nature portfolio

Peer Review File

Seismic risk mitigation at Campi Flegrei in volcanic unrest

Corresponding Author: Professor Iunio Iervolino

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

A version of this paper was originally rejected for publication by Nature Communications, however that decision was reconsidered after appeal by the authors.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The study presents a comprehensive investigation of seismic activity at Campi Flegrei, a densely populated volcanic region near Naples, Italy, through a series of statistical analyses. While the authors purport to investigate the potential efficacy of structural retrofitting policies within this context – a claim mirrored in the manuscript's title – the research primarily presents a straightforward probabilistic seismic hazard analysis for the case-study area. Although the analysis is executed correctly, it offers scant insight into seismic risk mitigation policies.

The research primarily concludes that the reference earthquake scenarios, in terms of moment magnitude (Mw), for the considered case study area – based on historical (recent) seismicity – fall within the 4.4–5.1 range. This range is notably lower than the minimum magnitudes corresponding to ground motion exceedance for life-safety retrofit as per the Italian building code. This result is somewhat expected. It is well known that earthquakes induced by volcanic activity are generally significantly less severe than tectonic earthquakes. These tectonic earthquakes typically represent the majority of events considered in assessing probabilistic seismic hazard for building code purposes.

Furthermore, probabilistic seismic hazard analysis at the basis of building code relies on several conservative assumptions – at various analysis stages – for instance, in the selection of maximum magnitude, ground-motion model selection, uncertainty soruces, etc. The authors themselves state that "The magnitudes of the events occurred so far are small enough to be mostly neglected in the national probabilistic seismic hazard analysis (PSHA) that determines the design ground motions to be used for seismic design according to the current building code, which considers events of local magnitude starting at about 4.3." (page 2), and continue with similar discussions through different points on the same page.

Moreover, the case-study area is particularly complex, and many believe that the primary concern should be volcanic hazard and risk rather than solely focusing on earthquake-induced ground motions. Despite high volcanic hazards, the area has witnessed significant urban development and population growth. It is estimated that 20% of the buildings in the so-called "red zone" (a high-risk area due to volcanic activity) are unauthorized (illegal), particularly vulnerable and aging significantly. The existing roads and transport infrastructure are ill-equipped to support potential evacuations. This complexity makes mitigation efforts and securing the population challenging at the intersection of urban planning and public policy. The main (and only) message from the study, i.e., you should retrofit existing buildings in the area following the current Italian building code for new constructions, while essential, neglects a series of other critical issues, i.e., some of these buildings should simply not be there.

Unfortunately, the author missed an opportunity to address these pressing issues. A more comprehensive approach would have been to address the interplay between seismic and volcanic risks, the implications of unauthorized constructions, and the challenges of evacuation in the face of a dual-threat scenario. Such an approach would have provided a more holistic view of the situation at Campi Flegrei and could have offered valuable insights for policy-making and urban planning in similar high-risk areas, making the paper suitable for publication in a high-impact journal such as Nature Communications.

Reviewer #2

(Remarks to the Author)

The manuscript concerns the mitigation of seismic risk at Campi Flegrei. It aims to investigate whether the structural retrofitting policies based on the rules for new constructions could be a viable strategy in the Campi Flegrei area. The authors focus their attention on the study of possible reference scenarios. They approach this study based on fault mapping, geomorphological inference, high-precision earthquake relocation performed in previous work, stress-drop analysis, and ground motion modeling. The authors conclude that the reference scenarios, in terms of Mw, are in the range 4.4-5.1, and estimate the time-variant exceedance probability for these magnitudes. Furthermore, the article highlights that the minimum magnitudes expected to cause the exceedance of the ground motion thresholds for life-safety established by the Italian building code are generally larger than those of the reference scenarios.

Main comments:
The article is well-written. I appreciate its simplicity and the focus on the practical aspects of risk management in the area of the Campi Flegrei volcano, which is in unrest. However, I think that some parts of the article require a little further refinement. For example, the fault selection method to obtain the reference magnitude scenarios is not very clear to me. Furthermore, in the abstract a concept like: "the magnitudes of the events … are small enough to be mostly neglected in the national probabilistic seismic hazard analysis ... Nevertheless, the structural seismic actions are generally non-negligible ..." (from the introduction) could better introduce the reader to the focus of the article, which appears to be the relationship between the magnitude of earthquakes and the ground motion thresholds, in terms of Sa, and its possible implications on the adequacy of Italian building code for seismic risk in this volcanic area. In general, I find that the authors should better highlight (e.g. in the discussion) how their study is important for seismic risk mitigation at Campi Flegrei. In the abstract the authors say that, to aid decision making, it was investigated whether structural retrofitting policies based on the design goals for new constructions could be a viable strategy in the area. In the introduction, they pose at least three possible, non-alternative, motivations regarding the relevance of shaking intensity for relatively-low magnitude earthquakes at Campi Flegrei. However, in the text, and especially in the discussion, I do not feel that there are answers to these questions, or that the questions, posed very directly in the introduction, are discussed clearly in light of the results of the article. Also, the title is directly focused on seismic risk mitigation at Campi Flegrei, but this concept is not fully exploited and explained in the final discussion. For these reasons I recommend publication on Nature Communications after minor revisions.

Minor comments:

Line 8 I suggest deleting Phlaegraean Fields in the brackets and possibly inserting Italy.

Line 11 The authors should check the number of earthquakes with Md>-1.1, which should be greater than 18000, to date. Perhaps the authors are referring to located earthquakes.

Lines 18 - 21 I suggest reporting, possibly in parentheses, the exceedance probability for these magnitudes, and what is the minimum magnitudes expected to cause the exceedance of the ground motion thresholds for life-safety retrofit by the Italian building code.

Lines 23 - 24 “Campi Flegrei (Phlegraean Fields) is a volcanic area, the caldera of which was shaped by two major eruptions, about 39000 and 15000 years ago, and located …”. To improve the clarity of the text, I suggest reformulating the opening sentence of the introduction, also considering the previous suggestion (line 8).

Figure 1. In the inset there is the Campania region and not a point indicating the position of the Campi Flegrei. It seems to me that this could be misleading because it can make a reader who does not know the area well think that this is the Campi Flegrei caldera. I suggest using a symbol like a circle to indicate the position of the Campi Flegrei volcano.

Lines 69 – 71 I suggest adding some information about the seismic stations such as sampling rate and sensor type. Were these stations installed by the Federico II University? If so, when were they installed?

Lines 163 – 165 and Figure 5 The fault selection method is not very clear to me. Reading the caption of Figure 5, in particular concerning panel 5b, it seems that the morpho-structural analysis identifies the faults based on the alignments of vents. However, the faults and dykes, along which the eruptive vents are generally located, should not have the same direction with respect to the stress axes that characterize the area. Therefore, I think that this criterion may not be very effective for identifying potential seismogenic faults. I suggest reconsidering this fault detection criterion and checking whether the final subset of selected faults changes significantly.

Line 172 Please specify which sequence by reporting the date or date range (e.g. between 16/03/2022 until 04/10/2023).

Line 202 and Figure 6 Panels 6b and 6d are not easy to read due to overlapping colors. Please, try to improve these figures. Perhaps it might be better to use error bars.

Lines 215 – 224 In the first rows and first columns of tables 3 and 4 (within 1 km of the Rione Terra site) I read minimum moment magnitudes of strong earthquakes of 3.3 and 4.0. This seems to me to be at odds with the statement that these magnitudes are generally larger than 4.4 for both return periods. I find this to be a very delicate point of the article and for this reason I think that more discussion could be useful.

Lines 231 – 233 The reference to the uplift rate is not very clear. I don't understand how considering the variation of the uplift rate the probabilities to observe a M_w≥5.1 or a M_w≥4.4 go from 2.7% to 0.3% and from 10% to 1.3%, respectively. There are papers that study the relationship between the uplift rate and the occurrence rate of earthquakes that could be cited. In any case, for greater self-consistency of the discussion, I suggest the authors refer to the decreased rate of seismicity rather than the decreased rate of uplift (that enters the article here for the first time), as the cause of the decrease in probabilities.

Line 235 I find that the discussion is incomplete and not explicit. I suggest the authors expand the discussion to highlight the significance and possible positive effects of their study in terms of seismic risk mitigation at Campi Flegrei, as promised in the title of the article. I think it is useful to return to the statements and questions mentioned in the abstract and introduction and discuss them in light of the results of the article (are the structural retrofitting policies based on the rules for new constructions a viable strategy in the Campi Flegrei area? Etc., see main comment).

Lines 284 – 287 See comment relating to lines 163 – 165.

Reviewer #3

(Remarks to the Author)
This paper studies whether the seismic provisions of the current Italian building code adequately protect structures to withstand earthquakes that originate within the Phlegraean Fields, a volcanic area in Italy. The estimated maximum magnitude that an earthquake in the area can generate is compared to the minimum magnitude expected to cause an exceedance of the design levels of the code, concluding that, in most cases, the former magnitude is lower than the latter magnitude. Thus, the study implies that the current Italian building code would provide safety against these earthquakes (although this is not explicitly stated in the manuscript).

I find this study very interesting because it is an effort to answer a pressing real-life question that is important for the safety of many people in the region. However, before I can recommend the publication of the manuscript, I ask that the authors address my comments presented below.

1) I do not understand the point of the earthquake forecasting section. It is being used to compute probabilities of exceedance of certain earthquake magnitudes, but nothing is then concluded from this information. The end of the Discussion section is a perfect place to conclude something from this analysis.

2) The method used in this study is very ad-hoc to the specific region. Readers of this paper may be interested in how it applies to other regions or situations. I would at least add a comment on the applicability of the proposed method to other cases.

3) Equation 7: It seems indirect to me to be comparing earthquake magnitudes because the difference between the minimum magnitude of strong earthquakes and the reference magnitude does not mean anything by itself. In other words, the greater the difference between the magnitudes the better, but the difference is not very meaningful. Maybe it would be more informative to compute the probabilities of exceedance at each period, that is, to set the reference magnitude to consider (4.4 or 5.1) and the distance range ($R<z$) and compute the probability that the spectral acceleration at a given period exceeds the design spectral acceleration. I can understand if changing this part of the method is unfeasible at this point, but I would like to understand why the authors made this decision.

Some other comments and issues that I found while reviewing the manuscript that I think are less important but should still be addressed are:

4) Abstract: I would add a sentence at the end of the abstract that states the implications of the minimum magnitude that causes exceedance of the ground motion threshold being larger than the magnitude of the reference scenario.

5) Figure 1b: The y label on the right: “Nr. of earthq. recorded in the last year” is confusing. Consider changing “in the last year” to “per year”. Also, the dates on the x-axis are very crowded. I would use fewer dates and/or change the date format.

6) Figure 3: It may be a good idea to show the epicenters of the 4 selected earthquakes in Figure 1a.

7) Figure 3: panels (b) and (c) have gray lines that are not mentioned in the figure caption. What do these lines represent?

8) Table 2: Is this table really necessary? It just shows two numbers.

9) Figure 9: “for some magnitude” is repeated in the caption.

10) Equation 6: is the logarithm in base 10? If so, this should be clearer.

11) Equation 7: I recommend adding an equation that explains how the exceedance probability is computed from the mean and standard deviation of the GMM.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Reviewer #2

(Remarks to the Author)
The manuscript has been significantly improved. Authors implemented the main suggestions and focus of the study is now more clearly presented and thoroughly discussed. For these reasons, I believe the article is suitable for acceptance and publication.

Reviewer #3

(Remarks to the Author)
The authors have dealt with previous comments adequately. However, I think there is an error in the exceedance probability formula added after equation (7):

Line 417: Within the standard normal CDF, the numerator should have the difference between the base 10 logarithm of the spectral acceleration variable (i.e., sa^{Tr}) and the mean base 10 logarithm from the ground motion model, that is, (log^{10}(Sa(T)) – mu). However, the formula provided has(log^{10}(Sa(T)) – sa^{Tr}), which does not make sense.

I also have a few minor comments regarding the new inclusions to the manuscript. Specifically:

1. Line 447: Parameter η was already used in Equation (6) for the residual of the GMM. Please use another letter for the logarithmic standard deviation of the fragility functions.

2. Equation (9): There is an error in this equation because the total probability theorem was not used correctly. The right side of the equation gives you the hazard curve (i.e., mean annual frequency of exceedance of Sa), not the PDF of the Sa. To get the PDF of the Sa, the distribution being integrated should be a PDF, not a complementary CDF. An easy way to see that there is an error in equation (9) is to notice that the units on both sides of the equation do not match because the left side has units of 1/acceleration and the right side is unitless. Another more elegant way to solve the error is to keep the right side of the equation and write the hazard curve on the left side of the equation, and then modify equation (8) to use the hazard curve instead of the PDF of Sa.

3. Line 460: Why was the minimum magnitude set to 4.4 and 5.1? These were the maximum magnitudes obtained from the faults. Why use them as the minimum magnitudes of the analysis? The minimum magnitude for these types of analyses is not a seismological parameter, it is an engineering parameter that depends on the system being studied. For this case, it should be set at a level where we are sure that no collapse is possible in the buildings being studied. Thus, it has nothing to do with the magnitude of completeness. I would just select a single value of minimum magnitude following this guideline and modify Table 4 accordingly.

4. Line 460: Also, why was the maximum magnitude set to 6.5? Would it make more sense to use 5.1, which was the maximum magnitude obtained from the faults?

Reviewer #4

(Remarks to the Author)
The work addresses the issue of seismic and volcanic activity in the Campi Flegrei caldera, which is particularly relevant in Italy, as a recent evacuation drill for the local population was conducted. As declared by the authors, the study responds to a specific request from the local administration: “given that the current eruption hazard remains low according to volcanologists, and the current main hazard for the area is seismic (although related to the volcanic unrest), would be an effective risk reduction strategy to upgrade substandard existing buildings to the current code safety levels?”. To this end, the authors conclude that the design ground motions foreseen by the Italian building code are not expected to be exceeded by an earthquake occurring in the caldera if not in the case of multiple fault activation.

The changes and additions introduced by the authors following the reviewers' requests, clarify some points that were not well explained in the first version. For example, a specific discussion sub-section including risk analysis and making use of the forecasting results is added. The authors show that bringing the existing 3-stories RC buildings to the safety levels of new construction, as required by the building code, would cause a fatality risk reduction larger than 70%.

The manuscript has already undergone two rounds of review.
I carefully read the revised manuscript, the reviewers' comments, and the authors' replies.

Referees #2 and #3 gave a positive evaluation of the work, along with a series of comments and observations which I fully agree with. The authors replied positively to all the reviewers' observations, introducing substantial improvements to the manuscript.

As for referee #1, I understand his concerns, particularly regarding the last two comments (points 4 and 5) but I agree with the thorough responses provided by the authors.

I would just like to add two comments to which I would appreciate a response from the authors:

1. The soil classification adopted in the GMM developed for Campi Flegrei (equation 6) is rather rough, considering only the soil classes B and C and not the VS30 value. Moreover, the subdivision in class B and C (figure 10 c) is derived from the shakemap from Michelini and Faenza (line 390) that does not provide a detailed geomorphological classification.

2. It would be appropriate to clarify better the use, for magnitudes larger than 4, of the GMM of Bindi et al. that is not suitable for volcanic earthquakes as correctly discussed in lines 377-383.

Based on the previous considerations, I believe that the manuscript ca be published in its present form, provided that a response is given to my two comments.

Version 2:

Reviewer comments:

Reviewer #3

(Remarks to the Author)
The authors addressed my comments successfully. Thus, I believe the manuscript should be considered for publication.

Reviewer #4

(Remarks to the Author)
The authors accepted and responded positively to the third round of review as well, so I believe the manuscript can be published in its current form.

REPLY TO EDITOR'S AND REVIEWERS' COMMENTS

REVIEWER 1

1. The study presents a comprehensive investigation of seismic activity at Campi Flegrei, a densely populated volcanic region near Naples, Italy, through a series of statistical analyses. While the authors purport to investigate the potential efficacy of structural retrofitting policies within this context – a claim mirrored in the manuscript's title – the research primarily presents a straightforward probabilistic seismic hazard analysis for the case-study area. Although the analysis is executed correctly, it offers scant insight into seismic risk mitigation policies.

[Authors] Authors regret that this comment is technically incorrect. The study does not report about a PSHA. Probabilistic seismic hazard analysis provides the rates of exceedance of ground motion intensity measure (IM) values (e.g., accelerations) [2] at a site of interest, whereas the results of this study presented in the first version of the manuscript were threefold, none of which is exceedance rate:

- **a. the magnitudes of the largest inferred geological structures in the deforming area;**
- **b. the (time-variant) probabilities of exceedance of these magnitudes via operational earthquake forecasting;**
- **c. the *minimum magnitudes of strong earthquakes* [3] able – if occurring close to the building stock in the area – would cause exceedance of the code-mandated seismic design goals.**

None of these three goals is, or could be, obtained by PSHA.

As it regards the risk mitigation policies, the presented manuscript showed that the minimum magnitude expected to cause the exceedance of the design goals of new constructions is generally larger than those inferred for the earthquakes occurring in the currently deforming caldera. Nevertheless, to comply with this comment, an explicit risk analysis was carried out computing the fatality risk reduction of new-design reinforced concrete buildings at Pozzuoli with respect to analogous existing buildings referring to various design epochs. This further analysis shows that the new-design goals would reduce the fatality risk (of the most common building typology in the area) more than 70%. Please see motivation and approach (lines 127-129), discussion (lines 288-310), and methods (445-467).

References

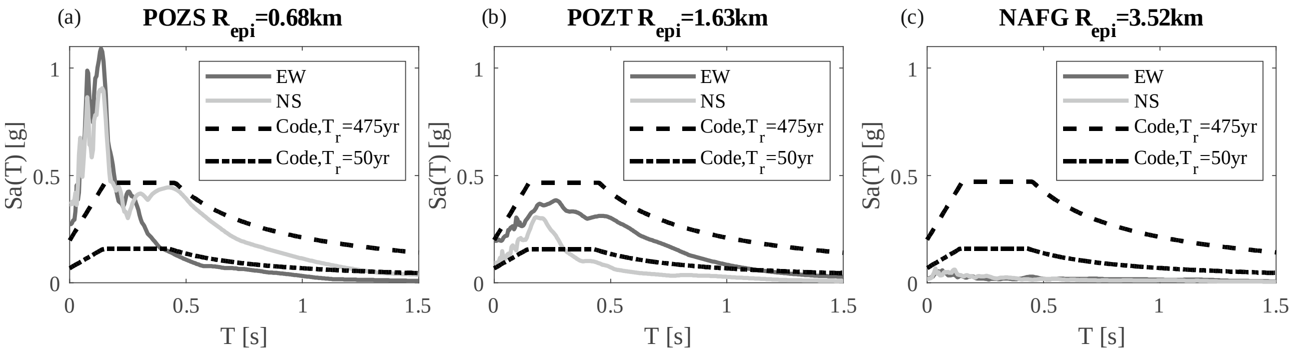
2. Baker, J., Bradley, B., & Stafford, P. (2021). Seismic hazard and risk analysis. Cambridge University Press.

3. Cito, P., & Iervolino, I. (2020). Rarity, proximity, and design actions: Mapping strong earthquakes in Italy. Annals of Geophysics, 63(6), SE671-SE671.

2. The research primarily concludes that the reference earthquake scenarios, in terms of moment magnitude (Mw), for the considered case study area – based on historical (recent) seismicity – fall within the 4.4−5.1 range. This range is notably lower than the minimum magnitudes corresponding to ground motion exceedance for life-safety retrofit as per the Italian building code. This result is somewhat expected. It is well known that earthquakes induced by volcanic activity are generally significantly less severe than tectonic earthquakes. These tectonic earthquakes typically represent the majority of events considered in assessing probabilistic seismic hazard for building code purposes.

[Authors] Authors regret noting that this comment, in its first part, is such that it seems that the manuscript was not read thoroughly, whereas, in the second part (about the severity of the events) it is seriously technically incorrect.

First, recent seismicity is used only to correlate the events to the known faults and to identify other geological structures possibly activated by bradyseism. Second, and most important, the exact point of the study is that the shallowness of these events is such that, in their epicentral area, they can be as severe as large magnitude events in other regions. To support this statement the figure below (Figure 4 in the manuscript) provides the response spectra of the largest magnitude event recorded in the area in the last years. It has moment magnitude about 4.0, yet it caused exceedance of the design spectrum with 475 years return period, from the nationwide PSHA study [4] lying at the basis of the seismic code in Italy.



The reviewer must beware that the area has been repeatedly (over the centuries) affected by low-magnitude fatality-causing earthquakes. For example, the Casamicciola earthquakes 1883, with magnitude estimated at 4.3, killed thousands [5]. The last deadly earthquake occurred in 2017, causing two fatalities, and its magnitude was estimated at 3.8 (http://wpage.unina.it/iuniervo/papers/Report_ground-motion_Ischia.pdf).

The fact that this area has these peculiar features, clearly not captured by national hazard studies due to the unavailability (up to recent years) of densely recorded data, is one of the main motivations of this study (please see manuscript lines 140-151).

References

4. Stucchi, M., Meletti, C., Montaldo, V., Crowley, H., Calvi, G. M., & Boschi, E. (2011). Seismic hazard assessment (2003–2009) for the Italian building code. Bulletin of the Seismological Society of America, 101(4), 1885-1911.

5. Rovida A., Locati M., Camassi R., Lolli B., Gasperini P. (2020). The Italian earthquake catalogue CPTI15. Bulletin of Earthquake Engineering, 18(7), 2953-2984. https://doi.org/10.1007/s10518-020-00818-y

3. Furthermore, probabilistic seismic hazard analysis at the basis of building code relies on several conservative assumptions – at various analysis stages – for instance, in the selection of maximum magnitude, ground-motion model selection, uncertainty soruces, etc. The authors themselves state that "The magnitudes of the events occurred so far are small enough to be mostly neglected in the national probabilistic seismic hazard analysis (PSHA) that determines the design ground motions to be used for seismic design according to the current building code, which considers events of local magnitude starting at about 4.3." (page 2), and continue with similar discussions through different points on the same page.

[Authors] This comment contains unsupported, if not incorrect, technical arguments. First, hazard and risk studies should not contain neither conservative nor conservative assumptions, as they could lead to biased results, which in turn could lead to non-rational decisions. Hazard and risk studies only have to reflect the knowledge on the phenomenon of interest, as literature discusses [6]. For example, if a hazard study would contain conservative assumptions the rates (or return periods) of ground motions would not be those the analysis provides as the results, and therefore they would not serve to control (in time) the performance of the structure designed based on such results. Second, there is no documented evidence that the PSHA at the basis of the Italian code has conservative assumptions, nor the review, for example the maximum magnitude in [4] are lower than those considered in subsequent studies [7,8].

In any case, it is not clear how also this comment is relevant for the presented study, in which the nationwide PSHA is considered only as it provided the design ground motions according to the seismic code, which would be used to upgrade the buildings in the area if a code-compliance risk reduction strategy would be enforced.

References

6. Erto, P., Giorgio, M., & Iervolino, I. (2016). About knowledge and responsibility in probabilistic seismic risk management. Seismological Research Letters, 87(5), 1161-1166.

7. Meletti, C., Marzocchi, W., D'amico, V., Lanzano, G., Luzi, L., Martinelli, F., ... & Seno, S. (2021). The new Italian seismic hazard model (MPS19). Annals of Geophysics, 64(1).

8. Woessner, J., Laurentiu, D., Giardini, D., Crowley, H., Cotton, F., Grünthal, G., ... & SHARE consortium. (2015). The 2013 European seismic hazard model: key components and results. Bulletin of Earthquake Engineering, 13, 3553-3596.

4. Moreover, the case-study area is particularly complex, and many believe that the primary concern should be volcanic hazard and risk rather than solely focusing on earthquake-induced ground motions. Despite high volcanic hazards, the area has witnessed significant urban development and population growth. It is estimated that 20% of the buildings in the so-called "red zone" (a high-risk area due to volcanic activity) are unauthorized (illegal), particularly vulnerable and aging significantly. The existing roads and transport infrastructure are ill-equipped to support potential evacuations. This complexity makes mitigation efforts and securing the population challenging at the intersection of urban planning and public policy. The main (and only) message from the study, i.e., you should retrofit existing buildings in the area following the current Italian building code for new constructions, while essential, neglects a series of other critical issues, i.e., some of these buildings should simply not be there.
[Authors] As stated in abstract, introduction, and motivation sections, this study responded to a specific request from local administration, which was the following: *given that the current eruption hazard remails low according to volcanologists, and the current main hazard for the area is seismic (although related to the volcanic unrest), would be an effective risk reduction strategy to upgrade substandard existing buildings to the current code safety levels?* The study presented represents a collection of the *best available science* [9] to answer such a question, showing that the minimum magnitudes to cause exceedance of retrofit ground motion goals are larger than those of the reference scenario. Given that the study responded to a precise request of decision makers to the scientific community, to consider any another goal, including the one suggested to the reviewer, would have meant failure of the scientific job (not entering the discussion whether the suggestion of the reviewer is sound, given that there's impending eruption and therefore no current interplay between eruption and seismic hazards according to volcanologists, see references).

References

9. Jordan, T. H. et al. Panel Review of the USGS 2023 Conterminous U.S. Time-Independent Earthquake Rupture Forecast. Bulletin of the Seismological Society of America 114, 572–607 (2024).

5. Unfortunately, the author missed an opportunity to address these pressing issues. A more comprehensive approach would have been to address the interplay between seismic and volcanic risks, the implications of unauthorized constructions, and the challenges of evacuation in the face of a dual-threat scenario. Such an approach would have provided a more holistic view of the situation at Campi Flegrei and could have offered valuable insights for policy-making and urban planning in similar high-risk areas, making the paper suitable for publication in a high-impact journal such as Nature Communications.
[Authors] The risk investigation of the study was not an academic exercise from the authors. As discussed in the reply to the previous comment, it was the response to a precise scientific request of the government. Authors cannot imagine a more impacting risk study, given that any decision taken based on these results would affect the life and safety of tens of thousands of people. Is not decision making (actual and not imagined) the ultimate goal of risk analysis, there's any larger impact of research the reviewer is looking for?

REVIEWER 2
The manuscript concerns the mitigation of seismic risk at Campi Flegrei. It aims to investigate whether the structural retrofitting policies based on the rules for new constructions could be a viable strategy in the Campi Flegrei area. The authors focus their attention on the study of possible reference scenarios. They approach this study based on fault mapping, geomorphological inference, high-precision earthquake relocation performed in previous work, stress-drop analysis, and ground motion modeling. The authors conclude that the reference scenarios, in terms of Mw, are in the range 4.4-5.1, and estimate the time-variant exceedance probability for these magnitudes. Furthermore, the article highlights that the minimum magnitudes expected to cause the exceedance of the ground motion thresholds for life-safety established by the Italian building code are generally larger than those of the reference scenarios.

Main comments.

1. The article is well-written. I appreciate its simplicity and the focus on the practical aspects of risk management in the area of the Campi Flegrei volcano, which is in unrest. However, I think that some parts of the article require a little further refinement. For example, the fault selection method to obtain the reference magnitude scenarios is not very clear to me.
[Authors] Agreed. Please see reply to comment #11 below.

2. Furthermore, in the abstract a concept like: "the magnitudes of the events ... are small enough to be mostly neglected in the national probabilistic seismic hazard analysis ... Nevertheless, the structural seismic actions are generally non-negligible ..." (from the introduction) could better introduce the reader to the focus of the article, which appears to be the relationship between the magnitude of earthquakes and the ground motion thresholds, in terms of Sa, and its possible implications on the adequacy of Italian building code for seismic risk in this volcanic area.

[Authors] Agreed. To comply with the comment of the reviewers the abstract now reads: "*Some earthquakes caused seismic structural actions generally non negligible, in their epicentral area, even if compared to the current-code design standards. Therefore, public concern was sparked with national government decision to allocate resources to mitigate seismic risk.*" Please see lines 13-15 of the revised manuscript.

3. In general, I find that the authors should better highlight (e.g. in the discussion) how their study is important for seismic risk mitigation at Campi Flegrei. In the abstract the authors say that, to aid decision making, it was investigated whether structural retrofitting policies based on the design goals for new constructions could be a viable strategy in the area. In the introduction, they pose at least three possible, non-alternative, motivations regarding the relevance of shaking intensity for relatively-low magnitude earthquakes at Campi Flegrei. However, in the text, and especially in the discussion, I do not feel that there are answers to these questions, or that the questions, posed very directly in the introduction, are discussed clearly in light of the results of the article.
[Authors] Agreed. To comply with this comment, an explicit risk analysis was carried out computing the fatality risk reduction of new-design reinforced concrete buildings at Pozzuoli with respect to analogous existing buildings referring to various design epochs. Fragility curves of the considered buildings are taken from relevant technical literature (developed by the authors in previous studies), whereas hazard is computed based on the operational earthquake forecasting results. It is shown that, even in the case of exceedance of the reference magnitude scenarios, to bring the buildings to the safety levels of new construction would cause a fatality risk reduction larger than 70%. Please see motivation and approach (lines 127-129), discussion (lines 288-310), and methods (445-467).

4. Also, the title is directly focused on seismic risk mitigation at Campi Flegrei, but this concept is not fully exploited and explained in the final discussion. For these reasons I recommend publication on Nature Communications after minor revisions.
[Authors] Because explicit (quantitative) risk assessment (reduction) is now shown in the study, authors prefer to keep the title as is, if possible.

Minor comments-

5. Line 8 I suggest deleting Phlaegraean Fields in the brackets and possibly inserting Italy.
[Authors] Authors would rather keep Phlaegraean Fields as the area is often called this way in scientific literature. Nevertheless, to comply with the comment of the reviewer, the sentence was slightly modified as follows (please see lines 8-9 of the revised paper): "*Campi Flegrei (Phlaegraean Fields) is a densely populated volcanic area in Italy, located nearby the city of Naples*".

6. Line 11 The authors should check the number of earthquakes with Md>-1.1, which should be greater than 18000, to date. Perhaps the authors are referring to located earthquakes.
[Authors] Yes, (about) nine-thousand is the number of earthquakes (with duration magnitude larger than -1.1) according to a high-precision relocation of the seismicity recorded in the last decade (Scotto di Uccio et al., 2024). This was clarified in the revised paper, reading (lines 10-11): "*…more than nine-thousand earthquakes with duration magnitude* (M_d) *above* -1.1 *were recorded,…*".

7. Lines 18 - 21 I suggest reporting, possibly in parentheses, the exceedance probability for these magnitudes, and what is the minimum magnitudes expected to cause the exceedance of the ground motion thresholds for life-safety retrofit by the Italian building code.
[Authors] Agreed. To comply with the comments, the revised paper reads (lines 21-27): "*Using earthquake forecasting tools, the probabilities of these magnitudes being exceeded, to account for unknown and/or extended ruptures, were also quantified (as of May 21 2024 they were 5.0% for* $M_w = 5.1$ *and 17.4% for* $M_w = 4.4$*). Probabilistic earthquake engineering analysis finally showed that the minimum magnitudes expected to cause undesired building performance in the case of code-based retrofitting are larger than those of the reference magnitudes. For example, at the site of Pozzuoli, within the caldera, such minimum magnitude are from* $M_w = 4.7$ *or larger, depending on the spectral ordinate of interest.*".

8. Lines 23 - 24 "Campi Flegrei (Phlegraean Fields) is a volcanic area, the caldera of which was shaped by two major eruptions, about 39000 and 15000 years ago, and located ...". To improve the clarity of the text, I suggest reformulating the opening sentence of the introduction, also considering the previous suggestion (line 8).
[Authors] Agreed. To comply with the comment, lines 30-32 of the revised paper read: "*Campi Flegrei (Phlegraean Fields) is a volcanic area located a few kilometers west of the city of Naples, in southern Italy.*[1] *The caldera of Campi Flegrei was shaped by two major volcanic eruptions, about forty-thousands and ten-thousands years ago.*"

9. Figure 1. In the inset there is the Campania region and not a point indicating the position of the Campi Flegrei. It seems to me that this could be misleading because it can make a reader who does not know the area well think that this is the Campi Flegrei caldera. I suggest using a symbol like a circle to indicate the position of the Campi Flegrei volcano.
[Authors] Agreed, the Campania region has been replaced by a dot representing Campi Flegrei. Please see revised Figure 1.

10. Lines 69 – 71 I suggest adding some information about the seismic stations such as sampling rate and sensor type. Were these stations installed by the Federico II University? If so, when were they installed?
[Authors] Agreed. To comply with the comment, lines 113-114 of the revised manuscript read: "…*and the design spectra for ordinary constructions at sites of considered stations. Data are from Rete Accelerometrica Nazionale and are available at https://ran.protezionecivile.it/IT/index.php.*".

11. Lines 163 – 165 and Figure 5 The fault selection method is not very clear to me. Reading the caption of Figure 5, in particular concerning panel 5b, it seems that the morpho-structural analysis identifies the faults based on the alignments of events. However, the faults and dykes, along which the eruptive vents are generally located, should not have the same direction with respect to the stress axes that characterize the area. Therefore, I think that this criterion may not be very effective for identifying potential seismogenic faults. I suggest reconsidering this fault detection criterion and checking whether the final subset of selected faults changes significantly.
[Authors] In fact, the morpho-structural analysis identified the fault traces based on several features, including rectilinear scarps, coastal cliff lineaments, linear subsequent streams, and alignments of the volcanic edifices' centers. The latter is intended as the eruptive vents alignment in some cases confirmed the fault derived by other studies or determined the tracing of new faults. The occurrence of such alignments has been widely discussed in the literature [10,11,12]. Therefore, authors believe there's no evidence for the need of revising the fault identification method. To clarify these aspects highlighted by the reviewer, the text was revised as: "*The morpho-structural features considered included rectilinear scarps, coastal cliff lineaments and linear subsequent streams. In some cases, these features follow the distribution (alignments) of the volcanic edifices' centers.*[40,44–46]" Please see lines 359-361 of the revised manuscript.

References

10. Di Vito MA, Isaia R, Orsi G, et al. Volcanism and deformation since 12,000 years at the Campi Flegrei caldera (Italy). Journal of Volcanology and Geothermal Research. 1999;91(2):221-246. doi:https://doi.org/10.1016/S0377-0273(99)00037-2.

11. Orsi G, Civetta L, Del Gaudio C, et al. Short-term ground deformations and seismicity in the resurgent Campi Flegrei caldera (Italy): an example of active block-resurgence in a densely populated area. *Journal of Volcanology and Geothermal Research*. 1999;91(2):415-451. doi:https://doi.org/10.1016/S0377-0273(99)00050-5.

12. Piochi M, Kilburn CRJ, Di Vito MA, et al. The volcanic and geothermally active Campi Flegrei caldera: an integrated multidisciplinary image of its buried structure. *International Journal of Earth Sciences*. 2014;103(2):401-421. doi:10.1007/s00531-013-0972-7.

12. Line 172 Please specify which sequence by reporting the date or date range (e.g. between 16/03/2022 until 04/10/2023).

[Authors] Agreed. The revised paper reads (please see lines 215-216): "…*based on the measured stress-drop of the events recorded between mid-March 2022 and the end of May 2024,…*".

13. Line 202 and Figure 6 Panels 6b and 6d are not easy to read due to overlapping colors. Please, try to improve these figures. Perhaps it might be better to use error bars.
[Authors] Agreed. Figure 6, that is, Figure 8 in the revised manuscript, was revised to improve its readability. Please check revised manuscript.

14. Lines 215 – 224 In the first rows and first columns of tables 3 and 4 (within 1 km of the Rione Terra site) I read minimum moment magnitudes of strong earthquakes of 3.3 and 4.0. This seems to me to be at odds with the statement that these magnitudes are generally larger than 4.4 for both return periods. I find this to be a very delicate point of the article and for this reason I think that more discussion could be useful.
[Authors] Agreed. To comply with the comment, the paper was revised as follows (please see lines 260-263): "*Taking as the reference (moment) magnitudes the values between 4.4 and 5.1 (see Reference magnitude scenarios), it emerges that these magnitudes are equal to or, in most cases, larger than 4.4 for both return periods associated to retrofit of ordinary constructions, when they would occur within 5 km from the site of interest.*" Moreover, lines 271-

285 read: "*According to the (extrapolation of the) GMM calibrated for the area, an earthquake with $M_w = 4.4$ occurring within 5 km from Rione Terra has a probability of exceeding the ground motion intensity with $T_r = 50yr$ [$T_r = 475yr$] at the site which is, in most cases, lower than 0.5, being it equal to 0.52 [0.17] for PGA, 0.38 [0.07] for $Sa(T = 0.3s)$, 0.19 [0.01] for $Sa(T = 1.0s)$ and 0.08 [<0.01] for $Sa(T = 1.5s)$; considering $M_w = 5.1$ these probabilities are always larger than 0.5 for $T_r = 50yr$, whereas they are lower than or about 0.5 for $T_r = 475yr$, depending on spectral ordinate. For earthquakes occurring closer, that is, within 1 km, the minimum magnitude expected to cause exceedance can be lower than 4.4; nevertheless, this especially applies to PGA, which is not generally relevant for seismic structural performance. For $T_r = 50yr$, the minimum magnitude of strong earthquake is lower than 4.4 also for $T = 0.3s$, which, however, can be considered a vibration period close to the lower bound of a large deal of common structures. This leads to conclude that the design ground motions enforced by the building code are – generally – not expected to be exceeded by an earthquake occurring in the caldera if not in the case of multiple fault activation or activation of unmapped or extended sources. This conclusion holds even when design spectral ordinates, at vibration periods equal to or larger than $T = 1.0s$, with $T_r = 475yr$ are reduced by 40%, while at relatively low vibration periods the exceedance is expected for magnitudes equal to or lower than $M_w = 4.4$, when they occur within 1 km from the site. Note that these results are consistent with what observed for the $M_w = 4.0$ event discussed above.*" Finally, the tables of the minimum magnitudes of strong earthquakes have been provided with a color code where green identifies strong earthquakes above the minimum of the reference scenarios, those within 0.3 magnitude units from the minimum of the reference scenarios are in yellow, and those below 0.3 magnitude units from the minimum of the reference scenarios are in light red. Please see revised tables 2 and 3 and lines 258-260: "*Color coding in the tables reflects magnitudes of strong earthquakes above the minimum of the reference magnitudes (green), those within 0.3 magnitude units from the minimum of the reference magnitudes (yellow), and those below 0.3 magnitude units from the minimum of the reference magnitudes (light red).*".

15. Lines 231 – 233 The reference to the uplift rate is not very clear. I don't understand how considering the variation of the uplift rate the probabilities to observe a M_w≥5.1 or a M_w≥4.4 go from 2.7% to 0.3% and from 10% to 1.3%, respectively. There are papers that study the relationship between the uplift rate and the occurrence rate of earthquakes that could be cited. In any case, for greater self-consistency of the discussion, I suggest the authors refer to the decreased rate of seismicity rather than the decreased rate of uplift (that enters the article here for the first time), as the cause of the decrease in probabilities.
[Authors] Agreed. The reviewer is correct that the probabilities of magnitudes inferred from earthquake forecasting are derived from the seismicity rates only, therefore now reference is solely made to such rates rather than uplift rates, please see lines 245-253. As it regards references of the relationship between the uplift rate and the occurrence rate of earthquakes, authors are – unfortunately – not aware about quantitative papers on this issue about Campi Flegrei. In any case, as said above, the reference is now solely made on seismicity.

16. Line 235 I find that the discussion is incomplete and not explicit. I suggest the authors expand the discussion to highlight the significance and possible positive effects of their study in terms of seismic risk mitigation at Campi Flegrei, as promised in the title of the article. I think it is useful to return to the statements and questions mentioned in the abstract and introduction and discuss them in light of the results of the article (are the structural retrofitting policies based on the rules for new constructions a viable strategy in the Campi Flegrei area? Etc., see main comment).
[Authors] Agreed. To comply with this comment, an explicit risk analysis was carried out computing the fatality risk reduction of new-design reinforced concrete buildings at Pozzuoli with respect to analogous existing buildings referring to various design epochs. Please see introduction (lines 127-129), discussion (lines 288-310), and methods (445-467). See also the reply to comment #3.

17. Lines 284 – 287 See comment relating to lines 163 – 165.
[Authors] Please see reply to comment #11.

REVIEWER 3
This paper studies whether the seismic provisions of the current Italian building code adequately protect structures to withstand earthquakes that originate within the Phlegraean Fields, a volcanic area in Italy. The estimated maximum magnitude that an earthquake in the area can generate is compared to the minimum magnitude expected to cause an exceedance of the design levels of the code, concluding that, in most cases, the former magnitude is lower than the latter magnitude. Thus, the study implies that the current Italian building code would provide safety against these earthquakes (although this is not explicitly stated in the manuscript).
I find this study very interesting because it is an effort to answer a pressing real-life question that is important for the safety of many people in the region. However, before I can recommend the publication of the manuscript, I ask that the authors address my comments presented below.

1. I do not understand the point of the earthquake forecasting section. It is being used to compute probabilities of exceedance of certain earthquake magnitudes, but nothing is then concluded from this information. The end of the Discussion section is a perfect place to conclude something from this analysis.

[Authors] Agreed. To comply with this comment a specific discussion sub-section which includes explicit risk analysis making use of the forecasting results is added. Please see lines 288-310. In particular, the fatality risk reduction of new-design reinforced concrete buildings at Pozzuoli with respect to analogous existing buildings referring to various design epochs, was computed. Fragility curves of the considered buildings are taken from relevant technical literature, whereas hazard is computed based on the operational earthquake forecasting results. It is shown that, even in the case of exceedance of the reference magnitude scenarios, to bring the buildings to the safety levels of new construction would cause a fatality risk reduction larger than 70%. Please see motivation and approach (lines 127-129), discussion (lines 288-310), and methods (445-467).

2. The method used in this study is very ad-hoc to the specific region. Readers of this paper may be interested in how it applies to other regions or situations. I would at least add a comment on the applicability of the proposed method to other cases.

[Authors] Agreed. The presented study is very specific for the Campi Flegrei area and its bradyseism situation. Nevertheless, the interdisciplinary approach integrating data and methods from geology, geophysics, and earthquake engineering, can be exported in other situations where rapidly assembling the best available science for risk management and decision making from authorities is needed. This is now specified at lines 108-111 of the revised manuscript: "*Although the study is very specific to the bradyseism of Campi Flegrei, the interdisciplinary approach integrating data and methods, from geology, geophysics, and earthquake engineering, intends to represent an example of rapidly assembling the best available science[20,21] to evaluate potential risk mitigation strategies and then aid decision making that can possibly be of reference for other similar emergencies.*".

3. Equation 7: It seems indirect to me to be comparing earthquake magnitudes because the difference between the minimum magnitude of strong earthquakes and the reference magnitude does not mean anything by itself. In other words, the greater the difference between the magnitudes the better, but the difference is not very meaningful. Maybe it would be more informative to compute the probabilities of exceedance at each period, that is, to set the reference magnitude to consider (4.4 or 5.1) and the distance range (R<z) and compute the probability that the spectral acceleration at a given period exceeds the design spectral acceleration. I can understand if changing this part of the method is unfeasible at this point, but I would like to understand why the authors made this decision.

[Authors] The exceedance probability of design intensities, given that an earthquake with M_w of 4.4 or 5.1 occurs within a certain distance from the site, is of course invaluable information, as the reviewer points: in fact, it is given by the integral in equation 7, written for $M_w = 4.4$ (or 5.1). On the other hand, authors believe that this probability alone is a useful yet somehow limited information, at least in the kind of study presented. This is because M_w 4.4 (or 5.1) is the reference magnitude scenario, that is, "*The reference magnitudes are defined here as the magnitude range corresponding to the mapped faults possibly activated during the volcanic unrest*" (lines 197-198) but, at the same time, it is not the only possible scenario. For these reasons, the revised paper quantifies the exceedance probabilities, in compliance with the reviewer's comment, yet leaving the discussion on the minimum magnitude of strong earthquakes. Please see lines 271-275 of the revised paper: "*…an earthquake with $M_w = 4.4$ occurring within 5 km from Rione Terra has a probability of exceeding the ground motion intensity with $T_r = 50yr$ [$T_r = 475yr$] at the site which is, in most cases, lower than 0.5, being it equal to 0.52 [0.17] for PGA, 0.38 [0.07] for $Sa(T = 0.3s)$, 0.19 [0.01] for $Sa(T = 1.0s)$ and 0.08 [<0.01] for $Sa(T = 1.5s)$; considering $M_w = 5.1$ these probabilities are always larger than 0.5 for $T_r = 50yr$, whereas they are lower than or about 0.5 for $T_r = 475yr$, depending on spectral ordinate.*".

Some other comments and issues that I found while reviewing the manuscript that I think are less important but should still be addressed are:

4. Abstract: I would add a sentence at the end of the abstract that states the implications of the minimum magnitude that causes exceedance of the ground motion threshold being larger than the magnitude of the reference scenario.

[Authors] Agreed. To comply with the comments, the paper was revised as follows (please see lines 23-27): "*Probabilistic earthquake engineering analysis finally showed that the minimum magnitudes expected to cause undesired building performance in the case of code-based retrofitting are larger than those of the reference magnitudes. For example, at the site of Pozzuoli, within the caldera, such minimum magnitude are from $M_w = 4.7$ or larger, depending on the spectral ordinate of interest.*".

5. Figure 1b: The y label on the right: "Nr. of earthq. recorded in the last year" is confusing. Consider changing "in the last year" to "per year". Also, the dates on the x-axis are very crowded. I would use fewer dates and/or change the date format.
[Authors] Agreed. The font of some dates was reduced to improve readability of the x-axis. Also, the y label of the revised figure is “Cumulative nr. of recorded earthq.”. Please check the revised paper.

6. Figure 3: It may be a good idea to show the epicenters of the 4 selected earthquakes in Figure 1a.
[Authors] Agreed. In the revised paper, Figure 1a shows the epicenter of the earthquake the spectra of which are represented in Figure 3. Please see revised paper.

7. Figure 3: panels (b) and (c) have gray lines that are not mentioned in the figure caption. What do these lines represent?
[Authors] There was a typo. However, this comment does not longer apply because the figure was modified. Please see revised paper.

8. Table 2: Is this table really necessary? It just shows two numbers.
[Authors] Agreed. The table was removed.

9. Figure 9: "for some magnitude" is repeated in the caption.
[Authors] Agreed. The caption was revised.

10. Equation 6: is the logarithm in base 10? If so, this should be clearer.
[Authors] Agreed. In the equation, low was replaced by $\log_{10}$.

11. Equation 7: I recommend adding an equation that explains how the exceedance probability is computed from the mean and standard deviation of the GMM.
[Authors] Agreed, at lines 417 it is now explicated that the exceedance probability is computed from the GMM via the cumulative complementary Gauss function. Please see revised manuscript.

REPLY TO REVIEWERS' COMMENTS

REVIEWER 3

1. The authors have dealt with previous comments adequately. However, I think there is an error in the exceedance probability formula added after equation (7): Line 417: Within the standard normal CDF, the numerator should have the difference between the base 10 logarithm of the spectral acceleration variable (i.e., saTr) and the mean base 10 logarithm from the ground motion model, that is, (log10(Sa(T)) – mu). However, the formula provided has(log10(Sa(T)) – saTr), which does not make sense.

[Authors] Agreed. It was a typo (calculations are correct). It is now fixed as $P\left[Sa(T) > sa_{T_r}|M_w = m, R = r\right] = 1 - \Phi\left[\left\{\log_{10}\left(sa_{T_r}\right) - \mu_{\log_{10}[Sa(T)]}(m,r)\right\}/\tau\right]$. **Also, lines 409-410 now clarify that *"[…], and $\boldsymbol{\mu_{log_{10}[Sa(T)]}(m,r)}$ is the mean of the logarithm of $\boldsymbol{Sa(T)}$ conditional to magnitude and distance according to the GMM.*". See revised manuscript.**

2. Line 447: Parameter η was already used in Equation (6) for the residual of the GMM. Please use another letter for the logarithmic standard deviation of the fragility functions.

[Authors] Agreed. The residual of the GMM is now denoted by γ in Eq.6. Please see revised manuscript.

3. Line 447: Equation (9): There is an error in this equation because the total probability theorem was not used correctly. The right side of the equation gives you the hazard curve (i.e., mean annual frequency of exceedance of Sa), not the PDF of the Sa. To get the PDF of the Sa, the distribution being integrated should be a PDF, not a complementary CDF. An easy way to see that there is an error in equation (9) is to notice that the units on both sides of the equation do not match because the left side has units of 1/acceleration and the right side is unitless. Another more elegant way to solve the error is to keep the right side of the equation and write the hazard curve on the left side of the equation, and then modify equation (8) to use the hazard curve instead of the PDF of Sa.

[Authors] Agreed. It was a typo (calculations are correct). $\boldsymbol{P[Sa(T) > sa|M = m, R = r]}$ is now replaced by $\boldsymbol{f_{Sa(T)|M=m,R=r}(sa)}$ in Eq.9. Please see the revised manuscript.

4. Line 460: Why was the minimum magnitude set to 4.4 and 5.1? These were the maximum magnitudes obtained from the faults. Why use them as the minimum magnitudes of the analysis? The minimum magnitude for these types of analyses is not a seismological parameter, it is an engineering parameter that depends on the system being studied. For this case, it should be set at a level where we are sure that no collapse is possible in the buildings being studied. Thus, it has nothing to do with the magnitude of completeness. I would just select a single value of minimum magnitude following this guideline and modify Table 4 accordingly.

[Authors] Agreed. The reviewer is correct that the minimum magnitude for integration is a choice of the analyst and reflects the minimum magnitude for which structural risk is not negligible. 4.4 and 5.1 were used for a combination of two reasons: (i) the fragility curves used for risk analysis are not developed for lower magnitudes (see fragility references); (ii) the structural response observed in the events recorded so far at Campi Flegrei shows that, lower magnitude events, than those considered determined can be factually neglected from the risk assessment, which helps using the fragility curves considered. Nevertheless, to (prudently) also use the completeness magnitude, enable supporting such a choice indicating that lower magnitudes do not largely contribute to the risk. To comply with the comment of the reviewer, this is now better clarified at lines 451-457 that now read: "*In fact, the parameter $\boldsymbol{m_{min}}$ is intended as the minimum magnitude for which the fatality risk is considered not to be negligible. $\boldsymbol{m_{min}}$ was set at the reference magnitudes, that is $\{\boldsymbol{4.4, 5.1}\}$ because the used fragility curves were developed for larger magnitudes than those observed so far in the area and to isolate the effect on risk of the reference magnitudes and larger. Using also $\boldsymbol{m_{min} = 1.5}$, enabled showing that lower magnitudes do not affect the conclusions on risk reduction.*" Please see the revised manuscript.

5. Line 460: Also, why was the maximum magnitude set to 6.5? Would it make more sense to use 5.1, which was the maximum magnitude obtained from the faults?

[Authors] Although the reviewer is right that 5.1 is maximum magnitude obtained from the faults, it cannot be excluded that the rupture will be on unmapped faults or extend beyond those mapped, as discussed in the specific section of the paper dealing with exceedance of the reference magnitudes. Therefore, a conservative maximum magnitude was used. Magnitude 6.5 is similar to the maximum magnitude adopted for the region by the national (official) model of Stucchi et al. (2011), which also considers tectonic earthquakes in the region. This is why it was chosen. To comply with the comment of the reviewer, this is now better clarified at lines 455-457, which now read: "*To set $\boldsymbol{m_{max} = 6.5}$, which is a value close to the maximum magnitude attributed to the larger region Campi Flegrei are enclosed in by nationwide studies (Stucchi et al., 2011),enabled accounting for what discussed in the section Exceedance probability of reference magnitudes*." Please see revised manuscript.

REVIEWER 4

1. The soil classification adopted in the GMM developed for Campi Flegrei (equation 6) is rather rough, considering only the soil classes B and C and not the VS30 value. Moreover, the subdivision in class B and C (figure 10 c) is derived from the shakemap from Michelini and Faenza (line 390) that does not provide a detailed geomorphological classification.
[Authors] EC8 B, C classes are the only site conditions for the recording stations considered. This information is consistent between Michelini et al. (2020), and the similar study by Forte et al. (2019). EC8 classification is used for three reasons: (i) Vs30 of seismic monitoring station sites is often not measured and often offered by surface geology; (ii) the stations are few, and to considers EC8 site classes enables more robust GMM calibration pooling different stations together, rather than discriminating the Vs30; (iii) fragility curves used for risk analysis are developed for EC8 site conditions rather than Vs30. To comply with the comment of the reviewer, lines 377-380 of the revised paper now read as follows: "*The site condition of the stations was taken from the soil classification used for the Italian implementation of ShakeMap (Michelini et al., 2020), in terms of class, and shown in Figure 10c. Such a classification, which is similar to that provided by other studies (Forte et al., 2019), shows that B or C class is attributed to most station sites. The number of records and stations did not enable to consider further EC8 site classes or more refined data such as the shear wave velocity at surface proximity.*". Please see revised manuscript.

2. It would be appropriate to clarify better the use, for magnitudes larger than 4, of the GMM of Bindi et al. that is not suitable for volcanic earthquakes as correctly discussed in lines 377-383.
[Authors] The GMM developed is the only one being calibrated ad-hoc for Campi Flegrei volcanic area to date. However, its use for magnitude (*M*) larger than 4, which is needed for risk analysis, requires extrapolation, which the authors do not trust. Other alternative GMMs for volcanic areas, such as Tusa and Langer (2016) and Lanzano and Luzi (2019), also refer to low magnitudes. The GMM of Bindi et al. (2011) was chosen, as it applies for magnitude [4-6.9] as already discussed (line 477). This is believed to be a conservative choice given the relatively rapid attenuation with distance specific of Campi Flegrei. The comply with the comment of the reviewer, lines 422-428 now read: "*Therefore, the results presented in Figure 5 and Figure 6, Table 2 and Table 3, were based on significant extrapolation of the GMM, which could not be solved even using the few other GMMs for volcanic areas available (Tusa and Langer, 2016; Lanzano and Luzi, 2019). To account for this issue, the minimum magnitude of strong earthquakes in the area was also computed using the GMM of Bindi et al. (2011) deemed to be one the best performing models for large scale PSHA in Italy (Lanzano et al., 2020). More specifically, such GMM was used in lieu of the one developed for Campi Flegrei for M_w larger than 4.0, as it applies for M_w between 4.0 and 6.9. This is considered a conservative choice given the relatively fast attenuation with distance observed in the area*". Please see revised manuscript.

Based on the previous considerations, I believe that the manuscript ca be published in its present form, provided that a response is given to my two comments.