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Interactive comment on “Annual South American forest loss estimates based on passive microwave remote sensing (1990–2010)” by M. J. E. van Marle et al.

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This is a good and interesting paper, presenting a dataset with significant potential for giving independent estimates of forest loss over South America, over a long period.

I have some suggestions for improvements. In particular I am slightly concerned with a circularity of argument: VOD is presented as providing independent data on forest loss, but then the results are calibrated against the Hansen et al. forest loss product. This is understandable, as ground truth data on biomass loss are clearly not available at a quarter degree resolution. I would have liked to see this calibrated against biomass change data, as might be available from SAR or LiDAR datasets in the future, but cur-

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rent data availability of that type of data in South America is very limited. However, more discussion of the results of using the Hansen data should be considered, and spatial maps showing where it agrees and where it disagrees with the Hansen dataset would be very useful. Equally, I think the correlation with the Hansen dataset in the Abstract, and to a lesser extent elsewhere, is overstated for two reasons. 1. the fact that the dataset is calibrated against the same Hansen dataset is not revealed in the Abstract, and 2. the comparisons are made as a total area of a country that is deforested, not its proportion - this inflates accuracy as area is on both axes.

Major comments:

Introduction section is somewhat short. I think it should contain a wider discussion of what is actually detected by VOD, compared to active microwave and optical sensors (radar and lidar), and what is seen by optical sensors. A discussion of the different effects of seasonality, and differing definitions of deforestation in the different products and the effect of different forest definitions on the abilities of the different sensors.

In the Methods section it would be useful to display a figure from one of the cited VOD papers showing the relationship between VOD and canopy cover based on real data. This would allow the reader to make more of an assessment of the validity to cut off at 0.6 and 1.2.

Either in the Methods, or Discussion, more should be made of the difference between what VOD and Hansen are actually detecting. While the VOD changes have been calibrated against Hansen et al. data to give forest loss per 0.25 degree grid cell, that is just due to an empirical calibration, with error. I think more should be made of this error - e.g. I would love to see RMSE values at a grid scale, plotted on a map and with statistics given in a table. VOD is really seeing something similar to net biomass change - i.e. an integration of deforestation, degradation, and regrowth (both natural within forests, and after previous clearance - as well as artefacts due to for example moisture changes). Hansen et al. is just gross deforestation. In areas where deforestation is

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the dominant change, the correlation will work, but in areas where it isn't this is not necessarily because they're seeing different levels of deforestation, as reported, but because other processes may dominate. I don't think there is much that can be done about it, but this must be discussed. I strongly feel a spatial map displaying, at a 0.25 degree grid scale, some metric of difference between PRODES, Hansen and VOD would be very useful in interpreting these datasets. Summing everything by country or by state is quite frustrating in this regard.

Figure 1 should be changed to display which pixels were cut off due to being above 1.2, and which cut off due to being below 0.6.

Figure 4 displays a somewhat spurious correlation. As it is in terms of gross forest loss, the area of each country is a significant factor on both axes. This increases the likelihood of a strong fit, even if there is little correlation between variables. I would like to see this replotted with forest loss in terms of proportion of country deforested per year. Only the area of the country considered by the analysis should be included in the area figure here, so it's somewhat similar to detectable forest area at the start of the period. It is okay for Figure 5 to be in terms of total area - though it would be interesting to see a deforestation rate figure like Figure 4 for PRODES vs VOD, separated by state.

The conclusions could state more grounds for further work. It could cover ways in which VOD could be converted to net biomass change, rather than loosely correlated with gross deforestation which is a somewhat frustrating way to display these very interesting results. Maybe comparisons with LiDAR and SAR-based biomass change maps would be an interesting route for the future? VOD has great potential for large-scale monitoring of whole-country net changes in carbon stocks, e.g. for REDD+: but that would

Minor points:

- Brazil - comparison to PRODES not just Hansen should be mentioned in the Abstract. This is very relevant because the calculations are not independent of the Hansen et

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al dataset, being calibrated again it - Page 11501 Line 27 - erroneously suggests that Landsat has had 30 m data since 1972. - Page 11502 line 7 - I feel that MODIS should be mentioned here, as halfway between say AVHRR and Landsat. Products such as Terral and the MODIS LCC product could be mentioned. Also spelling, coarser. - line 18 - PRODES uses other datasets too to help with cloud cover, e.g. CBERS and DMC. - 11503 line 11-12: given actual resolution given for Landsat, for comparison suggest give actual resolution of VOD sensors - 11505 section 2.2. I assume you did not filter the 'loss' dataset by the 2000 Canopy Cover layer as performed by Hansen et al. in their analysis? I do not think this is a problem, but it should be mentioned in 2.2. and discussed later, as some of the 'loss' changes thus compared to the VOD data will happen in pixels that were not forest in 2000. - 11516 - I do not agree with your argument particularly at the bottom of page 11516. This would be fine if VOD provided an independent metric of deforestation, but in fact it was calibrated by GFC, so biases due to differing scales should be corrected for in your dataset. The only possible difference could be due to Brazil having more small-scale deforestation than the rest of South America, but field experience suggests in fact the opposite is true. I think you need to at the least caveat this section more, or else think of some other possible explanations for this (interesting) discrepancy. I believe this could be due to the differences in gross deforestation (Hansen) vs gross forest biomass change (VOD), with there being extensive regrowth in some areas of Brazil.

- Somewhere in the general introduction might be good to mention active microwave remote sensing of vegetation change - mostly to avoid confusion among non-specialists. Various papers exist giving change based on L-band satellites, especially ALOS PAL-SAR - a recent example in South America would be Joshi et al. 2015 (Environmental Research Letters).

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