August 13, 2021

Dear Editors:

Please consider this revised version of article ERL-111271, "Ecosystem carbon balance in the Hawaiian Islands under different scenarios of climate and land use change", for publication in *Environmental Research Letters*. We thank the two referees for their reviews of this manuscript, and have fully addressed each of their comments in detail below. Our point-by-point responses immediately follow each referee comment, and we include line numbers of the revised manuscript in each response to identify where we made changes to the text.

Long-term model projections such as ours will be critical to understanding how future land use and climate change may interact to influence the achievement of climate mitigation goals, and so we expect our paper to be of immediate interest to a wide audience of carbon cycle scientists, ecosystem modelers, land managers, and policy makers. We hope you find our revisions and responses to referees' comments sufficient to merit publication of this manuscript in *Environmental Research Letters*.

Best regards,

Paul C. Selmants

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Referee 1, comments to the author:

The manuscript describes an impressive effort projecting ecosystem carbon balance in the Hawaiian Islands under climate change and land use change scenarios. This assessment of future regional terrestrial carbon dynamics can provide a support to local planning of climate mitigation strategies through ecosystem carbon sequestration. The manuscript is well written and organized. However, more clarification and justification are needed for methodology and assumptions about how future NPP and land cover change is modelled. And also, more details are needed such as maps illustrating how projected change in carbon flux (e.g., NPP and NBP) and carbon pools (live biomass, soil organic soil) is spatially distributed. I have provided detailed comments below.

As suggested by this referee, we have added clarification and justification about how future NPP and land cover change were modeled, as well as new figures illustrating the spatial distribution of current and projected future carbon fluxes. Our responses to individual comments below indicate where we made these changes.

L109, why need to assign each cell a state type? What is consideration for it? The state of each cell tracks a range of information about that cell, including land cover class, moisture zone, island and time since transition. This set of information is used to define the probabilities that each cell can undergo a particular type of transition, as well as how often each of these transitions can occur. We have added text and references to the first paragraph in the Methods section that better defines the terms ‘state’ and ‘transition’ and how they relate to each other (lines 102-106).

L114, transitions between ‘state type’? Is it ‘land cover classes’? According to the definition of state type as unique combination of moisture zones, islands and land cover classes at L110, is there transition from dry forest and mesic forest in the same island?
We agree this was unclear. We have added text to the first paragraph of the Methods section that defines the terms ‘transition’ and ‘transition pathway’ (lines 104-106) and we have re-written this section to make clear that transition pathways define which land cover classes can be converted to other land cover classes (lines 122-129).

L123-126, high land use scenario samples historical rate between median and maximum, but why not low land use scenario sample rates between minimum and median? What is the justification that low land use scenario will have rate lower than historical records?
Sampling from this range in the low land use scenario shifts the balance of agricultural land area change toward agricultural contraction, reducing over time the total land area under active cultivation in the Hawaiian Islands. We consider this to be a reasonable possibility under a low land use scenario, especially given the volatility in agricultural land area change experienced in the Hawaiian Islands over the recent historical period (1992-2010). We have added text that explains why we bounded agricultural land area change rates between zero and the minimum recent historical rates in the low land use scenario (lines 135 - 138).

L126, missing description for urbanization rate for high land use scenario.
Transition rates for urbanization in the high land use scenario were sampled from a uniform distribution bounded by the median and maximum historical rate, as stated on lines 121-123 of the original manuscript. We have re-worded this description to make it more clear that it includes urbanization (lines 132 - 135).

L133, what transitions can increase forest area such as low land use scenario shown in figure S5? Does tree plantation refer to reforestation or afforestation? If afforestation, what is the source land cover class for this plantation? Forest area increases in the low land use scenario because of agricultural contraction (i.e., abandonment of previously cultivated land). We have added text to the supplementary explaining that the increase in forest, grassland, and shrubland area in the low land use scenario between 2020 and 2040 is primarily due to agricultural contraction over the same time period (supplementary lines 82-83; supplemental figure 7). Tree plantation does not refer to reforestation or afforestation. We did not include expansion of tree plantations as a transition pathway, and so there is no land cover class that can be converted to tree plantation in either land use scenario. Once harvested, tree plantations can either be replanted to tree plantations or be converted to grassland or agriculture. We have added text to the Methods section that better describes tree plantation harvest rates and transitions (lines 144-153). The “source land cover class” for tree plantations was abandoned sugar cane land as described in the Introduction section (lines 74-78).

L136, it will be helpful to add average rotation cycle corresponding to the 75% and 40% historical harvest rate? Like how soon planted trees will be harvested in low and high land use scenarios.
We have added text defining the historical harvest rotation rate as between 5-7 years, as well as more detail on how tree plantation area is projected to change under both land use scenarios (lines 144 - 153).

L137, will wildfire take place only in forests? If so, what is the resulting land cover class? Would it still recover back to forest after a certain time?
Wildland fire in Hawai'i primarily ignites in non-native grasslands and shrublands, but can sometimes spread into forests. We have added text and references that describe where wildland fire occurs in the Hawaiian Islands (lines 164 - 169).

L140, is state type land cover classes? Since 'state type' is defined as combination of moisture zone, island, and land cover type at line 110, how can 'state type' be re-combined with moisture zone and island here?
We agree this was a confusing use of terminology. We have altered the text here to read "… each unique combination of moisture zone (supplemental figure 1), island, and land cover class (figure 1) for the years 1999-2019." (line 158-159). We have also changed the text to read 'land cover class' rather than 'state type' elsewhere throughout the manuscript when referring only to the land cover classification of simulation cells.

L158, moisture zone is included in state type as defined at line L110, isn't it?
Yes, this was a confusing use of terminology. We have completely re-written this paragraph based on this and other comments from this referee (lines 180-191), such that we no longer uses the term 'state type' at all.

L162, how litter and dead wood pools were initialized?
We have added text describing how litter and dead wood pools were initialized using values from the IBIS DGVM (lines 180-181) and allowed to equilibrate with spatially variable NPP using a spinup model (lines 205-210). GeoTIFF raster files of initial carbon stocks are available at https://doi.org/10.5281/zenodo.5198072.

L161, what is 30-year climate normal come from? Does it have consistent temperature and rainfall distribution with RCP projections during 2010 and 2020? Or is there any bias between 30-year climate normal and RCP projections?
The contemporary 30-year climate normals for rainfall and temperature come from the references Giambelluca et al. 2013 and Giambelluca et al. 2014, as cited on line 183 and depicted in supplementary Figure 3. We have added text to the supplementary that better describes statistical downscaling of CMIP5 temperature and rainfall projections and how they compare to contemporary climate data (supplementary lines 47-53).

L163, figure 2 shows total live biomass in 2010 is about 68 Tg C, but total forest aboveground biomass is estimated as 36 Tg C in Asner et al 2016 which is used for initialization here. Where is the additional 32 Tg C from? What is ratio of aboveground biomass to total live biomass in IBIS?
Asner et al. (2016) only quantifies aboveground forest live biomass. The additional 32 Tg C comes from the sum of belowground forest live biomass and total live biomass in grasslands, shrublands,

and agriculture. We have added text and citations describing how we used a power function applied to the Asner et al. (2016) abovegrould forest live biomass values to estimate forest belowground live biomass and, subsequently, estimate total forest live biomass, which was used to calibrate the IBIS DGVM (lines 185-187).

L163, what is calibrated GPP NPP rate? It will be helpful to list out the mean or the range. We have added text (lines 189-190) listing the range of GPP values from the Kimball et al. (2017) calibration dataset and added a supplementary table of IBIS-derived mean NPP rates for each unique combination of moisture zone and land cover class (supplementary table 1).

L166-168, is similar multiplier also applied to living biomass? If not, will this single NPP multiplier break the equilibrium between NPP and biomass? For example, for a simulation cell, NPP and biomass should reach dynamic equilibrium in IBIS simulation, a given NPP could only support carbon turnover of a certain number of trees. External decease NPP by a multiplier will in turn reduce biomass in order to match it, causing artificial reduction in biomass.
No, there is no set of spatial multipliers applied to living biomass. We have added text and figures to the supplementary that better describes how we derived and applied the NPP multipliers (supplementary lines 55-68, supplemental figures 5 & 6). We have also added text to the manuscript describing how the LUCAS initialization of carbon stocks allowed for these stocks to reach a dynamic equilibrium with NPP across the landscape of the Hawaiian Islands (lines 205-210).

Line 173, why assume Rh as ratio of NPP? Is the ratio constant over time and across climate change scenarios? Why not to estimate Rh based on soil moisture, temperature and soil carbon stocks? Because of lack of soil temperature in RCP 4.5 and 8.5? Need more justification for this flux ratio method. We have added text and citations justifying the fact that plant productivity is the primary factor driving variation in ecosystem carbon loss at annual timescales, and that the vast majority of carbon lost from soils on an annual basis was fixed by photosynthesis within that year (lines 200-203). Although we agree that gridded annual maps of projected future soil moisture and soil temperature could potentially improve predictions of ecosystem carbon flux in our model, these data products do not exist for the Hawaiian Islands.

L177-179, are the growth and decay multipliers varied at annual basis? How are multipliers derived for time periods CMIP5 projections do not cover? Such as period between 2020-2049? It is very necessary to include the distribution of these multipliers, are they enhancing or weakening NPP over time at two RCPs? Yes, the growth and decay multipliers vary annually. We have added text, citations, and figures that better describes how growth and decay multipliers were derived for intermediate time periods and how they vary on an annual basis (lines 211-216, supplementary lines 55-68, supplemental figures 5 & 6).

L182, the empirical model used here to relate NPP to temperature and rainfall is core to projections of carbon balance. It is necessary to present this empirical relationship in the paper, such as what the optimal temperature of NPP is and how it changes with rainfall. For example, figure S4 shows projections of temperature and rain, there should be corresponding projections of NPP change.
We have added a supplemental figure displaying the spatial distribution of projected future NPP

multipliers based on future climate projections (supplemental Figure 6). We have also added text to the supplementary that better describes how we employ the empirical NPP equations to calculate spatial NPP anomalies for both contemporary and projected future NPP (supplementary lines 55-68).

L184, what is the temporal coverage and resolution of these climate projections? Do them only cover the period of mid-century (2049-2069) and end-of-century (2070-2099) as described in L179?
Yes, climate projections only cover the period of mid-century (2049-2069) and end-of-century (2070-2099) as described on line 179 of the original manuscript. We have added text to the supplementary Climate section that provides more detail on climate projections (supplementary lines 45-53).

L239: is the NPP decrease during 2010-2020 evident in satellite observations? To what extent of this decrease is caused by climate conditions change rather than by forest area changes?
Statewide NPP declined by approximately 2% on average between 2010 and 2020 across all scenarios and Monte Carlo iterations (8.1 Tg y^{-1} in 2010 to 7.95 Tg y^{-1} in 2020), and the ranges of statewide annual NPP estimates broadly overlapped for each of these years (2011 range = 7.96 - 8.28 Tg $^{y-1}$, 2020 range = 7.85 - 8.1 Tg $^{y-1}$). This slight decline in modeled NPP over 10 years, which is well within the error range of moderate resolution satellite observations, was due almost entirely to land cover change - which includes but is not limited to forest area change. We have added text that describes this change in NPP over the contemporary (2010-2020) time period to the Results section (lines 275-277).

L242. It is misleading to state 'climate change led to increased Rh over time' as figure 3 shows decreasing trend of Rh under high land use scenario and two RCP climate change scenarios. It should be revised as something like 'switching from RCP 4.5 to 8.5 led to increased Rh'. We agree, this phrasing was misleading. We have altered the text to clarify that R_h increased in the low land use scenario in the latter half of the 21st century under RCP 8.5 (lines 283-286).

L244, why the increase in total soil carbon stock (figure 2) will not enhance Rh? Is it due to the assumption made by this work that Rh is only ratio of NPP and latter decrease over time? What are drivers of Rh decrease?
R_h only declines consistently under the high land use scenario in both RCPs (figure 3). In the low land use scenario, R_h remains relatively stable after 2060 under RCP 4.5 and increases after 2020 under RCP 8.5 (figure 3). As stated previously, we have added text and citations justifying NPP as a direct driver of R_h because the vast majority of carbon respired heterotrophically from terrestrial ecosystems was fixed by photosynthesis that same year (lines 200-203). Also, soil carbon stocks do not increase consistently under all scenarios (figure 2). Under RCP 8.5, soil carbon stocks begin to asymptote in the latter half of the 21st century in the low land use scenario and begin to decline in the high land use scenario (figure 2). We have revised this section of text to better describe the drivers of R_h change over time (lines 283-289).

L244, why Rh shows very sharp decrease before 2020 under all land use scenarios with all RCPs, such decrease rate seems to be higher than the rest period. Are these decreases supported by observations?

Land use change rates are set to be the same in both land use scenarios during the contemporary period from 2010-2020 (lines 153-154; supplementary figure 7) and climate driven changes in R_h are still small during this initial time period. This explains why R_h behaves consistently across all four scenarios from 2010-2020 and diverges afterward. The decrease in R_h before 2020 is driven almost entirely by land use change, specifically the increase in urban area at the expense of natural and agricultural land (supplementary Figure 7).

Line 341, what is the source of 'Hawai'i-specific climate data'? The Hawaiʻi specific climate data are from Giambelluca et al. (2013) and Giambelluca (2014), which are cited several times throughout the manuscript and are depicted in supplementary figures 3. We have added these citations again here, as well as a reference to the supplemental figure (lines 383-384).

Referee 2, comments to the author:

The manuscript entitled "Ecosystem carbon balance in the Hawaiian Islands under different scenarios of future climate and land use change" is interesting and suitable for publication but several mistakes in the manuscript should be corrected before acceptance.

Specific comments

Authors should carefully recheck the manuscript for the typos and also write the meaning of all acronyms because there are some which are not presented.
We have carefully checked the manuscript for typographical errors and have changed "600ppm" to "600 ppm" on line 241 as well as corrected several misspellings of the word "supplemental" in the revised manuscript. We have also confirmed that all acronyms are spelled out upon initial use in the main body of the manuscript.

All the figures are clearly presented but authors must check all the figure pixels again. Some of them are not very clear.
We have re-made all the figures to increase their resolution to 400 dpi in response to this comment.

I suggest that authors check the bibliographic references in the main text and revise the format of documents to meet the requirement of the Journal. We are using the Institute of Physics (IOP) citation style language (csl), which meets the guidelines for this Journal. No changes were made in response to this comment.

In the abstract, please add more information about the methods of the experiment. We have added text to the abstract that provides more information about the methods of this project (lines 22-26).

In the abstract, please don't write the same conclusion lines as they were written in the main conclusion section.
We have altered the text so that the conclusions in the abstract are not written identically to those in the Conclusions section (lines 34-36).

Hypothesis should be given. How this work is different from the available literature? A hypothesis statement would be warranted if the purpose of this study was to test existing theory using a controlled experiment, but that was not the case. The purpose of this study was to use existing theory to make projections about the future, and so we believe a statement of objectives is sufficient (see lines 94-98). We describe in detail how this work compares to the available literature in section 4.2 “Comparison other studies” (lines 370-395), as recommended by the author guidelines for this Journal. No changes were made in response to this comment.

Objectives should be rewritten in detail and comprehensively. We have altered the text to add more detail to our stated objectives (lines 94-98).

How did the authors perform the calibration and validation process? We have added text describing the calibration of the model to the Methods section (lines 182-191). Validation of ecosystem carbon balance estimates in the Hawaiian Islands is problematic given the paucity of data (described in section 4.2 “Comparison to other studies”, lines 370-372), as well as the fact that we used much of the available statewide data to calibrate our model (see lines 182-191). However, we were able to compare our results to those from previous studies and provide details on factors driving differences in specific results (lines 372-395).

How did they select the sensitive parameters of the model that they used? We are unsure what the referee is referrring to in this question, as they do not point to a particular portion of the manuscript. We assume they are referring to our test of model sensitivity to different rates of a CO_2 fertilization effect (CFE). We provide information and citations about how we selected parameters for the CFE sensitivity test in the Methods section (lines 230-242). No changes were made in response to this comment.

Discussion, the discussion part is not related to the results part. Why you discuss these? What does it have to do with the result? We followed the author guidelines for this journal by discussing the significance of our results and comparing them with previous work using relevant references. We therefore disagree with this referee that our Discussion section is unrelated to the Results section. Throughout the discussion, we put our results in the context of emissions mitigation for the State of Hawaiʻi, discuss the relative impact of future global emissions reductions versus local land use decisions on statewide ecosystem carbon balance, compare our results to those of previous studies, highlight how our model could be improved, and describe the significance of our results. We hope the editor will agree that the text of our Discussion section provides context to our results and highlights their significance without simply reiterating what we wrote in the Results section. No changes were made in response to this comment.