We thank both reviewers for the constructive comments on our manuscript. We addressed these comments in a revised version of the manuscript. Please find below a detailed response to the single points raised by the reviewers.

The reviewer comments are in blue and our answers in black. All new text in a revised version of the manuscript is printed italic.

Reviewer #1:

A) only one emission scenario that is being used – A1B. The results show a great deal of variability from the two climate projections (17 and 62% increases) and these are significant as wildland fires release the equivalent of 20- 30% of present day fossil fuel emissions as the authors point out.I believe it would be useful to have an A2 emissions scenario as well.

We choose the A1B scenario because it is the most extensively studied one of the IPCC SRES marker scenarios. We agree that the inclusion of more scenarios would increase the spread of simulated future fire emissions, but unfortunately availability of computer resources restricted the number of simulations feasible for such a study. Instead we decided to use one scenario but two different climate models to demonstrate the spread in fire emissions that can be caused by different climate model projections based on one emission scenario. This has been paid little attention in previous work on future fire projections. Pechony and Shindell (2010), studied the response of fires to three different IPCC SRES scenarios (B1, A1B, and A2) and find a range between ~15 and 40% increase in future fire activity with a very similar response pattern for the different scenarios. This range is smaller compared to the difference we find when we use the same scenario but different climate models. We extended in a revised manuscript the paragraph in the "Discussion and Conclusions" chapter that discusses this limitation of our study by a reference to this result:

"*Due to computational limitations we were only able to apply climate projections from two different climate models using one future emission scenario, which, however, showed large differences in the simulated future fire emissions. In the future it will be desirable to apply a larger set of future climate model projections. CMIP5 (Taylor et al., 2009) will form here an ideal consistent basis. This will also allow to explore future fire emissions for different emission scenario projections (different RCPs). Pechony and Shindell (2010) investigated the response of future fire activity for three different SRES marker scenarios applied to the GISS climate model. They found that future fire activity increases by 15, 19, and 35% in 2100 compared to preindustrial times for the scenarios A1, A1B, and A2, respectively. This range is smaller compared to the difference we found in future fire activity when two different climate models forced with the same emission scenario were used.*"

B) I am unclear if the emissions include those from smouldering fires that can burn in deep organic layers like peatlands for long periods of time. Some argue that our estimates of emissions may be conservative due to underestimation of smouldering fires (Turetksy et al. 2011 as cited by the authors).

In this study we did not include peat fire emissions. Global vegetation carbon models up to now do not include adequate parameterization of carbon accumulation in peat that forms a prerequisite for simulating peat fire emissions. We added a section to the "Discussion and Conclusions" chapter to clarify this:
"*We did not include peat fire carbon emissions in this study. While this excludes a large emission source especially from the tropical peat regions under present day conditions (e.g. Page et al., 2002) it also does not account for possible future permafrost thawing in the boreal regions that might expose large amount of soil organic carbon to burning (Turetsky et al., 2011). Peat fire emission modeling will require an adequate parameterization of carbon accumulation in peat areas. This is currently under development for several global carbon vegetation models (e.g. Wania et al (2009), Kleinen et al. (2011)) and might from the basis for future studies.*"

C) The current model does not take into account changes of vegetation; this is a significant caveat that deserves more attention. It is assumed that the burned area affect the different PFT's in proportion to their abundance. This is a large assumption and in some parts of the world may not be appropriate (see papers by Cummings).

Yes, these are limitations of our model. In the "Discussion and Conclusion" we noted the limitation caused by static vegetation cover: **"**However, fire emissions will be also indirectly controlled through changes in vegetation structure that is in turn partly controlled by fire itself. This feedback can only be accounted with dynamic global vegetation models (DGVMs), which should be the preferred tool to study fire-climate interactions in the future."

Regarding the distribution of the burned area among the different PFTs in the model, we added in a revised manuscript in the "Model" chapter:
" .... *The burned area was assumed to affect the different PFTs in proportion to their abundance. This assumption may not be appropriate for all landscapes, but will impact simulated fire carbon emissions significantly as for example herbaceous and woody fuels differ by a order of magnitude (van der Werf et al., 2010)."*

D) The model was validated against global carbon emissions for 1997-2004 from satellite observations; this is a short period and satellite observations although may be the best available global data are still a crude approximation. The climate anomalies were defined from a base period of 1948-1972 but the baseline for carbon emissions was 1985-2009 – why not the same time period for both?

The climate anomalies were defined relative to a reference period 1948-1972 that did not experience significant climate change. This allowed us to included the trend in climate as projected by the different climate models (that might be different from the observed one) consistently from 1973 onwards in our simulations. We choose, however, to report the results relative to the present day period (1985-2009) as well as preindustrial period as these periods are more commonly used in climate studies.

E) I would like to see more discussion on the relationship between human population and fire occurrence. What about the role of arson? Also, fire management may be at the point of diminishing returns in some regions such that even modest increases in fire occurrence will lead to significantly more escapes (Flannigan et al. 2009).

We extended the description of how human ignition and fire suppression is parameterized as a function of population density in the "Model" chapter in a revised manuscript:

"*Human ignition probability was accounted for following a relationship given by Venesky et al. (2002), which assumes that an average person is more likely to cause a fire in sparsely populated regions, as they interact more with the natural ecosystems, compared to persons living in densely populated areas. Densely populated areas were defined as areas exceeding 300 inhabitants/km^2 on grid box average. Fire suppression also depends on the population density. Fire suppression will more likely take place in densely populated areas were typically high property values are at risk compared to sparsely populated areas (Stocks et al., 2003; Theobald and Romme, 2007). We parametrized fire suppression similar to Pechony and Shindell (2009) assuming that in densely populated areas 90% of the fires will be suppressed.*"

We implicitly assumed that fire management strategies are always successful and do not account for the fact that fire management strategies with the purpose to reduce fire activity might lead to an unforeseen unintended increase in fire activity.

We would also like to stress here that we mentioned in the "Discussion and Conclusions" chapter the fire-human relationship as very weakly constrained parameter in our model: "... there are a number of processes in the model that are based on a rather incomplete understanding of human-fire interactions (Pechony and Shindell, 2010). Human fire interactions are to a large part socio-economically controlled with varying fire management practices ranging from active fire suppression efforts, typically applied in regions were high property values are at risk, to the use of fires for conversion of vegetation (Bowman et al., 2009). These fire management practices in turn depend on fuel characteristics that will change with a changing climate (Flannigan et al., 2009). This makes it particularly difficult to take changing fire management practices into account for future fire emission predictions. Comprehensive information on past fire activity, e.g. reconstructed from charcoal sediments (Power et al., 2010), explored in more detail in conjunction with reconstructed land use change pattern will hopefully allow for improvements in our current understanding of human-fire interactions."

Minor points

1. Title – should specify wildland fires

   Our work includes besides wildfire emissions also deforestation fire emissions. We therefore rather keep "fire" in the title instead of the suggested "wildfire". See also comment 1) of reviewer #2.

2. Abstract and throughout the paper the term fire behavior is used and this may not be the most appropriate term. For example, at the end of the first paragraph in the Abstract. . .changes in fire behavior will turn impact climate – fire activity may be more appropriate.

   In the revised manuscript we changed fire behavior into fire activity.

3. The shift of up to 4 months in the maximum burning period is surprising.

   In the manuscript we misleadingly stated: "Regions that showed a change in the month of maximum burning had a tendency of shifting the maximum burning later into the year (up to +4 months) for many regions at mid and high latitudes of the Northern Hemisphere (Fig. 7a)."

   We found a maximum shift of +4 months in a few grid boxes on average, however the most areas showed a shift of +2 months. In a revised manuscript we changed the paragraph into:

   "*Regions that showed a change in the month of maximum burning had a tendency of shifting the maximum burning later into the year for many regions at mid and high latitudes of the Northern Hemisphere (~ +2 months, Fig. 7a).* "

4. Population estimates may be optimistic as we will hit 7 billion this month (October).

   Thank you for pointing that out. We checked our Figure 1 which shows the population development from 2000 to 2100 and realized that we had erroneously shifted the time axis by 10 years. We corrected the figure. In 2010 the population is 6.9 billion, which is reasonable given that population passed 7 billion in October 2011. We apologize for the confusion caused by the wrong figure.

5. There has been some recent future fire occurrence modeling for Canada – see Wotton et al. (2010). Flannigan, M.D., Stocks, B.J., Turetsky, M.R. and Wotton, B.M. (2009). Impact of climate change on fire activity and fire management in the circumboreal forest. Global Change Biology, 15: 549-560. DOI: 10.1111/j.1365-2486.2008.01660.x. Wotton, B.M., Nock, C.A. and Flannigan, M.D. (2010). Forest fire occurrence and climate change in Canada. International Journal of Wildland Fire,19,253-271.

   We cited for future fire occurrence a comprehensive review article by Flannigan et al., 2009, which compared results from around 40 published studies on future fire activity. We extended in a revised manuscript in the "Discussion and Conclusion" chapter the paragraph discussing regional estimates on future fire activity by more recent studies, including the one for Canada.

   "Flannigan et al. (2009) analysed around 40 published studies that investigated the implications of changing climate for global wildland fire on different scales and with models of different complexity. They concluded that fire activity will generally increase in the future, but there will be regions with no changes and regions with decreases. For the boreal and temperate regions they found an overall consensus that the fire season will be lengthened in the future, which was consistent with our results".

   Was changed into:

   "*Flannigan et al. (2009) analysed around 40 published studies that investigated the implications of changing climate for global wildland fire on different scales and with models of different complexity. They concluded that fire activity will generally increase in the future, but there will be regions with no changes and regions with decreases. For the boreal and temperate regions they found an overall consensus that the fire season will be lengthened in the future, which is in line with more recent studies (e.g. Balshi et al., 2009, Spracklen et al., 2009, Wotten et al., 2010, Westerling et al., 2011), and was also reproduced in our results*".

Reviewer #2:

A) It is rather unclear what was the primary objective of this study: investigating how wildifre emissions may change in the 21st century (as stated not once in the text), or demonstrating how fire emissions projections may depend on climate model used (to which much of the paper is dedicated). In the first case use of only single climate scenario is clearly insufficient, while in the second case it is quite justified. The authors should decide what is the main goal pursued, and tune the study and/or the text accordingly.

The primary purpose of our study was to investigate how individual forcings on fires impact future fire emissions. We therefore investigated how changes in population, land use and climate will impact future fire carbon emissions. We think that this is also sufficiently reflected in the manuscript title: "The impacts of climate, land use, and demography on fires during the 21st century simulated by CLM-CN". We, however, decided to also include an estimate of future fire emissions choosing one combination of land use change and population development. For the climate we included projections from two different climate models, as the sensitivity studies showed that the simulated fire emissions were very sensitive to our choice of modeled climate projection. We realized that by choosing only two combinations, we did not cover in this study the entire range of possible future fire trends. As, however, there are only a few global studies published that report future fire activity and our study is this first one that explicitly simulated future fire carbon emissions, we decided that these estimates, even though the representativeness is limited, would be a valuable contribution to the manuscript.

B) In this study the SRES A1B scenario is used for the climate projection along with the corresponding A1 population projections. While land-use and harvest rates were prescribed according to the RCP 45 scenario. However, the RCP45 scenario is quite different from SRES A1B, projecting lower fossil and industrial emissions, higher landuse emissions etc. Climate, population growth and land-use are closely inter-linked, and it is not clear that such mix of scenario components is meaningful.

The SRES scenarios provide emissions resulting from land use change. To our knowledge only the IMAGE model also provided regional information on changes in land use causing these emissions. The SRES scenarios do not take into account wood harvest and are not harmonized in terms of land use to the historical period (up to 1990). As such the SRES scenarios were not applicable in our model. The newly developed RCPs, however, contain the information needed to account for land use change explicitly in our model by prescribing annually changing land cover maps and wood harvest rates, which were harmonized between the historical (observed) period and the future projections. Therefore, we choose to use the RCP projections for land use change in our simulation even though they are not consistent with the projected climate, which was based on the SRES scenario A1B. Climate projections consistent with the RCPs will just become available and should be used for future studies.
We noted this in the "Discussion and Conclusions" chapter: "Due to computational limitations we were only able to apply climate projections from two different climate models using one future emission scenario, which, however, showed large differences in the simulated future fire emissions. In the future it will be desirable to apply a larger set of future climate model projections. CMIP5 (Taylor et al., 2009) will form here an ideal consistent basis. This will also allow to explore future fire emissions for different emission scenario projections (different RCPs)."

The sensitivity experiments, performed in this study, are independent from the climate projections, as climate is kept identical in the experiments and only the land cover change projection is changing.

C) Comparison of findings to earlier studies is insufficient. At least in two earlier published works, both mentioned here (Krawchuk et al 2009 and Pechony and Schindell 2010) the impact of individual forcings on fires was explored. Krawchuk et al studied influence of individual factors on fires utilizing a statistical model. Pechony and Schindell studied this subject within a framework very similar to what is done in this paper. How do results for individual forcing in this work compare with their findings?

To our knowledge only the work by Pechony and Shindel (2010) investigated the impact of individual forcings on fires as we did in our study. We extended the "Discussion and Conclusions" chapter in a revised manuscript by a comparison of our findings to their results:
"*Pechony and Shindel (2010) investigated the impact of individual forcings on fire activity and came to similar conclusions as we did: the trend in future fire activity is largely driven by changes in climate and the direct human impact through ignition and fire management becomes less relevant. Decreasing fuel loads induced by land use change led in both studies to a decrease in natural fire activity.*"

D) Provided description of the model is insufficient (even if more details can be found elsewhere), since it lacks some details immediately needed to understand results presented in the paper. A more specific description of how anthropogenic ignitions and fire management relate to population density in this study is necessary. Also, it appears from the text that the fire management affects only densely populated areas. What is regarded as "densely populated"? To what extent this assumption is justified, given that modern fire-management efforts are clearly not limited to the vicinity of large human settlements?

In a revised manuscript we extended the “Model” chapter by a more detailed description of how human ignition and fire suppression was parameterized in our study. This was originally published in Kloster et al. (2010) and is repeated here. See details in our response to reviewer#1 (E), who raised a similar point.

E) Furthermore, it has to be guessed here if vegetation dynamically changes with climate in the model, or is it only prescribed/scaled according to RCP land use and harvest rates (as appears from the text, but then it is unclear why authors regard it as advantage over Pechony and Schindell work where fuel loads were prescribed according to SRES land-use scenarios).

In CLM-CN the land cover is fixed, i.e. not responding to changes in climate, and scaled to the RCP projections. However, the fuel load is simulated dynamically, i.e. the changes in land cover are applied to the simulated biomass in the model and will result as a direct response in emissions of carbon into the atmosphere and a redistribution of carbon on land. The resulting fuel load is then used in the fire parameterization. This is indeed similar to the method applied by Pechony and Shindell (2010), who prescribe the fuel load from the IMAGE model, that took the effect of land use change on fuel loads into account. What is different, however, is that in our approach changes in fire activity will directly impact the fuel load as well. This is not taken into account by Pechony and Shindell (2010) as fuel loads are prescribed and not dynamically simulated. We changed in a revised manuscript the “Discussion and Conclusion” chapter to make this more clear:
“*The simulation presented here showed that climate also had a substantial indirect control on future fire carbon emission through the alteration of fuel availability partly caused by changes in fire activity itself. A coupled vegetation-carbon model as used in this study allowed us to account for this, in contrast to other studies that were solely based in statistical relationships (Krawchuk et al., 2009) or did prescribe biomass density (Pechony and Shindell, 2010).*”

Additional remarks:

1) *Considering the focus on fire emissions in the paper, "fire" in the title should be substituted with "wildfire emissions".*

   Our work includes besides wildfire emissions also deforestation fire emissions. We concentrated in our results on reporting carbon emissions from fires. However, we also simulated with our model approach changes in burned area. We therefore rather keep “fire” in the title instead of the suggested “wildfire emissions”. (see also answer to comment 1 by reviewer #1)

2) *Section 2, Model, below eq. (1): "...and mort the mortality factor..." - "mort" should be in italic. Same for "cc" further in same paragraph.*

   changed accordingly in the revised manuscript

3) Same section, last paragraph: " The model was able to capture much of the observed mean and variability in fire carbon emissions" - in present-day fire carbon emission - pls be specific here.

   A previous publication (Kloster et al., 2010) includes a detailed evaluation of the model applied in this study. We, therefore, choose to just repeat here the main findings of this study:

   “The model was able to capture much of the observed mean and variability in fire carbon emissions (Kloster et al., 2010). Global annual mean fire carbon emissions varied between 2.0 and 2.4 PgC/yr for the time period 1997 to 2004, which lies within the uncertainty of satellite based estimates. The best match with observations was found when the model accounted for both human ignition and fire suppression as a function of population density. The simulated trend in fire carbon emission over the

20th century was broadly consistent with observational constraints."

4) *Table 1. human ignitions: "constant value of 0.5" - provide units, otherwise this information is meaningless.*

0.5 is the human ignition probability which is unit less. We changed in the revised manuscript "human ignition" into "human ignition probability" to clarify this in Table1.

5) *Figure 3. Yellow line and label text are hard to see; Also use of "related" colors (like blue and cyan) for both climate lines would be much appreciated*

Changed accordingly

6) *Figure 4 and all those showing maps are very hard to track (despite the explanations in the captions). Please add brief title to each map and units to colorbars.*

Were possible we added titles to the figures and units to the colorbars in the revised manuscript. This should make it easier for the reader to track the results.

7) *There is no reference to Figures 7 and 8 in the text.*

Figure 7 is referenced on page 10 and 11 in the section "Response of fire to future changes in climate" We added a reference in the revised manuscript to Figure 8 in the section "Response of fire to future changes in population density"

8) *Last sentence is teasing: "A fire model as presented in this study is a first step towards this direction." - Many steps were already taken in this direction. This study might be a further step forward, but hardly the first.*

We changed in the revised manuscript "A fire model as presented in this study is a first step towards this direction." into "*A fire model as presented in this study forms one step towards this direction.*" . Thanks for pointing out this misleading formulation.