Review by Anonymous Referee #2 & author's response

General Comments:
The authors outline an enhancing modification of the JULES land surface model, termed JULES-BE, where BE stands for bioenergy. They describe a change to the dynamics of how cropland expands based on the assumption that new cropland will be planted, rather than being filled by the natural expansion of existing cropped area. They show that this change makes the area in bioenergy crops more faithfully conform to that prescribed by the driving IAM scenario in a 21st century simulation. They also present a PFT parameterization for the popular bioenergy crop Miscanthus. This PFT reproduces growth and structural characteristics of Miscanthus for a site in the United Kingdom but doesn't capture the the full variability of yields observed globally. The PFT also tends to predict unrealistically high yields for hot regions. They also added the ability to simulate coppice, rotation forestry, and litter harvest for bioenergy using existing woody species PFTs. They conclude the paper with demonstration forest bioenergy simulations and initial comparisons to European observations.

The authors convinced me that JULES-BE model represents a useful advancement that will help address important questions in the field. The paper is well written and outlines the technical aspects of the model clearly. I also appreciate the fact that the limitations of model and possible ways to address them are clearly identified. However, there are a few issues in the text that could be clarified or improved, which I detail below.

Specific Comments:
Section 2.2.1 (page 4, line 3):
The rationale of the 30% litter harvest assumption should be briefly described. While this is an existing model assumption the authors chose not to changed it and must therefore feel it is supported. The cited reference does not provide the reasoning for this assumption.

> Added the following explanation to Section 2.2.1:
> "Setting the harvest rate to 30 % of litter production approximates the estimate of 8.2 Pg C year^{-1} of human-appropriated net primary production from crop harvests globally in 2000 (Haberl et al., 2007). Future development of JULES-BE will allow the harvest rate to be user-prescribed for each PFT."

Section 2.5.4 (page 7, line 14):
Explain the rationale for cutting to 1 m in height.

> 1 metre height was used as an illustrative example. In short-rotation coppicing, thin stems are cut near the base from a thicker trunk, rather than to a specific height. 1 metre height equates to an above-ground biomass of 117 g C m^{-2} for *P. nigra* and 153 g C m^{-2} for *P. x euramericana*. Shorter cutting height would result in longer re-establishment times.

> The relevant sentence in section 2.5.4 has been updated and now reads:
> "Harvesting occurs on a 3-year rotation on day 270 of the year, when trees are cut to 1 metre height, allowing sufficient remaining biomass for rapid regrowth the following year."

Section 3.1 (page 7-8):

Lines 24-28: It also seems notable that the model shows onset of growth much earlier than the observations.

Agreed – this is worth mentioning.

Added this sentence to Section 3.1:
"The modelled crop also increased in height and LAI earlier in the season compared to observations."

Lines 29-30: The model underestimated the height somewhat for the simulation period (figure 2B) and slightly underestimates the observed aboveground biomass for the UK (Linchonshire) site for the modeled heights (below _2.6m, figure 2D). Given this it would seem that aboveground biomass should be low compared to observations. How then does the modeled yield exceed that at the Linconshire site by over 60%?

Added the following text to the end of Section 3.1:

"The model underestimated height during the growing season but overestimated the yields. This suggests that ratio of height to aboveground biomass was lower at this site than the sites used for calibration in Figs. 2(d) and S1. However, height at harvest time was not recorded; peak height occurred around August to September while harvest was in February or early April. It is usual for Miscanthus to lose biomass over autumn and winter; the preference for harvesting in mid/late winter is not for largest yields but for improved fuel quality and reduced nitrogen loss from the system."

Section 3.4 (page 9):
This section is underdeveloped. While the authors do make it clear that the simulations are mainly proof of principle they will be of considerable interest to many readers as they demonstrate the culmination of the model changes presented. In particular, the residue forestry panels in figure 9 are not even mentioned. These results suggest that litter harvest can provide roughly the same biomass yield as coppice while having very little impact on forest growth (comparing to the first 40 years of the rotation panels). This is a very provocative initial result and should be contextualized in the text as is done for the coppice and rotation simulations.

We will find an appropriate case in the literature to inform a new simulation of residue forestry to strengthen this proof of principle.

Section 4 (page 10):
Sentence line 22-23:
The interpretation of this sentence depends on the definition of 'crop'. Throughout the paper the term crop is used generically with section 2.4 explicitly stating "JULES-BE can represent any type of plant as a bioenergy crop" and in a few places is explicitly qualified, e.g. 'crop grasses'. Please clarify the meaning here. If the statement pertains only to annual crops like grasses I accept the conclusion. However, if trees are included in the definition of crops I would expect that the day of harvest has some potential to impact yield of short rotation coppice but will have very limited impact on predicted yield for longer forest rotations.

I agree that for forestry, altering the harvest day-of-year would have little impact on yield, but would affect other ecosystem properties. This sentence has been updated and now reads:

"Allowing harvest day-of-year to vary regionally would improve global-scale assessment of any bioenergy crop, as harvest timing is dependent on local climatology and affects local land-surface properties, such as roughness length, albedo, and transpiration rate, which in turn affect the climate."

Last paragraph starting line 26:
I am not convinced by the authors' contention that the TRIFFID completion scheme can be made to inform the choice of bioenergy crops appropriate to a given location. The authors present potential changes to the competition scheme that, if I am reading it correctly, would allow PFTs placed in the same land class to compete on the basis of aboveground biomass and / or post season yield calculations. Even if these changes were made it is not clear how this would add greater insight than performing independent simulations with potential PFTS and comparing yields directly. More fundamentally yields do not seem to be the appropriate metric for comparing bioenergy crops in the context of an ESM. If yields were the main concern species specific crop models would probably be sufficient for this purpose. While yield is certainly important for the economics of species selection, it is not sufficient for climate relevance. The value of an ESM is that it allows the impact of bioenergy crops to be examined holistically. Assessing alternatives requires considering the status of carbon stocks and biophysical feedbacks alongside the offset of emissions from crop yields. I do think JULES-BE will be useful in performing such an analysis, just not in the manner described here.

Thanks for this interesting and thoughtful critique. I do agree that the DGVM competition mechanism may never be an appropriate instrument for evaluating suitability or preferability of different BE crops in the same grid cell or bioregion.

Added the following sentence to the end of Section 4:
"Ultimately however, a yield-based competition scheme would still ignore the biophysical, economic and environmental factors that influence choice of crop type. As such, JULES-BE may always be more useful for informing these land-use decisions based on its output, rather than integrating these decisions into the existing model."

Figures 2 and S1:
Consider providing goodness of fit statistics for figure 2C, 2D, and for at least the selected model (case 1) in figure S1.

The root mean square errors of the modelled relationships to observations have been added for Case 1 of Fig S1 (all panels). These are the same relationships as in Figs. 2(c)-(d), so they have not been added to Figure 2.

<u>Technical Corrections:</u>
Section 2.2.2. (page 4, line 4): For consistency with the remainder of the formula litC, on both sides of the equation, should have time subscripts.

Added time subscripts to lit_c and *harvest* in Eqs. (1)-(4).