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Discussions

# *Interactive comment on* "Estimating Grassland Curing with Remotely Sensed Data" *by* Wasin Chaivaranont et al.

**Anonymous Referee #2**

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GENERAL COMMENTS

This paper proposes to investigate the improvement to satellite estimation of the degree of curing (DOC) of grassland by introducing vegetation optical depth (VOD) data derived from passive microwave sensors, compared to the DOC that can be estimated using normalised difference vegetation index (NDVI) data alone. The paper then, as a second investigation, proposes to compare the reliability of the grassland fire danger index (GFDI) calculated from DOC estimated dynamically from VOD and NDVI with GFDI calculated from DOC fixed at its maximum value of 100%.

GFDI is a key parameter in operational fire management in Australia, and DOC is an essential input to the calculation of GFDI as well as being an important quantity in its own right for fire management. Thus the subject matter of this paper is certainly



important and within the scope of the journal. There has been substantial interest in recent years in using satellite data to routinely monitor DOC. Most work has used optical sensors by way of NDVI or other band combinations, and VOD offers a potentially useful complementary source of information on DOC that is more immune to gaps due to cloud and noise due to atmospheric effects.

However, the analyses that this paper presents are not done in a way that clearly demonstrates that including VOD gives better DOC estimates than using NDVI alone, let alone giving a clear indication of the extent that VOD can contribute to DOC monitoring. Also, the comparisons of GFDI calculated with different inputs do not say anything significantly new or useful. Thus this paper should not be published without a substantial revision of its analyses and other modifications. I expand on these points below.

The selection of field sites in Section 2.2 raises concerns. The rejection of sites without the expected negative correlation between VOD and curing seems very dubious: if such sites are rejected then of course the performance of a VOD-based estimate over the set of remaining sites must improve. Only five sites are accepted for the training dataset, which is a small number. Which five sites are used is not stated but should be: are they all the same grass type (improved pastures or native grasses)?; is the hummock grass site Lorna Glen included (the field data from site has very little DOC variation)?

VOD is primarily sensitive to vegetation water content (fuel moisture content), as you note, whereas NDVI and some other optical indices are sensitive to chlorophyll content. As grassland senesces the water and chlorophyll contents both decrease but not necessarily in perfect correlation, and also the relationship between DOC and FMC varies between species of grass (your reference Dilley at el. 2010). Curing is usually assessed in the field - whether by the destructive, visual or Levy rod method - by using the colour of the grass to distinguish live from dead. Since the colour is controlled by the amount of chlorophyll, a remote sensing method that responds to vegetation water would be a less direct estimate of DOC than a method that responds to chlorophyll.

Furthermore, I understand that VOD estimates the *absolute* amount of water in the vegetation and so would be controlled by the vegetation amount (biomass) as well as its fractional water content. The optical methods to estimate DOC and FMC aim to estimate *fractional* amount of live vegetation and water respectively while mitigating the confounding effects of contamination of the scene by persistent green vegetation (trees) and bare soil. Therefore the VOD method is expected to be a less direct assessment of DOC and so its use must be solidly justified. The fact that VOD is less direct suggests that perhaps a VOD anomaly approach, such as a relative VOD by analogy with relative greenness, is called for, but the authors note they found such an approach ineffective. How does VOD handle the presence of trees or bare soil in the grassland scene (pixel)?

Page 5, lines 18-21 discuss the authors' failure to reproduce Newnham et al.'s result that relative greenness (RG) predicts DOC better than plain NDVI, noting the sensitivity on the time range used to calculate RG. Newnham et al. examined the dependence on time range, but you don't cite their result or mention using it; you have not stated what time range you used. More importantly, Newnham found that the simple per-pixel RG performed worse than NDVI, as you have. Newnham only found that RG could improve on plain NDVI by using a version of RG that normalised NDVI by a spread based on climate zone, rather than per-pixel. While you were right to consider RG as a possible improved way of using NDVI to predict DOC, against which a VOD-based method should be compared, to do this properly you would have to use Newnham's preferred variant of RG. However, Newnham et al. note that the improvement is small over plain NDVI, so it may be enough just to quote Newnham's value for RMSE.

Page 5, lines 25-27 states that your search for correlations between VOD anomalies and DOC was unsuccessful. Also, page 6 line 35 to page 7 line 1 says the DOC versus VOD regression indicates that VOD alone is not reliable enough to estimate DOC. These two statements both suggest that VOD is a poor predictor of DOC, and in particular that VOD is poor at explaining the residual DOC variation beyond that



explained by NDVI. Related to this, it is surprising that Equation 10 does not include a term linear in VOD but instead VOD appears only in the cross-term (VOD)(NDVI).

It is important to acknowledge that a regression of DOC against NDVI together with any other predictor variables must mathematically give at least as good a fit as does a regression against NDVI alone. The question then becomes how well the new predictors explain the residual after the NDVI regression. From Table 1 it appears that the ratio of MODIS band 7 to band 6 (used in Method B) explains the residual better than does VOD ("Evaluation" line in Table 1). The different evaluation datasets (23 sites for the VOD method, 37 sites for Method B) make the comparison of the statistics that the authors compare for the two methods more uncertain.

Page 6, line 35 says Table 1 includes the DOC versus VOD regression results but in fact it does not. It would be instructive to see a scatter plot of DOC against VOD, or of the residual DOC unexplained by the NDVI prediction against VOD.

DOC as a function of VOD alone could be a useful alternative to optical indices in situations of prolonged cloud such as northern Australia during the monsoon. The two approaches could even be used together if they could be harmonised to be practically interchangeable, but this needs much more work.

Page 7, lines 20-35 analyse the spatial and temporal variation of DOC. It is not clear that this says anything new or that the quantitative measures of variability are useful. It is well known that DOC varies spatially and temporally, including having interannual variations. The spatial patterns and standard deviations of these are not obviously useful. What is critical is the uncertainty of estimates at any particular location and date. In any case, it is dubious to calculate these variations over the entire continent which includes non-grassland regions (e.g. forest, heath) and substantial arid or semi-arid regions for which remotely sensed characterisation of the sparse (or absent) vegetation is challenging.

The paper would be improved by including discussion of the drawbacks of VOD. For instance, noting the magnitude of VOD errors resulting from imperfect separation of the VOD, soil surface and soil moisture contributions to the microwave radiometer signal. Also, citing any validation of the VOD dataset over Australia. A spatial resolution of 0.1 degree ($\sim$ 10 km) is fine enough for regional DOC assessments in extensive grasslands but not quite fine enough for some purposes such as input to fire behaviour models or operational GFDI calculations (currently on a 3 km or 6 km grid depending on state), or in landscapes where grassland is fragmented on small scales. You have mentioned the drawback that VOD cannot be estimated near the coast where much grassland is located.

I now turn to the analysis of GFDI with different input DOC. The conclusion that can be drawn from Figure 8 is no more than that a realistic dynamic DOC better predicts fire risk than one fixed at 100%. An NDVI-only DOC might do as well or better. It would have been more useful for you to demonstrate that GFDI is better calculated from DOC based on NDVI and VOD rather than DOC based on NDVI alone, but you have not attempted that.

Page 9, lines 20-21 notes that “the recalculated GFDI places the largest percentage of unburned pixels in the low–moderate GFDI severity class”. I think that this improvement in the distribution is inevitable no matter how good or bad the DOC estimate, simply because now some fraction of pixels has DOC < 100% and so GFDI is lower for those pixels. There is no comparison for DOC estimates (< 100%) with and without VOD.

The analysis of GFDI has serious problems, which are acknowledged in Section 5.3 (e.g. forests and prescribed burns are included), that make the conclusions doubtful. Also, GFDI should be calculated from simultaneous meteorological parameters. The maximum wind speed (page 4, line 42) is often at a very different time of day and with a very different value from the 3 pm wind speed.

In light of my previous three comments, I suggest omitting the analysis and discussion of the effect of DOC on GFDI from the paper.



SPECIFIC COMMENTS

Here I list some minor points that need correction or improvement, but the list is not complete.

Page 2, line 23: You correctly note that optical based remote sensing products, including NDVI, are affected by aerosols but fail to mention that atmospheric correction can, if appropriate aerosol data is available, mitigate that. However, it could well be that VOD sidesteps this issue by being insensitive to aerosol.

Page 3, line 24: Nijs et al. (2015) is not in the reference list.

Page 4, line 10: Give the RMSE too, as well as the bias.

Page 6, line 1: There is more than one version of DOC products available, from different sources each related to the Bureau of Meteorology. State exactly how/where the data were obtained, e.g. URL of website or server.

Page 8, lines 1-2: This should also state that higher GFDI also indicates higher ignition probability (a separate factor from rate of spread of an already ignited fire).

Page 9, line 20: AVHRR data can be obtained at 1 km ($\sim$0.05°) resolution (Local Area Coverage (LAC) or High Resolution Picture Transmission (HRPT) formats).

Page 10, line 19 and Page 11, line 4: As well as NCI, it is important to acknowledge the CSIRO who produced the NCI datasets by mosaicing and regridding the tiled data provided by NASA.

Page 11, line 7: Also acknowledge the Bureau of Meteorology, who continue to generate and distribute the data products set up by the AWAP project.

Page 13, line 28: Fix spelling of “Reflectances”.

Page 24, lines 18-20: Write the band wavelength ranges as, for example, “620 to 670 $\mu$m”. The wavelength unit is $\mu$m, which equals 10ˆ-6 m, not mˆ-6.

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