Draft manuscript: 'Representation of disturbance in the Joint UK Land Environment Simulator Vn4.8 (JULES)" submitted to GMDD

Response to Reviewer 2

Anonymous Referee #2

We thank the reviewer for taking the time to share their opinions on our work. However, we wholly disagree with reviewer 2 regarding the novelty and quality of our work presented here. It is factually incorrect to say that what we present was common place in the 1990s in the context of global land surface modelling.

A chronology of model complexity, as used in major international studies and IPCC reports clearly demonstrates this:

- The first ever inter comparison of multiple coupled climate-carbon cycle models, "C4MIP", was published in 2006. Friedlingstein et al (2006; J. Climate). Of the 7 full GCMs included, only 2 had dynamic vegetation enabled and neither included land-use change or fire.
- In 2013 the most recent IPCC Assessment report, AR5, carbon cycle chapter assessed results from 10-15 coupled climate-carbon cycle Earth System Models (Ciais et al., 2013; WG1 Chapter 6). 4 models included dynamic vegetation and land-use change, but only 2 of these included fire (See table 6.11). Both WG1 and WG2 Assessment reports identify fire as a key process which is not routinely included in future projections.
- Land surface models being run offline often have higher degrees of complexity and process inclusion, for example within the "TRENDY" set of models used each year for the Global Carbon Budget update (Le Quere et al., 2017) some now include dynamic vegetation, land-use and some form of fire, but few of these processes are routinely coupled into a GCM yet as outlined above. This is therefore still an active area of research.

JULES is central to major international scientific studies such as the Global Carbon Project and the "TRENDY" project, and forms the land-surface component of HadGEM3 and UKESM1. It is therefore clear that developing JULES to build on the existing vegetation dynamics and land-use representation is both vital and cutting edge. Such a model will contribute significantly to IPCC AR6 and future Global Carbon Budget updates, as well as significantly advancing this community land surface modelling capability available to a wide user base. Documenting this clearly in the literature and making this publicly available as a matter of transparency and as a basis for further developments is an essential element of this, which is what our paper attempts.

Furthermore, we would be interested to hear the editor's view of the scope and purpose of the GMD journal as we feel that the reviewer has a different interpretation of these to our own understanding, which is that the journal is intended to be a home for documentation of important underpinning science in environmental modelling. Here we are documenting updates to JULES, which as stated in our Introduction is the point of our paper, and we believe this is in line with what is expected for the scope of a Model Development journal.

We will revise the initial presentation to make some of this motivation clearer, and below we provide some more detailed responses to the points in the review.

Major comments:

1. The manuscript does not present a clear modelling concept of how the both processes are presented, in terms of qualitative description and/or supporting it by a flow-chart which would also guide the reader through the manuscript.

We would be happy to include a brief overview of the processes that will be presented through the course of the paper inserted at the end of the 'Introduction' section to guide the reader more clearly through the rest of manuscript.

2. The fact that land-use can be regarded as a disturbance is flawed because land-use change is a permanent, very often irreversible change in land-cover. It is reversible when people abandon their fields and that depends on socio-economic conditions that motivate human decision-making. Such a reflection is missing in the introduction.

We disagree that referring to land-use as a disturbance is flawed, as many other published studies refer to land-use as a disturbance, including the one that the reviewer is presumably citing, although the full reference is not provided (Foster et al., 1998). We do refer to agricultural abandonment in the manuscript 'Introduction', however we would be happy to include extra text to make the point that land-use change is often a long-term change, and both agricultural development and abandonment depends on socio-economic conditions and decision making.

3. The literature overview leaves the reader with an unclear message, other than it is very complex. However, the literature, also the cited does allow to conclude which processes are essential to incorporate land-use and fire disturbance in land surface models such as JULES. The problem statement that DGVMs have to properly consider disturbances has been identified already in papers in, e.g. Foster et al. 1998, and has been implemented in many ways in many DGVMs since then. This applies also to land-use.

Please see response at the start.

4. The methods section starts with explaining how the disturbance term is implemented in the major equation on quantifying changes in vegetation. And here starts the problem of the modelling approach: what is presented is a simply cookie-cutter approach to correct PFT coverage by the proportion of fire disturbance and land-use. Such an approach represents the level of science of the 1990ies. Since then many more advanced approaches also simple ones have been published from which this modelling concept can profit.

Please see response at the start.

5. The remainder introduction of equations in the methods section is referring to already published modelling studies and the text does not explain how this was adapted to the current model version or what was updated given the latest progress in science in that field. Therefore, I cannot identify any added scientific value in terms of modelling approaches from which other modelling groups would profit.

As stated throughout the text, the equations describe the addition of new processes of land-use change (αa_i) and fire (β_i,) which are being published here for the first time. We will signpost these additions again ahead of the equations in the text.

6. Variables in equation 1 are insufficiently defined or explained.

We will provide an additional summary table of all terms and units used in equations 1-8 within the 'Model description and developments' section.

7. The feedback of fuel availability on vegetation distribution is not explained (equation 3).

We have already stated that "Fire disturbance, βi, is included as a PFT-dependent burnt area which can vary in space and time" and following equation 3 that "The calculation of burnt area depends on fuel availability, including soil carbon density…". We will add a sentence that explicitly states that "The calculation of burnt area depends on fuel availability as documented in Mangeon et al., (2016) and which now includes the additional feedback of reduction in fuel from fire (equation 3). Also included in fuel availability is soil carbon density, C_s, providing additional mechanisms by which fire and land-use can feedback onto vegetation distribution" which we trust now makes this clear.

8. From this starting point or poor modelling concept and inadequate description, it makes in my view no sense to review the remaining part of the manuscript because it makes it impossible to judge if the results produced are based on solid ground or if they can be reproduced.

The JULES model code is all freely available, as is the suite used for this analysis, as described at the end of the paper to ensure reproducibility. The changes described in equations 1-8 are all included in the main trunk of JULES from version 4.8, as detailed in the 'Code Availability' section.

References

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