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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.

General comments

Overall I found that the manuscript accomplished its stated objectives using a novel approach to address the main limitation of LNS, was for the most part clearly written, and stands to make a contribution both conceptually in understanding the prevalence and rates of degradation, as well as methodologically through improving remotely sensed rangeland monitoring, areas of research in much need of advancement. In order of importance, I particularly welcome the use of shifting annual reference NPP pixels to demonstrably improve LCC classification (although the reliability of some reference sites might be questioned), the attempt to evaluate LCC classification using

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Discussion paper

independently-derived datasets measuring elements of land potential, and the generally pragmatic, conservative decisions made at several steps that improve the robustness of the analysis. This being said, I do think the manuscript could be stronger in several respects. Some assumptions are unaddressed or under-stated, the precipitation gradient in the region was not well utilized, and the organization and presentation of results could be much clearer, especially the tables.

Specific comments

"The method is limited spatially only by the capacity to classify the land," (page 1, line 24): I'm not sure exactly what this means, but I doubt it's true. A key assumption of the analysis is the accuracy of MODIS NPP in the study area. In tropical grasslands both dry and wet, this data can be unreliable for different reasons. In fact, it could explain why weak NPP and degradation gradients were observed. If there are relevant assessments for the region, cite them. If not, best to evaluate the MODIS data to the extent feasible, or use more than one method for NPP.

Authors' Response: Regarding the spatial limitation of LNS, the limitation is the spatial resolution of the satellite data that are used. If Landsat data were frequent enough to be used to estimate NPP, and was available with adequate frequency, the LNS analysis could be undertaken at that scale.

Authors' Response: The referee also states a key difficulty for virtually all remote sensing studies - reliability of the data. While the errors in the LNS procedure far outweigh those associated with the sensor, it is nevertheless true that the MODIS NPP product, based as it is on a light-use efficiency model, is frequently inaccurate. In the present case it was assumed that NPP errors would be minimized in the limited area (compared with global) that were analyzed.

Authors' Response: We do agree that rewording/removing will help avoid additional confusion.

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Authors' Response: Pg 1 line 24: Sentence deleted. "The method is limited spatially only by the capacity to classify the land."

Another assumption is that use of foliage projective cover (FPC) in defining LCCs did not unduly alter the analysis and conclusions. The soil and weather data are arguably independent of degradation, vegetation condition is not. While I understand the logic in using FPC, and it is not necessarily problematic, I'd prefer a mention of what factors the classification was robust to when included (or not), and a correlation matrix of factors used for LCC classification at minimum. Authors' Response: Foliage projective cover was used as a reference point to start to separate pre-2000 vegetation groupings. The point was to limit the opportunity of different, existing vegetation groups from being compared with each other and thus minimize false interpretation as degradation.

Authors' Response: A correlation matrix for 50 classes for each year over 14 years would be tedious (50x14= 700 cells) for the reader to evaluate. A correlation matrix for just one LCC could be included but would not be representative of any other LCC.

The manuscript missed an opportunity to use the (large) rainfall gradient in the region productively. Analyses were presented and interpreted at river basin scales, which to me is not the natural unit of aggregation for analysis in this case (as hydrology is not the primary focus). I would have preferred to see, for example, mean precipitation isohyets delineated at increments from the coast, and degradation trends analyzed specifically within and between these areas. Addressing rainfall explicitly would have greatly increased the amount of information produced by the analysis.

Authors' Response: Climate (including rainfall) was included in the creation of LCCs, although not in the form of climatological isohyets across the entire region, rather as annual rainfall. It is true, however, that long-term environmental differences, as captured to some extent by climatology, may create more homogeneity within LCCs, and we acknowledge that this should be explored in future studies.

Authors' Response: Second, river basins provided a more natural comparison with

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Discussion paper

management units which are of interest to policy-makers as well as managers and an important factor of concern in the Burdekin Dry Tropics is erosion leading to sediment transport, as mentioned in the Introduction, which contributes to the silting of the Great Barrier Reef.

With regard to the manuscript's presentation, most importantly, some numbers do not appear to add up, and their derivation must be checked and clarified. Table 2 gives -1.71 (non-degraded) and -3.90 (degraded) MgCm-2yr-1 as the average LNS values for these 2 degradation classes, which firstly form the basis for the whopping "2.14 MgCm-2yr-1" typo (hopefully) in the abstract, text, and Table 2.

Authors' Response: The 2.08 MgCm-2yr-1 is the average value for the entire study region, not the total. We think this is clear in the table.

Secondly, Tables 5 and 4 respectively provide -97.5 (non-degraded) -209.1 (degraded) gCm-2yr-1 as apparently the same values. If river basins must be used to organize the tables, they would be more effective if reorganized. Cutting down the table text and combining tables to align figures on degraded area, trend categories, and/or degradation classes would present the results much more clearly. Finally, including the reference NPP, rainfall, or some other indicator of overall productivity potential would make the reported values more meaningful. Alternatively, summarize such relevant statistics by basin in an appendix.

Authors' Response: The -209.1 gCm-2y-1 value from Table 4 refers to degraded areas, while the -97.5 value from Table 5 refers to the non-degraded areas – as the referee points out - but we are unsure why there could be confusion regarding these. Tables 4 and 5 are straightforward, presenting the average NPP loss, percentage loss and the area affected in each basin and the entire region. The point is that each river basin has different degrees of degradation and that degradation may be interpreted differently (e.g. NPP loss, percent loss) for each basin.

Authors' Response: Tables could be combined, but removing key data such as the

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Discussion paper

percent NPP loss would make for confusing analysis because LNS cannot be reliably interpreted across an LCC without using a scaled calculation of loss, such as a percent. Also NPP loss is essential for evaluation because it ties the results to a physical metric which may be compared to other land condition assessments. Part of the new approach presented in the manuscript is the scaled values of NPP and how they are interpreted. The LNS values represent how far the observed NPP is from the reference NPP.

Finally, it would have been nice to see a map with degradation class-by trend combinations, to show where is degraded, where is being degraded, and where is recovering.

Authors' Response: These were presented separately to avoid repeating the results.

Finally, some tables and figures should be shifted to supplementary materials.

Authors' Response: The tables and figures have been reviewed with this in mind and we concluded they are sufficiently important to the text that they are better left where they are. Their inclusion will not make the paper unusually long.

Technical corrections

Page 4, line 27: GLMLCC is static, not dynamic as in the UMDLCC approach here

Authors' Response: Changed this text to make that distinction once again, although it was implied in the Methods and made explicitly in the Discussion.

Page 5, line 18: "soil erodibility" was apparently not used

Authors' Response: Soil bulk density, soil water holding capacity and clay percentage were used in the LCCs. Soil erodibility was used (see figure 7) in the evaluation of LNS results.

Page 5, line 34: missing end parenthesis; what is a "distributary"?

Authors' Response: Page 5, line 34: Changed "distributary" to "tributary"

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Discussion paper

Page 7, line 5: “accounts,” not “allows”

Authors’ Response: Page 7, line 5: Changed “allows” to “accounts”

Page 8, lines 3-4: as compared to a reference mean of . . . what?

Authors’ Response: Sorry, we can’t find this text.

Page 8, lines 9-10: reword; typos

Authors’ Response: Page 8, lines 9-10: Changed “The sum of LNS values for entire class, as opposed to LNS per unit area revealed how the importance the size of each class in contributing to the overall reduction in NPP.“ to “The sum of LNS values for an entire class, as opposed to the LNS value per unit area, revealed the importance of class size in the overall reduction in NPP.”

Page 8, lines 14-16: “had”?

Authors’ Response: Page 8, lines 14-16: Changed “had” to “were”

Page 8, line 21: “smaller”? I think you mean “lower”

Authors’ Response: Page 8, line 21: changed “smaller” to “lower”

Page 9, lines 4-10: Does not match the figure legend.

Authors’ Response: Page 9, lines 4-10: Changed “Among degraded areas there was evidence of managed grazing, including abrupt differences in LNS along station boundaries (Figure 5b), but there were also gradients of LNS within some stations (Figure 5c), and others with low LNS spread across boundaries (Figure 5d). Other areas with evidence of management included forest clearing (Figure 5e) near station boundaries. There were also locations classified as degraded with little evidence of direct grazing management such as between the drainage lines of streams (Figure 5f).” to “Among degraded areas there was evidence of managed grazing, including abrupt differences in LNS along station boundaries (Figure 5b), but there were also gradients

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of LNS within a single station (Figure 5c), and others with low LNS spread across multiple boundaries (Figure 5d). Other areas with evidence of management included forest clearing (Figure 5e) near station boundaries. There were also locations classified as degraded with little evidence of direct grazing management such as between the drainage lines of streams (Figure 5f)."

Page 10, line 11: "were occurred in"?

Authors' Response: Page 10, line 11: Changed "were occurred in" to "occurred"

Page 10, line 18-24: naturally 'bare' ground is undergoing degradation?

Authors' Response: Page 10, line 18-24:Changed "The only negative trend was in the 'bare' class while 'removed' had the largest positive trend." to "The only negative trend was in the 'bare' class, presumably an indication that a small amount of vegetation was present, while 'removed' had the largest positive trend."

Page 10, line 33-36: reword

Authors' Response: Page 10, line 33-36: Changed "This indicates that degradation, as detected with LNS, were sites that were persistently below the potential, not simply subject to some short-term environmental deficiency, such a single-year with spatially patchy lower rainfall." to "This indicates that degradation, as detected with LNS, corresponded to sites that were persistently below the potential. This emphasized that these sites were not simply subject to some short-term environmental deficiency, such a single-year with spatially patchy lower rainfall."

Page 12, line 12: Table 2, not Table 1

Authors' Response: Page 12, line 12: Changed "Table 1" to "Table 2"

Page 12, lines 4-20: These numbers do not match the tables.

Authors' Response: The numbers do match, but I will ensure the number is presented exactly as in the table

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Discussion paper

Authors' Response: Page 12, lines 4-20: Changed "65%" to "65.3"

Also, permanent degradation cannot be inferred here.

Authors' Response: It is inferred owing to the irreversible nature of degradation

Authors' Response: Page 12, line 15: Changed "presumably" to "a possible indicator"

Page 12, line 29: "strong" correlation? What is the evidence?

Authors' Response: The evidence is in table 8.

Authors' Response: Page 12, line 29: Changed "strong correlation" to "good agreement" to be more precise.

Page 32: Clarify that points are years, not LCCs or something else

Authors' Response: Page 32: Changed ". . .lines." To ". . .lines for each year 2000 to 2013."

Page 34: Figure 3. . .?

Authors' Response: Previously Corrected

Page 35: Figure 4. . .?

Authors' Response: Previously Corrected

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