Interpretation of the inversion results", focused on whether or not the resulting pressure decrease could lead to collapse of the reservoir roof, is better suited to check the reasonability of the obtained results.

-If the volume of an eruption is approximately equal to the reservoir volume times compressibility times pressure change (Huppert and Woods, 2002), how does a pressure change of 30 MPa correspond to a reservoir volume-compressibility product for the given erupted volume? (Again the authors would need to provide this erupted volume estimate up front to do this calculation). Is the volume-compressibility product reasonable/consistent with previous studies of the Etna reservoir?

The equation the reviewer refers to relates the erupted volume to (i) the overpressure at which a given eruption starts (Δp_0), (ii) the chamber volume and (iii) the compressibility of the magma. We think it would not make much sense to apply this equation to the case under study, using, instead of Δp_0, the amplitude of the pressure decrease which developed throughout June to July 2021.

Minor comments:

-It would be nice to include some more context for why this time series was chosen, and specifically how the start and end time were chosen. Given that eruptions precede this study period and gravity continued to decrease following the time period, it seems that the study period was chosen to be the time coinciding with both gravity decrease and surface deflation.

We selected the start and end dates of the study period, taking into account the following criteria: (i) obtaining a deformation field as coherent as possible and with a favorable signal-to-noise ratio; (ii) encompassing, along the three gravity time series, most of the phase of higher decrease rate starting in early June; (iii) including the cluster of lava fountains occurring every less than 1 to few days, between early-June and late-July.

Gravity indeed continued to decrease for a couple of weeks after 31 July, but this further change is negligible, compared to the overall decrease since early June. Conversely, a less coherent pattern of ground deformation is obtained if the study period is extended until mid-August, probably due to the activation of sources other than the depressurizing reservoir at ~2.6 km b.s.l. (see lines 72-75 in the revised version of the ms).

-More information about the inversion is needed (perhaps in a supplement). The authors use a Pattern Search Algorithm that searches for parameters that minimize the misfit, but there is mention (line 95) of a weighted mean of square residuals. How were the two datasets weighted, how was the weighting decided, and how does it affect the results? Are there tradeoffs in model parameters?
The residuals used for the definition of the objective function are weighted by the inverse of the uncertainty associated with the measurements. In this way, standardized data of different types can be integrated into a single error functional, without having to introduce arbitrary weights. We added a sentence at the beginning of the "Data inversion results" section, aimed at clarifying this point (lines 100-101). Naturally, if the weighing scheme is changed, inversion solutions also change. However, we verified that, for limited random variations of the weights, the deviations of the optimal solution parameters from those reported in Table 1 is within the estimated uncertainty ranges. That is reported in a newly added "Methods" section ("Checks on the data inversion procedure"; lines 289-293).

-Did the authors provide references for Etna volatile contents?
See responses to points 7) and 9) from reviewer 1.

-At what temperature and pressure were the SOLWCAD models run? Are the solubility curves very sensitive to these choices? Are there references for the temperature and pressure of the Etna magma system?
In the revised version of the ms, we added information on the pressure and temperature conditions at which the SOLWCAD runs were performed (lines 198-199). Saturation curves do depend on the values chosen for these parameters. Nevertheless, while pressure is fairly well constrained by the inferred depth of the magma reservoir, consistent values were reported in previous works for the temperature of Etnean magmas (Spilliaert et al., 2006, *J. Geophys. Res. Solid Earth*, 111), implying that this parameter varies within a small range. These small variations do not affect significantly the results presented in Fig. 4.

-Figure 4: even if the authors want to keep the pressure range out to 100 MPa, why include such a huge density range? It makes it hard to see the more relevant part of the graph near the lower-pressure, lower-density range. I recommend cropping that to a more relevant range.
Following the suggestion from the reviewer, we restricted the range of $\Delta\rho$ (Y axis) in Fig. 4 and now it goes from 0 to -225 kg/m^3.

<u>Reviewer #4</u>

I congratulate the authors on a very interesting piece of multidisciplinary research – in my opinion, this kind of work is exactly what the field of volcanology needs. Although volcanoes monitored with both gravity and deformation networks are rare, the issue of a discrepancy between deformation and gravity sources, and between the magnitude of these sources and the volume of erupted products, is a recurring quandary. The holistic approach here of resolving that discrepancy definitely pushes the field forward. I also appreciate the authors' choice of well-documented tools and equations that would make reproducing this work and expanding it to other volcanic systems feasible.

My one serious misgiving with this paper is the value chosen for the shear modulus in the calculations. See below for more detail, but I am concerned that the value may be incorrect. As this value affects the calculated pressure change and thus also the cross-checking with the geochemical modeling upon which the conclusions of the paper rest, the validity of this value and the calculations would need to be addressed before I would be comfortable with this paper being published.
We chose a standard value of the shear modulus (30 GPa), which was utilized in several previous papers focused on the analysis of geophysical data from Mt. Etna (e.g., Lundgren et al., 2003, *J. Geophys. Res.*, 108; Palano et al., 2008, *J. Volcanol. Geotherm. Res.*, 169; Gonzales and Palano, 2014, *J. Volcanol. Geotherm. Res.*, 274; Bruno et al., 2017, *Geophys. Res. Lett.*, 44; Viccaro et al., 2019, *Sci. Rep.*, 9). Conversely, our choice to cite the paper by Heap et al. (2009), to support the selected value of the shear modulus, was not appropriate, as noted by the reviewer. That was a mistake of ours and, in the revised version of the manuscript, we cited the paper by Gonzales and Palano (2014), instead of the work of Heap et al. (2009).

Overall comments
While you state in multiple places that the gravity meters used in this work are “nearly driftless”, you never actually state what the drift of these meters is. Since the gravity change that your work is based on is essentially a linear change over a period of time, knowing the drift is very important to evaluating whether or not that gravity change is accurate. Being more precise about the drift is also important for drawing a clear distinction between the capabilities of superconducting and quantum gravimeters vs traditional spring meters that makes the kind of monitoring presented in this paper possible.
We mention that the AQG-B and the iGrav gravimeters are, respectively, free and almost free from instrumental drift effects at the beginning of the concluding section (“The availability of high quality time-series from almost driftless continuously recording gravimeters…”). That the AQG-B provides driftless absolute gravity data was reported in Antoni-Micollier et al. (2022), while the manufacturer of the iGrav SG (GWR) declares an instrumental drift of less than 0.5 microGal/month. This figure agrees with the findings of some papers, including that by Fores et al. (2017), who analyzed a 3-year time series from an iGrav and found a long-term drift of 4.5 microGal yr^{-1}. In the revised version of the ms, we expanded the sentence at the beginning of the concluding section (lines 262-264), to provide contest to the statement that the gravimeters utilized in our study are driftless, or almost such.

I’m concerned that the value you use for the shear modulus (30 GPa) in your calculations may be incorrect, or at the very least it does not match the reference cited. The paper you cite for that value, Heap et al. 2009, suggests a Young’s modulus for Mt. Etna between 22-32 GPa. Assuming a Poisson’s ratio of 0.25 (which is not in line with that paper, but I believe required by the equations you use), a Young’s modulus between 22-32 GPa yields a shear modulus around 7-8 GPa. If you use 7-8 GPa for the shear modulus, that yields a delta P/delta rho curve that intersects the calculations from SOLWCAD at very different points (around -95 and -75 MPa, by my estimations). Can you please clarify this discrepancy?
As stated before, we considered a value of the shear module which is commonly utilized in studies dealing with the analysis of geophysical data from Mt. Etna. The reviewer is right when he says that the selected value “*doesn’t match the reference cited*” and, as we mentioned before, we changed that reference to the paper by Gonzales and Palano (2014).

To what extent are your conclusions dependent on a spherical source geometry? Could an ellipsoidal source, for example, also fit the data? Would this change your conclusions with regards to the pressure change required?

To check whether consideration of an ellipsoidal source, rather than spherical, would lead to results different from those we obtained, we jointly inverted the gravity and deformation data using, as the forward model, the formulation recently published by Nikkhoo and Rivalta (2023, *Geophys. J. Int.*, 232, 643–655). This formulation permits to calculate the surface deformation field and gravity changes due to the pressurization of a finite ellipsoidal cavity in a half-space. Inversion results are very similar to those obtained with the Mogi model, thus confirming that the optimal solution parameters presented in Table 1 are not critically dependent on the assumption of a spherical source shape. That was discussed in the newly added "Methods" section, where we also reported results obtained from application of the Akaike information criterion (lines 294-312), allowing to determine which of the two formulations is more adequate to represent the process that generated the gravity and deformation data.

Line-by-line comments

Line 32 – Please specify whether the Earth tide corrections account for ocean loading effects – potentially significant given Mt. Etna's location.

The employed earth tide model does not account for ocean loading effects, since they were found to be negligible in the summit area of Mt. Etna (about 20 km away from the coastline). Indeed, after removing the theoretical tidal effect, calculated with a model not accounting for ocean loading, residuals are less than 1 microGal peak-to-peak, over both the diurnal and semi-diurnal components.

Line 66 – typo, replace "streaking" with "striking"

Done.

Lines 77-79 – The sentence "The amplitude … craters decreases" is a little hard to follow - consider rephrasing for clarity

Rephrased, also following a comment from reviewer #2.

Line 79 – Replace "time" with "temporal" for clarity

We prefer not to apply the suggested change.

Line 80 – Please explain how you calculated the uncertainties on the temporal gravity changes.

The uncertainty on each time gravity difference ($U_{\Delta g}$) was propagated from the uncertainties affecting the daily gravity averages on the start and end dates of the selected period (07 June and 31 July). In turn, the uncertainty on a daily average is defined as the standard deviation of the daily averages within a 15-day window around the selected date. Hence:

$$U_{\Delta g} = \sqrt{\left(\sigma_{g1}\right)^2 + \left(\sigma_{g2}\right)^2}$$

That was clarified in the revised version of the ms (lines 81-84).

Line 92 – A few more details on the joint inversion method would be helpful here, since the paper cited explores a few different inversion approaches. In particular, it would be helpful to know 1) how the gravity data are weighted relative to the GNSS data and 2) how this paper handled setting the geometry (ie, was everything fit all at once, or was the geometry solved for first and then the density?)

As for 1), see the response to the 2nd minor comment of reviewer #3. As for 2), the inversion scheme determines all the parameters at once. We added a sentence to the “Data inversion results” section to clarify that (lines 115-116).

Lines 98-107/Equation 1 – The way this section is written is gives the impression that only the gravity data are included in the inversion scheme. Are you only using equation 1 to link data to model, such that the deformation data is included implicitly, or are you using two separate equations for gravity and deformation data (similar to Chauhan et al 2020)?
To make it clearer the fact that we used the entire coupled model in our analysis, we expanded Equation 1, so that it now includes the formulations for horizontal and vertical deformations.

Line 106 – Given that the gravity network and measurements at Mt. Etna are so extensive, is there a more site-specific value you could use for the free air gradient? Has a value of the free air gradient been established at Mt. Etna?
The FAG was measured at a few sites in the summit area of Etna by Greco et al. (2012; *Bull. Volcanol.*, 74, 1745–1756). In particular, FAG measurements were accomplished in July 2008, at PDN, MNT and SLN. With respect to the standard FAG (-308.6 microGal/m), discrepancies ranging between about -30 and 50 microGal/m were observed at the three sites. A single FAG value can be used as input to the inversion scheme and, either if we use the average of the values measured at the three stations (-328.8 microGal/m), or the standard FAG, identical source parameters are retrieved from the joint inversion of the gravity and deformation data. That is also due to the limited vertical deformation, exceeding 1 cm only at ECPN (1.9 cm) and EPLU (1.6 cm). Under these conditions, we prefer to stick to the standard FAG value and, accordingly, we left the text unchanged.

Lines 124-125 – When you write “even though the sources of deformation and gravity coincide”, is the implication that the inversion scheme allows the deformation and gravity change sources to be in different locations? Or are you reiterating that the two sources are assumed to be the same location/geometry in the inversion scheme? Please make this clearer.
We meant that a common mass/pressure source provides excellent fit to the gravity and deformation data (see Fig. 3), implying that it is reasonable to assume that the two sources are placed in the same position. In the revised version of the ms, we removed the incidental sentence “*even though the sources of deformation and gravity coincide*”, in order to avoid possible misunderstanding (line 133).

Line 126 – What density are you using to determine this estimate of the volume change from gravity variations?
The assumed value of density is now reported in the text (also following a comment from reviewer #2)

Line 136 – Typo: replace “constrain” with “constraint”
Corrected.

Figure 4 – Given that you calculate uncertainties for delta M and delta V, and the conversion from those values to delta rho and delta P is relatively straightforward, it would be interesting and potentially bolster your conclusions to propagate those uncertainties and include them on the dashed lines in Figure 4.
In the new version of Fig. 4 we presented the uncertainty on the $\Delta\rho/\Delta P$ relation, propagated from uncertainties on ΔM and ΔV (grey area in Fig. 4).

14th Sep 23

Dear Dr Carbone,

I hope you are well?

Your manuscript titled "Gas buffering of magma chamber contraction during persistent explosive activity at Mt. Etna volcano" has now been seen again by 4 reviewers, and we include their comments at the end of this message. They all highlight that they find the manuscript to be much improved, so thank you for repsonding so comprehensively to the review comments. However, there remain several outstanding points that need to be addressed. 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.

- Please ensure that instrument drift is fully accounted for and included within modelling uncertainties.

- Please clarify further your discussion of the role of bulk magma compressibility, and the relationship with exsolved volatiles, when interpreting your results.

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.

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.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
[Link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

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,

Emma Nicholson, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0003-1749-9285

Joe Aslin
Senior 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 #1 (Remarks to the Author):

The authors have addressed all of my concerns and comments adequately.

In my humble opinion, this is now a great article, and it was a pleasure reading it! The revised manuscript leads the reader nicely through the study and explains clearly how all the different multidisciplinary parts come together to support the authors interpretation of the data. The multidisciplinary approach give highly interesting insights into the processes within the magmatic system, and the authors show the value of using continuous gravity measurements.

I would like to congratulate the authors to their upcoming publication of this article.

Reviewer #2 (Remarks to the Author):

Carbone et al., 2023, Nature Communications Earth & Environment - Review

“Gas buffering of magma chamber contraction during persistent explosive activity at Mt. Etna volcano”

I’ve enjoyed revisiting this manuscript. It has been greatly improved, and I thank the authors for their detailed responses to Reviewer comments. However, I now have a moderate concern, as outlined below, based on the comment of another Reviewer.

Reviewer 4 correctly queried the description of ‘nearly driftless’ gravimeters. The authors responded with expected instrument drift of <0.5 μGal/month for the iGrav gravimeters, with Fores et al. (2017) reporting ~4.5 μGal/year.
With these values in mind, I believe instrument drift is therefore a non-negligible component in two of the observed gravity signals, if they haven’t already been accounted for (and it should be clarified if this is the case).
This provides an additional aspect of uncertainty that must be appropriately considered within the modelling.
• The period of interest is from 7th June to 31st July, totalling 55 days. This equates to ~15% of the annual drift (55 days/365 days), giving ~0.7 μGal.
• At station SLN, the gravity changes from ~25 μGal to ~18 μGal, so a difference of ~7 μGal. The expected instrument drift is ~10% of the net gravity signal.
• As for MNT, the gravity appears to change from ~27 μGal to ~13 μGal, and so expected instrument drift would equate to ~5% of the signal.

Furthermore, the expected drift values should be included at the beginning of the manuscript, when describing the instruments, and not at the end.

These uncertainties should be incorporated into the modelling and analyses before I can recommend publication. Please find some line-by-line recommendations below.

Signed,
Matthew Head, University of Illinois at Urbana-Champaign

Line-by-line comments:

Line 76: “that occurred every less than 1 to few days in June to July” – consider rewording to something along the lines of “with a recurrence interval of less than a few days throughout June and July”.

Line 105: Can you include the objective function? Perhaps in the ‘Methods’ supplementary.

Line 121: “when they are inputted into…” – consider rewording to “prior to inversion”.

Lines 139-140: The authors still do not provide a calculation of $\Delta M/\rho$. It should not be left as an exercise to the reader to determine the volume change inferred from the gravity – the values referred to should be made explicit in the text.

Lines 154-155: “size is not entirely in-keeping with more recent structure-from-motion mapping results” – in what way? Is the assumed diameter greater or smaller than values derived by structure-from-motion? The tephra volume will then be either be a ‘maximum’ or ‘minimum’ estimate from these calculations.

Lines 163-165: This a joint inversion of the GNSS and gravity data? Therefore, it’s incorrect to state that it’s defined solely by the GNSS data. Again, the authors refer to a volume change inferred by observed gravity variations – but do not provide a calculation to demonstrate this (see comment for Lines 139-140).

Lines 190-191: Consider including the value of ΔM here, so that the reader does not need to return to Table 1. E.g., “ΔM resulting from the joint inversion (-1.4±0.15 x 1011 kg; Table 1)”

Line 209: “inputted” – input is also the correct past tense. However, this may be something flagged later during editing and typesetting.

Line 232: This is now equation 6.

Lines 270-272: As mentioned in my main comment, these details should be at the beginning of the manuscript.

Lines 297-299: Where are the uncertainties in the measurements given? Will this be in a supplementary data file?

Line 302: This sentence needs amendment – either “we consider whether an…” or “we check whether an…”

Line 304: Consider rewording to “using a formulation that calculates the deformation field…”

Line 305-307: You say the parameters are very close to those retrieved by the Mogi inversion – the full set of best-fit parameters of the ECM should be included here (in a similar way to Table 1). It helps to further support the use of spherical models if it is shown that the radii of the ellipsoid are approximately equal.

Line 310: “losing” not “loosing”.

Reviewer #4 (Remarks to the Author):

I thank the authors for their diligence in responding to my and other reviewers' comments and concerns - the manuscript is much improved. I have only two remaining minor comments that should be addressed:

1) Regarding the value of the shear modulus chosen - thank you for correcting the citation. However, since the 30 GPa value chosen is so much different than the 7-8 GPa value (and similar) suggested by Heap et al. (2009) and other works such as Schultz (1995) derived from rock mechanics measurements, this is worth addressing in the paper. I would suggest adding a sentence or two defending the choice of a (I assume?) seismic-derived shear modulus vs. a rock mechanics-derived shear modulus.

2) Thank you for specifying the density value chosen (2800 kg m^-3) for the calculations. However, please provide a source and/or justification for choosing that particular value.

Reviewer #5 (Remarks to the Author):

The revised manuscript shows significant improvements in the structure and in clarifying the methods. It sufficiently addresses all minor comments made upon the initial review. Furthermore, the paper ultimately comes to what seems to be reasonable conclusions for a mechanism to explain the mismatch between the ΔM from the inversion and ΔV from the inversion but upon revision is still not supporting this conclusion with sufficient clarity and completeness. I hope now with the additional clarification from reading the revised manuscript and the rebuttal, these comments re-articulated in a way that helps the authors address the weaknesses that remain in the revised manuscript.

Major Comments

1) I recommend double-checking the sign on the first term on the right-hand side of the eqn 3 (ln 179). While I could not find a copy of Hagiwara, 1977 as it appears to not be digitized, when rederiving equation 3 how I anticipate one would, for a deflating spherical reservoir, I found $\Delta M = -(\rho' - \rho_r)\Delta V + \Delta\rho V$ for mass loss/deflating chamber. If you were computing ΔM of an inflating chamber, I believe you would recover equation 3 as currently written in the manuscript (first term on the right positive). While this potential sign error does not propagate into further analysis since regardless the term is small relative to the other terms, it should still be checked to ensure consistency.

2) In the rebuttal, you state clearly that you favor changes to bulk magma compressibility due to the exsolved volatile phase (both density change of the fluid and exsolution) however this does not come across clearly in the manuscript. Explicitly mentioning that "decompression driven density changes" ln 166 are related to changes in bulk magma compressibility would help clarify. This is particularly important because the following phrase "an increase in the proportion of exsolved gas to magma in the source reservoir" ln 167 could be easily interpreted as asserting that exsolution of additional volatiles buffer pressure changes to the exclusion of pressure buffering by gas density changes. Clarifying this language throughout the "Interpreting Inversion Results" section is critical for situating this work in the context of the existing literature which addresses this same topic the role of volatiles on chamber compressibility and thus the amount of magma erupted such as Wasser et al. 2022, Rivalta and Segall 2008, Townsend 2022, Huppert and Woods 2002 (and many more), many of which recognize that both exsolution of volatiles and the mechanical compressibility (β_g) contribute to bulk magma compressibility.

3) My understanding is that the melt density in SOLWCAD is not pressure-dependent. As a result, this study neglects the effects of the melt and crystal compressibility. If there is pressure dependence for the melt density in the modeling, this should be clarified in the manuscript. If it is not included, it would be incomplete to exclude those effects without further justification as there are plausible conditions where the contributions to the bulk magma compressibility by the melt and crystals are of similar magnitude as the contribution by the gas phase. β_m can be represented as the average of the compressibility of each phase weighted by the volume fraction of that phase (crystal,

melt, gas). If the melt and crystals have compressibilities on the order of 10^{-11}-10^{-10} Pa^{-1} (Rivalta and Segall, 2008) and the gas has a compressibility on the order of 10^{-9}-10^{-8}, there are plausible ranges of exsolved gas contents where the contribution of the crystals plus melt and the gas to compressibility would be equal. Returning in your analysis to how Δrho and ΔP impact bulk magma compressibility could help justify this. One fairly simple way to do this is to use the definition of $\beta_m = 1/\rho \, d\rho/dP$ and your SOLWCAD results to compute β_m and then compare that compressibility to the estimation of magma compressibility that you compute in your response using the relationship $\Delta V_m/\Delta V_{out}=1+4\mu\beta_m/3$.

4) Based on the clarification in the rebuttal, I now understand that the intent of the paragraph on the mechanical stability of the chamber was to discuss the plausibility of their ΔP values in the context of roof collapse during the study period. This paragraph (ln 249-264) remains confusing and incorrect as written. Ref 32, 34-37 all discuss the failure of magma reservoirs undergoing inflation, that have internal pressures greater than lithostatic, and Ref 33 discusses the relative importance of overpressure and buoyancy in fracture propagation; however, the author's findings suggest that the chamber is being underpressurized relative to lithostatic pressure. As such, the cited literature and their rationale using Grosfils et al is not suitable evidence for their claim. The mismatch between the discussion of literature that concerns over-pressurized chambers paired with the assertions about decompression by the authors makes the section very confusing. Most commonly, under-pressurizing magma chambers are thought to cause roof collapse via shear failure of the overlying rock and thus manifest at the surface through the formation of calderas, ring faults, or other subsidence structures. Literature concerning the initial formation of calderas or ring fault systems and shear failure around underpressurized magma reservoirs would be a more appropriate place to look to support the rationale. Some of the following references may be of use:

Aizawa, K., Acocella, V., & Yoshida, T. (2006). How the development of magma chambers affects collapse calderas: Insights from an overview. *Geological Society, London, Special Publications*, *269*(1), 65–81. https://doi.org/10.1144/GSL.SP.2006.269.01.05

Geshi, N., Miyagi, I., Saito, G., & Conway, C. E. (2023). Caldera collapse thresholds correlate with magma chamber dimensions. *Scientific Reports*, *13*(1), Article 1. https://doi.org/10.1038/s41598-023-34411-5

Gudmundsson, A. (2007). Conceptual and numerical models of ring-fault formation. *Journal of Volcanology and Geothermal Research*, *164*(3), 142–160. https://doi.org/10.1016/j.jvolgeores.2007.04.018

Holohan, E. P., Schöpfer, M. P. J., & Walsh, J. J. (2015). Stress evolution during caldera collapse. *Earth and Planetary Science Letters*, *421*, 139–151. https://doi.org/10.1016/j.epsl.2015.03.003

MARTI, J., ABLAY, G. J., REDSHAW, L. T., & SPARKS, R. S. J. (1994). Experimental studies of collapse calderas. *Journal of the Geological Society*, *151*(6), 919–929. https://doi.org/10.1144/gsjgs.151.6.0919

Roche, O., Druitt, T. H., & Merle, O. (2000). Experimental study of caldera formation. *Journal of Geophysical Research: Solid Earth*, *105*(B1), 395–416. https://doi.org/10.1029/1999JB900298

Tepp, G. (2021). Material failure and caldera collapse: Insights from the 2018 Kilauea eruption. *Earth and Planetary Science Letters*, *553*, 116621. https://doi.org/10.1016/j.epsl.2020.116621

Minor Comments

- Ln 121, change inputted to input for correct verb tense
- Ln 181, it would be useful to clarify if $\rho' - \rho_r$ is the density difference between host rock density and initial magma density or final magma density once decompression and exsolution have occurred. I believe it should be the latter
- Several locations, change emitted to erupted, emitted is more commonly used in reference to light, heat, or gases

Best,

Kathryn Scholz with input from Meredith Townsend

Gas buffering of magma chamber contraction during persistent explosive activity at Mt. Etna volcano

Manuscript: COMMSENV-23-0308B
By D. Carbone et al.

Point-by-Point Responses to Reviewers

We thank the reviewers for re-checking our manuscript after the first revision. We appreciated their further comments, which helped to substantially improved our manuscript.

Reviewer #1

The authors have addressed all of my concerns and comments adequately.
In my humble opinion, this is now a great article, and it was a pleasure reading it! The revised manuscript leads the reader nicely through the study and explains clearly how all the different multidisciplinary parts come together to support the authors interpretation of the data. The multidisciplinary approach give highly interesting insights into the processes within the magmatic system, and the authors show the value of using continuous gravity measurements. I would like to congratulate the authors to their upcoming publication of this article.

We thank the reviewer for his/her kind appreciation.

Reviewer #2

I've enjoyed revisiting this manuscript. It has been greatly improved, and I thank the authors for their detailed responses to Reviewer comments. However, I now have a moderate concern, as outlined below, based on the comment of another Reviewer.

Reviewer 4 correctly queried the description of 'nearly driftless' gravimeters. The authors responded with expected instrument drift of <0.5 μGal/month for the iGrav gravimeters, with Fores et al. (2017) reporting ~4.5 μGal/year.
With these values in mind, I believe instrument drift is therefore a non-negligible component in two of the observed gravity signals, if they haven't already been accounted for (and it should be clarified if this is the case).
This provides an additional aspect of uncertainty that must be appropriately considered within the modelling.
• The period of interest is from 7th June to 31st July, totalling 55 days. This equates to ~15% of the annual drift (55 days/365 days), giving ~0.7 μGal.
• At station SLN, the gravity changes from ~25 μGal to ~18 μGal, so a difference of ~7 μGal. The expected instrument drift is ~10% of the net gravity signal.
• As for MNT, the gravity appears to change from ~27 μGal to ~13 μGal, and so expected instrument drift would equate to ~5% of the signal.

Furthermore, the expected drift values should be included at the beginning of the manuscript, when describing the instruments, and not at the end.

These uncertainties should be incorporated into the modelling and analyses before I can recommend publication.

In the revised version of the ms, we added a new part at the end of the “Introduction” section, to clarify that the calculated uncertainties on the time gravity changes from the stations equipped with iGrav SGs already include the component arising from possible instrumental drift effects (lines 95-98). In the same part we also reported the expected amplitude of the instrumental drift effect during the considered period.

Line-by-line comments:

Line 76: “that occurred every less than 1 to few days in June to July” – consider rewording to something along the lines of “with a recurrence interval of less than a few days throughout June and July”.
We reworded the sentence.

Line 105: Can you include the objective function? Perhaps in the ‘Methods’ supplementary.
As suggested by the reviewer, we reported the objective function at the beginning of the “Methods” section (lines 293-298 in the revised version of the ms).

Line 121: “when they are inputted into…” – consider rewording to “prior to inversion”.
We changed the sentence to “…before they are fed to…”.

Lines 139-140: The authors still do not provide a calculation of ΔM/ρ. It should not be left as an exercise to the reader to determine the volume change inferred from the gravity – the values referred to should be made explicit in the text.
We indicated the amount of volume change inferred from gravity.

Lines 154-155: “size is not entirely in-keeping with more recent structure-from-motion mapping results” – in what way? Is the assumed diameter greater or smaller than values derived by structure-from-motion? The tephra volume will then be either be a ‘maximum’ or ‘minimum’ estimate from these calculations.
With the remark through lines 154-155 we wanted to highlight that the evaluation of the vent size is subjected to a certain degree of subjectivity, implying that “the volume of emitted tephra should be treated as indicative, rather than a precise estimate”. We didn’t mean that we consider one of the values reported in the literature more reliable than the other and we thus prefer to leave that part of the text as it is.

Lines 163-165: This a joint inversion of the GNSS and gravity data? Therefore, it’s incorrect to state that it’s defined solely by the GNSS data. Again, the authors refer to a volume change inferred by observed gravity variations – but do not provide a calculation to demonstrate this (see comment for Lines 139-140).
We changed “defined by the GNSS data” to “resulting from the joint inversion”. The amount of volume change inferred by the observed gravity variations is now reported at the beginning of the “Interpretation of the inversion results” section (line 140 in the revised version of the ms).

Lines 190-191: Consider including the value of ΔM here, so that the reader does not need to return to Table 1. E.g., “ΔM resulting from the joint inversion ($-1.4\pm0.15 \times 10^{11}$ kg; Table 1)”
Done.

Line 209: “inputted” – input is also the correct past tense. However, this may be something flagged later during editing and typesetting.
We changed “inputted” to “input”.

Line 232: This is now equation 6.
We corrected this mistake.

Lines 270-272: As mentioned in my main comment, these details should be at the beginning of the manuscript.
The information on the typical amplitude of the instrumental drift was moved to the “Introduction” section (lines 97-98 in the revised version of the ms).

Lines 297-299: Where are the uncertainties in the measurements given? Will this be in a supplementary data file?
The uncertainties are reported throughout the “Introduction” section. As for GNSS data, an uncertainty of 1 and 3 cm is associated with the horizontal and vertical components, respectively (lines 78-79 in the revised version of the ms), while the uncertainties on the time gravity changes are 1, 4 and 8 µGal, at SLN, MNT and PDN, respectively (lines 95-96 in the revised version of the ms).

Line 302: This sentence needs amendment – either “we consider whether an…” or “we check whether an…”
We changed the sentence to: “…we check whether assuming an ellipsoidal source…”

Line 304: Consider rewording to “using a formulation that calculates the deformation field…”
Changed as suggested.

Line 305-307: You say the parameters are very close to those retrieved by the Mogi inversion – the full set of best-fit parameters of the ECM should be included here (in a similar way to Table 1). It helps to further support the use of spherical models if it is shown that the radii of the ellipsoid are approximately equal.
In the revision version of the ms, we reported the best-fit parameters obtained from the joint inversion of the gravity and deformation data, using the ECM as the forward model (Table 3 in the revised version of the ms).

Line 310: “losing” not “loosing”.
We corrected this mistake of ours.

Reviewer #4

I thank the authors for their diligence in responding to my and other reviewers' comments and concerns - the manuscript is much improved. I have only two remaining minor comments that should be addressed:

1) Regarding the value of the shear modulus chosen - thank you for correcting the citation. However, since the 30 GPa value chosen is so much different than the 7-8 GPa value (and similar) suggested by Heap et al. (2009) and other works such as Schultz (1995) derived from rock mechanics measurements, this is worth addressing in the paper. I would suggest adding a sentence or two defending the choice of a (I assume?) seismic-derived shear modulus vs. a rock mechanics-derived shear modulus.
Following the reviewer’s suggestion, we stressed that this value is the most commonly used in papers dealing with the modelling of deformation data from Mt. Etna (lines 201-202 in the revised version of the ms.)

2) Thank you for specifying the density value chosen (2800 kg m^-3) for the calculations. However, please provide a source and/or justification for choosing that particular value.
In the new version of the ms, we added a reference for the assumed value of the magma density (Ferlito et al., 2017).

Reviewer #5

The revised manuscript shows significant improvements in the structure and in clarifying the methods. It sufficiently addresses all minor comments made upon the initial review. Furthermore, the paper ultimately comes to what seems to be reasonable conclusions for a mechanism to explain the mismatch between the ΔM from the inversion and ΔV from the inversion but upon revision is still not supporting this conclusion with sufficient clarity and completeness. I hope now with the additional clarification from reading the revised manuscript and the rebuttal, these comments re-articulated in a way that helps the authors address the weaknesses that remain in the revised manuscript.

Major Comments

1. I recommend double-checking the sign on the first term on the right-hand side of the eqn 3 (ln 179). While I could not find a copy of Hagiwara, 1977 as it appears to not be digitized, when rederiving equation 3 how I anticipate one would, for a deflating spherical reservoir, I found $\Delta M = -(\rho' - \rho_r)\Delta V + \Delta\rho V$ for mass loss/deflating chamber. If you were computing ΔM of an inflating chamber, I believe you would recover equation 3 as currently written in the manuscript (first term on the right positive). While this potential sign error does not propagate into further analysis since regardless the term is small relative to the other terms, it should still be checked to ensure consistency.
   The current form of eq. 3 is correct. What defines the sign of the first term on the right-hand side of the equation is ΔV, which, obviously, is positive in the case of inflation and negative in the case of deflation.

2. In the rebuttal, you state clearly that you favor changes to bulk magma compressibility due to the exsolved volatile phase (both density change of the fluid and exsolution) however this does not come across clearly in the manuscript. Explicitly mentioning that "decompression driven density changes" ln 166 are related to changes in bulk magma compressibility would help clarify. This is particularly important because the following phrase "an increase in the proportion of exsolved gas to magma in the source reservoir" ln 167 could be easily interpreted as asserting that exsolution of additional volatiles buffer pressure changes to the exclusion of pressure buffering by gas density changes. Clarifying this language throughout the "Interpreting Inversion Results" section is critical for situating this work in the context of the existing literature which addresses this same topic the role of volatiles on chamber compressibility and thus the amount of magma erupted such as Wasser et al. 2022, Rivalta and Segall 2008, Townsend 2022, Huppert and Woods 2002 (and many more), many of which recognize that both exsolution of volatiles and the mechanical compressibility (β_g) contribute to bulk magma compressibility.
   Here, the reviewer's point is that (1) exsolution of additional volatiles and (2) compressibility-related expansion of (pre-existing and newly-exsolved) gas both contribute to buffering the contraction of the magma reservoir and inducing the observed gravity change, while we seem to favor only (1). In order to better clarify this issue, in the new version of the ms, we implemented some changes to the "Interpreting Inversion Results" section, namely, to the paragraph in lines 171 to 177.

3. My understanding is that the melt density in SOLWCAD is not pressure-dependent. As a result, this study neglects the effects of the melt and crystal compressibility. If there is pressure dependence for the melt density in the modeling, this should be clarified in the manuscript. If it is not included, it would be incomplete to exclude those effects without further justification as there are plausible conditions where the contributions to the bulk magma compressibility by the melt and crystals are of similar magnitude as the contribution by the gas phase. β_m can be represented as the average of the compressibility of each phase weighted by the volume fraction of that phase (crystal, melt, gas). If the melt and crystals have compressibilities on the order of 10^{-11}-10^{-10} Pa-1 (Rivalta and Segall, 2008) and the gas has a compressibility on the order of 10^{-9}-10^{-8}, there are plausible ranges of exsolved gas contents where the contribution of the crystals plus melt and the gas to compressibility would be equal. Returning in your analysis to how Δrho and ΔP impact bulk magma compressibility could help justify this. One fairly simple way to do this is to use the definition of β_m =1/ρ dρ/dP and your SOLWCAD results to compute β_m and then compare that compressibility to the estimation of magma compressibility that you compute in your response using the relationship $\Delta V_m/\Delta V_{out}=1+4\mu\beta_m/3$.

   The calculations performed through SOLWCAD do account for the different compressibility of melt and gas, implying that the results shown in Fig. 4 already comprise all the contributions to compressibility within the system. Indeed, this was already stated in the ms, as we reported that:

   The density of the two-phase mixture (ρ_{mix}) at each pressure step is evaluated as:

$$\frac{1}{\rho_{mix}} = \frac{n}{\rho_g} + \frac{1-n}{\rho_l}$$

   where, n is the weight fraction of exsolved gas in the mixture, while ρ_g and ρ_l are densities of the gas and magmatic liquid, respectively, calculated through suitable models[30,31].

   In the revised version of the ms, we implemented some changes through lines 234-235, aimed to further clarify this point. We also added a sentence to indicate that the pressure dependent density decrease is mostly controlled by gas exsolution/expansion, while changes in the density of the liquid phase provide only a negligible contribution (lines 236-238).

4. Based on the clarification in the rebuttal, I now understand that the intent of the paragraph on the mechanical stability of the chamber was to discuss the plausibility of their ΔP values in the context of roof collapse during the study period. This paragraph (ln 249-264) remains confusing and incorrect as written. Ref 32, 34-37 all discuss the failure of magma reservoirs undergoing inflation, that have internal pressures greater than lithostatic, and Ref 33 discusses the relative importance of overpressure and buoyancy in fracture propagation; however, the author's findings suggest that the chamber is being underpressurized relative to lithostatic pressure. As such, the cited literature and their rationale using Grosfils et al is not suitable evidence for their claim. The mismatch between the discussion of literature that concerns over-pressurized chambers paired with the assertions about decompression by the authors makes the section very confusing. Most commonly, under-pressurizing magma chambers are thought to cause roof collapse via shear failure of the overlying rock and thus manifest at the surface through the formation of calderas, ring faults, or other subsidence structures. Literature concerning the initial formation of calderas or ring fault systems and shear failure around underpressurized magma reservoirs would be a more appropriate place to look to support the rationale. Some of the following references may be of use:

Aizawa, K., Acocella, V., & Yoshida, T. (2006). How the development of magma chambers affects collapse calderas: Insights from an overview. Geological Society, London, Special Publications, 269(1), 65–81. https://doi.org/10.1144/GSL.SP.2006.269.01.05
Geshi, N., Miyagi, I., Saito, G., & Conway, C. E. (2023). Caldera collapse thresholds correlate with magma chamber dimensions. Scientific Reports, 13(1), Article 1. https://doi.org/10.1038/s41598-023-34411-5
Gudmundsson, A. (2007). Conceptual and numerical models of ring-fault formation. Journal of Volcanology and Geothermal Research, 164(3), 142–160. https://doi.org/10.1016/j.jvolgeores.2007.04.018
Holohan, E. P., Schöpfer, M. P. J., & Walsh, J. J. (2015). Stress evolution during caldera collapse. Earth and Planetary Science Letters, 421, 139–151. https://doi.org/10.1016/j.epsl.2015.03.003
MARTI, J., ABLAY, G. J., REDSHAW, L. T., & SPARKS, R. S. J. (1994). Experimental studies of collapse calderas. Journal of the Geological Society, 151(6), 919–929. https://doi.org/10.1144/gsjgs.151.6.0919
Roche, O., Druitt, T. H., & Merle, O. (2000). Experimental study of caldera formation. Journal of Geophysical Research: Solid Earth, 105(B1), 395–416. https://doi.org/10.1029/1999JB900298
Tepp, G. (2021). Material failure and caldera collapse: Insights from the 2018 Kilauea eruption. Earth and Planetary Science Letters, 553, 116621. https://doi.org/10.1016/j.epsl.2020.116621

We understand the reviewer's concern about the fact that the papers we cited to check the plausibility of the resulting ΔP values may not be appropriate. However, while it is true that some of the above papers only focus on overpressure of the magma chamber, this remark does not apply to, e.g., the papers by Pinel and Jaupart (2005) and by Grosfils (2007). For example, the variable on the Y axis of Fig. 10 in Grosfils (2007) is pressure required to induce initial chamber failure (P), normalized by the lithostatic pressure (σ_z); hence, values of $P/\sigma_z < 1$ indicate underpressure, with respect to an initial equilibrium condition, where $P = \sigma_z$. It is also worth noting that, if we consider our results (pressure decreases of between -25 and -80 MPa, corresponding to values of R of between 950 and 645 m, DtC of 5.6 km and σ_z = 140 MPa) in the frame of Fig. 10b of Grosfils (2007), we came up with the two following [P/σ_z; R/DtC] points:

[(140-25)/140; 950/5600] [(140-80)/140; 645/5600], i.e.,

[0.82; 0.17] [0.43; 0.11]

Both points fall well to the right of the curve of initial failure, which indicates stability conditions.

That said, we agree with the reviewer that, in its original form, the part dealing with the mechanical stability of the reservoir is somewhat incorrect, as it does not properly address the distinction between overpressure and underpressure conditions, and may thus result confusing to the reader. Hence, we have rewritten this part (lines 252 to 263 in the revised version of the ms) and simplified it, also considering that the mechanical stability of the chamber is a side issue to the central aim of our study. As suggested by the reviewer, we dropped the references to papers dealing only with overpressure (refs. 32, 33, 35, 37 in the previous version of the ms.) and added a reference to the paper by Geshi et al. (2023), which is focused on chamber roof collapse, as a consequence of decompression.

Minor Comments

- Ln 121, change inputted to input for correct verb tense

Done

- Ln 181, it would be useful to clarify if $\rho' - \rho r$ is the density difference between host rock density and initial magma density or final magma density once decompression and exsolution have occurred. I believe it should be the latter

Eq 3 involves the comparison of the bulk conditions before and after the deflation process. The occurrence of magma chamber contraction means that the horst rock substitutes the material that was inside the magma chamber before the start of the contraction, hence ρ' is the initial magma density. In the revised version of the ms we added a sentence to clarify this point (lines 189-190).

- Several locations, change emitted to erupted, emitted is more commonly used in reference to light, heat, or gases

Changed, as suggested.

3rd Nov 23

Dear Dr Carbone,

Your manuscript titled "Gas buffering of magma chamber contraction during persistent explosive activity at Mt. Etna volcano" has now been seen by our 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 under the open access CC BY license (Creative Commons Attribution v4.0 International License).

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.

Please note that it may still be possible for your paper to be published before the end of 2023, but in order to do this we will need you to address these points as quickly as possible so that we can move forward with your paper.

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REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

Carbone et al., 2023, Nature Communications Earth & Environment - Review

“Gas buffering of magma chamber contraction during persistent explosive activity at Mt. Etna volcano”

With this revision, the authors have sufficiently addressed my concerns outlined in previous reviews. The authors approach provides novel insight into magmatic processes at Mt. Etna; clearly presenting the components of the study, supporting the authors interpretations of the joint GNSS-gravity inversion and geochemical modelling.
It has been my pleasure to be part of the review process for such an interesting multidisciplinary study, and I would like to congratulate the authors on the upcoming publication.

Signed,
Matthew Head, University of Illinois at Urbana-Champaign

Reviewer #5 (Remarks to the Author):

The authors have adequately addressed my comments. I would like to thank them for time they took to further clarify the role of compressibility in their analysis and in correcting their discussion of mechanical chamber stability within the manuscript.