RESPONSES TO REVIEWER'S COMMENTS FOR THE PAPER:
Modelling post-fire vegetation recovery in Portugal
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Responses to Referee #1

We have made an effort to answer to all the issues raised by the reviewer. In order to facilitate the reading we have split and numbered all different items raised by the reviewer.

RC 1 – "The paper does not represent any substantial contribution to the scientific progress in forest fires, mainly because this manuscript does not introduce anything new really robust to that published by Gouveia et al. 2010."

As clearly stated in the abstract and in the introduction, the main goals of the paper were: (i) to assess the accuracy of the vegetation recovery model previously developed by Gouveia et al. (2010); (ii) to assess the model's performance, namely its sensitivity to initial conditions, to the temporal length of the input dataset and to missing data; and (iii) to study vegetation recovery over areas affected by large wildfire events that occurred in the fire seasons 2003-2005.
The first goal is certainly innovative at least in the sense that, to the best of our knowledge no attempt was made to check the performance of the proposed model beyond the period of model adjustment. The second goal is essential to insure the robustness of model results, and therefore from the point of view of applicability of the model. In fact, the exponential nature of the proposed model could lead to misleading results if parameter adjustment is not performed in an appropriate way. This requires an assessment of sensitivity both to initial conditions and to the availability of data within the study period. Finally, the model was applied to nine fire events (i.e. two fire events already considered in Gouveia et al. (2010) and seven new fire events) and new results are presented for two of the new cases studied.
All the above mentioned issues will be addressed below in detail and it will be demonstrated that the paper does indeed contribute to a better insight in the process of vegetation recovery following large fire events.

RC 2 – "I think that the longer length of the time series or the application of the method to three new fires is not enough to accept this manuscript as a innovative or original paper."

The extension of the time series was not intended as an innovation but as an indispensable mathematical procedure. In fact, the authors consider that a comparison of results obtained when applying the model of Gouveia et al. (2010) to time series of different lengths is an indispensable procedure to assess the robustness of the **original** (proposed) model. This aspect was already raised in the final section of Gouveia et al. (2010) where it is stated that "the proposed model only requires time series of NDVI of a relatively modest length, the stability of estimates being ensured partly by the fact that a single parameter is estimated by linear regression from the data". It is worth stressing that this statement was based on the data **available at the time** (i.e. up to 2006, which does not include the entire recovery period) and

therefore checking its validity is essential in what respects to the applicability of the model under more general conditions. Besides the exercise of extending the time series was also required to assess the sensitivity to initial conditions and to missing data.
In what respects to **innovation**, it is worth noting that results from the three distinct types of sensitivity studies led to an **improvement** of the methodology namely in what respects to a **new way** of setting of parameter *a* as well as to a **new way** of dealing with post-fire data associated with distinct phenology.
As stated before, the number of cases analysed ads up to **seven new** fire episodes and not to just three new ones as stated by the reviewer. As shown in both Table 1 and Figure 1, nine distinct burnt scars were chosen, which correspond to nine different fire events that occurred in three different fire seasons (2003, 2004 and 2005) with different climatological characteristics, one of the years including a very strong heat wave (2003) and another one including a severe drought (2005). Moreover, the nine cases are distributed in distinct regions of the country with distinct topographic characteristics and land-cover types.
Finally, in what respects to **innovation**, it may be worth recalling that the analysis performed led to the introduction of a **new parameter** ($NDVI_{DIFF}$) that to the best of our knowledge has not been used before in the literature.

RC 3 – "Morover, the relationships between recovery rates and fire damage or previous land cover types are not scientifically robust. Their conclusions are based only on a qualitative assessment and on erroneous assumptions resulting in confounding results."

This comment is unclear since the referee does not explain what is meant by "not scientifically robust". Nor does he explicitly mention what qualitative arguments and erroneous assumptions he is referring to. Nevertheless the authors would like to point out that a serious effort was made to use statistical techniques that have proven to be robust, namely unsupervised classification and statistics derived from sample percentiles (e.g. inter-quartile ranges). Moreover, whenever required, confidence levels are shown both in Tables and in Figures. Finally consistency of obtained results was systematically checked against independent data (e.g. burnt areas from the national service, CLC2000 together with the updated CLC2006).

RC 4 – "In the case of recovery vs fire damage, as the authors indicated in the introduction, and later, they contradicted, pre-post NDVI difference introduce error in fire severity/damage estimation. Using an absolute measure as this could lead to incorrectly characterizing burn severity in pixels which contain less pre-disturbance chlorophyll on average than the surrounding landscape due not only to differences in the amount of cover but differences in the type of vegetation present (Miller and Thode 2007)."

The authors are in total agreement with the referee about the existence of problems associated to the use of an absolute measure of NDVI difference when evaluating fire damage. This was indeed the reason why a relative measure of NDVI ($NDVI_{DIFF}$) was used in our work instead of an absolute measure. As shown in Eq. 4, $NDVI_{DIFF}$ consists in the normalised difference of pre and post-fire NDVI and using such normalized measure (instead of an

absolute measure) has the advantage of mitigating the undesirable effects associated to less pre-disturbance chlorophyll which were mentioned by the reviewer.

The reviewer is wrong when he states that "in the case of recovery vs fire damage, as the authors indicated in the introduction, and later, they contradicted, pre-post NDVI difference introduce error in fire severity/damage estimation". This is simply not true as it was clearly stated in lines 11-14, p. 4569 of the original manuscript. However, for the sake of clarity, an explicit reference to the effect of low levels of pre-disturbance chlorophyll will be introduced in the manuscript both in section 1 and in section 3.4. Accordingly the last statement of the second paragraph in p. 4563 (lines 19-21) now reads:

> *When using differencing methods based on an absolute measure of NDVI , low levels of pre-disturbance chlorophyll may, however, introduce some confusion in the measurement of absolute change, raising the need for relative measures such as the one proposed by Miller and Thode (2007).*

whereas the last statement of the first paragraph of section 3.4 (lines 11-14, p. 4569) was changed as follows:

> *In order to attenuate the differences induced by the presence of several vegetation types, as well as to low levels of pre-disturbance chlorophyll (Miller and Thode, 2007), a normalized difference of pre- and post-fire mean NDVI values was used to evaluate fire damage, as given by (...).*

Incidentally, the normalized parameter $NDVI_{DIFF}$ was used as a measure of **fire damage** (and not of burn severity as mentioned by the reviewer). Differences between fire damage and burn severity are in fact explicitly mentioned in section 3.4 (lines 5-11, p. 4569) of the manuscript

> *The difference in vegetation activity, as obtained by NDVI, during the pre and post fire periods, was used to estimate fire damage, instead of burn severity, since the latter definition is often associated with a large amount of factors which include the impacts on atmosphere, on soil composition, the amount of organic material consumed or by effects in vegetation such as the amount of char on shrubs, scorch height and crown scorch, tree mortality or the presence of colonizers (Jain and Graham, 2004; Miller and Thode, 2007).*

Last, but not least, it may be noted that the methodology originally proposed in Gouveia et al. (2010) **does not require having to consider the surrounding landscape** (as mentioned by the reviewer) in order to estimate post-fire vegetation recovery or fire damage.

RC 5 – "Hence, the relationship between severity/damage (NDVI diff)and recovery (derived from NDVI)are mainly based on NDVI byself and then, it is a circular relationship without any physical basis."

The authors fully agree with the reviewer when he states that recovery is based on NDVI and that severity/damage is based on $NDVI_{DIFF}$ which is also based on NDVI. However the reviewer's statement that the fact that both parameters are derived from NDVI leads to "a circular relationship without any physical basis" is simply not true.

Let us first demonstrate why there is no circular relationship. As clearly defined in the manuscript,

1. Recovery is the time elapsed from the fire event up to the time when lack of greenness (y), which is an NDVI-derived quantity, reaches a pre-defined threshold – it is therefore an NDVI-based quantity, i.e. a quantity based on a zero-order difference of NDVI (the same is to say on NDVI itself as correctly stated by the reviewer).
2. On the contrary, severity/damage is related to $NDVI_{DIFF}$ which is based on a temporal difference of NDVI, i.e. on a first-order difference of NDVI.

The reviewer is therefore plain wrong when he claims that there is a circular relationship in any reasoning involving a given quantity (say recovery) derived from a given quantity (say NDVI) together with another quantity (say severity/damage) derived from a first order difference of the previous one (say $NDVI_{DIFF}$). Such a (wrong) statement is in fact equivalent to stating that one is led to a circular relationship whenever using a quantity based on price together with another quantity based on inflation... or using a quantity based on position together with another quantity based on average speed!

Now let us demonstrate that there is indeed physical basis in our reasoning. The usefulness of NDVI to study problems related to vegetation dynamics and to vegetation recovery has been demonstrated by a large number of studies, some of the most relevant are mentioned in the introduction of our manuscript. In fact, suggesting (without demonstrating of referring to any previous studies) that the use of relationships derived from, or based, on NDVI is deprived of physical basis is refuting results from a large number of studies such as those by Myneni et al. (1995), Díaz-Delgado et al. (1998), Días-Delgado and Pons (2001), Hope et al. (2007), Fox et al. (2008), Gouveia et al. (2009), Gouveia et al. (2010). Besides, our proposed use of a normalized difference of satellite data to evaluate fire damage was inspired by other works (e.g. Epting and Verbyla, 2005; Kokaly et al., 2007; Miller and Thode, 2007; De Santis and Chuvieco, 2007; Fox et al., 2008; Miller et al., 2009) which were accepted by the scientific community and are considered as relying on a physical basis.

RC 6 – "In relation to recovery vs land covers authors said that recovery was faster in regions with higher cover of transitional woodlands-shrublands than in conifer woodlands; but results indicated (table 4) that clusters 1 and 3 in RIII showed similar % of coniferous forests but different recovery times; rejecting such hypothesis, and giving place to incongruencies between data tables and results."

The reviewer is correct when he states that the percentage of coniferous woodlands is similar in cluster 1 and 3 in the case of region RIII. The reviewer is also correct when he states that

recovery times of these two clusters are of different magnitude. These two statements are in fact a direct result of Table 4 when the considered percentages of coniferous and transitional woodlands **are the ones observed on the year 2000**. This is a crucial point since it implies that the "incongruencies between data tables and results" mentioned by the reviewer are **nothing but apparent**. This is clearly explained in the original manuscript (p. 4580, lines 23-29) where it is stated that:

> *For RIII, the area occupied by coniferous dramatically decreased from 71%in 2000 to 16%in 2006, while transitional woodland-shrub increased more than four times from 16 to 68%. Additionally, the analysis of vegetation composition of recovery time clusters (Table 4) indicates steeper increase in transitional woodland-shrub areal coverage in the centroids corresponding to lower and intermediate mean recovery time, especially in the latter, where areal coverage is more than 8 times higher in 2006 than in 2000.*

A **correct** reading of Table 4 **further** implies considering the percentages of coniferous and transitional woodlands that were observed on the year **2006,** which reflect the new land cover composition that was developed during recovery. In fact, taking this aspect into account no "incongruencies" are to be found since clusters 1 and 2 were almost completely replaced by transitional woodland-shrub. When analysing recovery times one has therefore to take into account land cover composition both previous to the fire event and following recovery.

This aspect will be nevertheless emphasized on the revised manuscript by introducing new statements (highlighted in bold) in the text beginning at line 15, p. 4580 and ending at line 6, p. 4581:

> ***Results for RVII*** *suggest that vegetation recovery tends to be quicker for transitional woodland-shrub while coniferous forests tend to recover more slowly, which is consistent with the distinct plant traits that characterize each vegetation type. Mediterranean dwarf/shrub vegetation is in fact well adapted to fire, being composed by many species of resprouters which recover very rapidly after fire events. On the contrary, coniferous rely on post-fire seed germination to recover after fire, a process that requires more time.* ***This is however not the case in RIII where the cluster associated to the longest recovery time (63 months) presents a land cover composition very similar to the cluster with the shortest recovery time (32 months). The contradiction is nevertheless apparent when one takes into consideration the land cover composition that developed during the recovery process.*** *In order to evaluate whether the faster recovery of transitional woodland-shrub* ***was indeed associated to*** *changes in ecosystem composition, CLC2000 (pre-fire) and CLC2006 (post fire) data for RIII were compared, as shown in Table 3. For RIII, the area occupied by coniferous dramatically decreased from 71%in 2000 to 16%in 2006, 25 while transitional woodland-shrub increased more than four times from 16 to 68%. Additionally, the analysis of vegetation composition of recovery time clusters (Table 4) indicates steeper increase in transitional woodland-shrub areal coverage in the centroids corresponding to lower and*

intermediate mean recovery time, especially in the latter, where areal coverage is more than 8 times higher in 2006 than in 2000. The area suffered a dramatic change in composition of the ecosystem, as coniferous forest was replaced by transitional woodland-shrub after the fire, a result that supports those obtained in the study of the dependence of recovery time with vegetation type. Results are further consistent with previous studies (Naveh, 1975; Pérula et al., 2003; Viedma et al., 2006) documenting the trend for domination of dwarf/shrub vegetation following fire events, due to its rapid regeneration process.

RC 7 – "The results are plenty of parragraphs that later, they are repeated in discussion. The paper is full of assumptions without significant evidences. There are a lot of errors in figures and captions. Hence, I think this paper would be a oral presentation in a congress or gray paper, and I reject it to be published as a top scientif paper."

There were errors in the editing process, which the authors have duly corrected in a comment to the on-line discussion. Since the reviewer has shown to be so sensitive to errors that were beyond our control, it might be worth mentioning that the figures were correct in the original document submitted to edition.

As for the other considerations by the reviewer, it is the authors' opinion that making general comments about errors (editorial or other) without indicating precisely to which errors one is referring to is not appropriate in a serious review. Nevertheless, an effort will be made by the authors to improve the quality of the paper.