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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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The authors are most grateful for these unusually detailed and comprehensive reviews.

Scientific Questions

". . .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,

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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 humaninduced 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

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grazing system choice on vegetation cover in the eastern Karoo. J. Arid Environ. 2004, 57, 381–408.

Authors' Response: The referee lists very relevant studies that do seek to isolate management effects from climatic variability. The referee also correctly points out that only two of these works were cited in the manuscript. The two that were cited are the most closely aligned with the focus of the current manuscript. However, we believe that the reader missed the point made was in regard to the limitations which exist in current methods. For example, in the first paper provided, a rule-based approach was used. The aim of the current manuscript was to produce a repeatable method which does not rely of intimate knowledge of the rangeland system. To accomplish this we sought to allow objective, unsupervised data clustering to decide homogeneous units. Furthermore, the current manuscript develops land capability classes which are not a reflection on vegetation types whatsoever, as had been presented in many of the supplied references, but rather is solely based upon measurable characteristics of the regional environment. This way long term transitions in land condition which result in changes in vegetation type (e.g. invasive species and encroachment of unpalatable woody species) are included in our definition of degradation.

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..

Authors' Response: Interpolation was not used because the data was used in a cluster algorithm, which sought to distinguish homogeneous areas based upon actual data. In

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the Burdekin, as in the rest of Australia, a high quality climate data set exists for which necessary data smooth already exist. The use the average mean LNS value over multiple years also creates a smoothing effect that is more closely related to actual climate values.

Authors' Response: The study region is largely flat so the use of slope and aspect only drive up model error. Furthermore soil properties used to define land capability classes are related to topographic features. Additionally measure of soil erosion, another closely related variable with slope and aspect were used for comparison with LNS results. Authors' Response: Finally, fine scale differences in aspect and slope are a naturally occurring phenomenon in each LCC. Low LNS values in these areas are also a valuable indication of degradation and may be compared across LCCs. Pg 7 ln 15: it would be interesting to field check these all year reference sites. Authors' Response: Great point, noted. 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. Authors' Response: The Australian climate data has an overall accuracy of 84% and the study region falls in an area where a dense network of weather stations exist.

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)? Authors' Response: This comment is in response to the standard deviation values in northern basins and the proposed CV provides the same information, specifically because CV is simply the standard deviation divided by the mean.

Pg 11 ln22-27: "âĹij 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. Authors' Response: A data driven regression tree is another future alternative to the development of LCCs.

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

Authors' Response: In figure 7, the agreement is presented. As stated earlier elements related to topography may be related to the chosen soil properties.

TECHNICAL CORRECTIONS

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

Authors' Response: Corrected

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.

Authors' Response: The maximum referred to in the text is the best estimator of the potential value, which is the 85 percentile. NPP values higher than this were omitted (as stated in the manuscript), so no assumptions are made about their distribution. The goal was to 'model' the unmanaged portion of each LCC. The mean square variation was used for exactly the reason the referee pointed out (i.e. minimizing the effect of outliers while still analyzing variation within the population of maximum values). The differences in the maximum values found were then assumed to be naturally occurring differences, unrelated to management. In a highly managed rangeland such as the

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BDT, this assumption should hold true.

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.

Authors' Response: This was done, the regression was performed at 1km, then down-scaled to 250m. The spatial scaled of 250m was used because degradation related human management is most relevant at spatial scales finer than 1km for many reasons (e.g. grazing enclosure size, differences across property boundaries, highly variable vegetation, etc.)

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)

Authors' Response: Pg 6 ln 4: Changed "reference pixels" to "the potential"

Authors' Response: Pg 6 ln 12: Figure 3 is introduced on the same line at Pg 4 ln 12, so no correction is needed there

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

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management. . ..

Authors' Response: Bare ground data in Australia is typically limited to scales that aren't relevant to regional degradation mapping (e.g. 30m - Landsat, 20m – ASTER versus the 250m –MODIS used in the manuscript). The major problem with using additional aspects of the degradation, such as bare ground is that, if they may be derivatives of the same vegetation indices used for validation of the argument is circular!.

Authors' Response: The substantial agreement of hillslope erosion, a metric highly related to slope, should alleviate most of the danger. To a lesser extent it is impossible to remove all elements of weather.

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".

Authors' Response: Pg 7 ln 24: Changed "Figure 4a" to "Figure 4b"

Authors' Response: Pg 7 ln 24: Changed "Figure 4b" to "Figure 4a"

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.

Authors' Response: Precipitation was used because it is the primary environmental factor which drives differences in potential productivity. This means that if the LCCs can reduce the within-group variation and maximize the between group variation, they are outperforming the GLM map. This gets to a previous point made in the manuscript that it is impossible for all symptoms of the environment to be removed, instead we must manage the impact of the most important environment variables

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

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Authors' Response: Thanks.

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

Authors' Response: The river area was masked so steep slopes associated with riparian zone were minimized. As the text states, it was only the interfluves that were included. It is true that severe erosion can take place on river banks and riparian health has become a major problem in the study region and has resulted in abundant resources to remedy resulting erosion from these zones. This type of degradation was excluded owing to its finer scale than the 250m data that were available.

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