Review #2: please note that the review of our manuscript is in italics, with our responses given in a regular font.

*The manuscript by Landry and Matthews documents how CO2 emissions generated from global wildfires differ from those generated by fossil fuel combustion in terms of atmospheric fraction and temperature. The authors use a coupled model to assess this, and also show temperature effects from altered land surface albedo. There are some potentially interesting and useful results, including the net vs. gross fire emissions after sustained changes in fire frequency, some of the climate feedbacks, and additions to the literature on atmospheric fraction and impulse response functions. However, I find much of the study design and the paper's presentation to be ill-conceived, and therefore cannot recommend publication in its present form. My major criticisms are below.*

>> 2.1 We thank you for your time and thoughtful comments that resulted in a substantial improvement of our manuscript; please see our responses below for more details. At the onset, we would like to note that the part of the "*study design*" you considered "*ill-conceived*" was not related to our central results, but to one implication of these results that we presented in a single paragraph of the Discussion. As we explain below, we have now removed this paragraph from the manuscript, and have also included clarifications in several places in the revised text. <<

*Major comments:*

*(1) Much of the language throughout focuses on the fact that gross fire emissions are not equal to net because of ecosystem regrowth, and that somehow this concept is novel and not accounted for in past studies. I find this off-base. Studies that attempt to calculate the effect of changing fire regimes on carbon stocks or fluxes obviously need to account for regrowth. Differences in carbon stocks and hence net transfers to the atmosphere will of course only be realized if mean fire frequencies or ecosystem characteristics (vegetation type, etc.) change in a way that affect mean standing carbon stocks. This is not a new concept, and has been extensively published on.*

>> 2.2 We are grateful for this comment, because it shows we were not clear enough on the difference between net and gross emissions. We agree that the role of post-fire regrowth is well recognized and that many stand- to regional-level studies have accounted for this $CO_2$ flux in various ecosystems. In the literature on the global-scale effects of fire, the regrowth flux has not been considered as frequently (most studies focussing on combustion-only gross emissions), but we presented in the Introduction three studies that did account for it in their estimate of net emissions (Seiler and Crutzen, 1980; Ward et al., 2012; Li et al., 2014). The difference between net and gross emissions also includes the decomposition of the vegetation that is killed by fire, but not combusted; this flux is important soon after a fire event, and explains why net emissions can initially reach higher values than gross emissions (e.g., Fig. 5, particularly visible in panels a and b). The three previous global-scale studies accounted for this process, but neglected several additional factors. First, fire-emitted $CO_2$ fertilizes the terrestrial vegetation at the global scale, thereby reducing the net land-to-atmosphere fire-caused $CO_2$ emissions (please remember that net emissions are defined as the difference in total land carbon storage between a control and a fire simulation). This effect has been shown to be meaningful for fire scenarios more realistic than the ones considered here (Landry et al., 2015). Second, the higher atmospheric $CO_2$ due to fire warms the climate globally, which can affect vegetation productivity and soil–litter decomposition, modulating once again the net emissions. Third, the post-fire local change in land surface albedo ($\alpha_L$) has overall the opposite effect, because on average it cools the climate. Other feedbacks are possible (e.g., the fire-caused change

in temperature affects ocean $CO_2$ solubility, which modifies atmospheric $CO_2$ levels and thus the $CO_2$ fertilization effect), but these are probably second- or third-order effects in the context of our study.

We have included the following clarifications in the Introduction (which we extensively revised to address various comments, the revised version appearing at the end of our response) "~~The latter two studies were however performed~~ *While the fact that vegetation regrowth offsets a fraction of gross fire emissions has been appreciated for some time, previous global quantifications of the difference between gross and net emissions have been performed with first-order estimates (Seiler and Crutzen, 1980) or* in offline terrestrial models *(Ward et al., 2012; Li et al., 2014),* ~~and were therefore unable to account for various climate–fire feedbacks, including fire-induced $CO_2$ fertilization and the impact of changes in surface albedo on temperature~~ *and have neglected relevant processes. Indeed, net fire $CO_2$ emissions differ from gross emissions because they include not only the gradual decomposition of the non-trivial fraction of vegetation killed by fire but not combusted (especially for trees) and the post-fire vegetation regrowth, but also the effects of various feedbacks like the fire-induced $CO_2$ fertilization of terrestrial vegetation, or the impacts on vegetation productivity and soil–litter decomposition of temperature changes caused by modified atmospheric $CO_2$ and surface albedo."* <<

*(2) The authors derive separate impulse response functions (IRFs) from global wildfires and from fossil fuel emissions. The premise is that studies that use the latter to inform on fire impacts are misguided (e.g. Randerson et al. 2006, O'Halloran et al. 2012). But there are fundamental differences in the way past studies and this one are conceptualized. The referenced prior work attempted to understand the long-term legacy of fire CO2 emissions in the atmosphere from one particular local fire event. In that case they accounted for both local ecosystem regrowth and other global land and carbon sinks as derived from fossil fuel CO2 pulses. In essence this isolates the effect of one model 'pixel' or 'grid cell', and assumes the rest of the world remains unchanged, i.e. a partial derivative. This, to me, mostly makes sense given the multitude of drivers for other future land and ocean carbon sinks. The present manuscript, however, derives its fire IRFs from simulations where nearly every grid cell burns (ones that had some level of fire activity in the MODIS era) and is allowed to regrow. This IRF then is only applicable in the case of a drastic global wildfire event. If we are scientifically concerned with changes to local to regional regimes (as is the case generally and in the two aforementioned studies), the former approach seems appropriate. If, for some reason, we were attempting to understand the fate of CO2 in the atmosphere in the context of most of the land surface burning at once and being allowed to regrow, then we would use the fire IRFs derived here. That situation, however, does not seem relevant to most current research questions and issues. If I am misinterpreting their analysis of fire IRFs, then I apologize, but in that case the authors need to be more clear on exactly what their fire IRFs should and should not be used for. I have a serious concern that if published as is, the fire IRFs would be misinterpreted and used in contexts that are not applicable.*

>> 2.3 Thank you for this comment and another closely related one below (*[15200, line 25]*); for convenience, we respond to both comments here. First, although we performed an additional analysis consisting of a much smaller fire pulse occurring in a single grid cell and still consider that our conclusion (i.e., the approaches of Randerson et al. (2006) and O'Halloran et al. (2012)—which used IRFs derived from previous simulations of fossil fuel $CO_2$ emissions to estimate the fire-caused atmospheric $CO_2$ anomaly—do not work as intended) is valid, we ended up removing this part of the text because we now realize that such an analysis deserves a dedicated study. We want to underline that this analysis was not a main part of our study, but a single paragraph we added to Section 4.1 (Discussion) for illustration purposes. As stated in the Introduction: "Our main objective is to compare the long-term effects of fire $CO_2$ emissions to corresponding levels of fossil fuel $CO_2$ emissions, for single fire pulses and stable fire

regimes. A second objective is to quantify the differences between gross and net fire $CO_2$ emissions over 1000 years following major changes in fire frequency."

Second, we agree that the fire IRFs we presented before could have been misused. In particular, simply replacing fossil fuel-based IRFs with our fire-based IRFs in the convolution approach (i.e., the one used by O'Halloran et al. (2012), which to our knowledge is the most common way to estimate the fate of the atmospheric $CO_2$ anomaly caused by fire and other terrestrial changes) does not work either, as we validated with our own results. The fire IRFs we presented in our former Table 2 excluded the first couple of years from the fit, as we mentioned in the original text, because the fire-caused atmospheric $CO_2$ anomaly increased for a few years (due to decomposition of the fire-killed vegetation) before gradually decreasing. Moreover, the maximum value of the fractional atmospheric anomaly was higher than 1.0 (because the size of the fire pulse was defined based on gross, not net, emissions). Over the first couple of years, there was consequently a major discrepancy between actual fire results and the corresponding fire IRF. This discrepancy is irrelevant for the long-term behaviour we want to illustrate, but is consequential in the context of the convolution framework where the same IRF is applied over and over to all yearly land–atmosphere $CO_2$ fluxes. Moreover, to be fruitfully applied to specific fire events, fire IRFs should probably be derived from regional instead of global pulses in order to reflect the spatial variations of fire-related impacts (e.g., northern forests vs. savanna, Fig. 1), in line with the spirit of your comments. Since Fig. 2c illustrates clearly enough the difference we want to highlight regarding the long-term behaviour of fire vs. fossil fuel, we decided to remove IRFs from the manuscript.

Consequently, the major revisions we brought consisted of removing: 1) the paragraph of Section 4.1, along with Fig. 3, where we analyzed the approaches of Randerson et al. (2006) and O'Halloran et al. (2012); 2) Table 2, in order to prevent a possible misuse of the fire IRFs we derived; 3) the part of the Introduction that presented IRFs and various studies, including Randerson et al. (2006) and O'Halloran et al. (2012), that used fossil fuel-derived IRFs to quantify the atmospheric $CO_2$ anomaly caused by fire or other terrestrial changes (p15188, from l8 to l25); and 4) the text referring to IRFs in the Results (p15195, from l1 to l8).

*(3) I generally found the justification for using a coupled model to address these issues lacking. I do not think the tool is inappropriate, but the authors seem to push the idea that only a coupled model can be used to answer these questions. To me, the benefits of using a coupled modeling approach are (i) that it can account for the CO2-climate feedbacks generated by changing fire regimes and (ii) that it can estimate the temperature response from CO2 and other forcing agents such as albedo and aerosols. In the case of (i), the authors do not actually simulate the effects of CO2 or climate on fire regimes; these are prescribed. Climate and CO2 do affect land carbon cycling in general, but the results have limited implications because the model simulations are highly theoretical (or experimental) and cannot easily be tied to actual future projections. In the case of (ii), the authors do discuss the impacts of land surface albedo on temperature. However, they do not include char, which can be one of the dominant albedo effects in many terrestrial systems such as grasslands and savannas (see Figure B1 in Ward et al. 2012). So the fire-albedo affects are incomplete. Moreover, the authors do not account for other non-CO2 gases or, more importantly, fire aerosols, which are likely the dominant impact of fires on climate. To be clear, I'm not arguing that the authors need to include these effects in this manuscript. But I am arguing that the major benefits of using a coupled model are not really being taken advantage of. I only stress this because much of the language seems to imply that a coupled model must be used to assess these issues. As W. Knorr pointed out, this is not true.*

>> 2.4 First, we agree that coupled climate models are not the only tools suitable to compare the climatic impacts of fire vs. fossil fuel combustion, and did not make such a claim in the manuscript.

However, we strove to identify the formulations that might have caused this impression and modified the text accordingly. The last sentence of the Abstract: "Overall, our study calls for the explicit representation of fire *activity*~~in climate models, rather than resorting to ersatz results coming from fossil fuel simulations,~~ as a valuable step to foster a more accurate understanding of its impacts ~~in the Earth system~~*on global carbon cycling and temperature, compared to conceiving fire effects as congruent with the consequences from fossil fuel combustion.*" We removed the following sentence (on p15188, starting on l3): "To date, the only study dedicated to fire in a coupled climate–carbon model with interactive vegetation dealt primarily with the consequences of major changes in future fire regime, but also found that net $CO_2$ emissions following changes in fire regime quickly became much smaller than gross emissions and progressively decreased over time (Landry et al., 2015)." On p15202, starting on l13: "Future studies on the differences in the carbon cycling and temperature impacts between fire and fossil fuel would nevertheless benefit from *considering*~~combining~~ the effects of non-$CO_2$ emissions ~~with climate–carbon feedbacks in climate models including interactive vegetation~~." On p15202, starting on l21: "More research is therefore needed to accurately represent the highly variable and poorly quantified fate of such exchanges of pyrogenic carbon ~~in climate models~~; meanwhile, their influence on our results is speculative, but is unlikely to challenge the main outcomes we obtained." On p15204, starting on l23: "The overarching message from the present study is that fire effects cannot be obtained from, and should not be conceived as akin to, fossil fuel emissions – rather, fire deserves its own explicit representation in ~~Earth system models~~*climate-related studies.*"

Second, we clearly stated in the Methods and Discussion that we did not consider the effects of non-$CO_2$ emissions (including the impact of char on $\alpha_L$, which would not occur if combustion led to 100% $CO_2$ emissions). Such an effective 'decoupling' of $CO_2$ from concurrent emissions of gases and aerosols is common in studies assessing the long-term fate of emitted $CO_2$ (Eby et al., 2009; Joos et al., 2013). We consider highly unlikely that accounting for non-$CO_2$ emissions would alter our main conclusions on the major differences between fire and fossil fuel, or on the differences between net and gross fire emissions.

Third, the reason we used a coupled climate model with interactive vegetation is partly related to the benefit "*(i)*" you mentioned. Although we did not study feedbacks between climate and fire occurrence, $CO_2$–climate feedbacks played a relevant role in our study, as explained in our response 2.2 (i.e., fire- vs. fossil fuel-induced $CO_2$ fertilization, and the impacts of fire- vs. fossil fuel-induced temperature changes on $CO_2$ exchanges). Using a coupled model ensured to capture these effects, which arguably ends up being easier than having to 'manually' adjust fluxes among independent models to account for them— and is probably why, to our knowledge, all previous studies deriving IRFs for fossil fuel used coupled climate models of varying complexity. Another reason we used such a model is that we were interested in the responses of the three major carbon pools, including the ocean which plays a major role in the $CO_2$ exchanges following emissions from fire or fossil fuel (please see Figs. 2 and 6). To clarify this point, we added the following sentence to the Introduction, just after stating that we use a coupled climate model for the study: "Using such a model allowed us to account for the various feedbacks mentioned previously (i.e., $CO_2$ fertilization and temperature–$CO_2$ interactions), as well as the major role of the ocean in the fate of the fire-emitted $CO_2$." <<


*Minor comments: -[15188, line 23] This is the type of language that I find ill-conceived. Studies such as O'Halloran et al. 2012 account for local ecosystem regrowth from a local fire event, which is essentially 'the fundamental' difference between fire and fossil fuel emissions the authors mention. Past studies such as this are generally not interested in the fate of atmospheric CO2 when the entire biosphere is regrowing from one large pulse fire event.*

>> 2.5 Please see our response 2.2 for the clarifications we brought to the text regarding other processes besides vegetation regrowth that play a major role in the differences between fire and fossil fuel emissions. Also note that this part of the Introduction has been entirely modified. <<


*-[15191, line 13] Parentheses should not be put around complete sentences*


>> 2.6 We removed the parentheses that previously enclosed the sentence. <<


*-[Figure 1] Qualitatively, the PFT and albedo succession curves match the mentioned observation-based estimates. But quantitatively they do not. The presented annual (?) albedo anomaly is considerably smaller for what's published in the North American boreal in winter/spring, but larger than what's published in summer (e.g. figures from Amiro et al. 2006). Hence it is difficult to compare. And the PFT regrowth takes much longer in the presented model (e.g. shrub PFTs generally last 20-30 years in Alaska and Canada as shown in Rogers et al. 2013, but last up to 300 years here). These differences should at least be mentioned.*


>> 2.7 Thank you for this comment. The goal of Fig. 1 was indeed to show that results agree qualitatively with observation-based estimates and have a reasonable magnitude. We clarified the text, stating more explicitly the quantitative disagreement with the results from Rogers et al. (2013): "In northern forests, the succession among the different PFTs (Fig. 1a) *was qualitatively similar to, but noticeably slower than,*~~agreed with~~ observation-based trajectories (Rogers et al., 2013)~~, while the impacts on biomass (Fig. 1c) and $\alpha_L$ (Fig. 1e) were consistent with field observations (Amiro et al., 2006; Goulden et al., 2011)~~. *Simulated fire-caused changes also appeared reasonable when compared with field observations for biomass (Fig. 1c) (Goulden et al., 2011) and $\alpha_L$ (Fig. 1e) (Amiro et al., 2006).*" Correspondingly, we added the following text to the Section on study limitations: "Fourth, the quantitative results we obtained were dependent upon the specific features of the UVic ESCM. For example, the simulated post-fire vegetation regrowth appeared too slow in northern grid cells (Fig. 1a), thereby overestimating the duration of both the $\alpha_L$-based cooling and $CO_2$-based warming following fire. The carbon–concentration feedback parameters from the UVic ESCM are close to the mean from other fully coupled climate–carbon models, but its carbon–climate feedback parameters are on the high end (Arora et al., 2013), meaning that the atmospheric $CO_2$ levels were more affected by temperature changes than would have occurred in most other models. Once again, these factors should not challenge the main outcomes we obtained."

Incidentally, we would like to note that the results from Rogers et al. (2013), which were not based on field data but on satellite measurements, are themselves uncertain. The succession trajectories they obtained very likely included many unburned patches, because mean tree cover immediately after fire "remained above 22%" (with +1 standard error going up to ~40%; their Figure 2b), thereby leading to higher levels of tree cover. Moreover, their results involved the combination of two MODIS datasets. The most relevant one here (MOD44B, providing percent tree cover) actually gave a stabilization of mean tree cover around ~60% for years 60–85, with no data afterwards (their Figure 2b). This differs noticeably from the final succession trajectories per plant functional type (PFT; their Figure 2c–e), which were based on MCD12Q1. This dataset provides a classification by PFT based on a 'winner-takes-all' approach; hence the entire pixel will be assigned to the needleleaf tree PFT from the time this PFT covers >50% of the pixel, thereby also leading to higher levels of tree cover.

Regarding $\alpha_L$, spatially-explicit results from the UVic ESCM are very demanding in terms of storage space (about 30 MB for each 'picture'). This is why we saved the mean value of each spatially-explicit variable once every 50 years only; saving seasonal values of $\alpha_L$ on a yearly basis would be far too

demanding. So the changes in annual $\alpha_L$ we obtained were indeed smaller than wintertime changes, but larger than summertime changes. A quick analysis shows that our results agree reasonably well with the values from Figures 1 and 2 of Amiro et al. (2006), which show considerable variability. The equation they provide for summer changes leads to a mean change of 0.050 over the first 50 years after the fire event when compared to 150-year old stands. (Their equation neglects the short-term decrease in $\alpha_L$ from surface blackening, which is appropriate given that we also neglected this effect in our study. Their linear regression further overestimates the initial increase in $\alpha_L$ because it excludes the initial 'plateau' before the values start decreasing.) For winter, their equation leads to a mean change of 0.134 over the first 50 years, compared to 150-year old stands. Our value for the mean annual $\Delta\alpha_L$ over the first 50 years is 0.054. Since annual $\alpha_L$ is much closer to summer than winter values in northern latitudes (because proper averaging accounts for the much higher solar radiation in summer vs. winter) and the 0.050 summer value we derived above was a little overestimated, the results agree reasonably well. The validity of the simulated $\Delta\alpha_L$ values was also supported by the following text (end of Section 3.1): "Second, the differences in $\alpha_L$ between the current fire regime and a no-fire world simulated by Landry et al. (2015) led to a global radiative forcing of $-0.11$ W/m$^2$ without the effect of surface blackening and $-0.07$ W/m$^2$ with surface blackening, in agreement with observation-based estimates (Ward et al., 2012) (note that we did not include surface blackening in the current study)." $<<$

*-[15193, line 11] Regarding above, the authors mention that lasting climate feedbacks are responsible for the overall slow regrowth in the model. To me this is interesting from the standpoint of a modeling exercise, but has limited application to reality. Vegetation will not be regrowing amidst immediate climate changes from a global conflagration. This is an instance where it would have been seemingly much better to run the model offline instead of in a coupled configuration.*

$>>$ 2.8 Please note that the purpose of Fig. 1 was not to provide a precise assessment of the performance of TRIFFID (the dynamic vegetation module within the UVic ESCM), but to give a flavour of simulated fire effects and their realism; a detailed quantitative assessment would likely require a dedicated study. But there was another reason why we showed these results: some people seem to believe, based on the study of Arora and Boer (2006), that TRIFFID is totally unable to simulate the coexistence of various PFTs (a senior research scientist recently tried to convince one of us this was absolutely the case!). As far as we know, there is nothing wrong with the results of Arora and Boer (2006), but the conditions for which they showed no PFT coexistence do not fully correspond to TRIFFID. We performed additional simulations to assess whether the explanation we provided (i.e., lasting climatic effects) was sufficient to account for the difference and found that it was not! Following the same fire pulse burning 88% of this grid cell only (with no fire in the other "fire cells", thus leading to a very small impact on global climate), the competition between C$_3$ grasses and shrubs was slightly altered, with marginal impacts on the regrowth of needleleaf trees. We performed more simulations with different pulse levels and found this element to have a greater impact. Simulated tree recovery was a little faster for a pulse burning 78% of the grid cell, which corresponds to the 22% initial tree cover of Rogers et al. (2013), and much faster for a pulse burning 50% of the grid cell. Consequently, we deleted the previously proposed explanation and, as mentioned in our response 2.7, acknowledged more explicitly this limitation in the Results and added a paragraph on this issue in the Discussion. $<<$

*-[15195, line 27] "Now, what if fossil fuel emissions were instead set equal to the net land-to-atmosphere emissions from fire?" What is the rationale for this experiment? What are the implications, either practical or theoretical? Some of this comes off like an entertaining modeling exercise with limited applicability.*

>> 2.9 We thank you for this question, which has led us to better explain the relevance of this set of simulations. Please remember that the main objective of our study is to "compare the long-term effects of fire $CO_2$ emissions to corresponding levels of fossil fuel $CO_2$ emissions, for single fire pulses and stable fire regimes." Now, what are "corresponding" levels? A first natural answer is for fossil fuel emissions to be equal to gross fire emissions (i.e., based on combustion only). However, a fossil fuel pulse increases the sum of carbon in the atmosphere, ocean, and land, but a fire pulse does not; it is therefore mathematically impossible for fossil fuel and fire to have the same effect on the three pools. Some readers might therefore feel that our first set of results were 'unfair' about the possibility of fossil fuel combustion having the same effect as fire. This is why we then present the second possible natural answer: let's simulate fossil fuel emissions that are equal to the net emissions from fire, year after year (this means that fossil fuel emissions become negative after a few years, as if atmospheric $CO_2$ was sequestered back into geological reservoirs). This represents a much more stringent test of the idea we put forward, thereby substantially strengthening our conclusion. We adjusted the text as follows: "Now, what if fossil fuel emissions were instead set equal to the net land-to-atmosphere emissions from fire *year after year over the entire simulation, a situation where we expect fossil fuel emissions to better mimick the effects from fire emissions*?" <<

*-[15197, line 4] Again, who is making the argument that yearly gross emissions should be used to assess the impact of fires on the land carbon sink? To me this is not a problem in the literature or the field in general.*

>> 2.10 Please note that the "cumulative impacts of fire regime shifts" we address in this Section on stable fire regimes go beyond the land carbon pool, and include the effects on the atmosphere and ocean carbon pools, as well as on the global temperature (Figs. 6 and 7). While we are unaware of people stating that gross fire emissions "*should be used*" to assess the impacts of fire on global carbon cycling and temperature, there seems to be a problem in the literature regarding the side-by-side comparisons of gross fire emissions to fossil fuel emissions (implying, implicitly at least, that they have similar effects), with no caveat regarding the difference between net and gross fire emissions. We have identified a dozen studies presenting such side-by-side comparisons over the last ten years, with five of them published since 2011. Even if many of these authors certainly understand that gross and net fire emissions differ, they end up implicitly promoting the idea that gross fire emissions are adequate to characterize climatic impacts—an idea that might very well confuse readers who do not have enough previous knowledge on these questions. <<

*-[15200, line 25] I may be confused here, in which case I welcome corrections from the authors, but I do not believe this setup mimics Randerson et al. 2006. The Randerson study accounted for a single fire event's impact on atmospheric CO2 by including local ecosystem regrowth and other global land and ocean sinks. The approach mentioned here seems to account for GLOBAL ecosystem regrowth from a global conflagration, coupled with the ocean CO2 sink from a simulation in which the land had not burned. It is not surprising that this IRF does not match the actual fire IRF where the atmosphere-ocean flux was affected by the regrowing land. But this is also not a simulation that, as far as I can tell, has any obvious application; nor does it replicate past work. The same critique applies to the following attempt at mimicking what O'Halloran et al. 2012 did. To me, again, the fundamental difference is that the past work mentioned considered local post-fire regrowth while this study considers global post-fire regrowth. I do not understand where the latter scenario is applicable.*

>> 2.11 Please see our response 2.3, where we responded to both comments. <<

*-[15202, line 2] I assume this is some sort of typo, in that the authors mean that including char albedo would reduce the albedo cooling effect, and that including other non-CO2 emissions (CH4, effects on O3, etc) would result in additional warming?*

>> 2.12 You assumed correctly. We modified the text as follows: "Accounting for *the short-term post-fire surface blackening caused by char*~~fire non-CO$_2$ emissions~~ would reduce the albedo cooling effect~~, due to the short-term post-fire surface blackening caused by char~~." <<

*-In the figures with multiple lines and colors, consider making the fossil fuel scenarios more similar to each other and the fire scenarios more similar to each other (e.g. dashes, or similar colors). This would make reading the graphs considerably easier.*

>> 2.13 Thanks for this suggestion. We distinguished fire from fossil fuel results in Figs 3a, 3b, 4b, 4c, 4d, 7a, 7b, and 7c, by using dashes for fossil fuel. <<

**Revised Introduction**

Fossil fuel emissions entail a net transfer of $CO_2$ from geological reservoirs to the much more active atmospheric, oceanic, and terrestrial carbon pools, thereby increasing the total amount of carbon in these pools and leading to an atmospheric $CO_2$ anomaly that decreases only gradually on a millennial timescale (Archer et al., 2009; Eby et al., 2009; Joos et al., 2013). This atmospheric $CO_2$ anomaly causes global warming that remains stable over thousands of years (Matthews and Caldeira, 2008; Eby et al., 2009). The atmospheric $CO_2$ anomaly also gives rise to a global $CO_2$ fertilization effect that decreases land surface albedo, due to dynamic vegetation expansion and generally higher vegetation cover, with an additional warming resulting from this fertilization-induced albedo decrease (Matthews, 2007; Bala et al., 2013).

Fire (also referred to as wildland fire, wildfire, and open vegetation burning) is a conspicuous disturbance in most terrestrial ecosystems, with considerable impacts on vegetation and climate (Bonan, 2008; Running, 2008; Bowman et al., 2009). Contrary to fossil fuel combustion, fire does not entail a net addition of $CO_2$ to the three active carbon pools of the Earth System, but simply redistributes the carbon already existing within these global pools. Except when used for permanent land clearing, fire usually triggers a strong local-scale vegetation regrowth response lasting years to decades depending upon the ecosystem (van der Werf et al., 2003; Goulden et al., 2011); hence the resulting atmospheric $CO_2$ anomaly and the concurrent global $CO_2$ fertilization are of shorter duration than after fossil fuel combustion. Fire also causes major modifications to land–atmosphere exchanges of energy through altered surface albedo and sensible/latent heat partitioning (Bremer and Ham, 1999; Amiro et al., 2006). Besides a short-term decrease due to surface blackening, local albedo generally increases after a fire event, thereby leading to a regional-scale cooling that is consequential at the global scale (Ward et al., 2012; Landry et al., 2015). For a given amount of emitted $CO_2$, fire therefore differs from fossil fuel combustion in terms of: 1) average lifetime of $CO_2$ molecules in the atmosphere; and 2) non-$CO_2$ climatic impacts. When comparing fire with fossil fuel combustion, the expression "$CO_2$ emissions" will henceforth implicitly include the consequences from these differences in atmospheric lifetime and surface albedo.

Fire currently affects around 300–500 Mha yr$^{-1}$ (Mieville et al., 2010; Randerson et al., 2012; Giglio et al., 2013), leading to gross emissions (i.e., accounting only for the combustion of vegetation and soil–litter) of 1.5–3 Pg C yr$^{-1}$ (Mieville et al., 2010; van der Werf et al., 2010; Randerson et al., 2012). The potential for modifications in the current fire regime to modulate climate change stimulated the explicit representation

of fire in the Lund–Potsdam–Jena (LPJ) Dynamic Global Vegetation Model (DGVM; Thonicke et al., 2001), and later on into various other similar process-based models of climate–vegetation interactions (Arora and Boer, 2005; Kloster et al., 2010; Li et al., 2014). These efforts have paved the way to studies that projected an increase in fire frequency and gross $CO_2$ emissions over the 21st century (Scholze et al., 2006; Pechony and Shindell, 2010; Kloster et al., 2012). The net effect of fire on global carbon cycling has however received less attention than the consequences from future changes in fire activity. In their seminal study, Seiler and Crutzen (1980) concluded that net biospheric emissions, coming mostly from fire, could range between $\pm 2$ Pg C yr$^{-1}$ by adding the effects of vegetation regrowth and other processes to their estimate of 2–4 Pg C yr$^{-1}$ for gross fire emissions. The net effect of fire on global terrestrial carbon storage has then apparently been left unaddressed for more than three decades, until Ward et al. (2012) suggested a fire-caused net reduction of $\sim 500$ Pg C in pre-industrial land carbon. They also found that this reduction could currently be slightly lower (around 425 Pg C) due to offsetting effects between fire and land-use and land cover changes (LULCC), but could increase to about 550–650 Pg C by the end of this century due to a climate-driven increase in fire activity. More recently, Li et al. (2014) concluded that net fire emissions were equal to 1.0 Pg C yr$^{-1}$ on average during the 20th century, compared to gross emissions of 1.9 Pg C yr$^{-1}$ on average over the same period. While the fact that vegetation regrowth offsets a fraction of gross fire emissions has been appreciated for some time, previous global quantifications of the difference between gross and net emissions have been performed with first-order estimates (Seiler and Crutzen, 1980) or in offline terrestrial models (Ward et al., 2012; Li et al., 2014), and have neglected relevant processes. Indeed, net fire $CO_2$ emissions differ from gross emissions because they include not only the gradual decomposition of the non-trivial fraction of vegetation killed by fire but not combusted (especially for trees) and the post-fire vegetation regrowth, but also the effects of various feedbacks like the fire-induced $CO_2$ fertilization of terrestrial vegetation, or the impacts on vegetation productivity and soil–litter decomposition of temperature changes caused by modified atmospheric $CO_2$ and surface albedo.

In this study, we used a coupled climate–carbon model with interactive vegetation to advance the current knowledge regarding the effects of fire $CO_2$ emissions on the global carbon cycle and temperature. Using such a model allowed us to account for the various feedbacks mentioned previously (i.e., $CO_2$ fertilization and temperature–$CO_2$ interactions), as well as the major role of the ocean in the fate of the fire-emitted $CO_2$. We focussed on non-deforestation fires that allow the different vegetation types to compete and grow back in the recently burned area, because they constitute the bulk of global burned area and gross emissions (van der Werf et al., 2010) and have been much less represented in climate models than the LULCC events associated with deforestation fires. Our main objective is to compare the long-term effects of fire $CO_2$ emissions to corresponding levels of fossil fuel $CO_2$ emissions, for single fire pulses and stable fire regimes. A second objective is to quantify the differences between gross and net fire $CO_2$ emissions over 1000 years following major changes in fire frequency. To facilitate the interpretation of results, we performed all simulations against a background climate corresponding to pre-industrial conditions.

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