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Interactive comment on "Uncertainty and sensitivity analyses in seismic risk assessments on the example of Cologne, Germany" *by* S. Tyagunov et al.

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1) General comments

The paper "Uncertainty and Sensitivity Analyses in Seismic Risk Assessments on the Example of Cologne, Germany" by Tyagunov et al., focuses at methodological aspects of considering aleatory and epistemic uncertainties in seismic risk assessments. The topic is certainly timely and the methods used are appropriate for the problem at hand. Nevertheless, I have some minor criticisms that the authors should consider to improve their discussion about handling epistemic uncertainties (see specific comments). My main concern is in the implication of assuming both -the original input data/models and models/data drawn from other sources- as equally uncertain for the cologne case.

Beyond the sensitivity analysis and considering the large uncertainty bounds obtained in the results, I think that the paper would benefit of assessing also the effects of the choice of the weights assigned to the branches.

Therefore, comparing the results obtained assuming equal weights with results assuming more informative, knowledge-based weights (i.e. better reflecting the knowledge that the authors have about the models/data used), would be an important element for discussing the uncertainty assessment following the implemented logic tree approach.

2) Specific comments

The authors implement a logic tree approach as a tool for analysing epistemic uncertainties associated with the parameters and models used in the hazard and risk analyses. However, regarding the uncertainty analysis, my main concern is about the ground assumption done by the authors, assuming that the original input data and models are as equally uncertain as the data and models drawn from other sources (p. 7297, Lines 17-19). This choice, that implies assigning equal weights to all the branches of the logic tree (e.g. p. 7299, Lines 6-10), is argued as a realistic situation in little-studied areas lacking reliable regional data.

My criticism regarding this point is that this choice may produce misleading uncertainty 'bounds' in the example used in the paper (Cologne), where specifically developed models and data are available. I think that assigning equal weights to all the branches of the logic tree in this example should be better discussed and argued by the authors, since I would suspect that those models and data available for the area could be considered as more 'informative' (for the case under analysis) than other models/data borrowed from other regions. In other cases, high resolution models may bear more detailed information that other with lower resolution. Therefore, if we know or can expect that some models perform better that others, why not providing a higher weight to them? This in fact, may better reflect the knowledge you have of the used data.

I agree with the authors that this choice may hold in little studied areas but, for a case

NHESSD

1, C2977–C2980, 2014

as the discussed example, I would wonder: Is the 'equal-weights' choice a way of rejecting a piece of 'known' information that eventually may contribute reducing epistemic uncertainties? I think this point is important for the discussion because epistemic uncertainties are the key topic of the paper. Some examples of alternative branches where one can wonder for the arguments to assume equal weights are:

a) In the seismic hazard modelling: using data, on one hand, form the Stromeyer and Gurnthal (2009) and Grunthal et al (2010) models for Central Europe, and on the other hand the model of Chandler and Lam (2002) for a region in China (p. 7298, L. 15-25);

b) In the modelling of the existing building stock: using two models with quite different levels of detail and resolution. On one hand, VM1 represents the whole city as one cell and is pointed out as an approach for studies over regional or national scales and for rough estimations of risk (p. 7300, L. 20-25); on the other hand, VM2 follows the approach of Wieland et al. (2012), where the territory is divided into a grid of cells that are relatively homogeneous in terms of their predominant building types (p. 7301, L. 10-15). Therefore, a high resolution against a low resolution model.

c) In the loss modelling: On one hand, LM1 is based on the damage classification of EMS98 and considered consistent with the European building typology; on the other hand, LM2 more appropriate for the building typology in the USA (p. 7305, L. 1-10). In this case, LM1 is likely more adequate for the problem, whereas giving the same weight to LM2 in probably just introducing unnecessary noise.

d) Somehow, also in the uncertainties of some parameters: in particular, the Mmax. In this case, a +-0.5 value respect to the original max. magnitude estimated by Grunthal et al 2010 is assumed (and therefore, a distance of 1.0M units between them). Note that the resulting Mmax values are not coming from different 'competing' models arriving to different values, but arbitrary bounds assigned as a plus/minus range around a single value. Therefore, I wonder about the implications of using equal weights in this case, since these two bounds may be orders of magnitude different.

NHESSD

1, C2977–C2980, 2014

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