Review by Anonymous Referee #1 & author's response

This study implemented Miscanthus into JULES-BE based on site observations from Lincolnshire, UK and a global bioenergy yield dataset. Future simulation was conducted to evaluate the implication of explicit representation of bioenergy crops for climate mitigation. Three simulations were carried to demonstrate the utilization of the new harvesting scheme in JULE-BE.

Miscanthus is one of the most important perennial bioenergy crops that are proposed as biofuel feedstocks for climate mitigation purposes in future scenarios, in particular for the Shared Socio-economic Pathways (SSPs) of the Coupled Model Intercomparison Project Phase 6 (CMIP6). Given the projected biofuel expansion at a global scale, this study constitutes an important step to explicitly represent bioenergy crops in land surface modeling to assess the implications of large-scale bioenergy expansion on water and carbon cycling.

However, there are a number of suggested comments that should be addressed prior to acceptance. For example, model configurations were not well described. A sensitivity analysis is missing. More discussions on the mismatch between simulation and observation is in need.

My main concerns are:
Page 2, on line 10-15: Several ESMs have represented bioenergy crops. Therefore, it will be better to conclude and compare what has been implemented in other ESMs.

> This section discusses BE crops in ESMs under the general banner of "DGVMs". Adjusted the language in this paragraph to make this more specific. This now reads:
>
> "Dynamic global vegetation models (DGVMs), by contrast, are models specifically developed to address questions about large-scale vegetation patterns and productivity, and their links with the climate and Earth system (particularly as part of the Earth system models of which they form the terrestrial components) (Sitch et al., 2008). However, this typically occurs at the expense of representation of specific plant species and detailed site and management information. There are differences between DGVMs (and ESMs) in representation of bioenergy crops and calculation of harvests (Krause et al., 2018): although some feature explicit representation of bioenergy crops and harvesting (e.g. LPJml (Beringer et al., 2011;Boysen et al., 2016); ORCHIDEE-MICT-BIOENERGY (Li et al., 2018b)), others use approximations based on generic plant functional types (PFTs) and calculate harvests as a fixed proportion of productivity (e.g. NorESM (Muri, 2018))."

The methods section lacks description for model configuration. For example, it is not clear how the authors chose the initial conditions?

> Added this explanatory paragraph at the start of Section 2.5:
> "Simulations were carried out to evaluate and illustrate the new functionality in JULES-BE. These simulations were all based on the JULES-ES configuration, a set of options designed for best representation of carbon cycle and climate dynamics over decadal to centennial timescales (ref). All simulations began with initial conditions from a spin-up to equilibrium, then included a transient spin-up period prior to the main run."

Page 5, line 24: Have you done any sensitivity analysis for the parameters?

PFT parameter choice is discussed in the Supplement and in Harper et al. (2018b).

Added this sentence to Section 2.4:
"(See also Harper et al. (2018b) for further information about PFT parameter selection.)"

Page 6, line 25: can you justify the consistency between the forcing used to drive the future simulation in this study and the forcing used to drive IMAGE 3.0 to generate the RCP2.6-SSP2 scenario?

The future simulation is driven by modelled meteorology from HadGEM2-ES, forced by the RCP2.6 concentrations of CO2 and other radiative forcing agents, and is therefore consistent with the IMAGE scenario of RCP2.6.

Added "(for RCP2.6)" to Page 6, line 26, to improve clarity.

Page 7, Line 25 and Figure 2, the simulated LAI is much lower than the observations, can you adjust some more parameters based on observations or add more discussions on the potential reasons?

Section 2.4 discusses PFT parametrisation, with further details provided in the Supplement. Leaf mass per area and leaf nitrogen concentration were taken from literature. Values for variables governing *vcmax* were determined via iteration to fit the GPP observations at this site. PFT allometry was optimised for height to above-ground biomass relationship.

Page 6, line 9: Only meteorological and soil properties data were mentioned. It will be great if the authors can also add some descriptions for the validation dataset here. For example, I can see it has some missing periods for the observed LAIs in Figure 1. And how about the land management practice for Miscanthus at this site? Why the durations for soil properties (2009-2010), meteorology (2006-2013), and model validation (2008-2013 for GPP, 2011-2013 for LAI) are inconsistent?

Added further detail to Section 2.5.1 for increased clarity. These paragraphs now read:

"Adjustment of PFT parameters for Miscanthus was performed using observational data collected from a commercial Miscanthus plantation in Lincolnshire, UK. The site is on a compacted loam soil previously used to grow wheat and oilseed rape. The site had mean annual temperature of 9.8 °C and mean annual precipitation of 621 mm. The net ecosystem exchange of CO2 was measured by eddy covariance methodology. Gross primary productivity (GPP) was calculated using the REddyProc method described by Robertson et al. (2017), after Reichstein et al. (2005). Manual measurements of height and LAI were taken over the growing season (Fig. 2). The site practices, harvesting regime and data collection are described by Robertson et al. (2016) and Robertson et al. (2017).

"JULES requires meteorological and soil ancillary (time-invariant) data to drive the model. Meteorological data were collected at the site on an hourly basis during 2006–2013 (shortwave and longwave radiation, wind speed, precipitation, temperature, air pressure, and specific humidity). Physical soil properties (soil albedo, heat capacity, thermal

conductivity, hydraulic conductivity at saturation, soil moisture at saturation, soil moisture at critical point, soil moisture at wilting point, Brooks-Corey exponent for soil hydraulic calculations, soil matric suction at saturation) were derived from measurements taken at the site between 2009 and 2010."

Table 3: the authors listed parameters values for C4 grass and Miscanthus for comparison purposes. Can you add more results from C4 grass (e.g., GPP, LAI, NEE) to have a better sense of the difference between these two PFTs?

Added a Figure S3 to the Supplement showing GPP, NPP, height and LAI at the Lincolnshire site for Miscanthus and C4 grass.

Figure 2: why the modelled LAI maintained a very high value until the next year and then suddenly became zero (e.g., around Feb 2012)? Especially give GPP did not exhibit similar behaviors in Figure 3.

LAI is suddenly reduced to near zero in February at the point of harvesting, because LAI scales with height (except where deciduous behaviour is present, but this is not the case for the Miscanthus PFT).

This explanation has been added to Section 3.1:

"The seasonal cycle of growth through to harvest in mid-February is illustrated by the seasonal fluctuation of height and LAI (Figs. 2(a)-(b)."

<u>Specific comments:</u>

Page 1, Line 12: it will be better if the authors can specify the model name here rather than at line 20.

The sentence now begins "We describe developments to the land surface model JULES …"

Page 1, line 17: what is the missing model component?

Changed the text on this line from:

"…suggesting missing model components that influence growth and yields"

to:

"…primarily owing to the model's lack of representation of crop age and establishment time."

Page 2, Line 15: change "global scale" to "global scales"

Changed to "…a global scale" as I believe this is more accurate.

Page 2, Line 34: what is “TRIFFID”? You only mentioned it later.

> TRIFFID was defined in this sentence, which has been rearranged for greater clarity, and now reads:
>
> “…but these approaches have not been integrated into TRIFFID, the DGVM within JULES which links plant productivity to soil carbon and the global carbon cycle.”

Page 6, line 5: it was mentioned that “net ecosystem exchange of CO2 was measured at the Lincolnshire site”, so why not show the comparison results for NEE?

> This is possible to do. However, NEE includes soil carbon respiration, which is heavily dependent on soil carbon content. The model was not specifically initialised with observed soil carbon density since soil carbon is a diagnostic variable in the model. Therefore while a comparison between observed and modelled NEE is possible, uncertainty and inconsistencies between modelled and observed soil respiration rates would prevent this analysis from providing clarity about the Miscanthus PFT.

Page 6, line 27: it will be great if the authors can specify RCP and SSP.

> Reference to the CO_2 concentration corresponding to RCP2.6 now specifies RCP2.6-SSP2 derived from IMAGE. There are minor differences in CO_2 concentration between different SSPs and models within an RCP (in the SSP database https://tntcat.iiasa.ac.at/SspDb).

Page 6, line 10: what are the main soil properties you are concerned with? Do they have significant changes during the simulation period?

> Soil properties are prescribed at the start of the model run and do not vary in time. They are used in the calculations of soil hydrology and thermal conductance.
>
> Added the list of soil ancillaries in a parenthesis in Section 2.5.1.

Page 9, Line 27: it will be great if you have individual titles for the Discussion part to summarize the main findings and limitations of this study.

> Separated Discussion into “Main findings and limitations” and “Further work”.

Page 11, Line 9: several other studies have implemented bioenergy crops into Earth system models. It should be the first step to get such processes implemented in JULES rather than Earth system models.

> I stand by the wording used: “This is the first step to getting **such processes** represented **mechanistically** within Earth system models.”
> The emphasis here is on having the relevant physical properties (such as the crop’s height at various times of the year, carbon harvested from the system at appropriate intervals) represented so we can explore the effects on the carbon cycle and climate system.

Page 11, Conclusion: can you discuss more implications of this study?

> Added the following paragraph at the end of Section 5:
> "Implications of this model functionality include the ability to study bioenergy cropping and harvests within a land surface model. Ultimately, this should facilitate climate change mitigation and climate modelling research to evaluate future low-carbon energy systems featuring bioenergy crops for their impacts on hydrology, climate and carbon storage."

Table 2: do you have any ranges for these parameters?

> [I assume the reviewer is referring to Table 3, since Table 2 shows TRIFFID parameters and lists allowed values.]
> PFT parameter choice is discussed in the Supplement and in Harper et al. (2018b).
>
> Added this sentence to Section 2.4:
> "(See also Harper et al. (2018b) for further information about PFT parameter selection.)"

Figure 4: rather than having two subplots show the observation and simulation results, could you add two more figures showing their spatial difference? Or report their spatial correlations?

> The difference between modelled and observed yields has been added to Figure 4.