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Discussions

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.

Anonymous Referee #2

Received and published: 25 June 2013

Many thanks for inviting me to review "Betts et al: 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."

First, may I apologise for taking two months to return this review.

This paper is important as it draws together the following key strands:

1. Newer generation GCM (i.e. HadGEM2-ES).

2. Use of RCPs as forcing.

3. Impacts implications, including river flows.

4. One of the first attempts to assess fire risk in a changing climate (although please see notes below).

5. On-going assessment of the role of terrestrial ecosystems in the global carbon cycle.

For all of these reasons, this paper will make a very valuable contribution to the climate change literature. I recommend "accept, with minor revisions". Below are some suggestions, which the authors might like to consider for a new version of the manuscript:

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).

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 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?

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).

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....

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....".

Related to the above, to determine fire risk, the authors state: "we use daily meteorological outputs from the model to calculate the McArthur Forest Fire Danger Index (McAthur 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 CO_2 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.

Similarly, (but opposite concern) where the feedbacks are clearly made by the authors – but the writing of the paper could mislead- is at line 25 of page 6182. Although the use of adjustments to the grass PFT to represent land-use/crops is not as sophisticated as might one day be modelled, it is still a valid technique, and associated changes to the land surface will feedback in the coupled HadGEM2-ES framework. So is this the best wording: "The projected land use change scenarios accompanying the different RCPs were not directly prescribed into the model". When, via effective parameter of enhanced turnover / forcing as grasses, land sue is included, then these effects (and feedbacks) are directly present in the coupled transient simulations?

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

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).

There are a few typos e.g. p6174, line 14 "imapcts"

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?