Response to Referee #3.

Ref 3: Bevan, Los and North use primarily remote sensing observations to identify differences in the response of characteristically tall versus short vegetation to the 2003 European summer drought. The topic is of interest to the readers of Biogeosciences but it is not clear to me how the findings here add new knowledge to the existing studies of Teuling et al. and others. The plant physiological mechanisms that are at play are the subject of additional speculation rather than clarity and advancement of knowledge. The results may reflect the shortcomings of using NDVI to capture variations in LAI rather than the impacts of drought on different vegetation classes. I recommend linking the findings to the proposed mechanism, rooting depth, in addition to vegetation height (a surrogate for rooting depth here) to draw a clearer link between vegetation state variables and climate mechanisms.

Reply: The reviewer raises various issues:
- add new knowledge: (1) We used a vegetation height dataset that shows continuous variations in height, rather than a discrete change in height (short / tall) obtained from land-cover classifications. The reduction in NDVI and increase in DTR associated with more severe droughts diminish continuously with height (2) our results, obtained for most of Europe, are consistent with the site based flux-tower analysis and remote sensing analysis; we thus demonstrate that the results of the flux-tower study and remote sensing study are valid for a much wider area (most of europe). Other results presented in our paper extend temporal coverage which adds to the significance of the results. (3) Based on the recommendations of the reviewer we added an analysis of rooting depth in the revised submission.

Ref 3: The abstract, which could benefit from some numbers to improve citeability, already brings up important challenges from the plant physiological perspective. Tall vegetation is often deeply rooted in places like Europe. In other areas like semiarid grasslands the situation is not as straightforward and differs by annual/perennial, by growth form, by species, by soil type, and more. Once trees get very tall, the upper leaves are effectively under drought stress as studies on redwoods and other very tall trees have shown. Height is important variable for canopy conductance. I understand the appeal from the remote sensing perspective to have a simple ‘tall/short’ basis for comparison, but I hope that a detailed discussion of the pitfalls of this viewpoint is made clear later in the manuscript. Upon further read they were not. People heaven forbid may think that growing vegetation taller will make it more drought resistant, when the opposite is likely be true in many situations. Height is a covariate, not the most important explanatory variable, which appears to be rooting depth, and a real root person may argue that the volume of soil mined for water by the roots is more important than the maximum depth itself.

We emphasise that this study does concentrate on Europe where, as the referee states, tall vegetation is often deeply rooted. Redwoods do not grow in Europe. Also, we do not rely on a tall/short classification but use height as a continuous variable. Our analysis of height versus reduction in NDVI or change in DTR does not show saturation effects for the conditions studied.
We added an analysis of rooting depth data; the global gridded products show a decrease in rooting depth with vegetation height; rooting depths from the point data base (ca 550 observations globally) show no relationship. Studies looking at maximum rooting depth find a

positive relationship between vegetation height and rooting depth (Canadell et al., 1996; Schenk and Jackson, 2002). The vegetation height data set has much increased spatial density (see Fig. 4 in comparison with new Fig. 10d) and because of this we can calculate correlations between height and NDVI anomaly for 50x50 km cells which is not possible to do with the rooting depth data.

Canadell, J., Jackson, R. B., Ehleringer, J. B., Mooney, H. A., Sala, O. E., and Schulze, E. D.: Maximum rooting depth of vegetation types at the global scale, 108, 583–595, 1996.

Schenk, H. J. and Jackson, R. B.: Rooting depths, lateral root spreads and below-ground/above-ground allometries of plants in water-limited ecosystems, Journal of Ecology, 90, 480–494, 2002.

On 16077 line 3 please specify summer 2003.
Changed

Ref 3: I find the Introduction interesting and well-written in principle, but am having a difficult time at the moment understanding what is new if Teuling, Zaitchik, and other manuscripts have already ascertained the importance of tall versus short vegetation on surface biogeophysics and biogeochemistry during the 2003 summer drought in Europe.

Reply: As mentioned in response to the first comment this study is the first in which remotely sensed continuous vegetation heights are used rather than just the short / tall land-cover classes considered in previous studies. This allows a more explicit relationship between drought tolerance and vegetation height to be explored (e.g. we can explore saturation effects). Our study confirms that results from the flux-tower site study of Teuling et al. (2010) are valid over a great spatial extent.

Teuling, A. J., Seneviratne, S. I., Stockli, R., Reichstein, M., Moors, E., Ciais, P., Luyssaert, S., van den Hurk, B., Ammann, C., Bernhofer, C., Dellwik, E., Gianelle, D., Gielen, B., Grunwald, T., Klumpp, K., Montagnani, L., Moureaux, C., Sottocornola, M., and Wohlfahrt, G.: Contrasting response of European forest and grassland energy exchange to heatwaves, Nat. Geosci., 3, 722–727,2010.

The problem statement appears to be the difficulty in simulating these effects in models, and contributions from the remote sensing community in improving these simulations. My answer would be to improve plant physiological mechanisms in the models, but the manuscript uses instead surrogates like NDVI (sometimes related to LAI). At the same time, NDVI is sometimes useful and often linked to models. If the purpose of the manuscript is to use remote sensing as a means for model improvement, I understand the justification, but this could be made more clear.

The separate processes in models are generally well known (e.g. effect of albedo on energy budget; effect of greater rooting depth on water availability; increased canopy resistance and stomatal closure on water vapour exchange). The problem is in finding out how these effects combine. This study provides clear guidance as to what the (height dependent) response of vegetation to drought should be. In addition, important improvements can be made to the specification of canopy parameters by taking height into account. The use of vegetation height vs NDVI and DTR allows model improvement via model validation. We choose to use

NDVI because of its near linear relation to fAPAR, and its well understood characteristics. We think saturation of NDVI is unlikely for the case studied, since we observe a similar response in the DTR. Finally, NDVI also at allows the possibility for a longer time series at some stage analysis as it becomes possible to utilise AVHRR, VEGETATION or SeaWIFS data.

Shortly before section 2 begins one may also ask if NDVI can change very much in coniferous forests, compared to grasses because of the limits of leaf area change in response to drought and the saturating NDVI/LAI relationship. EVI (Huete et al.) would be more sensitive and it has never been clear to me why NDVI is so ubiquitous when EVI offers so many advantages.

1) Over the years we found that most needleleaf forests show a seasonal cycle which can be explained by some seasonality in needle leaf out during spring and dropping of needles during autumn, by seasonal variations in the understory and by seasonality in deciduous trees that are mixed in with evergreen trees.
2) The reason we did not use EVI is that, in our opinion, its interpretation is not as straightforward as that of NDVI. Some studies have found a small improvement in MODIS EVI over MODIS NDVI when data were compared with ground measurements (Sjostrom et al 2009). EVI however appears to be less comparable across different sensors (MERIS EVI / VEGETATION EVI / MODIS EVI) and more difficult to cross calibrate because of differences in the way the blue band is processed (Fensholt et al 2006). The most important hesitation to use EVI is related to results found in the Amazon, where EVI shows higher than normal values during the 2005 drought (Saleska et al 2007). This increase in greenness found in EVI could not be confirmed in above ground biomass measurements since these showed a decrease in response to the 2005 drought (Phillips et al 2009). There is further evidence that the EVI failed to reproduce both the 2005 and 2010 droughts for large parts of the Amazon; whereas the NDVI did indicate a reduction in greenness (Samanta et al., 2010). Until this issue is resolved one way or another we prefer to base our analysis on NDVI.

Phillips, O., et al, 2009, Drought sensitivity of the Amazon rainforest. Science, 323:1344–1347, doi: 10.1126/science.1164033.

Saleska, S. R., K. Didan, A. R. Huete, and H. R. da Rocha (2007), Amazon forests green- up during 2005 drought, Science, 318(5850), 612, doi:10.1126/science.1146663.

Samanta et al., (2010), Amazon forests did not green-up during the 2005 drought, Geophys. Res. Lett., Vol. 37, L05401, doi:10.1029/2009GL042154.

Sjostrom, M. et al, 2009, Evaluation of satellite based indices for gross primary production estimates in a sparse savanna in the Sudan. Biogeosciences, 6:129–138, 2009.

Please define ‘resilient’, which has different meanings to different fields of science. A quantitative definition may be an improvement, something like the length of time that previous LAI was recovered? (This gets at the challenge of the definition, resilient could have time units or LAI units or biomass units etc.)

As we have not explicitly been measuring the vegetation ability to recover we will replace resilience with tolerance.

In the beginning of page 16081 increased temperatures and decreased precipitation are not always linked, but often are during large droughts. Please specify that this statement is relevant to this event but not universal.

We will add ‘although decreased rainfall and increased temperatures are clearly often linked’.

There are few minor usage errors throughout the manuscript but please note ‘within a 50 km in Germany’ on 16802 line 25 and a few other minor examples.

Corrected.

On page 16083 I would hope for the reader that more than just a working hypothesis can be delivered. Could rooting depth databases be mined? Could representative rooting depth for different vegetation types (i.e. Jackson et al.) be explored?

We added an analysis of vegetation height vs rooting depth (Schenk and Jackson, 2002; Canadell et al., 1996) and soil water availability (Kleidon, 2011) and the results were mixed. Canadell et al. (1996) report that across all biomes on a global basis the average ***maximum*** *depths at which trees, shrubs, and herbaceous plants are able to grow roots are 7.0 ± 1.2 m, 5.1 ± 0.8 m, and 2.6 ± 0.1 m which supports the our rooting depth hypothesis. However, the International Satellite Land Surface Climatology Project, Initiative II (ISLSCP II) (Hall et al., 2006) data on root depths (minimum of 95 % or 3 m) and soil water availability show a different picture. Point root-depth (minimum of 95 % and 3 m) data (Schenk and Jackson) did not allow any conclusions to be drawn on the relationship between vegetation height and root depth. There are limitations to the root data. Datapoints are very sparse (by contrast, the spatial coverage of the vegetation height data allows correlation analysis within in 50 km square) and the gridded data are coarse and are interpolated based on land-cover classifications. We find a negative correlation between vegetation height and the soil-water availability data (Kleidon, 2011); this may be because regions with generally taller vegetation have greater water availability (fewer droughts) and so in a model or optimisation sense require less storage to maximise productivity. In this case tall vegetation is able to maintain productivity better than short vegetation in spite of lower soil-water availability. Scatter plots of these data are included in the paper.*

Canadell, J., Jackson, R. B., Ehleringer, J. B., Mooney, H. A., Sala, O. E., and Schulze, E. D.: Maximum rooting depth of vegetation types at the global scale, 108, 583–595, 1996.

Schenk, H. J. and Jackson, R. B.: Rooting depths, lateral root spreads and below-ground/above-ground allometries of plants in water-limited ecosystems, Journal of Ecology, 90, 480–494, 2002.

Kleidon, A.: In Hall, Forrest G., G. Collatz, B. Meeson, S. Los, E. Brown de Colstoun, and D. Landis (eds.). ISLSCP Initiative II Collection. Data set. Available on-line http://daac.ornl.gov/] from Oak Ridge National Laboratory Distributed Active Archive Center, Oak Ridge, Tennessee, U.S.A., doi:10.3334/ORNLDAAC/1006, 2011.

Also, at what height is the diurnal temperature range (DTR) measured? Might the forests, regardless of drought, have a smaller DTR than non-forests because of their closer coupling to the atmosphere (Jarvis-McNaughton omega), or larger DTR because of their lower albedo, or lower DTR because they may contribute to a larger boundary layer? The discussion regarding DTR is important and worthy of further exploration to clarify the mechanisms.

We added clarification of the DTR (see data section); DTR is derived from surface skin temperature. The analysis of DTR was expanded to other years; non-drought years show no difference between short and tall vegetation.

It would also be revealing to go into additional detail regarding the groupling and use possibly an ANOVA to further identify if different classes have different NDVI and DTR dynamics during drought. Or even a classification tree. Is 'Forest' the most logical distinction with regards to the effects of drought? It might be, but other deep-rooted vegetation classes like dryland shrub/scrub may be more like forests than other short-statured vegetation and lend support to the rooting depth argument.

The land-cover analysis was added as a base line to compare against; but our main purpose was to see how height as a continuous variable would relate to a drought response. We believe a further distinction in land cover classes is too much of a distraction, and does not add to the question we would like to answer.

Most of the Discussion section sounds like expository material best found in an Introduction section. The findings of the manuscript are not discussed in the context of other studies.

We rewrote the discussion and focused on various mechanisms that could explain the observed dependency of NDVI change and DTR with height. We moved the first part of the discussion to the introduction.

In Figure 3, a pdf (that sums to one) rather than a histogram may be an improvement.
Accepted.

Figure 4 is interesting but more description of the flight lines is necessary to explain it to the reader.
We add footprint diameter size (~70 m) to the description in the data. And add 'Each point represents a height retrieval from data collected between 2003 and 2009, with an along-track sampling rate of about 172~m'. to the figure caption.

Red and green should not be used simultaneously in Figure 8. A different color can be chosen to assist our colleagues with red-green colorblindness.

The colour scheme will be revised.