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Interactive comment on "Impact of droughts on the C-cycle in European vegetation: a probabilistic risk analysis using six vegetation models" *by* M. Van Oijen et al.

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We thank the reviewer for their efforts and the very positive evaluation of our work. We are happy to see that our risk analysis method including the evaluation of its application here using NDVI-data and sensitivity analysis are appreciated as novel and useful.

We agree that some aspects of our risk analysis method, although indeed new for ecosystem modelling, are more common in other fields such as engineering, but there remain differences. We decompose risk as the product of hazard probability and vulnerability, suitably defined. Risk decomposition in engineering tends to focus on discrete events such as the failure of any system component. Fault tree analysis is a form of risk decomposition that quantifies the failure probability of the different components in a human-made system and this is modelled using discrete probability distributions. This approach is not suitable for our purposes in ecology where carbon fluxes are not binary: fluxes do not 'fail' when there is a drought but can change to any given degree. Therefore our framework for risk analysis uses continuous probability distributions and we define vulnerability (in Eq. 2) as a function of expectation values and not discrete probabilities. We have not been able to find Eq. 2 (nor the more full exposition of our approach in Env. Res. Letters, http://iopscience.iop.org/1748-9326/8/1/015032) in the engineering literature, although of course there is some conceptual similarity between the fields.

The reviewer states that we should have mentioned specific limitations of the six models that we used, and we agree with this criticism. In the earlier paper (Env. Res. letters, cited above), we had noted that "The quality of the risk analysis will depend on the extent to which the model is able to calculate how much these various adaptation processes decrease ecosystem vulnerability", but we did not mention this caveat here. Indeed, the six models simulate vegetation change (migration, acclimation, fire disturbance in some models, adaptation) only to a very limited degree and we shall add this comment to section 4.1 where we discuss strengths and weaknesses of our risk analysis. Three of the six models that we used are dynamic vegetation models which allow for replacement of functional vegetation types by others when the environment changes, but migration is not explicitly simulated. Physiological adaptation is simulated to some extent by the models (e.g. stomatal closure with increased atmospheric [CO2] and drought, increased allocation to roots when soil resources become limiting, temperature optimum of photosynthesis). Of course, prediction requires modelling and our suite of models arguably reflects the state of the art as used for IPCC CMIP5 projections, but we acknowledge the fact that every modelling study is limited by the quality of the models used. Having said that, we believe that our three main conclusions as listed in section 5 are robust because additional adaptation processes - if indeed important - would be likely to reduce vulnerability, whereas our risk decomposition already identi-

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fied increased hazard probability as the greater threat. This also means that changes in vulnerability would have to be extreme and somehow much more favourable in the Mediterranean area than elsewhere to overturn our prediction that the southern part of Europe is at the greatest risk. Regarding the drought hazard itself, a consistent drying trend in Southern Europe, with increased drought extremes, is also predicted in the recent work of Jacob et al. (Reg. Environ Change (2014) 14: 563-578). However, future improvements to modelling capability may conceivably produce lower estimates of the magnitude of the risk than what we found with currently available models (>0.25 g C m-2 d-1 for NEP in the south). We hope that this first risk estimate will stimulate further development of models and application of our risk analysis method.

The reviewer suggests including further analysis of the factors affecting NEP (beyond what we presented on p. 8344 and 8347-8348), and we have made additional calculations. The suggestion to plot vulnerability and risk of NEP at all grid cells against those for NPP and RH would lead to too many graphs (plotting these pairs of variables for six models examined in two time periods would lead to 48 graphs), so we must restrict ourselves to correlation analysis. We found that the vulnerability of NEP is indeed far more closely correlated to NPP vulnerability (correlation coefficients ranging from 0.70 to 0.96 across all models and both time periods) than to RH vulnerability (-0.65 to 0.11). The results for risk are similar. These results clarify our statement (p. 8348) that drought response of NEP tended to follow that of NPP more closely than that of RH for most models, and will be added to section 4.2.The reviewer asks us to clarify the meaning of a SPEI-value less than 1, as referred to in the Introduction (p. 8330, l. 6) and in section 2.5.2. (p. 8341, l. 21). That explanation is given in the Materials and Methods, section 2.2 on p. 8332, and we shall include cross-links to that section in the paper.

In the Introduction (p. 8329), we specify the five variables that we examine in this paper and the reviewer suggests adding a sentence explaining why these variables were selected. We did argue the selection in the paper, but very late in the paper

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(Section 4.1, p. 8347), and that text should indeed be moved to the Introduction.

We shall try to reduce the length of the Discussion (the reviewer suggests by perhaps 10%) but will aim to keep a degree of stand-alone readability in this section. Our analysis is fairly complex and we expect that some readers may benefit from the redundancy.

We conclude by reiterating our thanks to the constructive comments of the reviewer.

Interactive comment on Biogeosciences Discuss., 11, 8325, 2014.