nhess-2017-101-RC2 Author Comments

Major Comments

A.1 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)?

Author Response: While only five sites are accepted for calibration, these five have the most number of sequential DOC records available, totalling up to 122 observations (out of 238 observations from 23 valid sites). The selected five sites are: Majura, ACT (improved pasture), Tidbinbilla, ACT (mixed grass), Ballan, VIC (improved pasture), Murrayville 1, VIC (native grass), and Murrayville 2, VIC (improved pasture).

Lorna Glen, WA (native grass - hummock) is not included in calibration, but is included in the evaluation.

A significant limitation of VOD is the large pixel size (0.1° x 0.1°). Such a large area frequently includes many land cover types and may not be dominated by grass even though an observation field site falls within the pixel. The sites without the negative correlation with VOD also had no correlation with NDVI suggesting other land cover types where dominating the signal.

Changes in Manuscript: The name of the selected sites for calibration will be stated in section 2.2 (page 4). Total number of observations (238 DOC records) will also be added to the end of section 2.2 (page 4). Above explanation will also be added to end of line 29, page 4.

A.2 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.

Author Response: While this is true, given the different capabilities of VOD (ability to see through cloud, smoke etc) we would like to explore the possibility of whether adding the VOD (responding to vegetation water content) will improve DOC prediction when compared with existing NDVI (responding to vegetation greenness) based DOC prediction model.

Changes in Manuscript: Add the above reasoning to the first study objective (page 2, line 42) for clarification.

A.3 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.

Author Response: We did not attempt to compare the spread based RG, but only range based (per pixel) RG. In Newnham et al. (2011), while range based RG performance is not as good as preferred spread based RG (r^2 = 0.62 and RMSE = 14.2 %), it is still better than plain NDVI (NDVI had r^2 = 0.50 and RMSE = 16.4 %, while 2.5 years range based has r^2 = 0.57 and RMSE = 15.1 %). Note that while we cannot exactly reproduce 10 years time range Newnham et al. (2011) used, since our study time frame is 9 years, we tried various 2.5 years time ranges that overlapped with Newnham et al. (2011) study period, but did not achieve the r^2 and RMSE as high as 0.57 and 15.1 %, respectively.

Changes in Manuscript: Cite Newnham et al. (2011) range based RG results and additional explanation (as stated in the above Author Response) to section 3.1.

A.4 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).

Author Response: While it is true that VOD alone is a poor predictor of DOC, it is found that when using stepwise fit to calibrate NDVI, VOD, and (VOD)(NDVI) terms with the selected DOC observation sites records, the addition of (VOD)(NDVI) term improves the calibration r^2. The VOD term does not appear as the related p-value does not meet the acceptance threshold.

Changes in Manuscript: No changes needed.

A.5 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.

Author Response: Method B does have better evaluation performance than the proposed model. Note that both Method B and MapVic are evaluated with the same 23 sites as our proposed model, as stated in section 3.2 (page 6, line 12-14). It should be noted that Method B used all sites in its calibration, so it is not being evaluated against any independent data.

Changes in Manuscript: No changes needed.

A.6 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.

Author Response: Remove left over reference to Table 1 in page 6, line 35. Thank you for pointing this out. Scatter plot for calibration of DOC against VOD, NDVI, and (VOD)(NDVI) terms is as shown in Fig. RC2-1. Another scatter plot for residual DOC unexplained by NDVI (differences between observed DOC and NDVI-based DOC) against VOD and (VOD)(NDVI) terms is as shown in Fig. RC2-2.

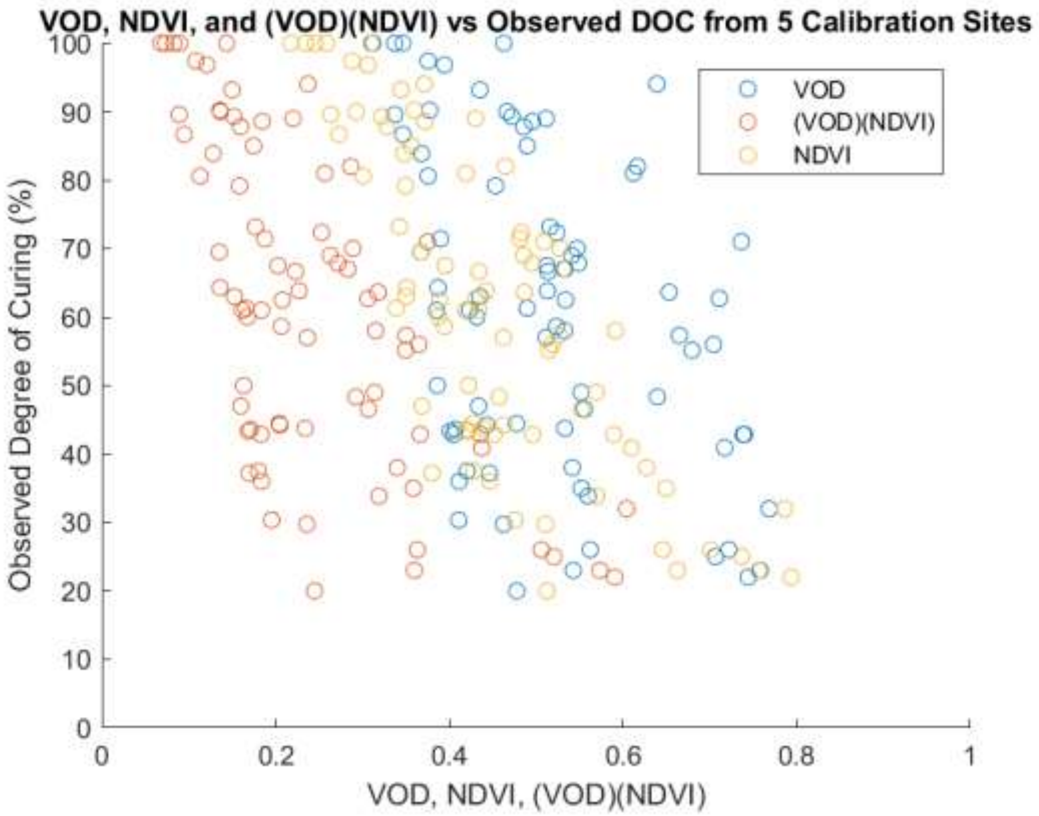

Figure RC2-1

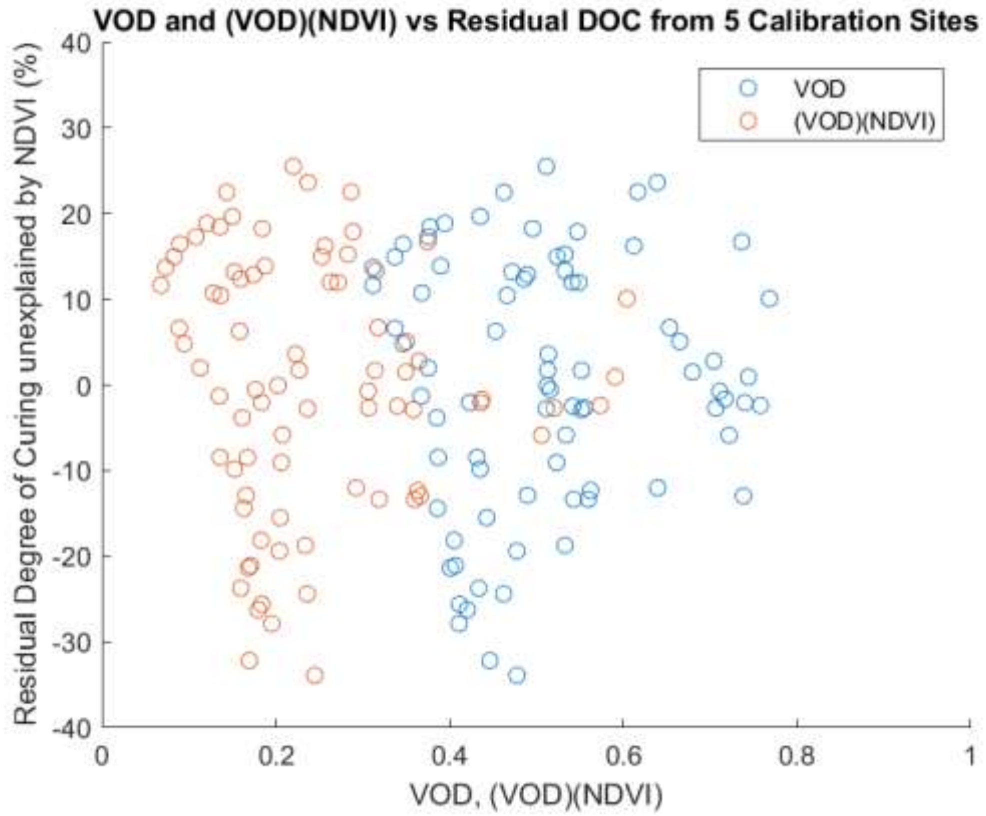

Figure RC2-2

Changes in Manuscript: Change the following sentence from "Using the linear model, as described by Eq. (3), Table 1 summarises the curing and VOD correlation result with a significant relationship and an r^2 of 0.20 with RMSE of 20.80 %" to "Using the linear model, as described by Eq. (3), the DOC and VOD correlation result has a significant relationship and an r^2 of 0.20 with RMSE of 20.80 %." The scatter plots in Fig RC2-1 and Fig RC2-2 will also be added to section 4.1 (page 6-7).

A.7 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.

Author Response: While we initially planned on predicting DOC as a function of VOD alone, we found that VOD (and its modified forms, such as VOD anomalies and seasonal based VOD) are not robust enough to be used as a lone DOC predictor.

Changes in Manuscript: No changes needed.

A.8 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.

Author Response: The temporal and spatial variability provides an indication of areas that are likely to be more or less difficult to predict, as well as a measure to compare prediction errors to. That is, if the variability is larger than the prediction error then the prediction is likely to be useful and vice versa. Also, we are not aware of any peer reviewed literature that provides this information. Forest areas are now excluded from the analysis and Fig. 4 in the original manuscript will be replaced with Fig. RC1-1 (as shown in Referee Comments 1). The continental mean spatial DOC standard deviation is updated from 21.70 % to 20.39 % (page 7, line 28). Additional spatial variability information is added as suggested in Referee Comments 1 (RC1, A.2); see Table RC1-2 in RC1 document for more information.

Changes in Manuscript: Add the information stated in the author response of RC1, A.2 and Table RC1-2 to the results in section 4.1 (page 7). Fig. 4 in the original manuscript will be replaced with Fig. RC1-1.

A.9 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 (~ 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.

Author Response: The following changes in manuscript limitation section will be added as suggested. The validation of VOD over Australia is already cited in Liu et al. (2013b, 2015) works (page 2, line 38). While their works were focused on the global scale, they also covered Australia.

Changes in Manuscript: Update section 2.1 (page 3, line 14) with the following statements: "The VOD dataset used here is derived using the LPRM approach from which soil moisture and VOD are retrieved simultaneously. Several assumptions are made in the LPRM approach, including: canopy surface temperature equal to soil surface temperature, a constant single scattering albedo, same vegetation parameters for both Horizontal and Vertical polarizations, and minimal effect of surface roughness (Meesters et al., 2005; Owe et al., 2001). Uncertainties in soil moisture and VOD retrievals are expected with these assumptions. The evaluation of LPRM soil moisture over Australia showed that the temporal patterns of satellite-based and in situ soil moisture agree very well (Draper et al. 2009; Gevaert et al. 2016). This agreement suggests a reasonable separation of temporal patterns of soil moisture and VOD, while uncertainties may exist in the absolute magnitudes of these two variables."

Then, add the following paragraph to the beginning of section 5.6 (page 9): "It is worth noting here that in an operational setting atmospheric interference by clouds or smoke will cause gaps in the optical and near-infrared (NDVI) data, though the VOD data remains unaffected. We also note that while the VOD data use here was derived from the AMSR-E sensor, which is no longer operational, VOD data derived from currently operating passive microwave sensors, such as Advance Microwave Scanning Radiometer 2 (AMSR2), could be used in an operational setting. It should also be noted that VOD moderately coarse resolution of 0.1° might not be fine enough for using in a small scale operation GFDI calculation and simulating small fires in fire behaviour model."

Draper, C. S., Walker, J. P., Steinle, P. J., de Jeu, R. A. M. and Holmes, T. R. H.: An evaluation of AMSR–E derived soil moisture over Australia, Remote Sensing of Environment, 113(4), 703–710, doi:10.1016/j.rse.2008.11.011, 2009.

Gevaert, A. I., Parinussa, R. M., Renzullo, L. J., van Dijk, A. I. J. M. and de Jeu, R. A. M.: Spatio-temporal evaluation of resolution enhancement for passive microwave soil moisture and vegetation optical depth, International Journal of Applied Earth Observation and Geoinformation, 45, 235–244, doi:10.1016/j.jag.2015.08.006, 2016.

A.10 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.

Author Response: While this is true, the point of Fig. 8 is to demonstrate that GFDI with dynamic DOC can reduce overestimation in GFDI with 100 % constant DOC. The comparison between the proposed model, Method B, and MapVic models for predicting DOC and computing recalculated GFDI (with dynamic DOC) is as stated in section 4.2 (page 8, line 24-38) by using burned area map.

Changes in Manuscript: No changes needed.

A.11 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.

Author Response: The purpose of that specific line and Fig. 9 are to illustrate the different between reference GFDI (with 100% DOC) and recalculated GFDI (with dynamic DOC). The comparison between the proposed model (with VOD), Method B (without VOD), and MapVic models (without VOD) for predicting DOC and computing recalculated GFDI (with dynamic DOC) is as stated in section 4.2 (page 8, line 24-38) by using burned area map.

Changes in Manuscript: No changes needed.

A.12 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.

Author Response: Daily GFDI is calculated using the daily maximum wind speed (while daily relative humidity is from 3pm). While it is true that prescribed burns are included in the burned area map, we already masked out the forest area (as mentioned in Fig. 7 caption, page 19). Unfortunately, we found that burned area map (MCD64A1) is our only viable choice here (see Referee Comment 1, RC1, A.3 for other alternative we already tried). We already acknowledge these problems in the limitation section (section 5.3) as stated.

Changes in Manuscript: No changes needed apart from updates on burned area analysis suggested in RC1, A3.

A.13 In light of my previous three comments, I suggest omitting the analysis and discussion of the effect of DOC on GFDI from the paper.

Author Response: While there are some acknowledged limitations in our analysis and discussion, we feel that these elements are still important elements in the paper. That is to emphasis the different between the GFDI computed with a constant 100% DOC and dynamic, satellite based DOC whether VOD is included or not.

Changes in Manuscript: No changes needed apart from updates on burned area analysis suggested in RC1, A3.

Specific Comments

B.1 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.

Author Response: Thank you for pointing this out. The particular line will be updated with information about atmospheric correction on optical based remote sensing product.

Changes in Manuscript: Add the following line to the end of page 2, line 25: “However, if appropriately detailed aerosol data is available, atmospheric correction can mitigate the aerosol effect on NDVI”

B.2 Page 3, line 24: Nijs et al. (2015) is not in the reference list.

Author Response: Thank you for pointing this out; missing citation will be added.

Changes in Manuscript: Add the following citation to the reference list:

Nijs, A. H. A. de, Parinussa, R. M., Jeu, R. A. M. de, Schellekens, J. and Holmes, T. R. H.: A methodology to determine radio-frequency interference in AMSR2 observations, IEEE Transactions on Geoscience and Remote Sensing, 53(9), 5148–5159, doi:10.1109/TGRS.2015.2417653, 2015.

B.3 Page 4, line 10: Give the RMSE too, as well as the bias.

Author Response: The levy rod method DOC has RMSE of 13.5 % with a bias less than 1 % (Newnham et al., 2011).

Changes in Manuscript: Add RMSE of 13.5 % to page 4, line 10.

B.4 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.

Author Response: The product page can be found via the following link: http://data.auscover.org.au/xwiki/bin/view/Product+pages/Grassland+Curing+MODIS+BoM

DOC datasets are downloaded from the following catalogue (Method B and MapVic): http://opendap.bom.gov.au:8080/thredds/catalog/curing_modis_500m_8-day/aust/netcdf/catalog.html

Changes in Manuscript: Add the above links to Data Availability section.

B.5 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).

Author Response: Thank you for pointing this out.

Changes in Manuscript: Add the following sentence at the end of page 8, line 2: “Though the higher GFDI does indicate higher probability of fire ignition.”

B.6 Page 9, line 20: AVHRR data can be obtained at 1 km (~0.05°) resolution (Local Area Coverage (LAC) or High Resolution Picture Transmission (HRPT) formats).

Author Response: Thank you for pointing this out.

Changes in Manuscript: Remove the following statement from page 9, line 20: "…the advantage of high resolution offered by MODIS (0.05° for AVHRR, but 0.005° for MODIS), or…"

B.7 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.

Author Response: Thank you for pointing this out.

Changes in Manuscript: Add acknowledgement to CSIRO in page 10, line 19 and page 11, line 4.

B.8 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.

Author Response: Thank you for pointing this out.

Changes in Manuscript: Add acknowledgement to the Bureau of Meteorology in page 11.

B.9 Page 13, line 28: Fix spelling of "Reflectances".

Author Response: Thank you for pointing this out.

Changes in Manuscript: Fix the spelling as pointed out in page 13, line 28.

B.10 Page 24, lines 18-20: Write the band wavelength ranges as, for example, "620 to 670 µm". The wavelength unit is µm, which equals 10ˆ-6 m, not mˆ-6.

Author Response: Thank you for pointing this out.

Changes in Manuscript: Correct m^{-6} to 10^{-6} m.