Remote sensing in rangeland fire ecology: Comparing imagery to measured fire behavior, and burn severity across prescribed burns and wildfires

Response to second round of reviews

AR: Author Responses (AR) appear below in this green text.

Reviewer 1, second report

accept

AR: I appreciate the clear recommendation to accept the manuscript as it was initially revised.

Reviewer 2, second report

Thank you for the opportunity to review this manuscript. I have read the authors' responses, and I would like to reiterate my key concerns in light of their replies.

1. On the authors' response style The tone of the author's reply is rather combative in several places—for instance, dismissing suggestions as "self-contradictory" or claiming not to understand their purpose. This does not foster constructive revision and is unhelpful in moving the manuscript forward.

AR: While I certainly did not mean to come across as "combative" and apologise for any hurt feelings, the reviewer ought not to take offense to having instances in which comments are not helpful too personally.

2. Specific issues that must be addressed

(1) Introduction

The current version remains too broad. Please sharpen the focus by trimming general background and instead clearly position the study within the relevant literature. A more explicit comparison of key domestic and international work would help clarify the research gap. Most importantly, the specific novelty and necessity of integrating field sensors with satellite dNBR in rangeland fire studies should be stated more directly.

AR: Although I disagree with the reviewer, I have removed text placing the study of wildland fire within the broad historical trends of science. THe briefer Introduction directly addresses the "specific novelty and necessity" of the comparison focused on here.
But the reviewer does not at all provide the additional details I requested about what justifies or necessitates an explicit discussion of "domestic" vs "international" work, which is a distinction I have never seen in any Introduction that does not have as its stated goal such a comparison.
To the reviewer's final point, I have made a direct statement about the novelty and necessity of integrating field sensors with satellite data.

(2) Methods

dNBR image selection: The description is still vague. Please specify concrete temporal criteria (e.g., "the last cloud-free image within 30 days before ignition" and "the first cloud-free image from the first post-fire growing season").

AR: But these statements are simply not true. The details the reviewer adds are incorrect—they are not steps in the process. The process is exactly as described—the description is not vague, the process is simple.

Sampling rationale: The choice of a 20-m buffer ("two Sentinel-2 pixels") and 100 sample points remains arbitrary without methodological or literature support. Please justify these choices explicitly—for example, by citing similar buffer distances used in comparable studies or by explaining how the sample size was determined.

AR: A citation to a publication that used this same methodology has been added.

(3) Results The reported "positive correlation" would be more informative if accompanied by significance levels (p-values) alongside the regression coefficients.

AR: Both the initial submission and the revised draft the reviewer had access to included the P values; only in response to another review were the regression coefficients added to the existing test statistics and P values.

(4) Conclusions The conclusions are still presented as a single dense paragraph. Restructuring them—for instance, using subheadings or bullet points—would greatly improve readability and help readers quickly grasp the key takeaways.

AR: The Conclusions have now been broken into two paragraphs, and a set of bullet points have been added in a "Key takeaway" section as well, per the reviewer's request.

Reviewer 4, first report

The study presents an investigation with potentially significant findings. While the research demonstrates scientific merit, we suggest addressing the following aspects:

1. Based on current research, there exists a discrepancy between remotely sensed burn severity indices and ground-truth measurements, which typically necessitates calibration using field-observed severity data. However, this study did not account for such calibration. Could this omission introduce uncertainty into the research findings?
2. Given that spatial autocorrelation is known to significantly affect ecological interpretations, would supplementing the current statistical analysis with geospatial approaches provide additional insights?
3. Results Section currently stands as the least robust portion of the manuscript. Integrating its content with Discussion Section could yield a more substantive and logically flowing discussion.
4. The study would benefit from deeper theoretical elaboration regarding the underlying mechanisms driving the statistical results. Providing more robust explanatory frameworks would significantly strengthen the interpretation.

AR: I decline to consider this review as I do not believe it complies with reviewer policies regarding AI use. This review was exceedingly general—it makes only vague reference to actual content or and makes no reference to line numbers where specific items ought to be addressed. Nor did it give specific recommendations, instead posing several general questions.

In fact, this review felt so "off" that I put it into an online AI content detector, which concluded it was AI-generated, with 81% of the text returning a high confidence for AI generation.