Response to Reviewer 1

Response to Reviewer 1		
No.	Comment	Response
R1C1	The paper discusses some enhancements to the H08 global hydrologic model to simulating bioenergy yield over a history. The authors compare the results to previous assessments and some observed yield values around the globe. The paper is a good contribution, and I recommend its publication. However, the paper has several sections that require some additional clarity/details. Below I provide a detailed summary of some of these issues.	Deer Reviewer, thank you very much for taking time to carefully read our manuscript. We are pleased to see your recommendation for publication. Your valuable comments enabled us to clarify a number of points that we previously unaware of, and we hope that we have increased the quality of the manuscript substantially. We have revised the paper by trying to incorporate all relevant comments and remarks. We have also tried to respond to all the comments meticulously as you may see below. Please find our responses to each comment below.
R1C2	It is not clear how this work builds on previous work by the authors (Yamagata et al. 2018 and Wu et al. 2019) or by the work of Trybula et al 2015.	We apologize for the unclear description. Here, let us further explain how our work builds on that of Yamagata et al. (2018), Wu et al. (2019), and Trybula et al. (2015). In the whole, the first bioenergy crop implementation in H08 was conducted by Yamagata et al. (2018). Using outputs from the same model employed by Yamagata et al. (2018), Wu et al. (2019) predicted future global bioenergy potential. Our study is a substantial upgrade to the portion of Yamagata et al. (2018) purely dedicated to the improvement of bioenergy crop modeling. In this upgrade, we referred the parameters reported by Trybula et al. (2015), which provided crop parameters for the leaf area development curve. To be specifically, in the work of Yamagata et al. (2018), the bioenergy crop modeling was realized in two steps. First, crop parameters (see the old values in Table 2) for *Miscanthus* (refer to *Miscanthus giganteus* in this study) and switchgrass (refer to *Panicum virgatum* in this study) were adopted based on the settings of the

		SWAT model 2012 version (Arnold et al., 2013). However, the default parameters could not well reflect the characteristics for *Miscanthus* and switchgrass and could lead to serious bias based on the result in Trybula et al. (2015). Second, because both *Miscanthus* and switchgrass are perennial, the potential heat unit was set as unlimited (see the old values in Table 2). However, this potential heat unit is far from the observations reported by Trybula et al. (2015) (see the new values in Table 2). Here, further enhancements were therefore conducted as follows. First, we changed the leaf area development curve by adopting the potential heat unit (Hun) and leaf area related parameters (dpl1 and dpl2) proposed by Trybula et al. (2015). The potential heat unit can determine both the total cropping days and the leaf development. Here, we set it at 1,830 and 1,400 degrees for Miscanthus and switchgrass, respectively, as recommended by Trybula et al. (2015) based on their field observations. This modification changed the original heat unit index (Ihun) and the development of the leaf area index curve. Second, we modified the algorithm for water stress that was used to regulate the radiation use efficiency. We took the ratio of actual evapotranspiration to potential evapotranspiration as the water stress factor for any point in the simulation, similar to the description of the soil moisture deficit used in other studies (Anderson et al., 2007; Yao et al., 2010). Third and the most important, we conducted a systematic parameter calibration and evaluation for both *Miscanthus* and switchgrass with the best available data.
R1C3	Better documentation of the methodology section to allow for reproducibility including the equations, and the explanation of the	Thank you. We have added the equations related to yield estimation to Section 2.1; added an explanation of the parameters in

	various parameters. A schematic would be also help.	Table 1, and provided the original values of the parameters in Table 2. In addition, we have described the original implementation of bioenergy crops in Section 2.2; rephrased the calibration process in Section 2.2; revised Fig.1 to include both the submodules of H08 and the specific biophysical processes of crop module; and improved Fig. 2 by adding the climate zone information originally presented in Fig. S6 to better illustrate the site locations.
R1C4	Sections 2.1 and 2.2 leave the reader wondering about the specifics of the two-step approach discussed, and how the adopted enhancements build on the previous approach. These two sections deserve more details of the methodology with greater levels of details that what is being offered. This will help the reader understand exactly how this work differs and builds on the two previous studies by the team, how to interpret the results and the difference between the 'original' and 'enhanced' versions of H08 (figure 3), how to interpret the various variables shown in Table 1, and to facilitate reproducibility.	We agree with the Reviewer's concern and have largely revised the Sections 2.1 and 2.2. The main modifications are as follows: we added the most important equations used for crop yield modeling to Section 2.1; we revised Section 2.2 to illustrate the original implementation of the bioenergy crop (two-step approach) in H08 and our enhancement; we included the original parameter settings in Table 2 and the physical meanings of the parameters in Table 1; and we clarified the six submodules of H08 in Section 2.1 and revised Fig.1 by adding a schematic diagram of the connections for each submodule.
R1C5	The paper shows some validation results for the rainfed module, and not for the irrigation module, but then show results for both when simulating both globally. The validation step for the irrigated module should be shown and discussed in the main text.	Thank you for this good suggestion. We have moved the validation results (site-level) with irrigation in Fig. 6. The main text has been edited as follows (lines 240–246): "We also investigated the performance under the irrigated condition (shown in Fig. 6). We used the reported observed yields for ten sites globally (Table S3). We found that the simulated yields were within or close to the observed yields for five sites located in China, the UK, and France (see Table S3), but were overestimated for the remaining sites. This was due to the assumption of irrigation. H08 assumes that irrigation is fully applied to crops and hence the yield represents the maximum

		potential yield under irrigation condition. Therefore, if the reported yield is within the range of the simulated yield between rainfed and irrigated conditions, it is considered reasonable. This was found to be the case, as shown in Fig. 6." We also included the validation results with irrigation (country-level) in Fig. S3, which indicates good performance. The corresponding text is on lines 268–270: "An additional comparison under the irrigated condition is presented in Fig. S3. The correlation coefficient of the yield simulated by H08 and LPJmL, as shown in the scatterplot (Fig. S3), was 0.95. A t-test showed that the correlation was significant at the 0.01 level."
R1C6	I would suggest shortening the title. How about something like "Simulating second-generation bioenergy crop yield using the global hydrologic model H08"	We have shortened the title to "Simulating second-generation herbaceous bioenergy crop yield using the global hydrological model H08 (v.bio1)".
R1C7	Line 4. Why is Miscanthus capitalized and italic but not switchgrass?	*Miscanthus* denotes *Miscanthus giganteus* and switchgrass indicates *Panicum virgatum* in this study. *Miscanthus* is the genus to which the studied species belongs, which is always capitalized and italicized in Binomial nomenclature. Therefore, we have used this conventional expression (capitalized and italicized) for *Miscanthus*. The same expression has been used in previous reports such as Trybula et al. (2015).
R1C8	Line 7: 'enhanced H08' Doesn't H08 keep track of different version numbers that can be used here instead of calling something an enhanced model version?	Thank you. We have changed 'enhanced H08' to 'H08 (v.bio1)'.
R1C9	Line 13: Add a sentence into the abstract to introduce the term BECCS if you are going to start the introduction section with this term. Preferably, I would suggest confining the framing around bioenergy crops rather than BECCS since the latter term never appears again in the text.	Thank you for noting this issue that we previously unaware of. We have taken your suggestion to focus on the bioenergy crop plantation and removed the abbreviation of BECCS.

R1C10	Lines 26, 30, 34: LPLmL should be LPJmL	We have corrected this error.
R1C11	Lines 30-32: It is not just LPJmL based on the following paragraph. It is also H08 based on the two recent publications using H08 (Yamagata et al. 2018 and Wu et al. 2019).	Thank you. We have added H08 here.
R1C12	Line 34: change 'biogeny' to bioenergy	We have corrected this error.
R1C13	Line 41: Hanasaki et al 2008a/b are repeated twice in the list.	This citation is listed as "(Hanasaki et al., 2008a, 2008b, 2010, 2018a, 2018b)". We checked and did not find any repeated citations.
R1C14	Line 44: the reference Wu et al. 2019 is missing in the list of references at the end.	The reference to Wu et al. 2019 was listed on lines 446–448 of the original manuscript. Sorry, we noted that it is not in an alphabetical order and we have now put it after the reference of Weedeon et al. (2014).
R1C15	Lines 49-50: I would suggest omitting the sentence "However, it is noted that the model performance for the simulated bioenergy crop yield was not validated at all" as an argument to justify the novelty of the work. I doubt the authors are claiming that the previous two studies using H08 with representation of bioenergy yield ignored properly validating the model and this study contributes this novelty. I would suggest that authors replace this sentence with an explanation of how the new work builds on the two- step approach documented in the two previous papers (Yamagata et al. 2018 and Wu et al. 2019).	We agree with the Reviewer's concern and suggestion. The novelty of this paper lies in its systematic parameter calibration using the best available multi-site data. The first bioenergy crop implementation in H08 (Yamagata et al., 2018). Using the same bioenergy crop scheme, another recent study also used H08 estimates of yield for *Miscanthus* and switchgrass to predict global bioenergy potential (Wu et al., 2019). Our paper is based on the work of Yamagata et al. (2018). We have rephrased the sentence as follows (lines 47–48): "Based on the work of Yamagata et al. (2018), here we improved the bioenergy crop simulation in H08 by performing a systematic parameter calibration for both *Miscanthus* and switchgrass using the best available data."
R1C16	Line 61: 'The six sub-modules', You have not introduced what those six submodules are yet. I would start by listing them or at least list them in () right after this phrase.	Thank you. We have added the six submodules to the sentence, as follows (lines 62–64): "The six sub-modules (land surface hydrology, river routing, crop growth, reservoir operation, environmental flow requirements, and

		anthropogenic water withdrawal) are coupled in a unique way (Fig. 1a)."
R1C17	Line 75: I would expand on this section to show the two-step approach here before talking about model enhancements in the next section (2.2). Even if those were presented in the two previous publications, I would at least include them in SI to make this manuscript a standalone piece.	We have rephrased the enhancement section and included the two-step approach as follows (lines 125–133): "The original bioenergy crop implementation in H08 (Yamagata et al., 2018) was conducted in two steps. First, crop parameters (see the old values in Table 2) for *Miscanthus* (refer to *Miscanthus giganteus* in this study) and switchgrass (refer to *Panicum virgatum* in this study) were adopted based on the settings from the SWAT model 2012 version (Arnold et al., 2013). However, the default parameters did not reflect the characteristics for *Miscanthus* and switchgrass well, which could lead to serious bias based on the result in Trybula et al. (2015). Second, maturity was defined by either undergoing an autumn freeze (i.e., the air temperature was below the minimum temperature for growth) or the exceedance of the maximum of 300 continuous days of growth. Because both *Miscanthus* and switchgrass are perennial, the potential heat unit was set as unlimited (see the old values in Table 2). However, this unlimited potential heat unit is far from the observations (see the new values in Table 2) reported by Trybula et al. (2015)."
R1C18	Lines 76-85: I would suggest including all the equations and steps for how yield is simulated to shed more light about the method and to allow reproducibility of the approach.	We have added the equations and text related to yield simulation. Since this addition is quite long, we have not included it here; please see details in Section 2.1.
R1C19	Line 90: 'as an output item' Are you saying that can you simulate water consumption as a new output variable? It is not clear.	You are correct. We have added a new output variable for water consumption to the crop module.
R1C20	Line 91: 'Fifth, we fixed the bug in the original code'. What Bug? One could say 'we fixed a bug in the original code'. But this is so vague and does not really give the reader any additional information. I would suggest	The bug is related to the improper use of ".eq." in place of ".ge." Since this is too trivial to report, we have taken your suggestion removed it from the main text.

	dropping the fifth point. Such details are best documented in SI.	
R1C21	Lines 105-110: can you mention the number of data points and years being used?	We have added the numbers and years on lines 169–175, as follows: "To independently calibrate and validate the performance of H08 in simulating the bioenergy yield, we collected and compiled up-to-date site-specific (varied from 1986 to 2011) and country-specific (varied from 1960 to 2010) yield data from both observations and simulations (Clifton-Brown et al., 2004; Searle and Malins, 2014; Heck et al., 2016; Kang et al., 2014; Li et al., 2018a). For *Miscanthus*, the yield data used covered 72 sites (64 rainfed and 8 irrigated; observed) and 15 countries (simulated). The simulated country-specific data is from MISCANMOD and LPJml. For switchgrass, the yield data used covered 57 sites (55 rainfed and 2 irrigated; observed) and 16 countries (simulated). The simulated country-specific data are from HPC-EPIC and LPJml."
R1C22	Line 115: what variable is being calibrated here? H08 simulates many output variables. How does the calibration process ensure that the adopted calibration process does not offer a gain in better matching one variable at the expense of another variable? For example, did the authors calibrate runoff first and then yield, or is it done all at once? If it is the latter, then showing some results on runoff would be necessary. I am not asking the authors to necessarily do additional work, but rather to better explain their approach.	Here, we calibrated the five key parameters of radiation use efficiency (be), maximum leaf area index (blai), base temperature (Tb), maximum daily accumulation of temperature (Hunmax), and minimum temperature for planting (TSAW) that influence the yield simulation in the crop module. The standard H08 model uses a priori parameters; therefore we did not calibrate other variables such as runoff in the land surface hydrology module.
R1C23	Line 117: 'the enhanced h08'. Does this mean that the second simulation was only done for the enhanced model?	You are correct. This simulation is used to analyze the effect of irrigation on yield, water consumption, and water use efficiency. Based on your suggestion (R1C34), we have reorganized the simulation setting section as follows (lines 187–193):

		"After calibration, four different kinds of simulation were run with different purposes. The first simulation was conducted using the original model without irrigation to investigate its performance. The second simulation was conducted using the enhanced model without irrigation to investigate its performance under rainfed condition. The third simulation was conducted using the enhanced model with irrigation to investigate its performance under irrigated condition. These three simulations were conducted at a daily scale with annual meteorological data from WFDEI for the period 1979–2016. The last simulation was conducted using identical model settings to the third one, except using different meteorological data from S14FD for the period 1979–2013. Note that irrigation in this study means uniform unconstrained irrigation."
R1C24	Lines 124-125: A bit unclear. Was the calibration done as a multi-objective optimization process to optimize both the RMSE and R values. For example, how do you decide an optimal parameter set when the two goodness-of-fit variables disagree? Figure 3 only shows RMSE, so I would suggest that you stick to this one and drop the R coefficient. Also, it is not clear if observed data is available for several individual years or only a single average year is available. If a time series exists, then I would suggest using goodness-of-fit measures such as Nash-Sutcliffe.	We apologize for the unclear description. Let us further explain the method. From a statistics perspective, root mean square error (RMSE) measures the standard deviation of prediction errors compared to the observations. The correlation coefficient (R) measures the correlation between the prediction and observation. Here, we gave the priority to RMSE, as it is a better metric for measuring errors in the predicted yield compared to R. We have added a figure showing the variations of RMSE and the corresponding R values in Fig. S1. It shows good agreement between the lowest RMSE and corresponding relatively high R. The majority of the yield data fall within a single period instead of an individual year. Thank you for the suggestion of using the Nash-Sutcliffe model efficiency coefficient, we did not use it due to the lack of time-series yield data.

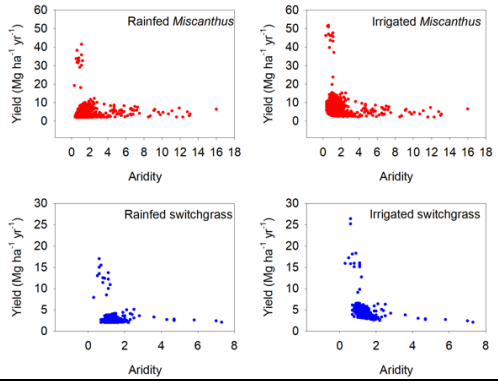
R1C25	Line 137: 'because the few sites that were irrigated'. Please rephrase.	We have modified the sentence, as follows (line 216): "because only a few sites were irrigated"
R1C26	Line 139: 'previous reports' Please add citations to support this claim. The single sentence that comes afterward is insufficient. What about other parameters?	Thank you for noting this issue. We have rephrased the sentence by adding a citation, as follows (line 217): "These values are similar to those of Trybula et al. (2015)" Other variables, such as base temperature, and maximum leaf area indices are also similar to those of Trybula et al. (2015).
R1C27	Lines 140-145: how does this work differ from Trybula et al 2015? This is not discussed in the intro. Also given that the adopted approach follows the SWAT implementation in Trybula et al 2015, and almost all of the parameters taken from the literature are also taken from Trybula et al 2015, would not it guarantee that you get similar parameter values for the other calibrated values to match those in Trybula et al 2015? What about other studies?	We apologize for this, let us further explain it here. Basically, we conducted a global calibration and evaluation with the best available data, while the work of Trybula et al. (2015) is based on one site observation and validation. The work of Trybula et al. (2015) is the first report of updating the SWAT for bioenergy crop simulation based on field observations. It provides a valuable reference for our study, as the crop module of H08 is similar to that of SWAT. Therefore, in our model enhancement process, the crop parameters related to leaf area development (potential heat unit, optimum temperature, maximum leaf area index, and two complex number; see details in Table 1) were based on their field observations (Trybula et al., 2015). For other parameters, including radiation use efficiency (be), maximum leaf area index (blai), base temperature (Tb), maximum daily accumulation of temperature (Hunmax), and minimum temperature for planting (TSAW), we conducted a systematic multi-site calibration and evaluation based on the parameter ranges reported in other studies (see Table 3). Our finalized parameters obtained through this approach are generally similar to those reported in

		Trybula et al. (2015), and are well within the range of other studies, as shown in Table 3.
R1C28	Lines 148-154: although the results are better than the original version, the results still seem to show a tendency to underestimate based on the results shown in figure 3.	Site-specific yield simulation and validation of traditional crops is a major challenge for global models (Müller et al., 2017), notwithstanding the bioenergy crop, which are being added to existing global models. For example, underestimation or overestimation have been reported in other global models like LPJml and ORCHIDEE that including the bioenergy crops. We added a new figure (Fig.3) of the calibrated results. It illustrates very good performance. Fig. 4 shows the validation of the model. Although it shows much better performance than the original simulation, it also shows a tendency toward underestimation. However, if we separately analyze each site, as shown in Fig. 5, most yield estimates were similar to or within the observed yield ranges. Therefore, our simulation appears to be reasonable at the global scale. We have further quantified the bias to illustrate the improvement of the model and rephrased the text as follows (lines 229–238): "Points in a scatterplot comparing simulated yields derived from the enhanced H08 with observed yields are well distributed along the 1:1 line. It can be seen that the performance of the enhanced H08 was improved over that of the original H08. For *Miscanthus*, the bias of original model ranged from –84% to 80% with a mean of –52%, while the bias of the enhanced model ranged from –59% to 53% with a mean of –9%. For switchgrass, the bias for original model ranged from –78% to 338% with a mean of 25%, while the bias for the enhanced model ranged from –52% to 109% with a mean of –7%. Note that it also shows a tendency toward underestimation for some sites, especially for *Miscanthus*. More detailed site-specific results

		are shown in Figs. 5a (*Miscanthus*) and Fig. 5b (switchgrass). To depict the uncertainties in the observed yield, the minimum and maximum observed yields are shown as error bars in Fig. 5. It was found that the simulated yields were within or close to the range of the observed yields. The simulated relative error was randomly distributed, was substantially smaller than the range of the observed yields, and showed no climatic bias."
R1C29	Line 158: 'well at sites 1, 2, and 10' so how many sites are under irrigation? You should mention it here.	There are ten sites with irrigation. We have modified the sentence, as follows (lines 240–241): "We also investigated the performance under the irrigated condition (shown in Fig. 6). We used the reported observed yields for ten sites globally (Table S3)."
R1C30	Lines 166-175: Did you drop the missing value from the significance test analysis (e.g., Finland in Fig 5d, Mongolia in Fig 5e)? I am still unsure whether the yield values from the other studies are average values over a particular period, and if it is the same period as in this study.	First, we did not drop the missing values. Note that the yield from MISCANMOD is reported with yield less than 10 Mg ha^{-1} yr^{-1} excluded (Clifton-Brown et al., 2004); therefore, we used the same method to make the comparison consistent. As the simulated yield for Finland is less than 10 Mg ha^{-1} yr^{-1}, therefore there are no values for Finland. For Mongolia, our estimated value was 0.4 Mg ha^{-1} yr^{-1} and was rounded to 0 Mg ha^{-1} yr^{-1}. Second, based on your comment below (R1C32), we moved the text related to the study period, as follows (on lines 253–256): "The periods of climate data used as inputs were 1960–1990, 1980–2010, and 1982–2005 for MISCANMOD, HPC-EPIC, and LPJmL, respectively. Here, the comparisons were conducted using exactly the same period as that of HPC-EPIC and LPJmL. For MISCANMOD, however, we used the data from 1979–1990 due to data availability."

R1C31	Lines 188-189: 'This can also be inferred from the validation results in Heck et al. (2016)' Please elaborate.	We have added an explanation, as follows (lines 276–277): "This can also be inferred from the validation results (Fig. 1a) in Heck et al. (2016) since the LPJml-simulated yield is close to the yield of *Miscanthus* compared to those of switchgrass."
R1C32	Lines 196-201: This information should appear earlier in the manuscript, so the reader is left wondering about such details. Also, if there is annual data from the other studies, then why not look at the timeseries instead of simply comparing the average value over a time period? To say a model can capture the long term mean over different basins is one level of validation, but to say that the model can also capture the interannual variability of yield from year to year, then this is a much more desirable level of validation.	As noted in a previous reply (R1C30), we have moved this text to the beginning of the section. Unfortunately, all values reported in previous studies are in mean annual terms. We used the average values for each component to ensure consistent comparison.
R1C33	Lines 203-220: This section comes as s surprise as it was not mentioned earlier as part of the framing of the paper in the front sections.	We apologize. This section shows the spatial distribution of yield, which is helpful for clarifying its geographical differences among climate zones. Based on your suggestion, we have added a sentence (in bold below) to notify readers of this information in the last paragraph of the Introduction Section, as follows (lines 51–55): "The following sections of this paper will: 1) describe the default biophysical process of the crop module in H08, 2) explain the enhancement of H08 for *Miscanthus* and switchgrass, 3) evaluate the enhanced performance of the model in simulating yields for *Miscanthus* and switchgrass, **4) map the spatial distributions of the yield of *Miscanthus* and switchgrass**, and 5) illustrate the effects of irrigation on the yield, water consumption, and WUE (defined here as the ratio of yield to water consumption) of *Miscanthus* and switchgrass."
R1C34	Line 206: It is confusing how many simulations were done in the study. The	Thank you for this constructive suggestion. We have reorganized the simulation setting

	authors talk about two simulations twice, but are referring to different ones. I would suggest including an experimental design section as part of the methodology section to explain the different simulations to be conducted over a historical period (rainfed/irrigated, original/enhanced, ...).	in Table S1 and the description is now as follows (on lines 187–193): "After calibration, four different kinds of simulation were run with different purposes. The first simulation was conducted using the original model without irrigation to investigate its performance. The second simulation was conducted using the enhanced model without irrigation to investigate its performance under rainfed condition. The third simulation was conducted using the enhanced model with irrigation to investigate its performance under irrigated condition. These three simulations were conducted at a daily scale with annual meteorological data from WFDEI for the period 1979–2016. The last simulation was conducted using identical model settings to the third one, except using different meteorological data from S14FD for the period 1979–2013. Note that irrigation in this study means uniform unconstrained irrigation."
R1C35	Lines 211-220: The validation results shown and discussed in the main body of the manuscript only talk about the rainfed simulations. It is unconvincing to skip the validation step for the irrigation module, and then show results and draw conclusions using that irrigation modeling capability. In this section, results from this study are shown, but they are not contrasted with estimates from previous studies.	Following your comment (R1C5), we have added validation results under irrigated condition, as noted in a previous reply (R1C5). The results are comparable to previous reports, as the discussion on lines 307–310: "The spatial distributions of yield increases due to irrigation simulated by H08 were very similar to those simulated by LPJmL (Beringer et al., 2011). At the continental scale (e.g., Europe), yield increases were located mainly in southern Europe, consistent with the findings obtained using MISCANMOD (Clifton-Brown et al., 2004)."
R1C36	Lines 223-233: Is this based on some aggregated regions, or on all the grids that belong to each climate zone? How do these results compare to other studies that were discussed in section 3.4? assuming this was based on a grid-level analysis, why not plot the results for all the grids and show a scatter	This is a very good point. Yes, it is based on all grid cells belonging to specific climate zones. However, we used the results for grid cells with yield higher than 2 Mg ha^{-1} yr^{-1} (low-yield productivity). Based on your suggestion, we constructed a scatterplot diagram between yield and

	plot (yield on the y-axis, and aridity or some other index that allows for distinguishing among the different climate zones on the x-axis)? This would allow the authors to fit a line to the data and talk about the results in a more compelling way.	aridity (shown below). However, we found it difficult to directly differentiate the effect of climate. Meanwhile, our current figures clearly show the differences among different climate zones. This section provides additional analysis of the predicted yield, which may not affect the main conclusion of this study. Therefore, please let us retain the original presentation of these results. 
R1C37	Line 241: 'WUE, which is defined in this study as the ratio of yield to water consumption' This should have appeared the first time the term is mentioned in the manuscript.	We have defined this term in the introduction, as follows (lines 54–55): "5) illustrate the effects of irrigation on the yield, water consumption, and WUE (defined here as the ratio of yield to water consumption) of *Miscanthus* and switchgrass."
R1C38	Line 246: 'The WUE values for Miscanthus were higher than those for switchgrass, which is inconsistent with values in previous reports' Please add a sentence to articulate why?	We are sorry for using the incorrect word; it should be "consistent", as the results are similar (WUE of *Miscanthus* is higher than that of switchgrass). We have therefore changed the word "inconsistent" to "consistent".
R1C39	Line 263: 'which was useful for optimizing bioenergy land with better consideration of water protection' – I am not sure what this means?	We have changed the sentence, as follows (lines 361–362): "which was useful for bioenergy land-scenario design. For example, more land can be allocated to the areas with greater WUE."
R1C40	Lines 266: 'and our results are reproducible with the transparent parameter disclosed.' Just sharing the parameters sets does not	Thank you for noting this issue. We have deleted the sentence.

	guarantee reproducibility. I would suggest omitting that phrase.	
R1C41	Line 277: why was not this yield map used in the previous sections as part of the validation exercise? Also, I would suggest moving figure S7 out of SI and into the main text.	We have added this yield map to the Method Section 2.4, as follows (lines 182–184): "A global yield map of *Miscanthus* and switchgrass that was generated using a random-forest algorithm (Li et al., 2020) was also used to compare the results. This yield map provides a benchmark for evaluating model performance because it is largely constrained by the observed yield ranges, denoting the yields achievable under current technologies (Li et al., 2020)." We also moved the corresponding result into the main text to Result Section 3.4, as follows: "As shown in Fig. 8, we compared our simulation with the latest available global bioenergy crop yield map, generated from observations using a random-forest (RF) algorithm (Li et al., 2020). This RF yield map provides a benchmark for evaluating model performance because it is largely constrained by the observed yield ranges, denoting the yields achievable under current technologies (Li et al., 2020). As shown in Fig. 8a and Fig. 8b, there were small differences between our estimated yield and RF yield for switchgrass, whereas larger differences were found for *Miscanthus*, especially in tropical regions. There is a similar case for ORCHIDEE, as shown in Fig. S21 in Li et al. (2020). We also compared the differences in the mean values for *Miscanthus* and switchgrass because they are not distinguished in LPJmL. As shown in Fig. 8c and Fig. 8d, the differences between our estimations and the RF yields were generally lower than those between the LPJml estimations and RF yields. In summary, our

		estimations were well within the ranges of those of ORCHIDEE and LPJml."
R1C42	Figure 4: To be consistent with the black error bars, the blue/red ones should also reflect max/min. Also, why include all the years for observations? Should not these be for the years for which there is an associated observed yield value?	We have included the maximum and minimum values for *Miscanthus* (red) and switchgrass (blue) in the revised manuscript. Since the observed yields are from varying periods, we followed the methods of Heck et al. (2016), Beringer et al. (2011), and Li et al. (2018), comparing the mean simulated yield within a historical period to the observed yield. This was done in part due to missing records of harvest year for some observations.
R1C43	Figure 7: why is the y-axis for panel b flipped around as if the values should be negative? I would suggest keeping it consistent with the other two panels (0 at the bottom left corner, and the bar chart goes upward for positive values).	We agree with your suggestion and have modified the y-axis in Fig. 7b.
R1C44	Table 1: Please add another column to define the different parameters and what they mean physically.	Thank you. We have added a new table to show the definition and physical meaning. For details, please see Table 1.

References:

1. Arnold, J. G., Kiniry, J. R., Srinivasan, R., Williams, J. R., Haney, E. B., and Neitsch, S. L. (Eds.): SWAT 2012 Input/Output Documentation, Texas Water Resources Institute, USA, 2013.
2. Beringer, T. I. M., Lucht, W., and Schaphoff, S.: Bioenergy production potential of global biomass plantations under environmental and agricultural constraints, GCB Bioenergy, 3, 299–312, https://doi:10.1111/j.1757-1707.2010.01088.x, 2001.
3. Clifton-Brown, J. C., Stampfl, P. F., and Jones, M. B.: Miscanthus biomass production for energy in Europe and its potential contribution to decreasing fossil fuel carbon emissions, Glob. Change Biol., 10, 509–518, https://doi:10.1111/j.1529-8817.2003.00749.x, 2004.
4. Hanasaki, N., Inuzuka, T., Kanae, S., Oki, T.: An estimation of global virtual. water flow and sources of water withdrawal for major crops and livestock products using a global hydrological model, J. Hydrol., 384, 232–244, https://doi.org/10.1016/j. jhydrol.2009.09.028, 2010a.
5. Hanasaki, N., Kanae, S., Oki, T., Masuda, K., Motoya, K., Shirakawa, N., Shen, Y., Tanaka, K.: An integrated model for the assessment of global water resources – Part 1: Model description and input meteorological forcing, Hydrol. Earth Syst. Sci., 12, 1007–1025, https://doi:10.5194/hess-12-1007-2008, 2008a.
6. Hanasaki, N., Kanae, S., Oki, T., Masuda, K., Motoya, K., Shirakawa, N., Shen, Y., and Tanaka, K.: An integrated model for the assessment of global water resources – Part 2: Applications and assessments, Hydrol. Earth Syst. Sci., 12, 1027–1037, https://doi:10.5194/hess-12-1027-2008, 2008b.
7. Hanasaki, N., Yoshikawa, S., Pokhrel, Y., Kanae, S.: A global hydrological simulation to specify the sources of water

used by humans, Hydrol. Earth Syst. Sci., 22, 789–817, https://doi:10.5194/hess-22-789-2018, 2018a.

8. Hanasaki, N., Yoshikawa, S., Pokhrel, Y., and Kanae, S.: A quantitative investigation of the thresholds for two conventional water scarcity indicators using a state-of-the-art global hydrological model with human activities, Water Resour. Res., 54, 8279–8294, https://doi.org/10.1029/2018WR022931, 2018b.
9. Heck, V., Gerten, D., Lucht, W., and Boysen, L. R.: Is extensive terrestrial carbon dioxide removal a 'green' form of geoengineering? A global modelling study, Glob. Planet. Change, 137, 123–130, https://doi:10.1016/j.gloplacha.2015.12.008, 2016.
10. Li, W., Ciais, P., Makowski, D., and Peng, S.: A global yield dataset for major lignocellulosic bioenergy crops based on field measurements, Sci. Data, 5, 180169, https://doi:10.1038/sdata.2018.169, 2018a.
11. Li, W., Yue, C., Ciais, P., Chang, J., Goll, D., Zhu, D., Peng, S., and Jornet-Puig, A.: ORCHIDEE-MICT-BIOENERGY: an attempt to represent the production of lignocellulosic crops for bioenergy in a global vegetation model, Geosci. Model Dev., 11, 2249–2272, https://doi.org/10.5194/gmd-11-2249-2018, 2018b.
12. Li, W., Ciais, P., Stehfest, E., van Vuuren, D., Popp, A., Arneth, A., Di Fulvio, F., Doelman, J., Humpenöder, F., Harper, A. B., Park, T., Makowski, D., Havlik, P., Obersteiner, M., Wang, J., Krause, A., and Liu, W.: Mapping the yields of lignocellulosic bioenergy crops from observations at the global scale, Earth Syst. Sci. Data, 12, 789–804, https://doi.org/10.5194/essd-12-789-2020, 2020.
13. Müller, C., Elliott, J., Chryssanthacopoulos, J., Arneth, A., Balkovic, J., Ciais, P., Deryng, D., Folberth, C., Glotter, M., Hoek, S., Iizumi, T., Izaurralde, R. C., Jones, C., Khabarov, N., Lawrence, P., Liu, W., Olin, S., Pugh, T. A. M., Ray, D. K., Reddy, A., Rosenzweig, C., Ruane, A. C., Sakurai, G., Schmid, E., Skalsky, R., Song, C. X., Wang, X., de Wit, A., and Yang, H.: Global gridded crop model evaluation: benchmarking, skills, deficiencies and implications, Geosci. Model Dev., 10, 1403–1422, https://doi.org/10.5194/gmd-10-1403-2017, 2017.
14. Searle, S. Y., and Malins, C. J.: Will energy crop yields meet expectations?, Biomass Bioenerg., 65, 3–12, https://doi:10.1016/j.biombioe.2014.01.001, 2014.
15. Trybula, E. M., Cibin, R., Burks, J. L., Chaubey, I., Brouder, S. M., and Volenec, J. J.: Perennial rhizomatous grasses as bioenergy feedstock in SWAT: parameter development and model improvement, GCB Bioenergy, 7, 1185–1202, https://doi:10.1111/gcbb.12210, 2015.
16. Yamagata, Y., Hanasaki, N., Ito, A., Kinoshita, T., Murakami, D., and Zhou, Q.: Estimating water–food–ecosystem trade-offs for the global negative emission scenario (IPCC-RCP2.6), Sustainability Sci., 13(2), 301–313, https://doi:10.1007/s11625-017-0522-5, 2018.
17. Wu, W., Hasegawa, T., Ohashi, H., Hanasaki, N., Liu, J., Matsui, T., Fujimori, S., and Takahashi, K.: Global advanced bioenergy potential under environmental protection policies and societal transformation measures, GCB Bioenergy, 11(9), 1041–1055, https://doi.org/10.1111/gcbb.12614, 2019.