Biogeosciences Discuss., 7, C3205–C3209, 2010
www.biogeosciences-discuss.net/7/C3205/2010/

Biogeosciences Discussions

Interactive comment on "Remote sensing of ecosystem light use efficiency with MODIS-based PRI – the DOs and DON'Ts" *by* A. Goerner et al.

Anonymous Referee #1

Received and published: 7 October 2010

This paper reviews possibilities for estimating light use efficiency via PRI with the goal of establishing whether a generic model that would be able to estimate GPP globally can be established My main concern about this paper is that this paper tries to evaluate PRI, but it uses satellite derived FAPAR, which may plays a significant role in this. Also, it is well known that the MOD17 product does a reasonable job in some cases but by far not in all cases. I don't really think it is valid comparing MODIS PRI to MOD17 and then drawing conclusions about the usefulness of one or the other Many studies have shown (and this study discusses) that -at least conventional- PRI measurements are dependent on a lot of things, so we would hte authors have expected there to be a reasonable relationship in the first place? It is quite well established MOD17 does not work everywhere (see for instance Heinsch et al., 2002) and it is also known that just doing a MODIS PRI has its problems as well.. In this respect,I think the paper does

not add too much new information.... That being said, I still think it should be published because it is a nice example of how neither of those methods can just be applied (as is also concluded in the discussion) One other thing, the title doesnt match the contents. I was expecting to read something more like a review paper from the title alone. This paper does not really discuss the do's and dont's but it is more a comparision between MOD17 and MODIS derived PRI

Other comments p 6936 line 25: "Numerous approaches to model GPP have been taken (Beer et al., 2010),among which light use efficiency based models are very popular." Awkward wording, please adjust line 27: "Light use efficiency models are based on the assumption that photosynthetic assimilation of vegetation is a function of the amount of photosynthetically active radiation absorbed by plants" This is not really an assumuption, is it..The monteith definition of GPP is very generic

p6937 line 7: Wasn't that the goal of htis study to find out whether this model can be applied globally? You should probably add something like "in theory" line 13: "Eq. (2) is generally agreed upon" You can delete this, eq 2 is the definition of fapar, nothing to agree upon here line 15: you should probably add a few sentences here explaining what Lue is. Say that it is driven by the most limiting of may factors, and changes in space and time line 23: While studies using airborne fluorescence measurements had promising results, there is no space-borne sensor yet. WEll the reason for that is that fluorescence faces similar, if not even more difficult issues than PRI. For instance the signal is incredibly small compared to all the background effects

p6938: "However, the PRI has some well known limitations (Grace et al., 2007). Multiple studies showed that the PRI signal is affected by the viewing and illumination geometry, 10 including the fraction of sunlit and shaded leaves seen by the sensor, canopy structure, and background reflectance (Barton and North, 2001; Nichol et al., 2002; Su′arez et al., 2008; Sims and Gamon, 2002; Louis et al., 2005; Drolet et al., 2008; Hilker et al., 2009; Middleton et al., 2009)." Not so sure about this. First of all I would argue that the limitations are not erally well known. In fact, I have the impression

that many people just use PRI without really considering the extraneous effects and then find or don't find some relationships, which of course can never be applied on a larger area, because the extraneous conditions will change Also, I would disagree that the influence of canopy structure and geometry (shading) on PRI is a limitation. quite the opposite is true. I would think that this is a valid signal we are seeing, because obvouisly PRI is driven by the amount of excess light, which is higher in a sunlit crown than in a shaded part of the canopy

p6942: "Albeit, from a previous study (Goerner et al., 2009) and preliminary experiments we know that correcting MODIS reflectances with readymade bidirectional reflectance distribution function (BRDF) parameters either has no effect on the PRI signal (when using POLDER/PARASOL based parameters (Bacour and Br′eon, 2005), see Fig. 2 in supplementary material) or only seems to increase 10 noise in the PRI signal (when using the MODIS MOD43 product, see Fig. 3 in supplementary material)." Hilker et al., 2009 (cited in this paper) gives an explanation for this. PRI changes as a funciton of the sun observer geometry. Hall et al., 2008 showed that this effect is a result of the photosynthetic downregulation in leaves, which is due to different proportions of shadow fractions. Conventional atmospheric corrections are not designed for multiangular acquisitions, hence they would destroy this directional observation of downregulation. As a result it is wrong to state that "readymade bidirectional reflectance distribution function (BRDF) parameters either has no effect on the PRI signal (when using POLDER/PARASOL based parameters (Bacour and Br′eon, 2005), see Fig. 2 in supplementary material) or only seems to increase 10 noise in the PRI signal ". It depends on how this is applied (see for instance hilker et al., 2009 and Hilker et al., 2008 RSE)

6947: "On a site level, LUEMOD17 has in every setting much less agreement with observations than LUEPRI. LUEMOD17, opt. performs much better. However, only for one setting, 5 the pooled FR-Pue observations, it is slightly superior to LUEPRI. The agreement between LUEMOD17.opt and the reference LUE increases slightly (with-

out changing any of the statements above) when using faPAR from MODIS collection 4 instead of 5 to calculate LUEMOD17 because the MOD17 parameters have been optimised based on collection 4 data (not shown)." Well that is not suprising though, because if at that site VPD and temperature are the limiting factors, of course the model would perform well. The point is, that this is not the case everywhere. You cannot really pick these few sites and then conclude that on a site level the MOD17 product is superior to MODIS PRI

6948: "The the fraction of PAR absorbed by the vegetation is rather stable throughout the year for the evergreen sites" What does that mean? Again I find it very problematic to use a satellite derived fapar to derive PRI and then compare it to MOD17

6950: "Unsurprisingly, the gain in accuracy through using PRI is highest for evergreen sites where changes in LUE are largely independent from greenness and 25 changes in leaf area (see also Running and Nemani, 1988; Gamon et al., 1992)." Why would LUE (not PRI) be dependent on leaf area and greeness in the first place. It is per definition the efficiency with which absorbed PAR can be used to produce biomass

6952 " Hilker et al. (2009) found that most of the directional effects on the LUE-PRI relationship can be attributed to atmospheric scattering." Hmm, not really . Hilker et al showed that since 6s uses an isotropic surface assumption which fundamentally disagrees with the concept that different amounts of shading will yield different lue values (because the amount of excess light energy varies). As a result, when you apply an isotropic atmopsheric model such as 6s, you may eliminate atmospheric effects but you will also eliminate some of the real PRI effects. MAIAC can work without an isotropic assumption and it can therefore eliminate atmospheric effects without destroying the pri effects Consequently you can measure surface PRI with maiac. This is the reason why forward AND backward scatter data showed good correspondence ot the multi-angular tower based data. Note that Drolet et al had to eliminate backscatter observations for that very reason

Interactive comment on Biogeosciences Discuss., 7, 6935, 2010.