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Discussions

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Response to RC1

Aaron J. Neill et al.

Author comment on "Structural changes to forests during regeneration affect water flux partitioning, water ages and hydrological connectivity: Insights from tracer-aided ecohydrological modelling" by Aaron J. Neill et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-158-AC1, 2021

HESS_2021_158: Author responses to RC-1

Summary

The authors present a modelling study analysing the effect of forest regeneration on blue and green water fluxes for a catchment in the Scottish Highlands, which have undergone dramatic decreases of native pinewoods since the 17th century. The authors use the tracer-aided ecohydrological model EcH2O-iso (Kuppel et al., 2018a) to model flux partitioning, water ages and hydrological connectivity under three different conditions (i.e., baseline conditions, thicket forest and old-open forest) representing different stages of natural forest regeneration.

The model results highlight that the thicket forest stage leads to the greatest changes in flux partitioning, water ages and hydrological connectivity especially during low flow, while establishment of old-open forest will likely result in the system returning to similar ecohydrological fluxes as during baseline conditions.

The authors argue that this study demonstrates the importance of considering different stages of regeneration as well as their spatial and temporal impact on ecohydrological partitioning to accurately inform landscape restoration.

Response to Summary: Thank you to the reviewer for taking the time to read our work and provide constructive comments to strengthen the manuscript.

General comments

RC-1.1: The study fits the scope of Hydrology and Earth System Sciences and represents an important contribution to investigating the effect of landscape restoration. The study uses existing concepts and methods, but applies them to different landscape scenarios than previous research. Hence, the paper represents a substantial contribution to scientific progress in this field. The paper is well-written and considers an appropriate amount of related work. The figures and tables are well chosen to support the results and conclusions of the study.

Response to RC-1.1: Thank you for recognising the significance and quality of our work.

I do not have major general comments, but I am missing some more in-depth discussion as to (1) the added value of the isotope module and (2) the likelihood of the two land-cover change scenarios under climate change.

RC-1.2: For (1), the authors refer to the validation by Kuppel et al. (2018a), but it would be useful to discuss in the paper to what extent the isotope data helped constrain model parameters and whether the model parameters sensitive to the isotope data are crucial for this study. In view of the uncertainty bounds in the behavioural solutions and to illustrate the value of the isotope data, the authors might want to include a baseline simulation without isotope data and compare the model uncertainties to those of the tracer-aided simulation.

Response to RC-1.2 & RC-1.16: We respond jointly to this comment and RC-1.16 given that they are related. Thank you for the suggestion of running a baseline simulation without isotope data; however, we would argue that this is not within the scope of this paper. We feel that the use of isotopes to constrain storage and mixing volumes in hydrological models to improve confidence in process realism is already well established. Indeed, previous work using simpler models in the Bruntland Burn catchment has shown this to be the case (e.g. Birkel et al. 2011). More recent work has also shown the value of isotopes in better constraining internal process representation and water partitioning in more complex process-based models, such as EcH$_2$O-iso (e.g. Holmes et al., 2020; Smith et al., 2021). In revision, we will make the established value of incorporating isotopes in modelling clearer in the Introduction (e.g. page 3 L84-85).

The width of the uncertainty bounds in this study are likely symptomatic of the wider and well-documented issue that models such as EcH$_2$O-iso have many degrees of freedom both in the form of free parameters and spatial patterns of simulated fluxes. Recent work has shown that whilst isotopes can improve simulation of general catchment functioning, they may not be sufficient to fully constrain the detail of all individual processes operating within a system, leading to a persistence of uncertainty in model outputs (e.g. Holmes et al. 2020). This can be further compounded by scaling issues (e.g. Smith et al., 2021) and the extent to which isotopes can constrain GW fluxes >5yrs (e.g. Stewart et al., 2010). The skill of EcH$_2$O-iso at simulating a range of ecohydrological and isotope datasets in this work increases confidence in its ability to provide plausible realisations of catchment functioning to the extent we can say something useful regarding important issues such as landscape restoration. However, it may also be the case that these realisations could be refined further with additional data. What sort of data would have most value in this regard is an open research question and one that that the authors intend to contribute to in future work; however, we again feel that any significant discussion of this is beyond the scope of this paper. In revision, we will try to communicate this sentiment more explicitly and briefly comment on some of these wider issues in the context of needs for future modelling studies of land-cover change (e.g. Section 5.1 P. 26-27 L. 477-483; Section 6).

RC-1.3: Related to this is also the discussion of changes in water ages with progressing regeneration (section 5.2), which should underline more why this information is highly beneficial for assessing regeneration changes as opposed to looking at the changes in blue and green fluxes only (and thus why we need the isotope data).

Response to RC-1.3: The age of fluxes and their associated storages, along with how these change in response to regeneration, can enhance our understanding of the spatial and temporal aspects of catchment storage-flux interactions and their sensitivity to change. Thus, this is a key advantage of using tracer-aided ecohydrological models and we will highlight this point more clearly on revision.

RC-1.4: Regarding (2), given that the full regeneration to old-open forest might take several decades, I am wondering whether changing climate might lead to a different trajectory of change than the one depicted in the study. More specifically, how realistic is it that the system can meet increased evaporative demand during summer (e.g., Werritty and Sugden, 2013)? Would it be possible to test this for the study catchment with the EcH2O-iso model (see page 30, lines 595–599)?

Response to RC-1.4 & RC-1.15: We respond jointly to this comment and RC-1.15 given that they are related. Thank you for this comment; we agree that the discussion surrounding the likelihood of the simulated scenarios could be improved. It is difficult to say how climate change may affect the development of the old forest given the large degree of uncertainty in how rainfall distributions will change and, consequently, interact with increased evaporative demands in summer. Exploring this question would be possible with EcH_2O-iso if vegetation growth dynamics were switched on; however, in this application they were switched off so as to minimise the number of processes requiring constraint when only simulating "snapshots" of regeneration. To address this comment in revision, we will add a short final section to the Discussion to cover scenario uncertainty. Along with the current final paragraph of Section 5.1, this will include a brief discussion regarding how details of the old forest structure may be uncertain due to climate change and/or different possible trajectories of regeneration.

RC-1.5: I also have a comment on the data availability. According to the HESS data policy, "data and other information underpinning the research findings are "findable, accessible, interoperable, and reusable" (FAIR) not only for humans but also for machines". If the data cannot be made publicly available, there should be "a detailed explanation of why this is the case". Please provide the data in an open repository or explain why this would not be possible.

Response to RC-1.5: Data will be made publicly available via an institutional repository in revision.

Specific comments

RC-1.6: Page 6, line 150: do you mean that there is an exponential decrease of roots in each layer with depth? Please clarify.

Response to RC-1.6: This will be changed to "The fraction of roots in each layer is determined by an exponential function describing how root fraction decreases with depth."

RC-1.7: Page 6, line 160: could you briefly comment on the impact of this assumption of complete mixing? With a total soil depth of around 30 m in some simulations, how does this assumption affect the water age simulation? I could imagine that the L3 soil layer might contain a relevant proportion of older water, which might bias the water age of transpiration towards older ages using the complete mixing assumption.

Response to RC-1.7: Given the wet, low energy environment of the BB, the complete mixing assumption is likely a reasonable approximation under most conditions given the model grid size and daily time steps. Previous empirical work in the catchment has shown limited evidence of ecohydrological separation beyond minor evaporative enrichment of increasingly mobile waters in the upper soil (e.g. Geris et al., 2015); consequently, the main limitation of the complete mixing assumption is likely to be over-enrichment of water in L1 which may be translated throughout the soil profile (Kuppel et al., 2018) rather than adverse effects on transpiration ages. The effect of L3 on the latter will further be mediated to some extent by the more limited presence of roots in this layer (most vegetation types have roots within 20-50 cm of the surface, most of which will often fall in L1 and L2).

RC-1.8: Page 6, line 168: "soil types were assumed to be spatially uniform". I am not sure I understand. Do you mean there is only one soil type per cell (as in Fig. 1a) or what exactly is spatially uniform? Also it is not clear to me how to read Table 1: should the percentages across all vegetation types (including bare soil) for each soil type add up to 100%? Could you explain this in a bit more detail in this paragraph?

Response to RC-1.8: By this we meant that the properties of each individual soil type are uniform in space. We will update to: "The properties of each soil type were assumed to be spatially uniform". It is correct that % cover of all vegetation types including bare soil should sum to 100% for a given soil type. To improve clarity, Table 1 will be reorganised by soil type rather than vegetation type, and it will be noted that vegetation fractions sum to 100% on Page 6 L176.

RC-1.9: Page 9, line 214: how many simulations meet the criterion of simulated saturation areas < 60%? Why are only 30 runs of those retained as behavioural results? This is probably a small proportion of the first subset, but it still gives large uncertainty bounds, for example, in flux ages.

Response to RC-1.9: Approximately 11,000 simulations met the criteria of saturation area <60%. However, many of these simulations will have performed poorly with respect to the other calibration targets used as part of the multi-criteria approach. We retained 30 behavioural runs to strike a balance between the need to illustrate uncertainty in model outputs and the increased computational demand of running the model when producing spatial outputs required for the change analysis but not calibration. We will provide this justification on Page 9 L 214. Also see Response to RC1.2 & 1.16 for comments on the width of uncertainty bounds.

RC-1.10: Page 12, lines 282–283: I do not fully understand. What kind of threshold and what is the role of reinfiltration along a flow path? Please clarify.

Response to RC-1.10: In some connectivity analysis (e.g. Turnbull and Wainwright, 2019) a threshold of simulated overland flow is used to determine if a cell would be measurably connected in reality. In EcH$_2$O-iso, overland flow is accumulated along a given flow path *but with the potential for losses to re-infiltration, potentially preventing connectivity of cells to the stream. Because of this, we did not opt to impose a potentially arbitrary threshold of overland flow for a cell to be considered connected and instead inferred connectivity directly from simulated spatial patterns of overland flow. In revision, we will clarify this on Page 12 L280 with: "In EcH$_2$O-iso, losses to re-infiltration along a given flow path can prevent upslope cells producing OLF from connecting to the stream (Maneta and Silverman, 2013). Consequently, connectivity was inferred directly from simulated spatial patterns of OLF without imposing a minimum threshold of OLF below which a cell would be considered disconnected (c.f. Turnbull and Wainwright, 2019)."*

RC-1.11: Page 13, lines 290–291: could you also state the values of the performance metrics for behavioural runs?

Response to RC-1.11: The values for performance metrics are currently given in Table 2. Given the number of calibration targets, we would not be keen on moving these values into the text to ensure that readability is maintained.

RC-1.12: Page 27, lines 507–510: "Greater consistency…". I am not sure I understand. Do you mean that regeneration does not affect the fluxes during larger events because of sufficient amount of rainfall and stored pre-event water during these events?

Response to RC-1.12: This is correct. The relevant sentence will be changed to "The lesser impact of thicket forest on the simulated magnitude of high flows suggests that increases in storage capacities (Fig. 4) and "green" water fluxes (Table 3) were insufficient to moderate the combined influences of antecedent conditions and precipitation inputs that led to the largest events modelled here (Fig. 5b)."

RC-1.13: Page 28, lines 517–520: So, would that mean that the old forest state might be achieved much later or maybe not at all?

Response to RC-1.13: In this case, the old forest state would still be reached, however the configuration may be different to the one simulated here. In particular, whilst old forest would still be present on the hillslopes, it could be that drying out of the valley bottom results in the presence of a younger regenerating forest rather than persistence of bog woodland/vegetation. This will be clarified in the new final Discussion section highlighted in Response to RC-1.4.

RC-1.14: Page 29, lines 554–555: I do not see big differences in the connectivity changes between low / moderate summer events and the large winter event. Could you support this assertion by mentioning percentage changes in section 4.7?

Response to RC-1.14: Thank you for this comment. On reflection, our choice of events presented for the connectivity analysis did not best support our assertions regarding the effect of regeneration on connectivity as the August 2014 event was actually quite large with relatively wet antecedent conditions. In re-evaluating this part of the analysis, we have found we can add greater strength and nuance to our assertions regarding the effects of regeneration. This will result in the following changes:

- *Section 4.3: Highlight that, proportionally (as revealed by plotting ln(Q) in Fig 5b), decreases are greatest for low to moderate flows in late summer and during autumn/winter rewetting, whilst large winter events can somewhat "reset" the catchment towards baseline conditions, resulting in more limited divergence of subsequent winter/spring flows.*
- *Figure 9: Include a small/moderate event in late summer or during autumn/winter rewetting; with the drought event, this will help strengthen the point that decreases in connectivity are greater under these conditions rather than larger events in summer and, particularly, winter. In response to Reviewer 2, the figure will also be condensed by removing the histograms from this plot and instead plotting the spatial distribution of cells connected in at least 50% of behavioural simulations, coloured by their flow path lengths.*
- *Section 4.7: Update to accommodate new figure and include % changes in cell connectivity.*
- *Section 5.1, Paragraph 3: Refine text to indicate that storage deficits from reduced GW recharge and increased summer transpiration not only affect late summer baseflows but also delay rewetting and affect the magnitude of small/moderate events at this time.*
- *Section 5.3: More nuanced discussion of connectivity changes for low/moderate summer and rewetting events vs. larger events in summer and winter.*

RC-1.15: Section 5.3: see general comment (2): I would appreciate some words on the likelihood that regeneration would undergo these two forest stages in view of climate change. Could it be that less rainfall/higher ET in summer would lead to a diversion of pinewood regeneration as depicted in Fig. S1 such that increased transpiration demand of thicket forest could not be met and transition to old forest would not occur? This links to the statement on exploring trajectories of change made in the Conclusions.

Response to RC-1.15: Please see response above to RC-1.4.

RC-1.16: Page 30, line 590: see general comment (1): I am not sure about the benefits of the isotope observations here. Do we need the isotope module of the model or what additional validation data might be useful to constrain the uncertainty bounds? Could the authors comment on the uncertainty that would result from calibration without isotope data? Is it the comparably low temporal and spatial resolution of soil-water isotope data that limits the uncertainty reduction?

Response to RC-1.16: Please see response above to RC-1.2.

Technical corrections

RC-1.17: Page 5, line 142: gridded

Response to RC-1.17: This will be corrected.

Tables

RC-1.18: Table 1: how did the authors determine the exact proportional aerial coverage in the two scenarios? References are given here but it is not clear to me whether/how these numbers have been derived from the information provided in the references

Response to RC-1.18: Field descriptions from Steven and Carlisle (1959) was used to determine the understory composition of bog woodland. The sum of understory vegetation % cover in Table 1 of Parlane et al. (2006) was used to determine the fraction of heather under thicket and old-open woodland. Bog pine % cover was based on figures for uncut bog in McHaffie et al. (2002; p.214) and plan drawings from Summers (2018). Percentage covers of thicket and old-open pine were derived from plan drawings of relevant stand canopies in Figure 3 of Summers et al. (1997). These details will be briefly added to the notes of Table 1.

Figures

RC-1.19 Figure 1a: would it make sense to have more meaningful symbols and/or colours for the monitoring sites, grouping weather stations, soil types and vegetation?

Response to RC-1.19: We will update the symbols/colours of Figure 1a to better group monitoring sites.

RC-1.20: Figure 1: could you also include a digital elevation model, so it is easier to see the location of the hillslopes in the catchment?

Response to RC-1.20: We will add contour lines to Figure 1a to improve visibility of the hillslopes.

RC-1.21: Figure 9: could you also show the dates of the different snap shots directly in the figure? If not, the reader has to switch back and forth between the figure panels and the figure caption.

Response to RC-1.21: Event dates will be added to Figure 9.

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New references not currently in manuscript

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