

Edinburgh, 25 September 2014

Re: Biogeosciences manuscript bg-2014-157: Author's response

Dear Editor,

We hereby submit to you a revised version of our manuscript 'Impact of droughts on the C-cycle in European vegetation: A probabilistic risk analysis using six vegetation models', by M. van Oijen et al.

On the following pages of this letter, we specify the changes that we made to the manuscript after having received the comments by two reviewers. We conclude the letter with two Appendices with our direct responses to the comments from Reviewer 1 and Reviewer 2, respectively. These responses were published in the online discussion on 28 August 2014, and announced the changes that we now have made in the manuscript.

We believe that the revised version has benefited from the comments made by the reviewers and wish to thank them for their contributions. We hope that the paper in its present form will be acceptable to you and we look forward to seeing the work in print on the pages of Biogeosciences.

Yours sincerely,

Marcel van Oijen (on behalf of all authors)

## Changes made to manuscript bg-2014-157 following the interactive discussion

Here we list the changes that were made to the manuscript. The *major changes* were all prompted by the comments from the reviewers, and were mentioned in the online discussion (see the two Appendices on following pages). The listed *minor changes* are additional smaller corrections.

### *Major changes*

1. We added a paragraph to the Introduction, explaining how our method for ecosystem risk analysis differs from methods common in other disciplines such as engineering.
2. Also in the Introduction, we explain the reasons behind our selection of the five ecosystem variables for which risk analysis was carried out (NEP, NPP, RH, SWC, ET). This explanation appeared too late in the original text (in 4.1).
3. Also in the Introduction, we added a sentence explaining the meaning of a value of -1 of the drought index (SPEI), referring to 2.2 for further details. The SPEI-value of -1 is the chosen threshold for drought in this paper, and thus needed to be explained early in the text.
4. We removed some repetition of text, allowing us to shorten the description of the six vegetation models in the Introduction, and also shortening the text on the SPEI and the six models in 4.1.
5. We provide more information on our use of the NDVI, rather than other data sources, in 4.1.
6. We added a paragraph to 4.1 where we discuss how the quality of our risk analysis depends on the quality of the models that were used. This focuses on the extent to which the models are able to represent processes by which vegetation can adapt to environmental change in general, and drought in particular.
7. In 4.2, we added a short paragraph with results from a new correlation analysis that we carried out during the period of online discussion following a reviewer suggestion. This analysis confirmed our conclusion that changes in NEE are indeed more closely related to changes in NPP rather than changes in RH.

### *Minor changes*

1. One author affiliation was updated.
2. A small number of textual and stylistic errors were corrected.
3. We added more detail to the description of the climate data in 2.1.

4. We updated the list of references, adding four recent works (Bevan et al. 2014; Jacob et al. 2014; Keenan et al. 2013; Rausand 2011) and providing full details of a paper that was still under review at the time of the original submission (Pinzon & Tucker 2014).

## Appendix 1. Response to referee 1 (copied from online Discussion)

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 [CO<sub>2</sub>] 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 identified 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<sup>-2</sup> d<sup>-1</sup> 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 (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.

## Appendix 2. Response to referee 2 (copied from online Discussion)

We thank the reviewer for the comments, for acknowledging the importance of ecological risk analysis and for qualifying our approach to this as interesting. The reviewer does suggest that our study may be hampered by insufficient quality of the models used, and by our results on future European carbon fluxes being in disagreement with "general foundations of biology" and by being inconsistent with "Darwin's evolution point of view". We shall discuss the seven points made by the reviewer in turn.

(1) The reviewer suggests that the six different models used in our study may not represent adaptation processes accurately. We agree with this criticism and refer to our response to Referee 1, who made a similar comment.

(2) This is an inquiry about possible differences in model parameterisation for present and future time periods with respect to "plants, soils, phenology etc.". We should perhaps first clarify that in process-based models like ours there are no fixed values of soil composition, plant physiological processes or rates of phenological development. All these are processes that depend on the internal state of the system at any given time and how it interacts with the contemporary environment. The amount of soil organic matter changes over time and is not a fixed parameter. Rates of photosynthesis adapt to elevated atmospheric  $[CO_2]$  (via stomatal closure), to temperature and atmospheric dryness, and soil moisture stress. Respiration responds to temperature but remains constrained by substrate availability, so again these processes respond to environmental change. There are no fixed days of the year associated with phenological stages in the models, instead temperature affects development rate. The parameters that underlie these dynamic processes, and constrain how they respond to the environment, are kept constant for the two time periods.

(3) Our simulations cover the whole of Europe as represented by more than 18000 grid cells (Table 1). The strength of our model evaluation using NDVI-data is that these data have a similar spatial extent and resolution. That allowed us to verify the spatial patterns simulated by the models, summarized by our main conclusions listed in section 5. A highly sparse set of eddy covariance towers (which also have footprints considerably smaller than the areas of grid cells) would not have afforded the same capability.

(4) The SPEI drought index indeed quantifies the environmental conditions, not the amount of drought stress experienced by the vegetation. Quantifying the latter is what the models were used for, and each model was run only for those grid cells for which it was able to represent the local vegetation, hence the different numbers of grid cells simulated by the different models (Table 1).

(5) It is always possible, as the reviewer does, to state about any given study that "uncertainties may be much larger than the result" that was found. It is difficult for us to argue against unquantified uncertainties. We believe that such reasoning should not preclude us from giving the best possible risk estimates that we are currently capable of.

(6) Including  $P(H)$  explicitly in Table 2 would expand its size by 50% but not add any information.  $P(H)$  is equal to  $R/V$  (the ratio of risk and vulnerability) both of which are specified. We refer to Equation (3).

(7) The reviewer states that the species in southern Europe "should have a better chance to survive in dry conditions" according to "Darwin's evolution point of view". While we applaud the ambitious use of evolutionary theory by the reviewer in arguing against our predictions of risks to carbon fluxes a century hence, we feel that this is stretching the applicability of Darwin's theory. We suggest that there may be several misunderstandings here:

- Evolutionary theory applies to longer timescales than 100 years with annual and perennial plant species.
- Even over the past millennia, changes in vegetation composition in the Mediterranean have been dominated by human interference rather than by gradual evolution.
- Our paper is not about survival but about carbon fluxes.
- The severities of drought at the end of the century will increase. Hence they are not those experienced by vegetation today, so even if vegetation were very well adapted to present-day conditions (not at all a given if we consider that some desertification is being observed in southern Europe), that would not imply resilience against future droughts or allow for plant adaptation within the coming decades.
- "Dry conditions" in Southern Europe are not the same as "dry conditions" elsewhere. Of course Mediterranean vegetation can handle summer droughts that would be detrimental to vegetation elsewhere. That is exactly why we use the SPEI drought index. It is a local index, i.e. a SPEI value of zero represents the local average of water availability (precipitation minus potential evapotranspiration) in any grid cell, and drought stress to the degree of  $\text{SPEI} < -1$  represents less water in already dry areas than in wet areas. The use of the SPEI thus ensures that drought hazard is quantified relative to the average conditions experienced by the affected ecosystem in each location.

We like to conclude by thanking both reviewers again for their efforts.