Dear Dr. Stevens:

The reviewers have completed their evaluation of your paper and Dr. Thomas Kitzberger, the subject-matter editor, has recommended acceptance pending minor revision. I have reviewed the recommendation and agree with Dr. Kitzberger. A revision that addresses the substantive concerns of the subject-matter editor and the reviewers will be re-evaluated by the editor before I make a final decision.

The comments from the reviewers and the subject-matter editor are attached here. You may also view the information in your AUTHOR CENTER of the ECOSYSTEMS MANUSCRIPT CENTRAL site (https://mc.manuscriptcentral.com/eco).

Please highlight the major edits in your revised manuscript text. This makes it easier for reviewers and editors to view your changes within the text.

When you are ready to submit your revision, return to your AUTHOR CENTER and click the Manuscripts with Decision category in the left-hand list. Then under Actions (on right), click Create a Revision. Follow the onscreen directions to respond to Subject-Matter Editor and Reviewer comments. In the text boxes provided, enter (type or copy and paste) your cover letter explaining, point-by-point, the changes (and text location) you have made in response to each reviewer or editor comment. Remember to save a copy of the letter for your records. Next, complete manuscript details, and upload revised files. Remember that after you have viewed your PDF and HTML proofs you MUST click SUBMIT in the lower right corner to complete the submission.

Your revision is due within 6 weeks of receipt of this letter. Revised materials received more than 3 months from the date of this letter will be regarded as a new manuscript.

Thank you for submitting your work to Ecosystems.

Sincerely,

Monica Turner

Co-Editor-in-Chief

SUBJECT-MATTER EDITOR'S COMMENTS

Subject-Matter Editor: Kitzberger, Thomas

Comments to the Author:

Authors have addressed the major concerns of both reviewers and myself. Accordingly, the manuscript has substantially improved in focus. There are a few minor (but nonetheless important) comments that need to be incorporated before final acceptance.

REVIEWERS' COMMENTS

Reviewer: 1

Comments to the Author(s)

Thank you to the authors for their efforts to improve the paper, which covers a timely and important topic in the area of forest restoration. I found the discussion of multiple lines of evidence coupled with areas of uncertainty to be robust and provide the community with a rich set of future lines of inquiry.

Minor comment:

Lines 132-133: It is not clear what period is referred to here.

Reviewer: 2

Comments to the Author(s)

The authors have submitted a revised manuscript that describes soil moisture dynamics in response to the long-term use of fire (allowed wildland fires) and compare a vegetative productive site to a drier less productive site. The contrast of soil moisture response is now more evident than before. To be sure, the initial paper was originally pretty good, and has since improved even more in the second version.

My initial criticism can be summed up as, 1) the soil moisture modeling approach to use a statistical model which relies on correlation rather than physical processes was not very robust and is therefore questionable to use to infer conditions not measured, i.e, pre-fire conditions. 2) A general lack of mechanistic connection between soil moisture observations and physical changes in vegetation. And 3) more focused comparison between the productive site and drier site. The authors have addressed each comment, particularly comment 3, and made small changes in response to comment 1 and 2. With regard to comments 1 and 2, I was looking for the authors to admit that while their statistical model and observational approach suggests functional relationships exist that can be associated with management actions and vegetation types, a process based view of the system response on the other hand would provide better understanding of how specific disturbances, management decisions, ect would determine ecohydrology change and magnitude of change. Here the authors have now included text that states the inclusion of physical hydrologic properties would help their model and provided minimal text outlining the mechanistical link between veneration and soil moisture. Certainly, I think more can be done here, but I am ok with the paper moving forward as is. As a whole the paper is good, and the science interesting.

***Response: 1) A statistical method avoids the potential errors caused by making assumptions about mechanisms that might alter soil properties following fire. There is still a great deal of uncertainty regarding the magnitude and duration of impacts such as fire-induced hydrophobicity, transpiration changes, and interception changes. 2) It is not possible to conclusively state which mechanisms are causing differences in soil moisture under different types of vegetation cover, since we are making observations of an existing system rather than performing controlled experiments, which limits us to identifying correlations. Mechanistic modeling could help explore likely mechanisms, but that is beyond the scope of this paper. We explain the potential mechanisms (e.g. reduced interception, reduced transpiration, and deeper peak snowpacks, lines 98-99), but our analysis is necessarily limited to the presentation of correlations and hypothesizing the potential causes, rather than observing mechanistic changes directly.***

A few minor comments:

L301-303: ‘Since random forest is a statistical model….. it does not require information about physical soil parameters in order to represent soil moisture, as long as the covariates used are correlated with soil moisture state.’ While, I’m ok with the use of the statistical model for this paper, you should non-the-less be upfront about weakness of such model, specifically that using this type of model to infer unobserved conditions (as stated at L308) requires the assumption that these correlations are true in unmeasured areas and prefire conditions.

***We have added the following text to acknowledge some of the limitations of this type of model: “Statistical models such as random forest provide multiple benefits, including their ability to fit nonlinear relationships without needing to make (potentially erroneous) assumptions about specific mechanisms of change. However, the model may not perform well when being used to infer conditions outside the range of observations, since there is no guarantee that the fitted relationships hold true for predictor values not included in the model fitting. While it was not possible to capture the complete range of predictors and their combinations present throughout the watershed, we selected our measurement sites in order to cover as broad a range of conditions as possible (in terms of fire history, vegetation type, water year type, and topography) in order to make the model validation applicable to a wide range of conditions.” And “This method also assumes that our model is able to capture pre-fire conditions accurately, despite the observational data being from burned areas. Although we could not access any completely unburned areas of the watershed for measuring soil moisture, we measured sites that had not burned since 1974 and/or burned only at very low severity; we believe such sites provide reasonable proxies for unburned areas and are therefore appropriate for fitting a model that is meant to simulate both burned and unburned conditions.”***

L435-437 (And in response to author comment L409-410): The increased soil moisture variability observed during the dry 2016 water year during and summer dry-down is likely caused from differences in physical soil characteristics, specifically residual saturation. This is well documented in hillslope hydrology see previously cited Grayson et al., 1997 and Famigliettie et al., 1998. I am not suggesting an additional analysis or discussion regarding this. However, because you are reporting soil moisture variability increasing in dry conditions as a result of your measurements, an acknowledgement of the known mechanistic process is warranted and within the scope.

***The referenced lines are about within-year variability, not spatial variability. The point was that wetter winters lead to wetter summers, rather than the soil reaching some minimum value every summer. This sentence has been reworded for clarity, it now reads “Summer dry-down was more pronounced in 2016 than in the wetter 2017 WY (Figure 7) despite July measurements being taken on the same dates each year. This suggests that even mid-summer shallow soil moisture is impacted by the previous winter’s precipitation.”***

***However, we agree that it is valuable to acknowledge known mechanistic processes related to our observations. We also found these citations to be useful in validating some of our choices of metrics. We have therefore added the following: “Other studies have shown that the drivers of soil moisture distribution vary with time since precipitation, with certain topographic and soil texture factors being more important predictors under dry conditions compared to wet (Famiglietti et al. 1998, Grayson et al. 1997). Under drier conditions, a decrease (or cessation) of lateral subsurface water movement, which decouples soil moisture from nonlocal controls such as upslope contributing area and instead leads to variations based on*** local ***topography, vegetation, and soil characteristics (Grayson et al. 1997). Our method includes a variety of local (e.g., vegetation cover, slope, aspect) and nonlocal (e.g. distance from nearest stream, upslope area) controls, and the use of the day of year as a predictor allows the model to account for late-summer changes in dominant controls, as suggested by Grayson et al. (1997).”***