Reply to comments of referee#2 on nhess-2021-385

General comments:

The article addresses a metodology to draw climate change related risk maps in a transboundary hydrological basin, taking as a case study the Upper Rhyne. The methodology is interesting and the article is well written, but despite this I think that the Authors should clarify three key aspects, before that the article might be recommended for publication:

1. The definition of risk and of its components. The Authors correctly report that many approaches are available in the literature to define risk (R) and its components. What remains unclear is the approach followed by the Authors and how the terms of hazard (H), exposure (E) and vulnerability (V) are definied and combined. As the focus is on natural risks, I would suggest to adopt the classical form R = H E V and to evidence, on the basis of the literature, why and how other authors' definitions differ from this form;

Thank you for this comment, which has also been addressed by reviewer 1. After the reviewers comments we agree that our initial conceptual framework needs improvements/clarification and will therefore perform the calculations on the basis of the simple formula Risk = Hazard*Vulnerability (Exposure + Sensitivity). We will highlight this more prominently. In our response to reviewer 1 we explain in more detail the initial rationale of our risk framework. See also figure 2 for the revised conceptual approach.

2. Climate homogeneities and risk unhomogeneities. The Authors states that in transboundary areas mapping faces the problem of harmonizing different regional data. Yet differences of regional data rather being considered a problem should in my opinion regarded to a source of information. They can be a consequence of different theoretical approaches, data collection methods, purposes of the procedure, historical risk perception. Moreover unhomogenities in risk mapping might arise also from different geological contexts (e.g. different slopes might differently react to precipitations and be differently prone to landslides, or the extradoxal area of river bends is generally more hazardous with respect to the intradoxal one) or by different population distribution (maps reported in the Supplementary material from page 20 to page 28 shade some insights on this aspect and require to be discussed with more detail in a geographical perspective). I therefore recommend (1) to deeper investigate the origin of the unhomogeneities they found in the regional risk mapping, and (2) to clearer state whether their approach homogeneizes these differences by working on the original data, or it goes beyond these differences by working on different, transboundary, datasets.

The reviewer raises an important concern, which, in our opinion consist of two dimensions; namely comparability and scale. The reviewer rightly mentions reasons for differences between similar data sets depending on the (sub)national context and we agree that valuable insights can be gained from studying this. However, cross-boundary comparability of risk and its subcomponents is limited if the underlying data sets are incomparable. It is in fact the purpose of our approach to achieve comparability between national entities despite the aforementioned challenges of the trinational situation, with its impact on the availability, homogeneity and resolution of comparable data sets. We explicitly explain in lines 175ff that we build on the paper by Scholze et al. (2020), where a deeper discussion on the issues mentioned above is provided. We had split the two articles because we felt it would exceed the page numbers of one article.

We will deepen the theoretical reasoning behind the inclusion of data sets in order to avoid the impression of arbitrariness and to highlight more clearly from which sources the data originated. We will move Table 2 from the supplement to the main body of the text in order to support this. We will provide a better explanation of how comparability is established and how the index is calculated.

We agree that different spatial patterns on a lower scale can add interesting insights. This is, however, a question of data availability. As we show in this paper, identifying suitable and comparable data sets is inevitably determined by the lowest common denominator within the trinational context. We therefore decided not to analyze data below the community scale and neglected some interesting data sets, that failed our indicator quality audit (See figure 2). The quality audit gives a measure of how suitable each indicator is in each administrative unit as well as the overall study area (lines 150-163). It addresses the inhomogeneities between the different administrative entities. The causes for the inhomogeneities are manifold and depend on the respective data sets. For example, different thresholds are being used to classify small and medium-sized enterprises (200 or 250 employees) or data such as unemployment rates are provided on different scales (community or NUTS-3 level). We realize that these imperfect data sets result in uncertainties, so we point this out throughout the paper, the figures and tables. As long as inhomogeinities in community data of different administrative origins exist, it remains a challenge to conduct transboundary assessments. This is, however, less of a problem on the NUTS-levels, which explicitly target this issue in Europe.

3. The crucial problem of arbitrariness in risk mapping. Risk mapping is a quantitative description of the potential damages or losses consquent to an adverse event. It passes through quantitative assessment and often also through classification, normalization and weighting of much different elements. In many cases these elements share the only property that they can be in some way quantified – as far as, e.g., ecosystem services are mostly not quantifyable. These procedures often introduce margins of arbitrariness which has effects on te final maps. On the other hand it is often difficult to have an estimate of the goodness of the introduced arbitrary choice. This can be done in case collected data sets of previous similar events are available. In case such data are not available the comparison of different procedure can guide the assessment of the validity of the procedure. In the lack of previous data or in the absence of the comparison with different mapping procedures, it is difficult to assess the goodness of the proposed mapping technique. The area investigated by the Authors has been urbanized for long time and it is reported that previous maps are available. At least a comparison with previous maps is recommended also to support this point.

The reviewer raises an important issue of the limitations of risk mapping approaches and composite indicators in general. We are aware that our approach aims at quantifying intangible aspects of risk, which is why we rely on indicators. We see it as a challenge to combine different climatic risks since they all affect the region and the people not independently/sometimes all at the time. We see it as an advantage to be able to reflect the multitude of climatic changes and the associated complexity. We focus on the overall socio-economic dimension of risk in the TMO, so naturally, the scope of the analysis is broader than would be for a single sector or a single risk. The following figure illustrates this complexity.

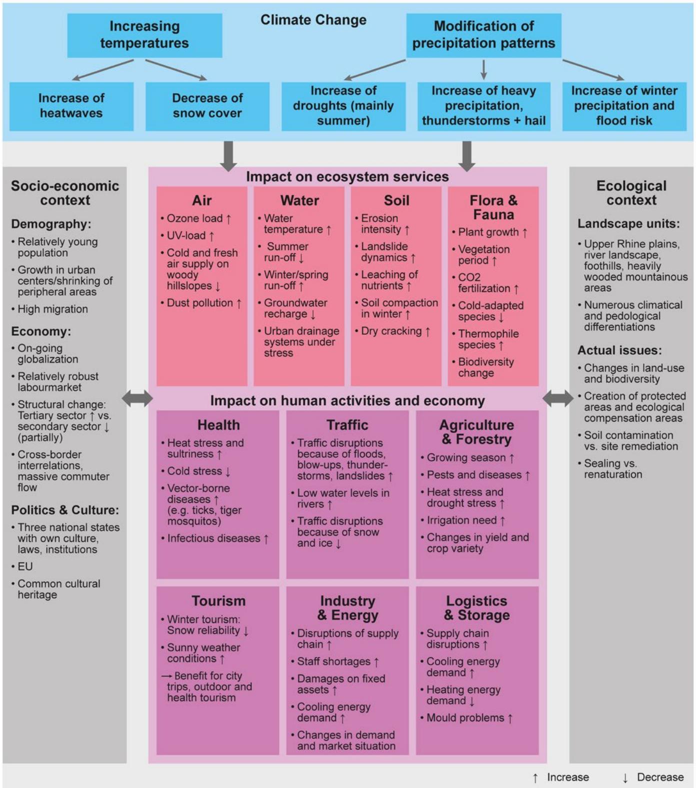


Figure 1: Schematic overview of climate change related Impacts in the study area

Unfortunately, no previous risk assessment of a similar scope exists for the study area. We therefore rely on an in-depth literature review (Scholze et al. 2020), in which we justify the selection and operationalization of indicators. Where it is possible (e.g. RCM ensemble), we quantify uncertainties. We critically reflect on sources of uncertainty, some being inherent to risk mapping/composite indicators, others as a result of the challenging data situation in the trinational context or both. Hence, we conclude that further research is needed to improve the quality of such multi-facetted risk assessments in a transboundary context. In this sense, we see our study as a starting point for the discussion on climate change related risks in the study area. We are aware of various internal and external validation (see for example Birkmann et al. 2022[1]) approaches and discussed the approach with stakeholders and experts. In spite of the absence of risk assessments of similar scope, we will adopt the recommendation of the reviewer and strengthen the discussion on other risk assessments as a form of validating our own results.

We thank for all the efforts and helpful remarks.

Kind regards,
NR, NS & RG

[1] https://www.sciencedirect.com/science/article/pii/S0048969721051408

Other minor comments:

l.6 “risk can be approximated” not clear what does it mean;

Thank you for this comment. By “approximating” we point out the difficulties of capturing the intangible characteristics of risk through an index. This aims at disclosing the limitations of the approach. We would prefer to keep it in the abstract as it is, and will explain further in the main text.

l.35 and followings: here it is important to detail some expectations (and uncertainties) of the considered climate change scenarios for the area;

Thank you for pointing this out. We will revise this section accordingly (see also figure 1 (above)). We will ensure the revision compliments the analysis of the climatic scenarios in the results section.

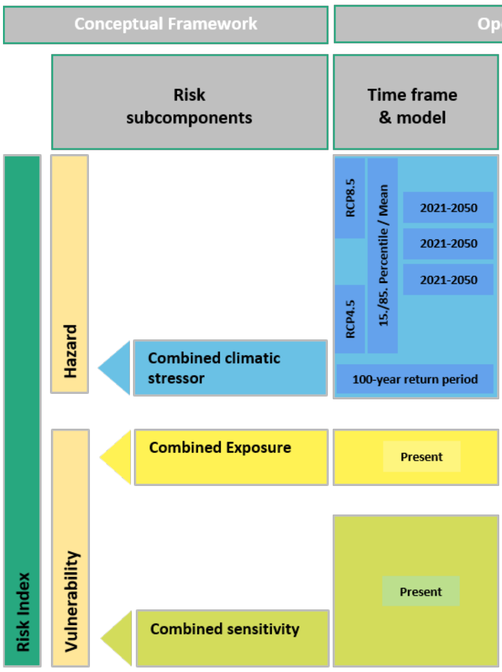
l.55 Introduce here a definition of risk and of its components;

Thank you for pointing this out. In line with our comments above, will include the revised definition of risk here in order to clarify our risk understanding and to improve readability.

l.67 “vulnerability of the funtion of exposure…” it is not clear, all these statement should be better detailed in a framework of a reference risk definition which should be introduced before;

l.145 “vulnerability = risk”: see above

Thank you for this comment, which we also addressed in the above sections. After revising the risk framework following the suggestions by reviewers 1 and 2, this section can be shortened substantially. We felt the need to deepen the theoretical discussion in order to explain why we followed the practice-oriented approach of the UBA (2017). We will also point out more clearly, that figure 2 conceptualizes the risk formula mentioned above.



Indicator Quality Audit

Indicator	France	Baden (D)	Palatinate (D)	Switzerland	mean suitability
Summer days	100%	100%	100%	100%	**100%**
Tropical nights	100%	100%	100%	100%	**100%**
Frost days	100%	100%	100%	100%	**100%**
Heavy precipitation	100%	100%	100%	100%	**100%**
Winter precipitation	100%	100%	100%	100%	**100%**
Summer precipitation	100%	100%	100%	100%	**100%**
Consecutive dry day periods	100%	100%	100%	100%	**100%**
HQ100 areas	90%	95%	95%	81%	**90%**
Built-up areas	100%	100%	100%	100%	**100%**
Critical infrastructure	81%	81%	81%	81%	**81%**
Population Density	100%	100%	100%	100%	**100%**
Population 15-65 years	100%	100%	100%	100%	**100%**
Business tax	81%	95%	95%	33%	**76%**
Unemployment rate	90%	86%	86%	86%	**87%**
SME employment	90%	81%	90%	90%	**88%**

Figure 2: Revision of conceptual approach

l.199 RCP4.5 and RCP8.5: introduce a small description of the scenarios

Thank you for this comment. We will include a description of the scenarios

l.205 At which time scenario are these data referred?

Thank you for this comment. In figure 2, we highlight that we utilize two sorts of time frames. The Hazard/Combined climatic Stressors refer to future data e.g. the RCP scenarios and Flood data. The Vulnerability data refers to present day (collected) data. In line with the reply to reviewer 1, we will move Table 2 from the supplement to the main body of the text and also include the respective time frames.

l.215 and around: how was the reliablility fo the scenarios assessed? I recommend firstly to make a comparison between measured data and the simulation of present time, to identify the biases and the proper downscaling (of simulations) / upscaling (of measurements) procedures and then apply the same biasing and, if necessary, downscaling, to future scenarios;

Thank you for this comment. We are not sure if we understand you correctly. The RCP scenarios project different climatic futures depending on the atmospheric greenhouse gas concentration, which can be translated into radiative forcing levels. The IPPC is clear that the scenarios are not associated with probabilities but serve to highlight the ghg-dependant corridor of plausible possibilities.

The projections we use in this study were provided by the German Weather Service (DWD). The global circulation model (GCM) members were assessed in the Coupled Model Intercomparison Project (CMIP5), the regional climate models (RCM) were assessed by the EURO-Cordex initiative. The DWD performed a bias correction. We additionally specify the ensemble percentiles in order to account for model uncertainties.

We hope we could clarify that the models have and continue to be monitored. However, an extensive evaluation of the models' performance is beyond the intended scope of this paper and we refer to the DWD.

l.222 rr > 20 mm: what does rr stand for?

rr stands for rainfall runoff. We will write it out to be more precise here.

ll.254---255 see point 2.

Thank you for this comment. We will refer more precisely to the results in the supplement. Here we have provided detailed model results for the individual climatic stressors.

ll.338---364 it seems being more a state of the art than a discussion. Many references are presented in an intridictory way: in this section they should be more detailed commented point by point in comparison with the presented approach.

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Thank you for this comment. We will revise this section in order to discuss more clearly the strengths and weaknesses of our results in relation to the literature.