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Interactive comment on "Degradation of net primary production in a semi-arid rangeland" *by* H. Jackson and S. D. Prince

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GENERAL QUALITY

I liked the approach used in this manuscript but wondered why they did not include slope / aspect criteria in their cluster inputs (big effects in the wester US in arid, boreal, and arctic systems which are ecologically known and accepted. I would be surprised if this was not also true of Australian rangelands). I particularly liked the diligence and innovative summarization of results (pg 8). I found that the Acronyms were numerous and wondered if this journal did a list of acronym section that would have aided the reader? Two prominent things stick out to me: 1) lack of familiarity with the literature and 2) major miss-numbering issues with Figures (assumedly) 6 and 7 (pg s 34 & 35).

SCIENTIFIC QUESTIONS

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Discussion paper

". . .there is currently no other method available, LNS was used" pg 3 ln 6. 1) Literature: Prominent articles that I am aware of focused on isolating management are included below. I was surprised the authors only found one or 2 of these. They do cite Wessels et al 2007 & 2008 (pg 2) which seem to me to be a viable and comparable approach):

a. Western US Rangelands i. Wylie, B.K., Boyte, S.P., and Major, D.J., 2012, Ecosystem performance monitoring of rangelands by integrating modeling and remote sensing: Rangeland Ecology and Management, v. 65, no. 4, p. 241-252, at http://dx.doi.org/10.2111/REM-D-11-00058.1. ii. Boyte, S.P., Wylie, B.K., and Major, D.J., 2015, Mapping and monitoring cheatgrass dieoff in rangelands of the Northern Great Basin, USA: Rangeland Ecology and Management, v. 68, no. 1, p. 18-28, at http://dx.doi.org/10.1016/j.rama.2014.12.005. iii. Rigge, M.B., Wylie, B.K., Zhang, L., and Boyte, S.P., 2013, Influence of management and precipitation on carbon fluxes in Great Plains grasslands: Ecological Indicators, v. 34, p. 590-599, at http://dx.doi.org/10.1016/j.ecolind.2013.06.028. iv. Gu, Y.; Wylie, B.K. Detecting ecosystem performance anomalies for land management in the upper Colorado River basin using satellite observations, climate data, and ecosystem models. Remote Sens. 2010, 2, 1880–1891. v. Rigge, M.B., Wylie, B.K., Gu, Y., Belnap, J., Phuyal, K.P., and Tieszen, L.L., 2013, Monitoring the status of forests and rangelands in the western United States using ecosystem performance anomalies: International Journal of Remote Sensing, v. 34, no. 11, p. 4049-4068, at http://dx.doi.org/10.1080/01431161.2013.772311.

b. Boreal forests i. Wylie, B.K., Rigge, M.B., Brisco, B., Murnaghan, K., Rover, J.A., and Long, J.B., 2014, Effects of disturbance and climate change on ecosystem performance in the Yukon River Basin boreal forest: Remote Sensing, v. 6, no. 10, p. 9145-9169, at http://dx.doi.org/10.3390/rs6109145. ii. Wylie, B.K., Zhang, L., Bliss, N.B., Ji, L., Tieszen, L.L., and Jolly, W.M., 2008, Integrating modelling and remote sensing to identify ecosystem performance anomalies in the boreal forest, Yukon River Basin, Alaska: International Journal of Digital Earth, v. 1, no. 2, p. 196-220, at

http://dx.doi.org/10.1080/17538940802038366. iii. NDVI prediction 1. Bunn, A.G., Goetz, S.J. and Fisk J., 2005. Observed and predicted responses of plant growth to climate across Canada. Geophysical Research Letters, 32, L16710, 14.

c. Africa i. Hermann, S.M., Anyamba, A. and Tucker, C.J., 2005. Recent trends in vegetation dynamics in the African Sahel and their relationship to climate. Global Change Biology, 15, 394404. ii. Wessels, K.J., S.D. Prince, et al., 2007, Can human-induced land degradation be distinguished from the effects of rainfall variability? A case study in South Africa. Journal of Arid Environments, 68, 271297. iii. Archer, E.R.M. Beyond the "climate versus grazing" impasse: Using remote sensing to investigate the effects of grazing system choice on vegetation cover in the eastern Karoo. J. Arid Environ. 2004, 57, 381–408.

Pg 4 ln 8-9: It seems that the nearest neighbor approach would merely retain the blockiness of the 5 k x 5 k data. Why not use an interpolation to smooth 5k 5k pixel boundaries? Say cubic or bilinear interpolation? Why not include slope and aspect? Known ecological difference occur related to certain conditions (south vs north aspect with moderate to steep slopes) in many ecosystems, particularly temperature limited (Arctic and Boreal) and moisture limited ones. In the northern hemisphere you would be showing all southern aspects as degraded when they are just drier because of higher transpiration demands from higher temperatures than north facing slopes. The same would be true for southern hemisphere, only with north slopes being drier..

Pg 7 ln 15: it would be interesting to field check these all year reference sites.

Pg 11 ln 1: Convection thunderstorm precipitation is HARD to map accurately. Often in remote areas with few weather stations, gridded precipitation can be unreliable when distant from a weather station.

Pg 11 ln 17: "largest spatial variations" Think of ecological tendencies for larger means to have larger variances. What if you use CV (coefficient of variation)?

Pg 11 ln22-27: "∼ need for comparison to pixel based estimated productivity" This sounds exactly what Wylie et al, Rigge et al. Gu et al. are doing but instead of a process-based model (classically heavily depend on precipitation which is notoriously problematic to map in remote landscapes) data driven regression trees were used to predict undisturbed productivity or potential productivity.

Pg 12 ln 39: "∼relationship to hillslope erosion)" Not convinced unless slope/aspect are taken into account in LCC.

TECHNICAL CORRECTIONS

The miss-numbered figures 6 and 7 seemed out of place in an otherwise very thoughtful paper.

Pg4 ln 36: Why not see if the 2 difference clusters/land groupings are consistent spatially? “mean square variance of their maximum NPP” was confusing. Re-word? I was confused if you only had one max value per LCC how you could get a variance of, that but later it became clear that you were looking a the variance of max-each pixel in the LCC. One statistical buddy told me that maximized variables have weird statistical properties and should be avoided (you also mention the maximum is susceptible to selecting "outliers"). We have used mean values from the upper quartile to avoid such issues. I see later (Fig3) you use 85 percentile. Why did you choose to use the maximum for the difference in clusters vs land grouping? I think it is “OK” but if you apply this elsewhere I would consider changing this.

Pg 5 ln 27-28: Why not downscale 250 m to 1km, run the regression at 1km (ndvi vs npp)? At least then you are comparing apples to apples. . . 250m variation is going to just be different than 1 km variation.

Pg 6 ln 4: “reference pixels” Glad to see acknowledgement of the limitations but I do not think the readers understand where the reference pixels come from because Fig 3 has not been presented. I was confused at this point before Fig 3 was introduced.

(also true at Pg 4 ln 12)

Pg 7 ln 1-7: In the US, the BLM (major federal land management agency for western arid rangelands) has locked in as percent bare ground as a good indicator of range condition. Are there any estimates of this you could use? I know there is a soil property mapping effort/research going on in Australia (Henderson et al. 2005, Geoderma 124:383-398) or continuous land cover (http://landcover.usgs.gov/pdf/canopy_density.pdf; http://glcf.umd.edu/data/treecover/) which could be used? Maybe remote sensing vegetation indices??. I am concerned that by not including slope and aspect in you r LCC determination that you maybe incorrectly identifying drier north slopes as degraded.. I guess your soil erodibility data is OK but soil texture differences could be a major driver in those determinations, not management. . ..

Pg 7 ln 24: "but between-LCC" Fig 4 miss labeled or text is wrong. Fig 4b has these statistics but was labeled "within LCC". I think the association with rain does not add much, particularly to assess the 2 clustering approaches. Why not plot variance vs your maximum NPP or reference NPP or mean cluster NPP? I think you are just using precipitation as proxy for productivity here. Higher variances with higher means is a common phenomenon in ecological data, thus often the coefficient of variance is used.

Pg 9 ln 16. I like your quantification of degradation in units of NPP.

Pg 9 ln 10: Fig 5f: I think I see possible difference associated with slope / aspect differences. . .

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