Review ‘How to measure the efficiency of terrestrial carbon dioxide removal methods’

Reviewer 1: Wei Li

This manuscript estimated the CDR potentials of afforestation/reforestation (AR) and bioenergy with carbon capture and storage (BECCS, Miscanthus) in the low-warming scenario using JSBACH3.2. The analyses of different factors that impact CDR such as locations, plantation area, and levels of FFS and CCS are important for evaluating the feasibility and climate benefits of AR and BECCS. More importantly, both AR and BECCS simulations were done within the same model, which makes the comparison more consistent. The model has also been well calibrated and validated against field observations. In general, the manuscript is clearly written, and the results are scientifically important and technically sound. I would recommend the publication of this manuscript after some minor revisions.

Response:

Dear Wei Li,

Thank you very much for taking the time to review the manuscript thoroughly and for providing valuable feedback. We are glad for your positive evaluation of the manuscript and have incorporated all the suggested changes (as described below).

Wei Li: *My main concern is about the treatment of AR in the model. The authors used the current preferences of existing forests as AR, but these forest PFTs are more parameterized by and considered as natural forests, so AR should refer to natural recovery if I understood correctly. In reality, AR may adopt some fast-growing species such as eucalypt and poplar, especially in Southern China, so they may have much higher yields than natural recovery. It may partly explain the lower potential of AR compared to BECCS (e.g., L280-282, L284).*

Response: Indeed, Indeed, the model represents natural forests rather than fast-growing species. This is common in most DGVMs and ESMs contributing to the CMIP6 framework. We have made that clear in the methods:

l. 151: Thus, AR is represented as natural regrowth rather than fast-growing wood plantations in the model.

In addition, we have added this to the discussion:

l. 497: Implementing forest plantations in LPJmL increases the total carbon uptake by up to 30% globally by 2100 compared to natural regrowth (Braakhekke et al., 2019).

Wei Li: *The lifetime of harvested wood from AR is also very important. The authors mentioned the wood harvest in the methods but didn’t report much in the results. Some simple calculations may be done by assuming a decay rate/turnover time in the wood product pool as assumed in the bookkeeping models.*

Response: Thank you for this valuable point. In the paper, we provide a simple estimate of additional wood harvest from AR, which was calculated during post-processing based on the LUH2 wood harvest in forest areas without AR. Since this additional wood harvest in forest areas with AR is relatively small compared to the total carbon stored in the vegetation, litter and soil by AR (1.49 GtC/53GtC =2.8%), we provide the estimated additional wood in the results section but do not include it further in the comparisons of efficiency between AR and HBPs. Also, because of this already very small cumulative value, we did not further separate the additional wood harvest in AR areas into distinct wood product pools with differing decay rates / turnover times.

The LUH2-based wood harvest in forest areas without AR is included in our model simulations and much higher (91.6 GtC cumulatively). Within JSBACH3.2 this wood harvest is further distributed with PFT-specific fractions into a slash fraction remaining on-site and three wood product pools (1 / 10 / 100 year lifetime) with exponential decay as in bookkeeping models. This wood harvest and wood product pool C storage is implicitly included in our calculations but since the wood harvest on forest areas without AR is similar in both our simulations with AR and without AR but HBPs instead, it does not affect our results. Since our study is intended to compare the efficiency of AR with HBPs excluding the C storage of existing forests, we do not discuss the C dynamics of different harvested wood products from forest areas without AR further in this paper, acknowledging that this can represent another form of CDR itself.

We have rephrased / added these aspects in our results section to make them more clear:

l. 293: The cumulative global wood harvest of forests without AR as estimated by the LUH2 data (whLU H2(2100)) between 2015 and 2100 is 91.6 GtC. Since this is similar in both simulations, it does not impact our results. Our estimate based on Section 2.3.1 reveals an additional wood harvest of AR areas whAR of 1.29 GtC. We argue that wood harvesting has a relatively small effect on the carbon cycle compared to the cumulative amount of 53 GtC sequestered by AR (Fig. 5) and thus we do not further consider it in our global estimates.

We have extended our discussion that increasing wood harvest might not necessarily increase carbon efficiency:

l. 493: Further, the benefits of storing wood carbon in long-lived products or using wood for bioenergy might be outweighed by decreased carbon sequestration in the forest and increased CO2 emissions to the atmosphere if used for bioenergy (Obermeier et al., 2021; Soimakallio et al., 2021).

The scientific literature is ambiguous about whether wood harvest reduces or increases the overall emissions of AR. The carbon removal potential of wood harvest depends on the permanence of carbon captured, the age of harvested wood, and the condition of the forest. As a consequence of wood harvest, AR, and natural disturbances that increase tree mortality, forest regrowth may constitute a substantial carbon flux to the biosphere, yet with highly uncertain estimates (Pugh et al. 2019, Houghton et al. 2012, Kondo et al. 2018). In contrast, Soimakallio et al. (2021) show that the increased biomass-based GHG emissions to the atmosphere together with decreased carbon sequestration in the forest very likely exceed the avoided fossil-based CO2 emissions.

***Wei Li:** Table 2, may also compare with CDR in Wang et al. (2023, https://doi.org/10.1021/acs.est.2c05253)*

Response: Thank you very much for pointing to the Wang et al. (2023) paper, which we have added to Table 2.

***Wei Li:** L410, L428 Although bioenergy crops may not need fertilization to improve yields, herbaceous bioenergy crops are harvested every year, so the nutrients in the aboveground biomass will be lost. Please see the nutrient demands by bioenergy crops in Li et al. (2021, https://doi.org/10.1021/acs.est.1c02238)*

Response: Thank you for emphasizing this point and providing us with this helpful reference. We don’t explicitly simulate fertilization by applying defined annual amounts of fertilizer at specific points in time. Instead, we follow the standard treatment of harvested N of crops in JSBACH3.2: The harvested N is returned to the soil after harvest over the year mimicking fertilizer application (Reick et al. 2021). Thus, we implicitly account for N fertilization that compensates for the loss of nutrients due to harvest in our model, even though we don't simulate explicit crop management as would be done on a field in the real world. We have emphasized this point in our methods including your reference and rephrased our discussion to make it more clear:

l.135: JSBACH3.2 does not explicitly account for crop management such as irrigation or fertilization. Instead, fertilization is simulated by returning the harvested nitrogen of HBPs to the soil over the year as is the default treatment for harvested nitrogen of crops in JSBACH3.2 (Reick et al. 2021). By doing this, we implicitly represent N fertilization to replenish nutrients removed by annual harvesting, which have been suggested to otherwise limit bioenergy production (Li et al. 2021).

l.482: However, second-generation bioenergy crops require less fertilizer than first-generation bioenergy crops (Li et al. 2018a) and their productivity increases only slightly using nitrogen fertilization (LeBauer et al. 2018). Still, Li et al. 2021 find that annual harvesting of aboveground biomass of HBPs leads to a loss of nutrients and a decrease of plant productivity if not compensated by fertilization. We implicitly represent fertilization in the model by adding the harvested nitrogen to the soil.

We have added the Li et al. (2021) paper to the discussion on nutrient demands of bioenergy crops:

l. 486: Although fertilizer application likely increases the carbon uptake of bioenergy plants, their usage results in additional costs and greenhouse gas emissions during production and transport, thereby lowering the efficiency (Li et al. 2021).

Open discussion comment Andi Krause

This is an interesting paper contributing to the debate on the role of CDR in climate mitigation. As the authors compare their results to one of my own papers (https://bg.copernicus.org/articles/14/4829/2017/) I want to note a few points:

- While this paper investigated LUC impacts on a range of ES indicators, we focused on carbon uptake in a subsequent paper (https://onlinelibrary.wiley.com/doi/full/10.1111/gcb.14144) which might therefore be more suited for comparison.

- We used a CCS carbon capture efficiency of 80% instead of 100%.

- In our 2018 paper we computed the forest area / carbon uptake relative to the baseline scenario by year 2099. Of course you can also use the present-day values as a baseline, just make sure to be consistent when comparing different studies.

Andreas Krause

Thank you very much for your valuable comments and positive evaluation of our manuscript.

We acknowledge that Krause et al. (2018) is more suited than Krause et al. (2017) for comparing the carbon uptake efficiency due to its designated focus and because additional simulations from other DGVMs are included.

We have adapted the values in Table 2 according to Krause et al. (2018) and changed the CCS capture efficiency to 80%.

Krause et al. (2018) use two different land use patterns (from MAgPIE and IMAGE) and four different DGVMs to calculate the carbon uptake from BECCS and ‘Avoided deforestation in combination with afforestation and reforestation’ (ADAFF). Thus, their range of tCDR efficiency is wider compared to Krause et al. (2017). Thanks to your comment, we now use the difference 2100 AR – 2100 BASE from Krause et al. (2018) to exclude climate effects on the carbon uptake of forests beyond AR between 2015-2100. This is consistent with the approach we used for our own simulations. Thus, the tCDR range of 99-125GtC for BECCS from Krause et al. (2017) is replaced by 20-122GtC from Krause et al. (2018) and the 106-113 GtC for AR is replaced by 19-144 GtC, respectively.

Reviewer 2: Irina Melnikova

Egerer et al. present an important work on comparison of two land-based carbon dioxide removal (CDR) options, namely afforestation/reforestation (AR) and bioenergy with carbon capture and storage (BECCS), under future low-emission scenario. They account for both CCS and fossil fuel substitution (FFS of bioenergy crops and show that BECCS is more beneficial for carbon removal in the long-term in many places of the world with some exceptions, and with many uncertainties. I find this study to be a good addition for existing literature on the topic and, thus, relevant and worth publishing; however, it requires some revision. Please consider my comments as friendly advice. If any of them don't make sense from your perspective, I am always happy to receive explanations.

Answer:

Dear Irina Melnikova,

Thank you very much for the valuable constructive, in-depth feedback. We are glad for your positive evaluation of the manuscript and have incorporated the suggested changes as described below.

1. The authors use SSP1-2.6 “sustainability” scenario, characterized by increase of both bioenergy and forest areas (Hurtt et al., 2020). Then it is not clear to me why the authors chose to focus on the bioenergy crop grids (and not afforested grids) defined by LPJmL/LUH2 for comparing the benefits of AR and BECCS. Because LPJmL in some direct or indirect way decided that the bioenergy crop grids are more suitable for crops (probably through socioeconomic and yield-related factors), then the AR and BECCS comparison is not fully fair. However, since the authors use SSP1-2.6 scenarios that has considerable increases in forested areas, why not trying to apply similar framework and plant bioenergy crops in those areas for comparison. I am aware that this would require a lot of effort (like running the model again etc.) but I think this would make this study more advantageous. If this is not possible, please at least add some discussion on the point.

   Response: Thank you for the valuable comment. Indeed, it would be a similarly fair approach to use the afforested areas comparing bioenergy crops and afforestation/reforestation. However, the focus of our study is to compare the two methods by introducing the different measures of efficiency (carbon uptake, FFS/CCS, timing, area) rather than to estimate realistic potentials. Therefore, it is important to apply both methods in the same areas, which could be both, either the one suited for bioenergy crops or AR. We decided on the former since the focus of the paper is more on bioenergy plants (we introduced their reimplementation in the model here for the first time and performed the comparison to observations). Furthermore, as you noted, the spatial distribution of bioenergy crops from IAMs is not primarily based on climate-related factors but rather on socio-economic factors, including exogenous assumptions on population, economic development, lifestyle, policies and technology change (van Vuuren et al., 2017). Thus, forests do not necessarily have a disadvantage in those areas. We have emphasized this point in the introduction:

l. 103: As the spatial distribution of bioenergy crops from IAMs is not primarily based on climate-related factors but rather on socio-economic factors (van Vuuren et al., 2017), forests do not necessarily have a disadvantage in those areas.

2. As I understood , the authors use the fCCS levels from the AR6 Scenarios database (Byers et al., 2022) by dividing primary energy production from biomass with CCS to total (lines 234-235). I suggest to use other data from this database (Carbon Sequestration|CCS|Biomass) that are available in order to evaluate their estimates on BECCS. I also think that for using AR6 scenario database, the recommended citation is Byers et al. 2022 (see https://data.ece.iiasa.ac.at/ar6/#/about). It is currently absent in the References.

   Response: Thank you very much for this hint.

   We have used the data for CCS biomass and compared them to the CCS of harvested biomass from our study. We find that CCS biomass is higher than our annual harvested biomass (up to 12.9 MtCO2 = 3.5 GtC CCS biomass vs up to 2.5 GtC harvested biomass in our study). By definition, CCS biomass does not exclusively include second generation bioenergy crops but also wood harvest, residues, and biomass of first generation bioenergy crops. Further, we find that the bioenergy crop production from the SSP1-2.6 IMAGE3.0 Scenario (Agricultural Indicators > Production > Crops Energy) is higher (up to 8000 Mt DM/yr = 4GtC) than the 2nd generation bioenergy harvested biomass (2.5 GtC) from our scenario, where the former also includes first generation bioenergy crops. We added Byers et al. (2022) to the reference list and referred to it accordingly (l.243).

   To make that point more clear, we have added a description of how fCCS levels were calculated in the methods:

   l. 244: fCCS is based on a decadal time series of primary energy production from biomass with and without CCS from the CMIP6 AR6 database (Byers et al., 2022) for the SSP1-2.6 scenario calculated with IMAGE3.0 (Fig. 4 and Eq. (2), van Vuuren et al. (2017); later on referred to as SSP1-2.6 CCS rates).

   fCCS = Primary Energy|Biomass|Modern|w/ CCS :
   (Primary Energy|Biomass|Modern|w/ CCS + Primary Energy|Biomass|Modern|w/o CCS) (2)

   Note that biomass here includes by definition purpose-grown bioenergy crops, crop and forestry residue bioenergy, municipal solid waste bioenergy, and traditional biomass. However, we assume that fCCS is similar for biomass from second-generation bioenergy.

3. Wouldn't it make sense to use the SSP Public Database (Riahi et al., 2017) https://tntcat.iiasa.ac.at/SspDb/dsd?Action=htmlpage&page=20 for estimating the fFFS (by looking at unharmonized emissions -> CO2 -> CCS -> Biomass). This would make the study more consistent with the SSP1-2.6 because the CCS and FFS is assumed by the IMAGE and is translated to CO2 concentrations and climate.

   Response: Thank you for pointing to the database. While it seems to be a very good idea to verify the CCS biomass using the database, unfortunately, fFFS or fCCS can not be estimated using the databases other than the energy fractions from biomass with and without CCS (for fCCS) and there are not suitable variables to estimate fFFS. In addition to CCS biomass, information on the total biomass from bioenergy plants is needed. As mentioned in 2., agricultural production of energy crops is given but lower than CCS biomass. Thus, most likely a substantial part of CCS biomass comes from wood harvest and residues.

4. Although SSP1-2.6 is a low-emission scenario, I expected to see at least some discussion on how the future climate affects (or does not affect) bioenergy crop production and forest. Besides, in the Methods, it is mentioned that MPI-ESM1.2-HR is a low climate-sensitivity model. Some information on the climate (e.g., global temperature change time series or spatial variation in 2100) in supplementary would be helpful for the reader.

   Response: Thank you for pointing this out. We already discussed parts of future climate effects on forests and bioenergy crops: The (permanent) storage of carbon will be limited by disturbances (i.e. fires, wind throw, droughts). However, we did not discuss the so-called CO2 fertilization effect yet. Due to elevated CO2 in the atmosphere, trees and bioenergy crops take up more carbon. Since Miscanthus is a C4 plant, the effect is most likely lower for bioenergy crops compared to most trees (that follow the C3 photosynthesis pathway). Additional simulations under SSP3.7 climate (not shown in this paper) support this. Further, there are different impacts of changes in temperature, humidity, etc. on carbon sequestration (Melnikova et al., 2023). We have added this point to the discussion:
   l. 377: Under climate change, forests and bioenergy plants likely take up more carbon from the atmosphere due to the elevated CO2. For most trees, the CO2 fertilization is most likely stronger compared to the C4 bioenergy plants. This effect is reinforced under high-emission scenarios.

   Response: We have added a map of spatial changes in temperature and precipitation of bias-corrected down-scaled climate forcing of MPI-ESM1.2-HR between present-day and 2100 in the Appendix. The temperature time series can be found on the ISIMIP webpage: https://www.isimip.org/protocol/isimip3b-temperature-thresholds-and-time-slices/.
   We have linked spatial changes in precipitation to tCDR potentials of AR vs HBPs in the results:
   l. 313: For example, forests benefit stronger from the precipitation increase in Southeast China towards the end of the century (Fig. A1), whereas dryer conditions in the Sahel are likely more favorable for HBPs.
   l. 343: In this specific SSP1-2.6 land use and climate scenario, the time until HBPs become more efficient than AR depends very much on the regional climate and soil conditions. For example, although the tCDR onsets in Southeast China and the Sahel appear after 2060, the time until HBPs become more efficient is much shorter in the Sahel compared to Southeast China due to a drying trend in the former and a wetting trend in the later region (Fig. A1).

5. While the authors conclude that “BECCS has a higher potential in the South American grasslands and Southeast Africa, whereas AR is more suitable in Southeast China”, I am concerned that authors come to this conclusion because under the considered scenario, originally bioenergy crops were applied earlier in the century in South America and South Africa and later in Southeast China (judging from Fig. 8c). But then, the potential benefits arise not from the region “suitability” but because BECCS can remove more carbon in the long term. It would be helpful to have some discussion on this caveat.

Response: Thank you very much for this important argument. Indeed, the suitability of regions is biased by the implementation time of the respective tCDR measure and thus the local environmental conditions are not the only criterion to evaluate the carbon uptake potential of a specific area. We can only approximate this potential impact within our specific scenario, rather than giving general statements of a region's suitability. We have integrated the discussion of regional suitability vs onset of HBPs in the results section and in the discussion. See also point 4. on the discussion about climatic conditions vs. late onset of tCDR.
l. 311: The higher CDR efficiency of AR in Southeast China is partially due to the late onset of tCDR in this area (Fig. 8). However, compared to areas with a similarly late onset (e.g. the Sahel zone), there is a much higher level of FFS needed for HBPs and thus climate and soil conditions are more favorable for AR.

Other minor/technical comments.

1. Title: would it make sense to make more specific and state "efficiency of bioenergy crops"? or reword the title to give a clue that AR and BECCS are compared?

   Response: Thanks for the suggestion to make the title more explicit. We have adapted the title to 'How to measure the efficiency of bioenergy crops compared to forestation'.

   However, the previous title referring to tCDR more generally should address that our method is applicable to other area-based tCDR methods, such as soil carbon sequestration, agroforestry, and biochar. We have added to the conclusions:

   l. 503: While we focus on BECCS and AR in this study, the measures of efficiency are applicable to other area-based tCDR, including soil carbon sequestration, agroforestry, and biochar.

2. Line 14: the sentence starts with "Hence" but is not logically connected with the previous sentence.

   Response: Right, we removed 'Hence'.

3. Line 23: decades to centuries?

   Response: We have changed this as suggested:
   l. 21: from conventional methods applied at large scale for decades to centuries such as afforestation and reforestation (AR)

4. Line 25: and geosphere?

   Response: Indeed, only the biosphere is meant here. The currently employed methods include mainly afforestation/reforestation and other forms of land management (e.g. see Smith et al. 2023), while the amount of novel forms of CDR such as BECCS or DACCS is still low. The novel CDR will most likely become more and more important in the future though. We adapted the wording to avoid misunderstandings:

l. 23: Nearly all currently deployed CDR depends on terrestrial ecosystems that store carbon in the biosphere (Smith et al., 2023).

5. Line 26 and throughout the manuscript (e.g., line 38, 50): I found it difficult to understand what is in the brackets. Is it uncertainty via SD? Please clarify.

   Response: The numbers in line 26 refer to the range of different (bookkeeping) models. We have updated the numbers to the most recent report, see Smith et al. (2024) Fig. 7.5. The numbers in lines 38 and 50 indicate the model/scenario mean and spread. We have changed the wording to make that clear:
   l. 24: . From 2013 to 2022, bookkeeping models aligned with estimates of the Global Carbon Budget suggest between 1.2 to 2.2 GtCO2 per year removed from the atmosphere through AR (Smith et al., 2024).
   l. 35: Across the scenarios that limit the warming to 2°C or below, agriculture, forestry, and other land use (AFOLU), mainly AR, remove on average about 2.98 (scenario spread of 0.23 – 6.38) GtCO2eq yr-1 and BECCS removes on average about 2.75 (scenario spread of 0.52 – 9.45) GtCO2eq yr-1 from the atmosphere in 2050.

6. Line 26: if all tCDR removes 2 ±9 GtCO2/year and AR 0.2-0.4 GtCO2/year, then what tCDR option dominated tCDR?

   Response: Thanks for the hint. We now refer to numbers from the latest State of CDR report indicating that almost all current tCDR is due to AR and forest management: See Smith et al. 2014, p. 124: Estimates of the volumes removed vary according to the approach used. From 2013 to 2022, models aligned with estimates of the Global Carbon Budget suggest an average of −1,860 (−1,160 to −2,230) MtCO2 per year from afforestation/reforestation. Country-level data on managed forests, adjusted using DGVMs, suggest −2,010 ± 620 MtCO2 per year over the same period, through both afforestation/reforestation and forest management. Both approaches agree on a slight slowdown in the rate of conventional CDR in recent years.

7. Line 34: please add citation

   Response: This refers specifically to Roe et al. (2019), which we have added to the sentence.

8. Line 45: please provide a simple reasoning for focusing on herbaceous biomass plantations (HBPs). Also, does the model consider only Miscanthus or other species as well?

   Response: HBPs such as Miscanthus are typically used in IAMs to represent second generation bioenergy crops (Daioglou et al. 2019; 10.1016/j.gloenvcha.2018.11.012). Our HBP re-implementation only represents Miscanthus. This differs from the original HBP implementation by Mayer (2017) into a previous version of JSBACH representing both Miscanthus and Switchgrass. While both plants don’t differ too much in terms of their phenology and harvest timing, plant physiological parameters from observations (e.g. Vcmax, specific leaf area) do differ (e.g. see Li et al. 2018, Fig. 1). Also, for HBP implementations in other DGVMs Miscanthus or Switchgrass are typically represented as separate PFTs (see Li et al. 2018, Littleton et al. 2020 or Cheng et al. 2020). Therefore, to facilitate comparisons and evaluations across DGVMs, we decided to base our parameter revisions solely on Miscanthus (see Nützel 2024).

We don’t represent woody biomass plantations (WBPs), since this would have required a fully different PFT (e.g. with different plant physiological parameters but also a different harvesting scheme). Since in both DGVMs and IAMs having HBPs is more common than having WBPs, we prioritized the model development of HBPs. We acknowledge though that both having other HBP species than Miscanthus or representing BECCS through WBPs could affect our results, but argue that already the comparison of one HBP species representative for BECCS with forestation is of scientific relevance. Further, our focus is on introducing the measures of efficiency exemplary for AR and BECCS rather than an exact estimation of their CDR potential.

9. Line 68-70: if you mention life cycle assessments (LCA), please provide a more detailed and clear description of it and add some discussion on LCA for this study in the Results. Please also try to either reword or split the sentence in two to improve its readability.

   Response: We have added a definition of life cycle assessments (LCA) and have adapted the sentences as follows:
   l. 69: However, in practice, biomass production losses and energy conversion reduce the FFS potential of biomass (Chum et al., 2011; Babin et al., 2021), e.g. due to transport emissions and indirect land-use change displacing the prior land use to other regions. Such and other emissions along the process chain can be captured by LCA. The goal of LCA is to quantify the environmental effects, including those of energy use, resource depletion, and emissions, across the entire life cycle of a product or service.

   Performing a comprehensive LCA is beyond the aim of our study and thus we include emissions from production losses implicitly in the FFS factor. We added to the discussion:

   l. 413: In addition, the FFS potential of bioenergy reduces through production and transport losses along the process chain (Babin et al., 2021). We do not perform an LCA in our study but rather include additional emissions through production losses implicitly in the FFS fraction.

10. Lines 80-81, I found it difficult to understand what scientific gap it is filling exactly, I think this has been done in (Melnikova et al., 2023).

    Response: Thank you for pointing this out. Indeed, to our knowledge, Melnikova et al. (2023) is the only other paper comparing both tCDR methods in the same area. Because the paper was just published recently, we missed that. We have adapted the text accordingly.
    l. 82: However, a direct comparison of the AR and HBPs carbon sequestration potential in the same areas was only conducted by Melnikova et al. (2023) within a consistent setup.

    That also supports general comment 1 as a similar method for land occupation was chosen. However, comparing both methods in the same area is not the main aim and research gap that we are filling with this study. It is only a prerequisite for applying the different measures we are introducing. We have made this clearer in the introduction.
    l. 95: This is to our knowledge the first study that compares BECCS to AR in the same location within a consistent setup to evaluate different measures of efficiency.

11. Line 88: To the best of my knowledge, I think this is the first study to account for FFS together with CCS by the land model. I commend authors for bringing this and encourage them to highlight this novel aspect more. I also suggest to add a more detailed description of CCS and FFS

    Response: In addition to 10., we have highlighted the simultaneous use of CCS and FFS more strongly to underline the novelty of the study. We have also added a more detailed description of CCS and FFS to the introduction.
    l. 97: In addition, this is the first study to account for FFS together with CCS by a land surface model.
    l. 27: Thereby, the CO2 that is emitted upon combustion of biomass can be captured in geological reservoirs for thousands of years.
    l. 67: . In addition to CCS, bioenergy crops are typically used for energy production, which enables fossil fuel substitution (FFS) meaning that the energy production from fossil fuels is replaced by bioenergy.

12. Line 121 “can be found in Nützel (2024)”: could not track the publication, the reference (lines 62-63) also does not provide much information on journal name or publisher.

    Response: Nützel (2024) refers to a Zenodo database. We apologize that it didn’t show up properly in the reference list. We have added the DOI to link to the database.

13. Line 188-149: as stated in my major comment, however integrated assessment model (IAM) allocates bioenergy crops to the grids more suitable for crops and forest – to the grids more suitable for the forest.

    Response: As mentioned above, the focus of our study is to compare the two methods by introducing the different measures of efficiency, rather than to evaluate realistic CDR potentials. Further, the suitability of tCDR methods in IAMs refers to socio-economic reasons rather than climate-related ones.

14. Section 2.4: would it make more sense to either put this section to supplementary together with Figs. 2 and 3, or make a more thorough analysis by dividing all the sites to three categories: best match, large underestimation, large overestimation sites (in Fig 3a)? Is it related to irrigation or fertilization? Does the mismatch have any implications on this study’s conclusions?

    Response: As discussed, the mismatch is partly due to the different spatial resolutions of models and observations. We find that the mismatch is not related to fertilization or irrigation (see Fig. 1; see also Li et al. 2018, Fig. S6 / S7 and Littleton et al. 2020, who compared their HBP implementations against the same observational dataset). Note that in Fig. 1, we show a comparison of single sites rather than averaging over a grid cell as in the paper. Thus, several observations that are located in the same grid cell correspond to a similar modeled yield. We conclude that the global mean is reproduced fairly well, whereas regional and small-scale differences are biased.
    We decided to keep the model evaluation in the main text because we provide the first peer-reviewed paper that shows the model implementation of HBPs in JSBACH3.2.

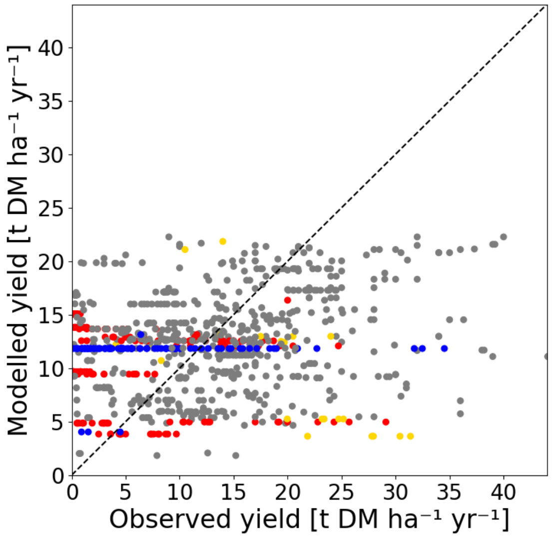

Fig. 1: Modeled and observed yields. Observed yields with fertilization and irrigation are highlighted in yellow, fertilization but no irrigation in red, no fertilization but irrigation in blue and neither in gray.

15. Lines 273-275: FFS and CCS is decided by IAM.

    Response: We assume that this comment points to the use of scenarios with no FFS/CCS or 100% FFS/CCS. We acknowledge that FFS and CCS values are provided by IAMs, which we use as a reference. Due to the high uncertainty of FFS and CCS in the future, we would like to provide an uncertainty range accounting for the full possibility spectrum.

16. Line 281: "1.29 GtC" – is this a cumulative value? If yes, then for what period?

    Response: Yes, this is a cumulative value from 2015 to 2100. We have changed the text accordingly (l. 291).

17. Figure 6 caption: also, for what period? Would it make sense to show 2090-2100 uptake and not all period for more just comparison (see my major comment 5)?

    Response: Figure 6 also refers to the year 2100 and thus includes all the carbon uptake until 2100 (with BECCS/AR implementation starting in 2015). As you pointed out correctly, BECCS are more effective in the long term, and thus only showing a ten-year difference would give a systematic advantage to AR. Thus, it would not make sense to only show the difference of the period 2090-2100.

18. Discussion around Fig. 7. I miss explanation on the potential causes of spatial difference. Is it because of the climate factors, soil conditions or allocation timing (see my major comment 5)? Are the tCDR options applied homogenously in all regions over time?

    Response: Fig. 8c shows that the tCDR options are not applied homogeneously over time, but the onset (and thus temporal evolution) differs spatially. Thus, climate and soil conditions as well as allocation timing most likely play a role. We have related Fig. 7 to Fig. 8c by indicating regions where the (late) onset is most likely the reason for a high level of FFS and where climate and soil conditions seem more essential. For example, even for a late onset of tCDR, there are regions (Eastern USA, Sahel) where a low level of FFS is needed for HBPs to

have the same carbon uptake as AR. Thus, we can infer that climate and soil conditions are more favorable for HBPs. However, to differentiate the impact of climate and soil conditions is beyond the scope of our study since a different simulation setup would be needed to separate both effects.
l. 343: In this specific SSP1-2.6 land use and climate scenario, the time until HBPs become more efficient than AR depends very much on the regional climate and soil conditions. For example, although the tCDR onset in Southeast China and the Sahel appears after 2060, the time until HBPs become more efficient is much shorter in the Sahel compared to Southeast China due to a drying trend in the former and a wetting trend in the later region (Fig. A1).

19. Section 3.4 (lines 333-341) is difficult to comprehend. Is this section needed? E.g., terms “positive / negative areas” are counterintuitive. Also, by “additional areas” mean additional are in the same grid as in the land-use scenario or any grids within the region?

    Response: ‘Additional area’ refers to the specific grid cell where the value is given. The area that is needed is an important measure of efficiency of a tCDR method because land use conflicts with e.g., agriculture and nature conservation emerge from tCDR implementation. Thus, it is essential for decision makers to know how much space is needed to fulfill a certain carbon sequestration goal. This is why this section is important. We have emphasized this point in the section. We have replaced the term ‘positive/negative areas’ by explaining the respective advantages of HBPs/AR.
    l.356: The area needed to reach a specific carbon removal target is an important measure of a tCDR method because land use conflicts with e.g. agriculture and nature conservation emerge from tCDR implementation. Thus, it is important for decision makers to know how much space is needed to fulfill a certain carbon sequestration target.

20. Section 4.1 is very important for this study. So, I recommend a more constructive comparison around Table 2 (especially lines 363 – 375) and text revision to make it a bit easier to follow for the reader, if possible.

    Response: We have revised the text to be more comprehensive in this section (l.384-406).

21. Line 410: “first-generation” comes out of blue, the study was on the second-generation biocrops.

    Response: Indeed this study refers to second-generation bioenergy plants. Thus, we have highlighted the advantages of second-generation bioenergy plants over first-generation bioenergy plants here (e.g. less water and fertilizer use).
    l. 456: Replacing first-generation bioenergy fuels, for example, by Miscanthus might save half of the land and one-third of the water use (Zhuang et al., 2013) and may increase soil organic carbon content (Longato et al., 2019; Melnikova et al., 2022).

22. Line 414: and societal acceptance?

    Response: We have added that monocultures and forest plantations are the substance of controversial societal debates.
    l. 450: Furthermore, the extensive cultivation of bioenergy plants, wood plantations,

and forest monocultures may harm biodiversity (Veldman et al., 2015; Hanssen et al., 2022; Searchinger et al., 2022) and have been subjected to societal debates over decades (Jönsson, 2024).

23. Line 419: please clarify “non-CO2 effects”, it may be misleading (e.g., non-CO2 greenhouse gasses)

    Response: We agree that the term ‘non-CO2 effects’ could be misleading. Here, we refer to biogeophysical effects. We have changed this accordingly.

24. Line 420: how about leakage risks of stored carbon?

    Response: We have added a review on the carbon leakage of CCS highlighting that studies on the permanence of CCS are still inconclusive.
    l. 470: However, studies on carbon leakage of CCS are still inconclusive, mainly because they rely on laboratory experiments that are not comparable with the field observations (Gholami et al., 2021).

25. Line 422: “the risk of disturbances” is also relevant for biocrops

    Response: We agree and have added to the text:
    l. 473: In addition, the risk of disturbances from fires, wind throw, droughts, and parasites increases with climate change and might limit the permanent storage of CO2 in trees but also in biocrops (Seidl et al., 2017; Anderegg et al., 2020, 2022).

26. Line 435: but wouldn't the carbon sequestration increase due to younger individuals?

    Response: Yes indeed, due to forest regrowth the carbon stored in the forest would increase. However, establishing a new forest will take some time during which carbon sequestration in the forest is lower (see also answer to Wei Li).

27. Line 454: what do you mean by “sensible balancing of interests”?

    Response: Sensible balance of interests refers to a fair negotiation of different land use interests, when conflicts between climate mitigation, food production and nature conservation arise by extending tCDR. We have added this as an explanation as follows:
    l. 516: … as a base for a sensible balancing of land use interests concerning climate mitigation, food production, and nature conservation.

28. Table 2. Melnikova et al. used the same area for biocrops and forest (if I am not mistaken 700 Mha). Also aren’t study periods different in different studies?

    Response: In Fig. S24 of Melnikova et al. (2023) we find a global cropland extension of BECCS between 500 Mha (REMIND-MAgPIE) and 550 Mha (IPSL) from 2040 to 2100. Aren’t those the correct values? Indeed the periods (and also the temporal evolution) differ between the models. We have added this to the caption of Table 2. From Fig. S21 we estimate an extension of around 500Mha for BECCS and 600Mha for forest since 2040, so the area seems to differ for both methods. Please correct, if this interpretation is wrong or if you can provide more accurate numbers.

29. Lines 629 and 632: duplicate citations? Line 635 missing title and some info?

    Response: Thank you for that comment, we have removed the discussion paper and referred to the final version of the published paper.

30. Lines 681 and 696: please add info on the publisher.

    Response: The recommended citation of this article is:
    Smith, S. M., Geden, O., Nemet, G., Gidden, M., Lamb, W. F., Powis, C., Bellamy, R., Callaghan, M., Cowie, A., Cox, E., Fuss, S., Gasser, T., Grassi, G., Greene, J., Lück, S., Mohan, A., Müller-Hansen, F., Peters, G., Pratama, Y., Repke, T., Riahi, K., Schenuit, F., Steinhauser, J., Strefler, J., Valenzuela, J. M., and Minx, J. C. (2023). The State of Carbon Dioxide Removal - 1st Edition. The State of Carbon Dioxide Removal. doi:10.17605/OSF.IO/W3B4Z
    We have added the DOI but we didn't find information on the publisher since this is a technical report.

31. Please consider making the code and data publicly available. I did not find any statement of the data and code availability.

    Response: We have made the code and data publicly available on Zenodo. We didn't do it earlier due to unknown changes in the manuscript during the review process. Preprocessing and postprocessing scripts are available at https://zenodo.org/uploads/13355458.