Dear Dr. Hisashi Sato,

Thank you very much for the time and effort you have dedicated to provide valuable feedback on our manuscript. We found your comments extremely helpful. We will incorporate changes to address your main concerns and hope you will find our replies to your comments on the manuscript satisfactory.

Our replies to the comments are highlighted in bold.

Best Regards,
Dushyant Kumar and co-authors

General comments
The authors modified a dynamic-vegetation-model aDGVM then applied it to the South-Asia. After evaluating the simulation results under the historical climatic conditions, the modified model was forced by predicted climates and CO2 trends, predicting major changes in geographical distribution of vegetation occurs by the end of the 21st century. A sensitivity test (i.e. comparing simulation results of four combinations of two CO2 scenarios and two climate change scenarios), authors concluded that South Asia will likely to function as carbon sink during the 21st century due to the CO2 fertilization effect. I evaluate that the manuscript is within the scope of the journal and it meets a basic scientific quality, however, authors need to address following items before publication.
Reply: Thank you for the positive feedback.

Major concerns
In the modification of aDGVM, a well-known functional relationships of leaves were introduced: SLA (specific leaf area) negatively correlates with Na (leaf nitrogen content per unit area), and Na positively correlates with Vcmax (maximum carbo-hydroxylation rate of Rubisco per unit leaf area). I should note that there is also a negative and strong correlation of SLA with leaf longevity (Wright et al. 2004), and actually, this correlation is much more intense than for the correlation of SLA with Na. Discounting the negative correlation between SLA with leaf longevity in the current model should favor higher SLA than for actual circumstances in nature. Author have to discuss how this discounting can skew the simulation results at least.
Wright, I. J., et al. (2004). "The worldwide leaf economics spectrum." Nature 428(6985): 821-827.

Reply: We appreciate that you highlight this important aspect of the SLA-N_a relationship. In aDGVM2, we are using the relation published by Sakschewski et al. (2015).
In aDGVM2, in addition to the SLA-N_a relationship, we have also implemented the A_{SLA} –LL trade-off in such a way that it affects the leaf turnover rates. Leaves with high A_{SLA} have higher turnover rates (i.e, shorter leaf longevity) than leaves with low A_{SLA}. We will add this detail in the revised manuscript. The A_{SLA} -LL trade-off implies that deciduous behavior is advantageous in dry regions because trees which do not invest much carbon into their leaves per unit dry mass (higher A_{SLA}) may shed them (lower LL) during the dry season without losing too much carbon. On the other hand, trees that invest more carbon into their leaves to enhance their structural stability (i.e., trees that make low-SLA leaves) have longer leaf turnover times and tend to emerge as

evergreen in aDGVM2, because being deciduous for them would be costly with respect to carbon use efficiency.

We will add these details to the text, together with the reference to the model description in the discussion part in section 4.1. We will also discuss the complete A_{SLA} -LL-N_a-V_{cmax} relationship and its impacts on vegetation state via trait trade-off. We will also include the A_{SLA} -LL relation in the model description.

Although the model was forced by various climatic variables, the manuscript only states influences of air temperature and precipitation. As authors themselves mentioned importance of VPD (vapor pressure deficit) on the transpiration rate in the manuscript (lines 406-409), other climatic variables controls the simulation. Accordingly, analysis and discussion how changes in other climatic variables influenced the results would be added. Besides, geographical distributions of all climatic variables, those are employed in the simulation, would be presented in the manuscript for both means of base-line- period and predicted trend during the 21st century.

Reply: Thank you for highlighting your concern on the significance of other climate variables controlling the vegetation dynamics. VPD is estimated in the model using relative humidity and saturated vapor pressure. Due to stomatal closure, photosynthetic rates under soil water stress conditions decline in aDGVM2 when atmospheric VPD increases.

Other than precipitation and temperature, aDGVM2 uses relative humidity, downwelling short and long wave radiation, and near-surface wind speed. We agree with the reviewer's idea of presenting both mean and predicted trend of these variables during the 21st century and will include a revised figure in the Supplementary Material (Figure S2).

We will add more detail and elaborate on the impact of other climate variables on simulated results in the revised manuscript to make this clear.

To quantify water use efficiency (WUE), authors scaled transpiration rate by leaf biomass (section 2.9). It's unusual. WUE is generally defined as carbon gain per unit water loss (i.e. photosynthesis rate per unit transpiration rate), because transpiration can be regarded as inevitable water lose during CO2 uptake through stomata for photosynthesis (Lambers et al., 1998). If authors use WUE of their definition, they need to clarify its underlining reason. Lambers, H., F. S. Chapin, and T. L. Pons (1998), Plant Physiological Ecology, Springer, New York.

Reply: Thank you for providing the reference. In this study, we have not used WUE and instead decided to present the change in transpiration at biome level. As both biome area and total amount of leaf biomass per biome are subject to change over time, changes in absolute transpiration quantity can result from either of the two changes, or from changes in water supply, or from changes in WUE. We therefore normalized transpiration to biome-level leaf biomass to eliminate effects caused by change in biome area and leaf biomass per biome. The normalization makes such differences in transpiration at biome level more comparable and independent of biome attributes such as area covered by a respective biome and biome-level biomass.

Choosing to normalize transpiration to leaf biomass integrates over both increased WUE combined with soil water availability constraints, and in our opinion in our specific case is therefore more suitable to characterize overall change in water balance over time at biome level, as it not only indicates water used to produce new biomass (as GPP over transpiration would express), but also includes water required to sustain existing biomass.

We will elaborate more precisely why we decided to normalize transpiration to unit biomass per area rather than choosing to go with WUE in section 2.9 of the revised manuscript.

Minor concerns
Line 15. "eCO2"
This term should not be used before its definition.
Reply: Thank you for pointing it out. We will make the correction in the revised manuscript and put the definition with the first use of the term.

Lines 70-72
I could not understand this sentence.

Reply: We will rephrase the sentence to make it clear in the revised manuscript.

Previous: "They were further limited when using contemporary environmental conditions to pre-define bioclimatic limits of plant functional types (PFTs), and when using fixed eco-physiological parameters, for example to model carbon allocation".

Rephrased: "**These studies were further limited by the fact that they all used fixed eco-physiological parameters and traits, e.g., fixed carbon allocation values to assign carbon to plant biomass pools, fixed SLA, as well as pre-defined bioclimatic limits that were derived from contemporary climatology in order to constrain the spatial distribution of plant functional types (PFTs) at global scale".**

Lines 74-75. "potentially disruptive effect of increasing CO2 on natural vegetation"
It's a misleading phrase. As authors repeatedly mentioned in this manuscript, higher atmospheric CO2 enhances photosynthesis rate and water-use-efficiency for C3 plants, although no major influences would be happed for C4 plants. Disruptive effects of higher CO2 can be expected only if we consider other factors such as lower leaf-cooling-effect due to lower transpiration rate.

Reply: Yes, we agree that the phrasing is misleading. We thank the reviewer for pointing it out and will rephrase the sentence in the revised manuscript.

Lines 81-82, "resulting from environmental filtering applied to traits of modeled plant individuals"
I could not understand this phrase.

Reply: Here we meant that in aDGVM2, the implemented novel process of selection and trait inheritance assembles plant communities that are well-adapted to given biotic and abiotic conditions. A detailed description about the implementation is given in Langan, 2019 and Scheiter et al. (2013).

We will rephrase the sentence to make this more clear in the revised manuscript.

Lines 156-164
Values for parameters should be presented.

Reply: In the revised manuscript, we will include the parameter values that define temperature limitation of carboxylation, as well as other parameter values used for calculation of leaf temperature.
We would like to point out that the air density in aDGVM2 is not constant and is derived from atmospheric pressure (101.325 kPa) at sea level by scaling according to air temperature and elevation following Burman et al. (1987).

Line 184-185
Due to the large inter-annual variability of precipitation, I cannot see apparent climate trends in the figure S1. Because predicted trends in precipitation considerably differ among regions of the South Asia (Figure S2b), it would be meaningless to discuss predicted trend of average precipitation over South Asia.

Reply: We agree that there is no clear overall trend in mean annual precipitation, so we will rephrase the text in the revised manuscript.

Line 185. "Western Ghats"
Line 262. "Brahmaputra basin"
Will you provide approximate longitude and latitude of this region?

Reply: The Western Ghats are located between latitude 73°- 77° E and longitude 8°N - 21°N, and the Brahmaputra basin ís located between latitude 28°N - 34°N and longitude 90°E - 96.5°E. We will include the coordinates in the revised manuscript.

Lines 191-198. Section 2.5
Geographical distributions of soil types and elevation would be presented. A table of soil properties of each soil type is also required. Also, please add explanation how elevation controls the simulation.

Reply: We have used the same soil data that were used in Langan et al. (2017) and the soil properties are given in the Appendix S2, Table1 in Langan et al. (2017) (https://onlinelibrary.wiley.com/action/downloadSupplement?doi=10.1111%2Fjbi.13018&file=jbi13018-sup-0002-AppendixS2.pdf). In the model elevation is used to calculate the atmospheric pressure at different altitudes. This is required to derive the respective partial pressures of O_2 and CO_2, and to scale air densities at different altitude. The partial pressure of oxygen is used in the photosynthesis routine to estimate the CO_2 compensation point (see more detailed explanation on the topic of how photosynthesis is implemented in aDGVM2 in our replies to the comments provided by anonymous reviewer 1).

We will add the soil type and elevation maps along with a table for soil properties in the supplementary material, and we will add a brief explanation in the methods section why elevation is required for aDGVM2 simulations.

Line 200. "four different scenarios"
Immediately after this term, an explanation would be needed that the four scenarios is the combination of two climate assumptions and two CO2 assumptions.

Reply: Thank you for pointing it out. We will rephrase the sentence and include the suggestion in the revised manuscript.

Lines 224-225. "simulated vegetation stands (1 hectare)"
It means that the simulation unit of the aDGVM is 1 hectare? But, all forcing data and validation data were converted to 0.5 degree, and all simulation results were presented at the 0.5 degree resolution. Please add explanation.

Reply: In aDGVM2, a "1 hectare plot" is assumed to be representative for a grid cell and results from these representative stands are scaled up according to the grid cell size in hectares, i.e., in the current study to a grid cell resolution of 0.5° × 0.5°. In our simulation we used 3600 different individuals per hectare, and photosynthesis and other physiological processes are calculated separately for each individual. Therefore, simulations are already computationally expensive and time-costly when conducting them for representative stands of 1ha. Even with modern technical means, simulating the number of individuals required to fill an entire 0.5° x 0.5° grid cell would exceed available computational power and/or require extremely long simulation durations that are simply not feasible. The "representative 1 ha approach" is therefore a pragmatic approach to balance adequate representation of trait diversity among individuals against technical constraints.

We will add this explanation in the revised manuscript.

Lines 230-231
Each tree can have multiple stem in the aDGVM? Need some explanation in the model description part of the supplemental material.

Reply: Thank you for the suggestion. In aDGVM2, the stem number of individual woody plants is a dynamic trait. This trait has been included by Gaillard et al., (2018) in order to allow simulation of shrubs vs. trees based on a functional trade-off between augmented access to soil water resources vs. height growth. It simulates shrubs as multi-stemmed woody plants whereas all woody individuals with a stem number between one and three are defined as trees. The classification of individuals into these two categories is done a posteriori, based on the model results. Stem numbers in a woody plant are emerging based on water availability, light availability and fire activity and are based on a trade-off between rapid height growth in single-stemmed trees and augmented efficiency of water uptake in multi-stemmed shrubs due to higher sapwood area per unit of woody biomass.

We will add this description in the model description section in the supplementary material.

Lines 231-233
I could not understand this sentence. Please rewrite.

Reply: Simulated woody plants in aDGVM2 can be trees if stem number is 3 or less, or shrubs if stem number is 4 or more (please see preceding explanation). We used the canopy cover of woody plants (including both trees and shrubs) to categorize the biomes. The canopy cover of woody plants in combination with grass biomass was used to separate woodland and savanna biomes. Grid cells with tree canopy cover greater than shrub canopy cover (i.e., a tree-dominated system) and a tree canopy cover between 5% and 45%, combined with a grass biomass below 100 kg/ha, were classified as woodland. However, a grid cell with the same characteristics was classified as savanna where grass biomass exceeded 100 kg/ha (Kumar et al., 2020).

We will add more details in the revised manuscript to make our point clear.

Line 271. "changes"
This word would be better to be replaced by "increasing trends".

Reply: Thank you for the suggestion, we will make the suggested change.

Line 290. "grassland"
It would be better to be replaced by "grasslands", which contain both C3 and C4 grasslands.

Reply: Thank you for the suggestion, we will make the suggested change in the revised manuscript.

Lines 335-336
Phrase "until 2090s" or "by 2090s" would be inserted somewhere in this sentence.

Reply: We will add the suggestion in the revised manuscript. Thank you.

Line 399. "resulted"
It would be better to be replaced by "coincided with".

Reply: We agree with the suggestion and will rephrase the sentence in the revised manuscript.

Tables S1 and S2
Units are missing.

Reply: We will add the missing units to the values in table S1 and S2 in the revised supplementary material.

Figure S5
No definition for the abbreviation “GRBM”.

Reply: It is an abbreviation for “grass biomass”. We will add the non-abbreviated term in the figure caption in the revised supplementary material.

Figure S3, S4, S6, and S8
For convenience of readers, captions of these figures might be better to be replaced by "Same as the Fig * except RCP8.5".

Reply: Thank you for the suggestion. We will address this concern in the revised supplementary material.

Typos
Line 116
Year is missing for "Gillard et al."

Reply: Thanks for pointing it out. We will add the year in the revised manuscript.

Line 129. "Spack"
It would be replace by "Spack". Unit for Spack should be also presented.

Reply: Thank you, we will add the suggestion in the revised manuscript.

Line 171 "C4 grasses" "C3 grasses"
Reply: Thank you, we will make the suggested correction in the revised manuscript.

Line 181. "Wm^2" "W/m^2"

Reply: We will make the suggested correction in the revised manuscript.

Line 314. "Ebiome"
It would be replace by "Ebiome".

Reply: We will correct the mistake in the revised manuscript.

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