Paper 712 Wilson Reviewer report

This is a disappointing paper. Its conclusions might be a useful indication of the interactions between defoliation frequency and severity on root growth in a range of *P. notatum* cultivars on waterlogged soils in southern Florida. But how relevant to normal pastures are root data of swards growing on waterlogged soil? I suspect not much.

The authors report data from a simple agronomic design of 4 replicates of 4 cultivars \* 2 cutting frequencies \* 2 cutting heights. The authors mention (Line 42) the need for “season-long” evaluations. The data they report are total harvested dry matter over just 16 weeks and a final measurement of root ingrowth at the end of this period. This is only a fraction of the growing season in sub-tropical Florida.

Given the short duration of the experiment in just one year, the data are of doubtful relevance to any practical cattle production system. Moreover, the authors provide no data of any sward characteristics such as LAI before and after the defoliation treatment that might give some insights into the effects of the imposed treatments.

Why was it necessary to forgo normal analysis of variance of the dry matter yields? Exotic analyses suggest that there may have been problems with the data. The raw yield data in Table 1 suggest that there were large yield differences between the four replicates of the same plots. Is there any reason for this? Is this the reason that the authors choose to use a statistical treatment different from traditional analysis of variance that was used to analyze the data from the same plots (Vendramini et al. 2013)?

The standard errors of the means in Table 1 appear to be much smaller than the crude data indicate. In the caption of Figure 3, “Where the entire 95% credible interval falls above or below zero . . .” (lines 323-4). But none of the data in Figure 3 come close to meeting this criterion, so that one can only conclude that there were no significant differences. Does this confirm my suspicion that the rather exotic statistical analysis is an attempt to disguise this conclusion? Irrespective, it is very difficult to see exactly what the results were in terms that might be useful to other readers.

How were the plots managed for the three years from 2010 to 2013? Are these the same plots reported in Vendramini et al. 2013? If so, how were they managed in the interim?

At over 7,300 words, the paper is far too long for the superficial nature of the work. The introduction (1232 words), materials and methods (1620 words), discussion and conclusions (1746 words) could each be reduced to about half their current length. In conclusion, the paper presents some data that may be of interest to workers in the south-eastern US. But given the short duration of the treatments, the lack of any ancillary measurements together with waterlogged soil and obscure statistical treatment, I cannot recommend that it is acceptable for publication in its present form. It may, however, merit a brief note if the authors are prepared to shorten it to about half its current length, explain the limitations of the waterlogged soil and use a more conventional statistical treatment.

A note for the editor: This paper is already available on the internet. What are the implications? Does this affect the paper’s acceptability for publication? The version is headed “bioRxiv preprint doi: https://doi.org/10.1101/763128. The copyright holder for this preprint (which was not peer-reviewed) is the author/funder. It is made available under a CC-BY 4.0 International license.” The link is <https://www.biorxiv.org/content/10.1101/763128v1.full.pdf+html>. It does not acknowledge *Tropical Grasslands*.