

Interactive
Comment

Interactive comment on “Climate and land use change impacts on global terrestrial ecosystems, fire, and river flows in the HadGEM2-ES Earth System Model using the Representative Concentration Pathways” by R. A. Betts et al.

R. A. Betts et al.

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Thank you for your review and supportive opening comments.

1. In the Introduction, maybe also mention that EO is showing some predicted trends known to be present in modelling exercises (for instance, Northern Latitude greening).

Response: Good idea, thank you. We will do so.

2. Introduction, section starting “Previous studies....” is valid to criticise “off-line” approaches, except in the instance where the removal of model biases to add anomalies

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to a known climatology results in especially large changes. This in some circumstances do may do more to generate correct projections than the risk of “violating conservation of physical quantities”? Similarly p6175, lines 4-12 do look overly defensive of use of a single model, and potentially unfair to those working hard to build and maintain the CMIP5 database – which is an especially valuable asset. Maybe re-word slightly?

Response: We agree, and the intention of our previous sentence was to try to say this, but we will make this clearer.

3. P6177, line 10. Can I just check the name of the land surface model? Isn't the latest version of the Hadley Centre surface model JULES (along with references more recent than 2003).

Response: Technically this is still MOSES not JULES (although scientifically they are virtually equivalent). JULES is the version of the model for which the code resides outside of the Met Office Unified Model system, whereas HadGEM2-ES still uses the older version within the UM system. We will however double check whether any more recent JULES references also relate to scientific updates within MOSES.

4. Please check throughout paper that all references are present in the reference list. e.g. p6177, lines 14 and 15. Missing Gedney et al (2004) and missing Mercado et al (2007). There may be others....

Response: Thank you - we will.

5. Whilst I like the comprehensive title, it is potentially misleading. When I read the title and Abstract, I thought an exciting feature of this paper was going to be Hadley model introducing an explicit representation of fire to MOSES/JULES. However p6177, line 23, “.....there is no representation of the effects of climate on disturbance regimes such as fire....”.

Response: This is a fair point - we will reconsider the exact form of the title.

6. Related to the above, to determine fire risk, the authors state: “we use daily mete-

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orological outputs from the model to calculate the McArthur Forest Fire Danger Index (McArthur 1973, Nobel, 1980). This is fine, except that the impression here is that this is done off-line? Doesn't this affect the stated view of the authors that it is better to perform simulations in a coupled-framework? Otherwise, if the coupling is retained (i.e. a correction is made to the land surface state at regular periods, and also fire adjusts atmospheric CO₂ concentration – or “allowed emissions” given RCP forcing), then this needs to be stated. Apologies if I've mis-understood something at this point. Please confirm whether the fire component predictions do feedback.

Response: The referee is correct, this is inconsistent with our philosophy of "online impacts", but we felt it important to highlight the potential importance of this missing process. It will be some time before an interactive fire model is operational with the Earth System Model, but we felt that our paper would be misleading if we gave the impression that plant physiological responses were the only important process, and ignored disturbance completely. We will see whether further clarification on this point can be made.

7. P6186 – line 4 “mean rainfall is projected to decrease by a few percent across this region” reads slightly vague.

Response: We will try to be more specific.

8. In my view, many of the novel features of this paper are taking impacts on to fire risk and more advanced descriptions of river flow. This paper is a long paper, and that is fine. But if the journal and/or other reviewers felt it needed to be shortened, then please retain an emphasis on Figures 13/14 (Fire) and 15-19 (River discharge).

Response: We feel the length of the paper is justified. If the editors request shortening, your comments will be useful in prioritising.

9. There are a few typos e.g. p6174, line 14 “imapcts”

Response: Thank you - typos will be dealt with.

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10. Diagrams – these generally look really nice, but in a couple of places small changes could be made to enhance readability. E.g. Figure 5, please move apart slightly the individual panels. In figures 9, 10, possibly large thickness for the curves (similarly figures 17, 18)? This might just be a printer issue, but Fig 13 global map - please try and keep in same style as the other global plots?

Response: Thank you, we will look into this.

Interactive comment on Biogeosciences Discuss., 10, 6171, 2013.

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