Interactive comment on “Impact of precession on the climate, vegetation and fire activity in southern Africa during MIS4” by M.-N. Woillez et al.

Response to reviewers.

We thank both reviewers for their positive comments and their suggestions to improve the manuscript. For easer reading we have reproduced below the reviewers’ comments in black and our responses are provided in blue.
Please also find attached the revised version of the manuscript.

Anonymous Referee #1

General comments

The study is well designed, well written and adresses a scientifically interesting topic with nice outcomes. The approach is well evaluation including an evaluation of the downscaling of driving data and the model’s present day performance. I therefore have only few comments. The main improvement I can suggest is to include a two additional datasets in the evaluation. Including a tree cover and burned area dataset based on remote sensing could strongly improve and simplify the evaluation for present day. Moreover it could help in the discussion.

Specific comments
p. 5392, l. 19/20: that fire activity strongly depends on vegetation type is well known. I suggest to be more specific: fire activity in southern Africa during MIS4 is mainly driven by vegetation cover.

The sentence has been modified accordingly.

p.5395 l.14-17: SPITFIRE does not resolve single fire effents, I am therefore not fully convinced the high resolution is needed, because fires are local scale events, but rather because the environmental conditions (moisture, vegetation type, fuel amount and type) may change. If all the drivers of the model would be spatially homogeneous, the fire patterns would not change with resolution.

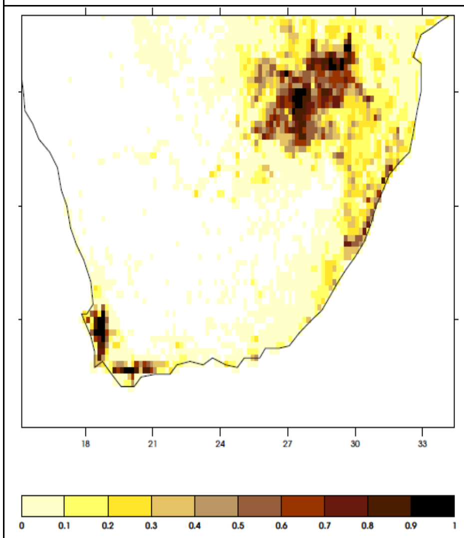
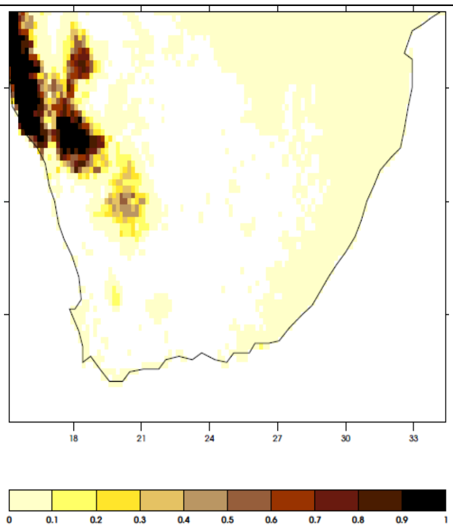
We agree with this comment, this was the reason for using downscaling. The sentence has been developed as follows:
"Fire occurrence depends on environmental conditions at local scale (moisture, vegetation type, fuel amount and type). Using the relatively coarse outputs from the GCM to force LPJ-LMfire would lead to unrealistic simulations of burnt areas. Therefore we use a statistical downscaling method to increase the spatial resolution from the GCM simulation. The high resolution climatic fields we obtain are used to force LPJ-LMfire and simulate fire activity at local scale".

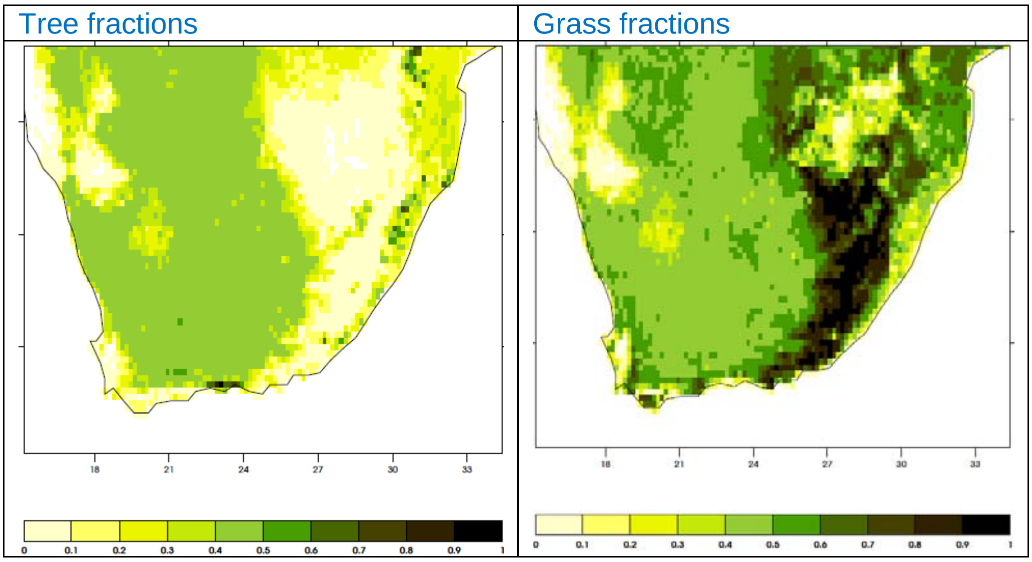
Figure 4a: It is a little difficult to compare figure 4a and b. Instead (or in addition) it would be very interesting to see bare soil, trees and grasses from remote sensing data. As there are datasets available on tree cover, there is no need to compare the modelled tree and grass cover to biomes. This is available based on modis data from the Hansen et al. (2003) dataset.

The Hansen et al (2003) dataset only provides tree cover. Therefore, we have used the Land Cover Types from MODIS data (average over the years 2001 to 2012). (https://lpdaac.usgs.gov/products/modis_products_table/mcd12c1).
This dataset unfortunately provides land cover types and not vegetation fractions. The following table gives the different types available in the dataset and the choices we made to transform this land cover types in fractions of bare soil/trees/grass:

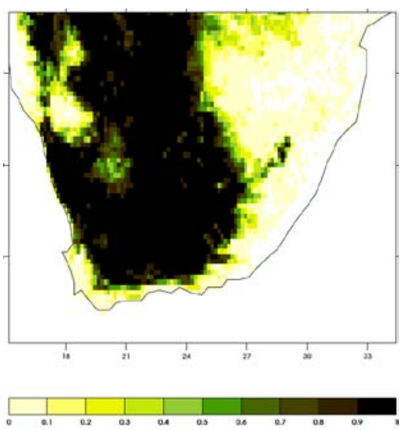
Land cover type from MODIS	Vegetation fractions
Evergreen Needleleaf forest	100% trees
Evergreen Broadleaf forest	100% trees
Deciduous Needleleaf forest	100% trees
Deciduous Broadleaf forest	100% trees
Mixed forest	100% trees
Closed shrublands	100% trees
Open shrublands	50% grass + 50% trees
Woody savannas	60% grass + 40% trees
Savannas	70% grass + 20% trees + 10% bare soil
Grasslands	100% grass
Permanent wetlands	100 % grass
Croplands	100% Human land use
Urban and built-up	100% Human land use
Cropland/Natural vegetation mosaic	100% Human land use
Snow and ice	100% bare soil
Barren or sparsely vegetated	100% bare soil

This classification leads to the following figures (with the same colorscale as Fig.4 of our manuscript):

Human land use fractions	Bare soil fractions
	



We can see on the first map a strong impact of human land use in the Cape region, along the West coast and in the North-East of southern Africa. Human impact is probably underestimated since the data do not show all human disturbances on the natural vegetation such as cattle grazing, forest managing and anthropogenic fires. In these regions, satellite data cannot be compared to model results.
But we also notice that the maps of grass and tree fractions obtained from the MODIS dataset are quite different from our modeled results.
MODIS grass fractions are higher in the East (eastern grassland region), as already mentioned in the manuscript from the comparison between Fig.4.a and Fig.4.b, but also over most parts of the western and central region. Similarly, the tree map shows tree fractions at 50% in the western and central region.
These areas are actually classified as “open shrubland” (cf Fig below) (corresponding to the Karoo region and part of the fine-leaved savanna, on Fig.4.a of the manuscript). The way we transform this biome in trees and grass fractions is obviously not appropriate. LPJ-LMfire does not simulate shrubs and we do not have any data to define accurately the percentages of bare soil/grasses/trees that should be attributed to this biome.



Fraction of grid-cell occupied by the
land type "open shrubland" in the MODIS database

We think that maps plotted from the MODIS data and our modeled results are not directly comparable and do not really help in the discussion since the choices to make the

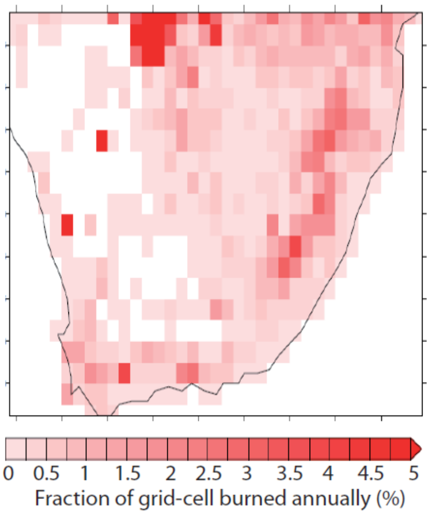
correspondence between land cover types and soil/trees/grass fraction is always questionable and more or less arbitrary.
We consider that since no satellite dataset providing soil/trees/grass fractions is available (to our knowledge), keeping the biome map on Fig.4 is the best way to show present-day observed vegetation, even if the comparison between Fig.4 a) and b) is not straightforward.
We leave the decision concerning the addition of the transformed MODIS dataset to the manuscript or to the Supplement to the Editor.

Figure 5: The same figure with the Hansen et al. (2003) dataset would be very useful and interesting.

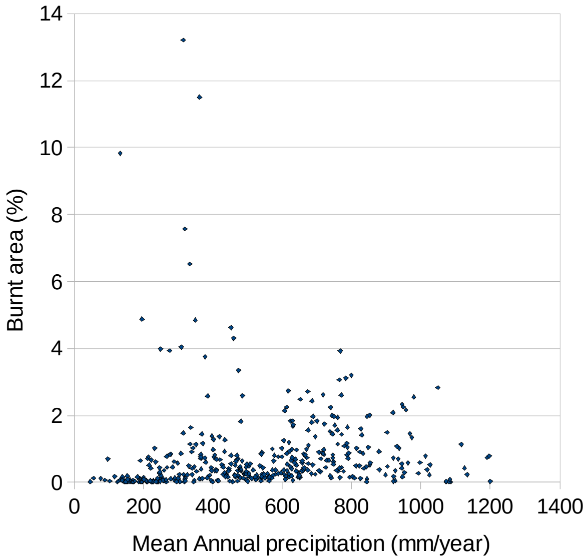
We agree, but as explained above we miss a dataset providing not only tree fractions but also grasses and bare soil fractions.

Figure 6: include a burned area dataset, for instance GFED which is already cited in the text. Although the absolute numbers of burned fraction are quite different, the authors suggest due to the lack of human influence in the model, the geographic patterns and the relation between burned fraction and precipitation should be similar.

Here are the GFED values averaged over the period 1997-2011 (spatial resolution 0.5°x0.5°,area without fire in white):



We have also plotted the GFED values averaged over the period 1997-2011 (spatial resolution: 0.5°x0.5°) against the total mean annual precipitation from the IRI LDEO database (see below) (http://www.ldeo.columbia.edu/research/other/irildeo-climate-data-library) :



Both above figures are very different from Fig.6 of our manuscript. Mean annual burned fractions remain below 2% over most parts of southern Africa, and no clear relationship appears between the annual burned area and the mean annual precipitation. The highest burned fractions are observed for mean annual precipitation between 200 to 500 mm/year, similar to our modelling results, but we have only few data available for this precipitation range.

The high discrepancy between our modelling results and observed data, suggest a high anthropogenic impact on the fire regime in southern Africa (either directly or indirectly through vegetation changes), as already mentioned in the manuscript. Therefore adding the above figures to the manuscript would not really improve the discussion.
The above map plotted with the GFED data has been added as a supplement.

Figure 9: The information of figure 9 is already available from Figure 7, I would suggest to remove this figure, as the number of figures in the paper is quite large.

Fig.7 only shows summer and winter mean differences, at the coarse GCM resolution. Fig.9 shows the entire seasonal cycle of temperature and precipitation obtained over southern Africa after the downscaling and therefore provides more information than Fig.7. In particular, Fig.9 shows that climatic changes also occur during March and November.

p. 5409 l. 11-17: also here including the tree cover dataset would help the discussion. Higher tree covers in the model may be attributed to anthropogenic influences.

See above for the issue of including a satellite dataset. We could introduce a tree cover dataset, but no data concerning bare soil and grasses.

Technical comments

The manuscript contains some typos that should be corrected

All the typos mentioned by Referee#2 have been corrected in the manuscript.

References
Hansen, M. C., R. S. DeFries, J. R. G. Townshend, M. Carroll, C. Dimiceli, and R.

A. Sohlberg (2003), Global Percent Tree Cover at a Spatial Resolution of 500 Meters: First Results of the MODIS Vegetation Continuous Fields Algorithm, Earth Interactions, 7 (10), 1-15.

Anonymous Referee #2

The manuscript "Impact of precession on the climate, vegetation and fire activity in southern Africa during MIS4" by Woillez et al. presents novel insights on the interactions between climate, vegetation, and fire disturbance in southern Africa during Marine Isotope Stage 4. By combining the general circulation model IPSL_CM5A with the dynamic global vegetation model LPJ-LMfire, the authors investigate how changes of the precession index over the course of MIS4 affected the African monsoon and precipitation patterns, and present the resulting effects on vegetation cover and fire activity. Their findings from this study emphasize that natural fire activity in southern Africa strongly depends on the prevailing vegetation type, which in turn is driven by precipitation patterns.

General comments:

The manuscript is well-written and meets the requirements for publication in Climate of the Past. The authors nicely combined two different models to address an interesting scientific question, and present their methodological approaches as well as their results in a mostly concise and transparent way. However, some open questions and points that would benefit from further clarification remain in this discussion version of the manuscript and are pointed out below under "Specific comments". In addition, a number of typos and language issues that need correction are listed in the "Technical comments" section. With respect to the figures presented in this manuscript, Fig. 4 is not very useful to evaluate the performance of LPJ-LMfire under present-day conditions as it is rather hard to compare the quantitative cover fractions in Fig. 4b to the qualitative biome categories in Fig. 4a. I therefore suggest to replace Fig. 4a with a map showing fractional tree cover based on remote sensing products, e.g. the Global Land Cover Facility (GLCF) tree cover data set (DeFries et al., 2000), and base the discussion of the model performance on that comparison.

Specific comments:

5393, line 1: "which would change the amount of fuel": please briefly mention that a shift in vegetation composition is likely not only going to affect the fuel quantity, but also the qualitative composition of the fuel, e.g., the ratio from coarse fuels to fine fuels and the flammability of fuel components, which in turn will affect fire intensity and frequency.

These precisions have been added to the text.

5392, line 11: "during Marine Isotopic Stage 4" - please define the duration of Marine Isotopic Stage 4 in years BP once in brackets, to make it easier to see right away what time frame you are talking about.

The duration has been added in brackets.

5394, line 17: "This interpretation has been confirmed by many numerical models for the Holocene": You are interested in the Pleistocene, not the Holocene. So does the same explanation also hold true for the Pleistocene? Are there any studies using numerical models that have focused on the Pleistocene with respect to the relationship between precession index, pressure, and monsoonal strength? If so, please cite, or else indicate why it is a valid assumption that the situation during Pleistocene and

Holocene are likely to be comparable.

Masson et al (2000) performed climatic simulations at 175 ka BP (marine isotopic stage 6.5) and showed that high insolation can generate an increase in monsoon activity even with surface glacial conditions.
The relationship between insolation and monsoon intensity which has been largely studied for the Holocene seems to remain a valid assumption for strong glacial conditions such as MIS6.5. Therefore we can also expect this assumption to remain valid for MIS4 (when ice sheets are smaller than during MIS6.5).
The reference to Masson et al (2000) has been added in the manuscript.

5396, line 4/5: "the vegetation is prescribed and fixed": on what basis? present-day climate?

The vegetation is fixed according to its present-day distribution, including agriculture. This is now specified in the text.

5397, line 13: "The fractional coverage reflects both the productivity and individual density of the PFTs": The fractional coverage of the PFTs is not necessarily reflecting the productivity of a PFT. A specific PFT can have a high fractional coverage albeit having low biomass values (i.e., low productivity) and a high individual density (small average individual, e.g. due to frequent disturbance which keeps tree PFTs from growing to tall trees). A better measure for the productivity of a specific PFT than its fractional coverage therefore would be its biomass. The fractional coverage is often a better indicator for the competitive balance between different PFTs present in the same grid cell.

We agree with this comment. A high fractional coverage may be the result of high individual biomass or high individual density (or both) and these parameters vary independently.
This sentence merely refers to the calculation of the fractional coverage in the model (Krinner et al, 2005):

$v = v_{max}(1-e^{-k\lambda})$ and $v_{max} = \rho c$

with v the fractional cover of a PFT, v_{max} the maximum foliage fractional cover, λ the LAI, wich depends on the leaf biomass, ρ the density of individuals and c the crown area, which depends on the biomass of the individual.

The sentence was modified as follows:
"The fractional coverage of a given PFT depends on both the productivity and individual density, which vary independently".

5397, line 14: "The spatial resolution is the same as the climatic forcings...": Please phrase this more clearly. Is this the 3.75_ x 1.9_ spatial resolution used for the atmosphere simulations in the GCM that you are using when running LPJ-LMfire?

The spatial resolution of any LPJ-LMfire simulation only depends on the spatial resolution of the climatic forcing fields chosen by user. Here we performed simulations only over southern Africa, at a spatial resolution of 0.16° (see section 2.2.2). We have added "...climatic forcings chosen by user, 0.16° in our case (see section 2.2.2) "

5398, line 2: "to a spatial resolution of 0.16_": is there a specific reason to chose exactly this resolution?

This resolution corresponds to the spatial resolution of the CRU data used in the downscaling method. This is now in the text.

5398, line 14: "...thus keeping present-day interannual variability": Do you have any information whether the interannual variability during your simulation time period (MIS4) would have been comparable to present-day interannual variability.
To perform LPJ-LMfire simulations we need climatic forcings with inter-annual variability. Using a constant mean climate is expected to lead to strong biases in the results. As we did not have any MIS4 climatic simulation at high resolution to extract inter-annual variability we chose, by default, to keep the present-day one. We agree we do not have any information on how much the modern and MIS4 climatic variability differ at the fine scale reached thanks to the downscaling procedure. This assumption is now made clear in the text, the sentence "We add the anomaly to the detrended version of the 20th Century Reanalysis climatology, thus keeping present-day interannual variability." has been replaced by "We add the anomaly to the detrended version of the 20th Century Reanalysis climatology. We therefore make the assumption that there has been no change in interannual variability, which is the best approach we could follow here given the available data. It would be interesting to test this hypothesis with high resolution RCMs in a future work."

5400, line 8: "defined as cubic splines in our case": on what did you base your decision to use cubic splines as the most suitable functions in this case?

The choice of cubic splines functions was made following Vrac et al. (2007). As explained in this publication, spline functions are piecewise third order polynomial functions evaluated at four knots. Each function has at most 12 parameters (please refer to Vrac et al. 2007 for more details). Such a model has both a great flexibility and a (relatively) limited number of parameters to compute. This is now specified in the text.

5400, line 15: "... at a regular spatial resolution of 0.16_...": I guess this is the reason why you chose the 0.16_ spatial resolution as your high-resolution spatial scale? If so, please mention this on p. 5398, line 2, to justify why you chose exactly that spatial resolution.

Yes, this is now mentioned p.5398.

5401, line 15: "If the predictors values [...] are outside the calibration range..." How often does this happen? I think this is important to know because it influences the uncertainty of the downscaling procedure.

This issue is discussed in section 3.1. As mentioned is the text, most of the predictors values are within the range of calibration, except temperature (see below for the issue of temperature)

Statistical downscaling method using GAM: What is the advantage of using this method of downscaling compared to other methods also including topographic effects (e.g., thin plate spline, Kriging...)?

The Kriging method is only a spatial interpolation of the statistical parameters of the variables, taking into account only topography; whereas GAM also takes into account other parameters, which we have selected because they had a physical basis (see Tab.2). Generally speaking other downscaling methods are often more constrained than the GAM we use here. A comparison of different downscaling methods was made by Wilby et al. (1998) (***Statistical downscaling of general circulation model output: a comparison of methods***) and showed the advantages of GAM. This reference is now in the manuscript.

Table 2, and explanation on page 5403: The % of variance explained for precipitation is rather low compared to the variance explained for the other three parameters. Why would that be so? Is it because the AOGCM does not do very well in simulating precipitation to start with, so that the derived predictor for precipitation is already not very good, or is it more likely due to the spatially highly variable nature of precipitation?

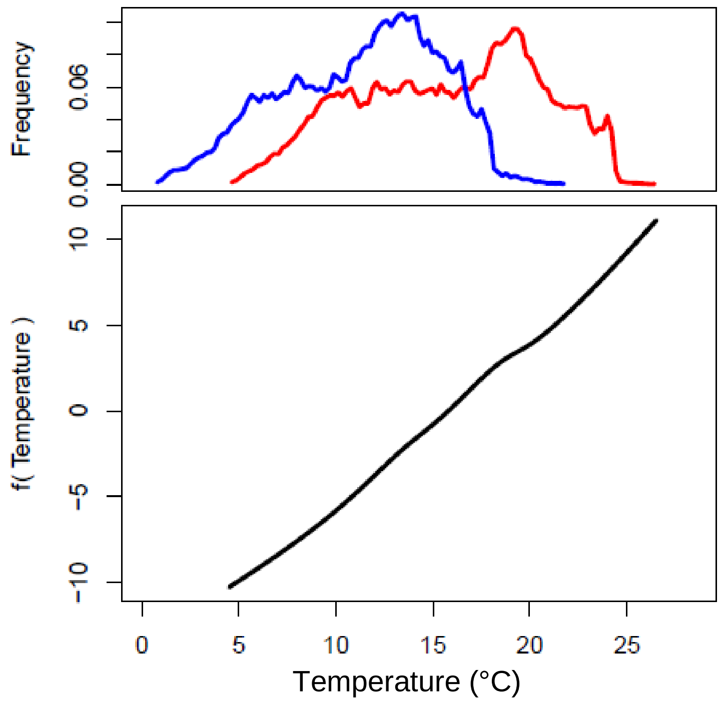
Vrac et al (2007) or Levavasseur et al. (2011) obtained similar % of variance explained for precipitation, even if they did not use the AOGCM precipitations as a predictor.
The lower % obtained compared to the other three parameters is therefore more likely due to the spatially variable nature of precipitation (now in the text), which is always quite difficult to capture properly.

5402, line 10: "...100 % very remote from any ocean, corresponding to a purely continental air parcel." At what distance from the ocean would DCO become 100 %?

The value of 100% is reached about 300 km from the ocean, which is now specified in the text.

5402, line 16: "Some of the MIS4 monthly temperature values . . . are lower than the calibration values..." Could you give a percentage value of how often it approx. happens that you are outside the calibration range? And does this happen scattered over the entire simulation area, or are there areas in particular where you tend to be outside the calibration range? Does it only happen for temperature that you are sometimes outside the calibration range, or also for the other parameters?

The following figure shows the histogram of monthly temperature values over southern Africa for the present-day IPSL simulation (red) and the MIS4_min simulation, which is the coldest (blue) (Top). On the bottom is the spline depending on temperature used for the downscaling.



The histogram for MIS4_min is shifted towards lower temperatures values by about 5°C. Values outside the calibration range correspond to temperature in the centre of southern Africa during the austral winter (which is the coldest region during JJA). This precision has been added in the text and the above figure is now in the Supplement.

The other variables from MIS4_max and MIS4_min used as predictors remain in the calibration range of the present-day simulation (see section 3.1).

5404, line 4: “we compare qualitatively” - instead of comparing your simulation results to the biomes, why don’t you do a direct forest-grassland comparison based on remote sensing products, e.g., the Global Land Cover Facility(GLCF) tree cover data set (DeFries et al., 2000)

As mentioned in the response to reviewer#1, we miss a dataset providing also grasses and bare soil fractions. The biome “coastal forests” (Fig.4.a) is easily classified as “forest” but the correspondence between other biomes and bare soil/trees/grass fractions is difficult and prevent a quantitative comparison.

5405, line 18: “... the model simulates the potential vegetation, i.e. without any anthropogenic disturbance...” - This is very important to keep in mind, since especially in Africa people still heavily rely on the usage of fire for agricultural and non-agricultural purposes and likely have influenced the natural vegetation over a long time. More detailed insights into the role of climate, humans and fire in southern Africa can for example be found in Archibald et al. (2008, GCB, doi: 10.1111/j.1365-2486.2008.01754.x), Archibald et al. (2012, PNAS, doi/10.1073/pnas.1118648109) and should be cited here for further information on the topic.

We thank the reviewer for these references, which have been added in the text.

5407, l. 4-16: While interesting, this paragraph could be shortened given the fact that this study focuses on south Africa rather than north Africa, and the North African monsoon sensitivity could be/will be the subject of another study.

Even if our study focuses on southern Africa, we chose to keep this paragraph to put our study in the larger context of the whole African continent and to provide information to the readers who might be interested in other African regions.

5407/5408, l. 25-l.1: So overall, precipitation is lowest during MIS4_min, and both MIS4_min and MIS4_max are lower than present-day if I understand this passage and Fig. 8 correctly. Please clearly state this once in a short sentence, because it will allow the reader to understand more quickly.

The sentence has been slightly modified to underline the point you have mentioned:
“The decrease of precession leads to an additional drying in the East (-50 to -100 mm/year over most part of the summer rainfall area) and in the center (-50 to -100 mm/year around 22°E)”

5409, l. 14/15: “The high tree percentages in that region seem to be more in qualitative agreement with pollen data from MIS4 than for present day.” This is an interesting observation that might indicate that for the present day mismatches between the vegetation model and actual observations are only to a certain degree due to performance issues of the DGVM, and the rest of the mismatch might indeed be attributed to the effects of humans and human land use.

The issue was pointed out in section 3.2.1. We have added the following sentence to section 3.3.2. to better highlight the results:
"This observation suggests that at least part of the present day mismatches between the vegetation model results and actual observations might indeed be attributed to anthropogenic effects."

5410, line 11/12: The decrease of 3 to 7 % for the woody PFTs due to the decrease in CO2 of 30 ppm is not totally surprising, given that LPJ is know to be a DGVM that reacts rather sensitively to CO2 concentrations.

We have added a reference to Köhler et al (2005) as an example to back this statement.

5412, line 4/5: "... no clear relationship appears between the amplitude of the annual precipitation changes and the fire activity." I guess that this might be due to the different reaction of east vs. central with respect to grass biomass. Overall, precipitation decreases almost everywhere between MIS4_min and MIS4_max, but in the East, albeit there being a decrease in precipitation, it is still wet enough to lead to an increase in grasses (and thereby easily incinerable light fuels) as you explain in the previous section, thus leading to an increase in annual burned area fraction, whereas in the center the total amount of precipitation remaining after the precipitation decrease is so low that the grasses decline, therefore leading to smaller amounts of light fuel and less fire in this part.

We agree with this comment. The response of fire regime to precipitation changes depends on the total amount of precipitation remaining for a low precession index, which is different in the East and in the center. This point has been briefly developed in the manuscript following this comment.

Technical comments:
All the technical comments listed below have been corrected in the manuscript.

p. 5392, line 4: please replace "Mediterranean-like" with "Mediterranean-type", also in subsequent occurrences throughout the manuscript
p. 5392, line 9: please replace "dynamical" with "dynamic"
5393, line 8: please replace "annual precipitations" with "annual precipitation amounts"
5393, line 18: please correct "developpement" to "development"
5393, line 23: please correct "analised" to "analyzed"
5394, line 4: "...lead to an decrease..." => "lead to a decrease"
5394, line 6: "... this study brings two interesting results" => "this study presents two interesting results"
5394, line 13: "precipitations": change to "precipitation amounts"
5394, line 21: "...are specially rare": please change to "...are particularly rare"
5395, line 24: "...96 x 95 points": replace "points" with"grid pixels" . Do the indicated numbers of grid pixels for atmosphere and ocean refer to a global grid? Please clarify.

Yes it refers to a global grid and the text has been clarified.

5397, line 9: "functionnal": please correct typo
5397, line 13: "fractionnal": please correct typo
p. 5399, line 22: replace "improve" with "increase"
5399, line 28:" ...preciptiations...": change to singular (precipitation), also for further occurrences in the manuscript

5400, line 4: “... so these two variables were simply bi-linearly interpolated at the same spatial resolution.” - “interpolated to the desired 0.16 _ spatial resolution.”
5400, line 18: “precipitations have to be” - precipitation has to be
5402, line 24: “satisfactoring” - satisfactory
5403, line 26: “distinguishes” - distinguish
5404, line 11: ”The higher grass fractions...” - “Higher grass fractions...”
5404, line 13: “forests fractions” - forest fractions
5405, line 8: “specially” - especially
5406, line 25, p. 5407, line 3: “decrease” - decreases
5409, line 27, p.4210, line 4, line 28: “decrease” - decreases
5410, line 3: “Arfica” - Africa
5413, line 2: “analised” - analysed
5413, line 7: “a cooling over Africa” - a cooling over southern Africa

This sentence refers to Fig.7 and to the global cooling simulated over the whole African continent during DJF (austral summer).

5413, line 24: “grasses fractions” - grass fractions
5413, line 26: “where trees are no longer sustainable”: please rephrase to “where tree cover declines”, after all the trees do not completely disappear.
p. 5413, line 28: “decrease (increase) on grid cells” - decreases (increases) on grid cells...