

## Response to Anonymous Referee #2

First of all, I would like to thank the anonymous referee #2 for his remarks and his constructive comments. Following, you can find my response to the general and the specific comments.

### Reply to Interactive comment:

The main requirement of the referee is to recognise that IBM is not a standalone method because it has no predictive power, however, it can improve and complement the existing VCs. This, according to the referee, should be shown in the paper in the form of a framework highlighting the contribution of each approach in vulnerability studies. My reply to each point of the referee in the general comments follows:

-Predictive power: I appreciate the comments of the referee and I totally agree with some statements, for example, “two methods that are founded on two different concepts” or “IBM mainly refers to the "intrinsic" susceptibility of a building to suffer damage”. However, as far as the predictive power of the IBM is concerned, I have some objections. VCs are developed based on empirical data and are not transferable. IBM may be used where empirical data or curves are not available and has a predictive power in the sense that it may identify the relative vulnerable buildings in a qualitative way. It may not predict monetary loss (or degree of loss) but it may indicate the buildings that will experience loss based on their characteristics. There are numerous disadvantages of the IBM method and space for improvement but the fact that it can be applied where no empirical data are available is considered an advantage. Moreover, as it was also suggested by the referee, it may be used to improve existing VCs.

-IBM is not stand alone: IBM as a qualitative method of assessing relative physical vulnerability is standalone. However, it is true that the IBM may benefit from information coming from VC and this will be shown in the holistic framework (required by the referee) which will be included in the revised version.

-Flexibility vs. subjectivity: the issue of subjectivity has been discussed in the paper and solutions have been proposed (e.g. correlation of real damage data to building characteristics). A sensitivity analysis would be certainly worthwhile but would probably provide the material for the next paper following this one. I could refer to the sensitivity analysis, however, in the section of recommendations for improvement and future developments (page 24-25).

-“Too many concepts refer to the same thing”: I agree with this statement and I recognise that a debate about vulnerability, different dimensions and definitions is missing mainly because it has been provided in previous papers by the author and colleagues. The revised version will definitely include more reference on this topic.

-Real interest of local authorities: The vulnerability curve presented in this paper was the product of very close collaboration of the local authorities in South Tyrol. The methodology for the development of the curve was based on the results of a stakeholder workshop. There was an effort to cover the needs of the end users and to also use their expert knowledge to develop the curve. There is definitely an interest from the side of the practitioners for the vulnerability curves and this is mainly because the curves provide a quantitative result.

Indicator based methodologies are not that attractive mainly because of the amount and detail of the data required.

-Importance of intensity: The fact that intensity is ignored has been recognised and discussed in the paper. The framework that will be included in the revised version will attempt to address this problem as well.

-The referee suggests that “the Author should provide a clear framework by which each of the methods gives its real contributions in vulnerability studies.” The author appreciated the comment and will provide a framework which shows the contribution and the interactions of the methods in the revised version.

- self-citations should be a little bit reduced: this was also required by referee #1. I will try to reduce them.

### **Reply to specific comments on the manuscript (supplement pdf)**

I would like to thank the anonymous referee for his comments regarding the spelling. I will make the recommended changes in the revised manuscript. As far as the other comments are concerned:

Abstract, line 18: The comment will be taken into consideration and the last sentence of the abstract will change as follows:

*“The comparison of the two methodological approaches and their results **is challenging since both approaches are dealing with vulnerability in a different way. However, it is still possible to highlight their weaknesses and strengths and to show clearly that both methodologies are necessary for the assessment of physical vulnerability and emphasise the need for a “holistic methodological framework” for physical vulnerability assessment.**”*

Page 2, line 3: I agree with the referee, however, there is a focus on debris flow (maybe I should stress this from the beginning of the paper) in the paper. Debris flow affects a limited amount of buildings and for this reason there is often not enough data to develop curves for each type of building. Nevertheless, even if we were able to do that for which characteristic of the building would we develop those curves? (for the building type? Age? Presence of openings? Surroundings?). In any case, a reference to the HAZUS curves is considered necessary (also from referee #1) and it will be included in the revised version.

Page 3, line 1: It is true that the sentence is general. I will remove the sentence in the revised version.

Page 3, line 3: I agree, I will remove “...reviewing methods for the development of vulnerability functions for tsunami”

Page 3, line 9: I will remove: “focusing on tsunamis”

Page 3, line 14: This is an interesting point that I will gladly include in the revised version.

Page 3, line 30: This is not about the specific study but about the use of indicators in general.

Page 4, line 34: this is exactly what I am saying “variety of building characteristics and surroundings” (the intensity is supposed to be the same)

Page 5, line 3: This issue is discussed later in the text (Page 24/lines 22-23, page 25/lines 5-7, Page 25, lines24-26)

Page 6, line 15: I will do so in the revised version

Page 7, line 4: The subjectivity issue is discussed later (Page 24/lines 22-23, page 25/lines 5-7, Page 25, lines24-26)

Page 7, line 4: I will add this reference at the beginning of the paper (page 2, line 10)

Page 8, Figure 2: No, there was no sensitivity analysis performed. I should probably clarify this in the text.

Page 11, Figure 5: The referee is right, however, for debris flow most curves to be found in the literature are lines and not buffered curves. A reference to this could be made in the next version.

Page 12, line 5: More information will be provided in the revised version.

Page 12, line 13: I agree with the referee. The comment will be considered in the revised version.

Page 15, line 17: more info will be added

Page 18, line 7: The referee is right, however, my statement is also right...

Page 18, line 17: The structures of the methods are indeed considerably different, however, they both claim to assess physical vulnerability and to provide a tool that can be used for risk analysis and ultimately for risk reduction. To which degree the two methods achieve that is one of the main aims of the paper.

Page 18, line 26: the fact that the two methodologies should complement and support each other is anyway one of the outcomes of the paper.

Page 18, line 27: This is true. Perhaps a good statement would be that: “although the intensity is not taken under consideration in IBM, information about intensity is hidden in some indicators (surroundings, building row etc.). However, such a statement has been already done at page 18/lines21-23.

Page 18, line 28: I do not agree 100%. The IBM does not have a predictive power when it comes to a specific scenario. However, the development of a vulnerability curve requires empirical data which are not always available. Transferring vulnerability curves from other places is possible only by similar housing design, materials and architecture. IBM could predict the relative vulnerability between buildings and indicate the ones that need reinforcement.

Page 18, line 31: This is exactly what the referee has already mentioned: although IBM is not a standalone procedure it could be used to support VC and on the other hand information derived by VC may support the IBM. For more detail see page 25, line 23 (Improved weighting of indicators).

Table 2: I appreciate the referee's point of view and I will consider it in the conclusions and discussion. I particularly like the suggestion that we do not need more methodologies but the improvement of existing ones.

Page 20, line 9: This is a point that the referee has made before in the text. It is true that "The efficiency of the integration of the methods still has to be proven". I will try to do so in the revised version. This is also a response to the point of the referee in the interactive comment "*the Author should provide a clear framework by which each of the methods gives its real contributions in vulnerability studies*".

Page 20, line 21: This is a comment which has been repeated often until now. It will be considered in the next version.

Table 3:

Measurable: I do not really understand the point of the referee. What does he or she mean with "event types"? hazard types (earthquakes, floods etc.)? It is probably my fault not to mention it earlier but we are talking about vulnerability to one hazard type at a time

Relevant: (C1) Not always, VCs require detailed empirical data which are not always directly accessible. Damage photos, for example, may give direct information about the relevance of indicators.

(C2) I can add this here although the comment has been also made elsewhere in the text

Policy-relevant: I agree with the comment and I will include it in the text.

Measure important: I agree with the comment and I will include it in the text.

Analytically and statistically sound: This statement is probably quite strong. I could write instead: "Although the indicators may give an overview of the actual situation, the links..."

Understandable/easy to interpret: No, we do not want this but this is not the right place to say it. I keep the statement for the discussion or conclusion chapter.

Reproducible: At this point the indicators are tested against the criteria. The statement will be made elsewhere in the text.

Page 22, line 8: I agree and will add this.

Page 22, line 19: I address this point in my response to the interactive comment above

Page 22, line 26: I agree with comment and will add it to the text.

Page 23, line 5: Perhaps it is more correct to say "practitioners use vulnerability curves as a prediction tool rather than to acquire information about specific buildings in an area and for this reason they ignore their spatial component".

Page 23, line 8: I agree with the referee, the word "process" will be removed.

Page 23, line 21: No, because it includes additional information that the VC does not include.

Page 25, line 12: This is a statement that belongs, in my opinion, at the beginning of the paper (probably at page 2, line 10.)

Page 25, line 32: "a comparison of the two methods may shed light to their advantages and drawbacks and may also inform the practitioners on their available methodological choices". I believe that the paper is doing this up to a point. Both methods are used in the same area and even if their one to one comparison is impossible for reasons that the referee has also pointed out, they are both scrutinized and their advantages and disadvantages as well as recommendations for improvement are presented. The recommendation of the referee for a "*clear framework by which each of the methods gives its real contributions in vulnerability studies*" certainly adds a lot to this comparison.

Page 27, line 1: It is true. In my opinion VCs show the relationship between intensity and degree of loss based on empirical data. They do not refer to specific buildings and they absolutely ignore building characteristics. The practitioners may derive only the following piece of information. E.g. in case a debris flow of 2,5m impacts any given building the degree of loss will be 0,5. This does not include any information regarding the building so, it cannot guide any retrofitting for a specific building. The required information for something like that may be given only by the IBM.

Page 27, line 19: A general framework is a requirement of the referee and it will be attempted in the revised version of the paper.

Page 27, line 20: The fact that the curve becomes steeper after the intensity of 1,5m (where the lower level of the windows of the first floor usually is) reveals the importance of the existence of openings. The fact that there are points showing considerable degree of loss with low intensity is connected to the existence of a basement or basement windows that allowed the entrance of material within the basement and of course the occurrence of additional damage. Moreover, buildings that although have experienced high intensity, have not experience high degree of loss are usually buildings with higher initial value due to additional floors. All these observations show that number of floors, existence of basement and openings play a significant role in the amount of damage that a building will experience. These observations will be included in the revised version.

Page 27, line 23: yes, a holistic framework for physical vulnerability will be provided in the revised version.