Review #1: please note that the review of our manuscript is in italics, with our responses given in a regular font.

General remarks:

The manuscript by Landry and Matthews entitled "Fire vs. fossil fuel: all CO2 emissions are not created equal" is a welcome addition to the literature on simplified carbon cycle response models based on more complex models, with the interesting difference that it does not consider a particular new part of the carbon cycle, but that it distinguishes between to combustion processes and their effects on the carbon balance of the terrestrial biosphere. This to my knowledge is a new angle on the problem that seems suitable for publication in Biogeosciences. However, I have some serious concerns about presentation which, if not addressed fully, will in my opinion rather reduce than increase clarity and impact. I am trying to detail this in the following comments. At times, I also find that there is a lack of clear definitions, which should be addressed through substantial revisions of the introduction and methods sections. What also seems to have been lost is a discussion of the vastly different scales between direct impacts of vegetation fires at the plot scale, regional impacts of albedo changes, and the diffuse impact of increasing or decreasing CO2 via CO2 fertilisation that acts only at a global scale due to the fast mixing time of the atmosphere.

>> 1.1 We thank you for your time and numerous comments that helped us improve our manuscript. We appreciate that you qualify it as a "*welcome addition*" and note that you did not have any issue with the results per se, although you suggested many changes to the presentation. As you will see below, we substantially modified the text to address the issues you raised. We feel that many of the concerns you expressed come down to the exact meaning one gives to the expression "CO_2 emissions". We of course agree, and already stated in the Abstract, that all CO_2 molecules in the atmosphere have the same effect. However, differences between fire and fossil fuel combustion imply, from the moment when CO_2 emissions are created, the existence of unequal non-CO_2 climatic effects (we considered albedo in this study) and a different atmospheric lifetime for the CO_2 emitted. Based on your comments, we believe that you agree with this view, but had issues with the way we expressed it; we therefore strove to reformulate the text along your suggestions. We also added to the Introduction the various spatial scales associated with different impacts, to address the last sentence of your comment (please note that we considerably modified the Introduction to address various comments, the revised version appearing at the end of our response). <<

Major comments:

(1) The fact that much of the carbon (not CO2) emissions of wildfires is consequently taken up again by the biosphere is by no means new. This manuscript is in large parts written as if it was.

>> 1.2 We agree that this idea is by no means new and did not want to take credit for it. This is why the second paragraph of our former Introduction presented previous studies, going back to year 1980, that aimed to quantify the net effect of fire on global land carbon storage. The new contribution of our study consists of quantifying this net effect in a coupled climate–carbon model with interactive vegetation, thereby accounting for effects that were neglected in previous global-scale studies (e.g., fire-induced CO_2 fertilization). To clarify this point, we substantially revised a sentence in the Introduction, which now reads: "~~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.*" Please note that the difference between net and gross fire emissions goes beyond vegetation regrowth, which we clarified by adding the following text after the sentence just quoted: "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." <<

Furthermore, the central result of this manuscript, e.g. presented on p. 15198, line 24ff "In this study, we have shown a consistent pattern of fundamental differences between the carbon cycle and climate effects of CO2 emitted by fire as compared to fossil fuel combustion" is simply wrong, which leads to significant confusion. The effect of the emitted CO2 is the same (e.g. if you had a power station next to an active forest fire, you could not distinguish between the effect of the CO2 coming from each one), but what differs is the effect of the emitting process, i.e. the fire in the combustion chamber (or whatever) vs. the grass, shrub or forest fire, including the involved flux of carbon. This confusion comes apparent in a sentence following within the same paragraph (p. 15199, l7) "Fire, on the other hand, gives rise to a much more dynamic land carbon response." Here, it is not the CO2 that is talked about, but the fire. My suspicion is that this confusion is deliberate in order to enhance the apparent urgency and novelty of the research results. I believe that this general thrust of the manuscript needs to be revised substantially.

>> 1.3 We thank you for raising this point, which boils down to a semantic issue that we clarified. For us, "CO_2 emissions" or "CO_2 emitted" in expressions like the "effects of CO_2 emitted by fire [or] fossil fuel combustion" implicitly included the fate of the atmospheric CO_2 molecules and the non-CO_2 impacts (e.g., changes in surface albedo) that accompany these emissions. So even though the radiative effect of each atmospheric CO_2 molecule is the same, one can easily distinguish a fire event from a power station through field and satellite observations of changes in albedo and vegetation. We clarified this semantic issue by adding two sentences to the Introduction: "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." We also clarified accordingly the first sentence cited in the comment (new text in italics): "In this study, we have shown a consistent pattern of fundamental differences between the carbon cycle and climate effects of CO_2 emitted by fire as compared to fossil fuel combustion*, which ultimately came from the net addition of CO_2 by fossil fuel combustion (contrary to fire), as well as the differences in atmospheric lifetime of emitted CO_2 and in non-CO_2 climatic impacts*." Concerning the second sentence cited in the comment ("Fire, on the other hand, gives rise to a much more dynamic land carbon response."), it actually refers to the impact of fire on land–atmosphere CO_2 dynamics, contrasting it with the impact of fossil fuel (i.e., CO_2 fertilization only). We consider this sentence to be adequate. <<

For that reason, in order to make this manuscript publishable with BG, I argue that the title should be changed in order to avoid confusing semantics: it is not the "CO2 emissions" that is different, in the sense of "emitted CO2", but the "act" of emission. I know that this is very subtle, but as argued before, gives rise to just the confusion I referred to, leading to the impression of the reader that what is reported here is largely unknown and novel (which it isn't). A further note is that fossil-fuel burning is also a *form of fire, so that a further distinction needs to be made. I would suggest a title along the lines of "Carbon cycle impacts of wildfire vs. fossil fuel emissions", or "The fate of emitted CO2 from wildfires and fossil-fuel combustion".*

>> 1.4 Our previous title read "Fire vs. fossil fuel: all CO_2 emissions are not created equal", in which the word "created" already made the point raised above about the "*act*" of emission. The fourth sentence of our original Abstract further made this point more explicitly: "While atmospheric CO_2 molecules are all alike, fundamental differences in their origin suggest that the effects from fire emissions on the global carbon cycle and temperature are irreconcilable with the effects from fossil fuel emissions." We nevertheless revised the title to: "Fire vs. fossil fuel combustion: the source of CO_2 emissions affects the global carbon cycle and climate responses". Concerning your "*further note*", we agree that CO_2 emissions from fire and fossil fuel both involve combustion, but we do not see the added value in distinguishing 'fire burning' from 'fossil fuel burning' on this basis. <<

In agreement with this, I would also like to see the last sentence of the Abstract changed. In particular I object to the use of the word "ersatz results", which unduly belittles compartmental approaches to quantifying the effect, and that in a study that does not report error bars. I am convinced that a perturbation-based, compartmental approach could deliver results with just the same level of confidence. I believe that this form of presentation is unfair, too absolute and lacks scientific modesty by over-emphasizing the significance of the result of a study based on a single model. To further increase clarity, and to avoid creating a false impression of novelty, instead of a "historical" introduction chronicling the development of approaches used in the various scientific communities, the manuscript should rather start by describing the current accepted state of knowledge: CO2 emitted from fossil-fuel combustion changes radiative forcing in the atmosphere, and leads to CO2 fertilisation on the land (leaving out the oceanic effects, like acidification). By contrast, wildfires lead rapid re-growth of vegetation leading to CO2 uptake, long-term changes in vegetation distribution and standing live biomass, changes in land surface albedo, plus the same effects of fossil-fuel emissions, but modified by the difference in net flux. The historical rundown on past and recent approaches can then follow.

>> 1.5 We thank you for this comment, as our previous text did not adequately reflect our idea. The last sentence of the Abstract previously read: "Overall, our study calls for the explicit representation of fire 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." The purpose of this sentence was not to state that only coupled climate–carbon models should be used to study fire, but that studying the impacts of fire on global carbon cycling and temperature should be based on the explicit representation of fire. The new version of the sentence now reads: "Overall, our study calls for the explicit representation of fire activity as a valuable step to foster a more accurate understanding of its impacts on global carbon cycling and temperature, compared to conceiving fire effects as congruent with the consequences from fossil fuel combustion." Although we disagree that our previous text conveyed a "*false impression of novelty*", we also modified the Introduction along the lines suggested. <<

A more minor but still substantial comment: it is ignored that for wildfires in particular, a substantial part of carbon emissions is not in the form of CO2. This should be discussed. (Much of CO and CH4 emitted will end up as CO2, of course, but I think the point needs to be included).

>> 1.6 Thank you for thoroughness. This point was already mentioned in more general terms in the Methods (penultimate sentence of Section 2.1): "In all simulations, we included only the CO_2-related effects of fire and fossil fuel combustion, and not the associated aerosols and non-CO_2 greenhouse gases.", as well as in the Section on study limitations (second paragraph of Section 4.2, starting with "Second, we neglected all non-CO_2 emissions from fire and fossil fuel."). Neglecting non-CO_2 emissions is a common approach in studies that focus on the (relatively) long-term impacts of CO_2 emissions. Nonetheless, we added the following text to the Methods (just after the sentence cited above): "We note that fire releases some carbon as carbon monoxide (CO) and methane (CH_4); however, these species constitute less than 10% of the fire-emitted carbon (Andreae and Merlet, 2001) and get mostly oxidized to CO_2 on a timescale shorter than the one of interest here (Ehhalt et al., 2001; Boucher et al., 2009)". <<

(2) A further major comment is that the manuscript makes the point that only fully coupled models are capable of quantifying the effects of fire emissions. There is, I believe, the danger of creating an undue monopoly for the owners of such coupled models. This runs counter to the fact, often forgotten, that the more complex models are also the ones that are more difficult to parameterise and validate. It is true that the albedo effect cannot be simulated without an atmospheric model, but whether it has to be simulated all in a single model depends on the size of the perturbation from the mean state. The temperature effects of the albedo perturbation could be estimated by a GCM and added to the temperature prescribed in an off-line terrestrial dynamic vegetation model. A further possible setup to simulate the carbon cycle effects of both emission processes is the following: force an off-line land model and some simple off-line ocean carbon cycle model (e.g. the HILDA model) with prescribed CO2 (e.g. from one of the RCPs) and burned-area scenarios, compute fire emissions, land and ocean uptake, and derive consistent fossil-fuel emissions as the residual to balance the atmospheric CO2 budget. In this setup, it would become obvious that the difference is in the process of emission, but that all CO2 molecules are equal. It is also a setup that does not require the use of coupled models. The possibility of adequate off-line approaches should be acknowledged, and the criticism of previous approaches, which were most likely used simply for convenience, emphasised much more.

>> 1.7 We agree that fully coupled climate–carbon model should not have a monopoly on climate-related studies. Although we did not actually make the point anywhere in the manuscript that only such models should be used to study fire effects, we found a few formulations that might have caused this impression and modified the text accordingly. 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*." The modification to the last sentence of the Abstract (please see our response 1.5) also addresses this issue.

In the last sentence of your comment, we assume you meant that the approaches of Randerson et al. (2006) and O'Halloran et al. (2012) should be criticized less (not "*more*"). In any event, following comments from the other Reviewer, we removed the two parts of the text (Introduction and Discussion) related to these studies.

Incidentally, we agree that independent land, atmospheric, and ocean models can reproduce the results we obtained, provided they exchange the adequate information on relevant perturbations frequently enough—after all, coupled climate models consist of sub-models that simply 'talk' to each other at a prescribed frequency! Using an already-coupled model or independent models with ad hoc linking is to some extent a matter of personal convenience. The issue of parameterization is a slightly different one: some fully coupled models use fewer parameters than complex stand-alone atmospheric models. Regarding the setup provided, we are not sure to fully understand the "*carbon cycle effects*" part (why "*balance the atmospheric CO2 budget*" when our objective is to quantify the fire-caused atmospheric CO_2 anomaly?), but in any event, please note that the "*temperature effects*" and "*carbon cycle effects*" parts of the assessment would need to interact together frequently, because these two effects affect each other. <<

(3) In the list of limitations, what is missing is the fact that we are dealing here with a single model only.

>> 1.8 Thank you for this suggestion. We added a fifth study limitation (that we put in fourth position in our text) to Section 4.2 (where α_L stands for the land surface albedo): "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 α_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." Please see our response 1.9 for more details on vegetation regrowth. <<

(4) The recovery rates shown in Fig. 2 of Rogers et al. (2013) are about 3 times faster than those shown in Fig. 1a. This should be stated up-front instead of saying "they agree" (which they don't) and then the difference being explained. It is also not clear whether the explanation is sufficient to account for the rather large difference. What would be needed are results from a simulation that show recovery times similar to the observed ones.

>> 1.9 We appreciate once again your thoroughness; here, our response consists of four elements. First, our previous text was: "In northern forests, the succession among the different PFTs (Fig. 1a) agreed with observation-based trajectories (Rogers et al., 2013), while the impacts on biomass (Fig. 1c) and α_L (Fig. 1e) were consistent with field observations (Amiro et al., 2006; Goulden et al., 2011). The overall slower return to pre-fire conditions compared to observations came from the lasting climatic effects from the extreme 200 Pg C fire pulse (see Sect. 3.2)." So we did state that the quantitative results differed, just after noting the agreement in the succession trajectories, which we consider to be a qualitative concept. Nonetheless, we reformulated the text (please see below).

Second, the results from Rogers et al. (2013) are adequate for a qualitative assessment, but 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 (satellite) 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. Here we are not negating that recovery simulated by TRIFFID (the dynamic vegetation module of UVic) is too slow, but the actual discrepancy is likely smaller than suggested by the quantitative results from Figure 2e of Rogers et al. (2013) and could also be affected by other factors, including the specific geographic location of the grid cell used in our Fig. 1a.

Third, the objective of the first paragraph of Section 3.1 (including Fig. 1) is to show that post-fire simulated land responses are reasonable. The outcomes of our study, which involved comparing the very different effects from fire and fossil fuel combustion over a 1000-year timescale, do not hinge upon a post-fire vegetation recovery being possibly delayed by up to several decades. An untold reason why we showed these results is the following: 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.

Fourth, we performed additional simulations to assess whether the explanation we provided 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 that this element had 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.

While the previous considerations are interesting, they are much too detailed to appear in a study that does not primarily aim to quantify post-fire land dynamics, but deals instead with the major differences between fire and fossil fuel combustion effects. We therefore limited ourselves to adjusting the previous text in the light of these findings: "In northern forests, the succession among the different PFTs (Fig. 1a) was qualitatively similar to, but noticeably slower than, observation-based trajectories (Rogers et al., 2013).", deleted the previous sentence starting with "The overall slower return to pre-fire conditions [. . .]", and added the study limitation presented in our previous response (1.8). <<

The same publication as well as Almiro at et al. (2006) also show albedo for summer and winter/spring, both of which differ substantially from the values shown in Fig. 1ef). Please explain why that is and why you believe the published values support your model results. Also, the way p15193, 1st paragraph is written suggests that Amiro et al. (2006) is a source for biomass changes. I could not find such results in that publication. Please associate references more clearly, e.g. Goulden et al. (2011) show changes in biomass that are roughly consistent with Fig. (1c).

>> 1.10 Please remember that the fire-caused impact on energy exchanges is related to the change in α_{L}. Consequently, there is little to explain regarding the values shown in Fig. 1f, where α_{L} is practically unaffected (which is normal as vegetation recovery is extremely fast in savannas). Concerning Fig. 1e,

our results for the mean change in α_L are consistent with the values shown in Figures 1 (summer, day of year 177–205) and 2 (winter, DOY 1–60) of Amiro et al. (2006). The equation they provide for summer 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 α_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 α_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 α_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, especially in the context of the major variability involved (Amiro et al., 2006). 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 α_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)." Finally, we reformulated the sentence to better assign references: "Simulated fire-caused changes also appeared reasonable when compared with field observations for biomass (Fig. 1c) (Goulden et al., 2011) and α_L (Fig. 1e) (Amiro et al., 2006)." <<

Minor comments:

(1) I could not find a map of the grid cells designated as fire prone. This should be provided to give the reader a better feel for the realism of the spatial distribution. It would also be good to have the distribution of burned area within 27 degrees of the equator against the remaining areas compared to the GFED4 data, in order to better judge the sensitivity study presented in Fig. 9.

>> 1.11 Thanks for this suggestion; Figure R1 (at the end of our response) will be the new Fig. 1 and we will refer to it where appropriate. About its "*realism*", please remember that the spatial distribution of "fire cells" directly reflects locations where fire occurred at least once between January 2001 and December 2012 according to GFED4. The sensitivity analysis we presented in our Fig. 9 did not aim to reproduce the quantitative distribution of burned area across grid cells from GFED4 data—it would be impossible to obtain a 100 Pg C fire pulse under such a constraint—so we do not think the comparison suggested would bring useful information. <<

(2) p 15187, l5: there should be separate citations for the emissions and for the burned area. Burned area studies cited should be from observations rather from models, and emissions at least from studies based on observed burned area. Some of the papers cited are fully prognostic models, and their estimates of burned area differ far too much from (still uncertain of course!) observations to be citable here.

>> 1.12 We modified the text as suggested and moved to another sentence the references to studies using prognostic models (in which we replaced Thonicke et al. (2010) by Arora and Boer (2005), as the former also uses LPJ). The text now reads: "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)*~~from the direct combustion of vegetation and soil–litter (Kloster et al., 2010; Mieville et al., 2010; Thonicke et al., 2010; van der Werf et al., 2010; Randerson et al., 2012;~~

~~Giglio et al., 2013; Li et al., 2014)~~. 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)*." <<

(3) Further down in the same paragraph, Pechony and Shindell only simulate number of fires, while fire frequency is often defined as fractional burned area.

>> 1.13 Correct. But since the same authors have previously shown their "fire count" metrics to be commensurate with burned area (Pechony and Shindell, 2009) and given that it would be very surprising for the number of fires to increase without burned area also increasing, we consider that this simplification is not misleading. <<

(4) Next paragraph mentions "climate-fire feedbacks". The studies cited before do not address feedbacks, and it is not clear which feedbacks you mean. Apart from that, see my major comment (1) and suggestions to restructure the introduction. The term "climate-fire feedbacks" could actually be dropped altogether at it is not directly addressed here.

>> 1.14 Thank you for the suggestion, we modified the sentence to: "The net effect of fire on *global* carbon cycling has however received less attention than the consequences from future ~~climate–fire feedbacks~~*changes in fire activity*." <<

(5) p15188, 1st paragraph: as explained above, a set-up with an offline terrestrial model can very well account for fire-induced CO2 fertilization if the effect is for example treated as a perturbation around a mean state. At best you could state that it would be more difficult and lack the same level of consistency, even though there are always other trade-offs like parameterisability and validity of the model.

>> 1.15 Here, we may be using the word "offline" to mean slightly different things. For us, in a strictly offline setting the terrestrial model is driven by external climate data and CO_2 levels unaffected by the terrestrial state. If atmospheric CO_2 does not change based on fire activity, no fire-induced CO_2 fertilization will occur. In any event, this specific statement was not on offline approaches in general, but on the specific studies mentioned (Ward et al., 2012; Li et al., 2014), which did not account for fire-induced CO_2 fertilization and other relevant fire-related impacts (e.g., on temperature); please see our response 1.2 for the revised version of this text. <<

(6) Same page, last sentence: I suggest that the introduction start with this sentence, include a more detailed description of the effects, then goes on with the histrocal run-down and continues to criticize previously used approaches.

>> 1.16 Thanks for this suggestion, which we brought to our revised Introduction. <<

(7) p15189, l12-16: here again the use of the word "emissions" is misleading, as what you mean is the process of emissions (i.e. fire in a combustion chamber vs. wildland fire), not the emission itself (the effect of emission could be e.g. the injection height).

>> 1.17 Please see our response 1.3 to see how we addressed this issue. <<

(8) p15194, l8: "found into the ocean", typo?

>> 1.18 We revised the text to: "taken up by the ocean". <<

(9) same page, "These two features illustrate a fundamental distinction between fossil fuel and fire: fossil fuel emissions represent a near-permanent addition of CO2 to the active (i.e., non-geological) carbon cycling pools, whereas fire pulses temporarily reshue the carbon already existing in these pools." This statement, by being rather obvious not only for specialists, rather belongs in the introduction, if it is at all needed. Here, it sounds overly pedagogical.

>> 1.19 We removed the sentence. <<

(10) p15195, l21: but note that the recovery time is longer than in the studies cited.

>> 1.20 We assume you meant line number 12, not 21? Even in this case, we fear we do not understand the comment. The studies cited (Matthews and Caldeira, 2008; Eby et al., 2009) found a pretty stable increase in global mean atmospheric surface temperature following pulses of fossil fuel CO_2 (no "*recovery time*" observed), consistent with what we obtained for our fossil fuel pulses (Fig. 3a). <<

(11) next page, l1-7: please use more objective and neutral language than "much more similar", "yet at a closer look", and "not actually equal". This sounds like a personal account of a researcher. Please leave room for a different impression created in the reader of the manuscript.

>> 1.21 We changed the text as follows: "for the atmosphere, however, the CO_2 anomalies were ~~much~~ more similar (Fig. 4a vs. Fig. 2b)*, though not identical as can be seen in Fig. 4b.* ~~Yet a closer look at the results reveals that the atmospheric anomalies were not actually equal (Fig. 4b).~~" <<

(12) same page, l8: "Based on CO2 alone" is misleading, because it sounds like as if it implies no albedo effect.

>> 1.22 This is actually what we mean: without the albedo effect, global temperature would be higher in fire than in fossil fuel simulations (because atmospheric CO_2 levels are higher in fire simulations). "Based on atmospheric CO_2 alone, one would thus expect T_s to be higher for fire than for fossil fuels, yet the opposite was in fact observed (Fig. 4c) due to the opposite impacts on α_L (Fig. 4d)." <<

(13) Same page, l19-20: Note again that fossil-fuel (burning!) emissions also come from fire, so the statement does not make sense as it is. It is also not the emission that makes the difference, but the fact that different things are combusted.

>> 1.23 We modified the sentence to: "The previous results provide relevant information regarding fundamental differences between *the effects resulting from CO_2 emissions created by* fire ~~and~~*vs.* fossil fuel ~~CO_2 emissions~~*combustion*, but were based on single pulses of fire activity." (please also see our response 1.3). <<

(14) Next page, l2-5: I am wondering who would be interested in cumulative gross emissions, or fluxes in general? I would suggest dropping these arguments, cumulative gross fluxes are more or less an oxymoron. It also contributes to the impression of over-selling the results.

>> 1.24 We appreciate your skeptical perspective, but we disagree for two reasons. First, most research on global fire-caused CO_2 fluxes has dealt with gross fluxes until now. Given that we still find comparisons of these fluxes with CO_2 emissions from fossil fuel in the literature, the distinction between gross and net emissions from fire has apparently not been fully assimilated by everyone yet. We thus want to clarify that gross emissions will keep increasing forever after a transition to a new stable fire regime, even though the much more relevant annual net emissions have been close to zero for a long time. Second, one thrust of our manuscript is to systematically compare fire with fossil fuel combustion. In this latter case, cumulative gross fluxes (since 1850 or even earlier dates) are still the matter of very active research, so some readers might wonder why we would not touch upon the equivalent for fire. <<

(15) p15198, l25: again, it is not the CO2 emitted that makes the difference.

>> 1.25 Please see our response 1.3 to this same comment. <<

(16) next page, l7: "wildland fire", not fire. The sentence is rather trivial, because a vegetation burning fire of course has a much more direct impact on land carbon than the indirect effect of CO2. We are here talking about effects at vastly different scales.

>> 1.26 We agree that the sentence presents an idea that is easily understood, but do not see what is wrong with this: explanations have to start somewhere, and progressing from what is known to new or more complex considerations is often advisable. About the use of the expression "*wildland fire*" instead of "*fire*", our entire manuscript uses the latter and we think it is preferable to avoid mixing terminology. However, we added a note in the Introduction about the other terms that are sometimes used with the same meaning as "fire" here (i.e., wildland fire, wildfire, and open vegetation burning). <<

(17) p15200: "These fundamental differences imply that fire impacts cannot be accurately estimated from simulations of fossil fuel emissions in climate models." This statement is too general and one would ask who would have the idea to do this. Rather, the manuscript should specifically criticise concrete examples of previous publication and then state that such and such approximation has been found to lead to unacceptable results.

>> 1.27 Once again, we are afraid we do not understand the comment. This sentence was the beginning of a paragraph in which we provided two such concrete examples (Randerson et al., 2006; O'Halloran et al., 2012). In any event, we removed this entire paragraph based on the comments from the other Reviewer. <<

(18) p15201: please don't use purely prognostic simulations as a source for global emissions (see above comment).

>> 1.28 We modified the text to: "Our fire regimes were therefore more severe than the current situation on Earth, as seen with our equilibrium results of $\geq$ 0.9 Gha yr^{-1} for burned area and $\geq$ 7.3 Pg C yr^{-1} for gross emissions under stable regimes (Table 3), vs. current values of 0.3–0.5 Gha yr^{-1} *(Mieville et al., 2010; Randerson et al., 2012; Giglio et al., 2013)* and 1.5–3 Pg C yr^{-1} *(Mieville et al., 2010; van der Werf et al., 2010; Randerson et al., 2012)*, respectively~~ (Kloster et al., 2010; Mieville et al., 2010; Thonicke et al., 2010; van der Werf et al., 2010; Randerson et al., 2012; Giglio et al., 2013; Li et al., 2014)~~." <<

(19) p15204, l10-11: I am not sure why I should expect anything but gross emissions to continue? Please explain what is new and unexpected here, or drop the statement.

>> 1.29 We modified the text to: "~~While~~*As expected,* non-zero gross emissions were maintained indefinitely following a stable fire regime change, *whereas* most of the net emissions actually occurred relatively quickly after the regime shift and net emissions progressively decreased to almost zero (Fig. 5)." Please also see our response 1.24 to a very similar comment. <<

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 ± 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.

References

Amiro, B. D., Orchansky, A. L., Barr, A. G., Black, T. A., Chambers, S. D., et al., 2006. The effect of post-fire stand age on the boreal forest energy balance. Agr. Forest Meteorol. 140, 41–50.

Andreae, M. O., Merlet, P., 2001. Emission of trace gases and aerosols from biomass burning. Glob. Biogeochem. Cy. 15, 955–966.

Archer, D., Eby, M., Brovkin, V., Ridgwell, A., Cao, L., et al., 2009. Atmospheric lifetime of fossil fuel carbon dioxide. Annu. Rev. Earth Pl. Sc. 37, 117–134.

Arora, V. K., Boer, G. J., 2005. Fire as an interactive component of dynamic vegetation models. J. Geophys. Res. 110, G02008.

Arora, V. K., Boer, G. J., 2006. Simulating competition and coexistence between plant functional types in a dynamic vegetation model. Earth Interact. 10, 1–30.

Arora, V. K., Boer, G. J., Friedlingstein, P., Eby, M., Jones, C. D., et al., 2013. Carbon–concentration and carbon–climate feedbacks in CMIP5 Earth System Models. J. Climate 26, 5289–5314.

Bala, G., Krishna, S., Narayanappa, D., Cao, L., Caldeira, K., et al., 2013. An estimate of equilibrium sensitivity of global terrestrial carbon cycle using NCAR CCSM4. Clim. Dynam. 40, 1671–1686.

Bonan, G. B., 2008. Forests and climate change: Forcings, feedbacks, and the climate benefits of forests. Science 320, 1444–1449.

Boucher, O., Friedlingstein, P., Collins, B., Shine, K. P., 2009. The indirect global warming potential and global temperature change potential due to methane oxidation. Environ. Res. Lett. 4, 44007.

Bowman, D. M. J. S., Balch, J. K., Artaxo, P., Bond, W. J., Carlson, J. M., et al., 2009. Fire in the Earth system. Science 324, 481–484.

Bremer, D. J., Ham, J. M., 1999. Effect of spring burning on the surface energy balance in a tallgrass prairie. Agr. Forest Meteorol. 97, 43–54.

Eby, M., Zickfeld, K., Montenegro, A., Archer, D., Meissner, K. J., et al., 2009. Lifetime of anthropogenic climate change: Millennial time scales of potential CO_2 and surface temperature perturbations. J. Climate 22, 2501–2511.

Ehhalt, D., Prather, M., Dentener, F., Derwent, R., Dlugokencky, E., et al., 2001. Atmospheric chemistry and greenhouse gases. In: Houghton, J. T., Ding, Y., Griggs, D. J., Noguer, M., van der Linden, P. J., et al. (Eds.), Climate Change 2001: The Scientific Basis. Cambridge University Press, Cambridge, UK, pp. 239–288.

Giglio, L., Randerson, J. T., van der Werf, G. R., 2013. Analysis of daily, monthly, and annual burned area using the fourth-generation global fire emissions database (GFED4). J. Geophys. Res. 118, 317–328.

Goulden, M. L., McMillan, A. M. S., Winston, G. C., Rocha, A. V., Manies, K. L., et al., 2011. Patterns of NPP, GPP, respiration, and NEP during boreal forest succession. Glob. Change Biol. 17, 855–871.

Joos, F., Roth, R., Fuglestvedt, J. S., Peters, G. P., Enting, I. G., et al., 2013. Carbon dioxide and climate impulse response functions for the computation of greenhouse gas metrics: a multi-model analysis. Atmos. Chem. Phys. 13, 2793–2825.

Kloster, S., Mahowald, N. M., Randerson, J. T., Lawrence, P. J., 2012. The impacts of climate, land use, and demography on fires during the 21st century simulated by CLM-CN. Biogeosciences 9, 509–525.

Kloster, S., Mahowald, N. M., Randerson, J. T., Thornton, P. E., Hoffman, F. M., et al., 2010. Fire dynamics during the 20th century simulated by the Community Land Model. Biogeosciences 7, 1877–1902.

Landry, J.-S., Matthews, H. D., Ramankutty, N., 2015. A global assessment of the carbon cycle and temperature responses to major changes in future fire regime. Climatic Change 133, 179–192.

Li, F., Bond-Lamberty, B., Levis, S., 2014. Quantifying the role of fire in the Earth system – Part 2: Impact on the net carbon balance of global terrestrial ecosystems for the 20th century. Biogeosciences 11, 1345–1360.

Matthews, H. D., 2007. Implications of CO_2 fertilization for future climate change in a coupled climate–carbon model. Glob. Change Biol. 13, 1068–1078.

Matthews, H. D., Caldeira, K., 2008. Stabilizing climate requires near-zero emissions. Geophys. Res. Lett. 35, L04705.

Mieville, A., Granier, C., Liousse, C., Guillaume, B., Mouillot, F., et al., 2010. Emissions of gases and particles from biomass burning during the 20th century using satellite data and an historical reconstruction. Atmos. Environ. 44, 1469–1477.

O'Halloran, T. L., Law, B. E., Goulden, M. L., Wang, Z., Barr, J. G., et al., 2012. Radiative forcing of natural forest disturbances. Glob. Change Biol. 18, 555–565.

Pechony, O., Shindell, D. T., 2009. Fire parameterization on a global scale. J. Geophys. Res. 114, D16115.

Pechony, O., Shindell, D. T., 2010. Driving forces of global wildfires over the past millennium and the forthcoming century. P. Natl. Acad. Sci. USA 107, 19167–19170.

Randerson, J. T., Chen, Y., van der Werf, G. R., Rogers, B. M., Morton, D. C., 2012. Global burned area and biomass burning emissions from small fires. J. Geophys. Res. 117, G04012.

Randerson, J. T., Liu, H., Flanner, M. G., Chambers, S. D., Jin, Y., et al., 2006. The impact of boreal forest fire on climate warming. Science 314, 1130–1132.

Rogers, B. M., Randerson, J. T., Bonan, G. B., 2013. High-latitude cooling associated with landscape changes from North American boreal forest fires. Biogeosciences 10, 699–718.

Running, S. W., 2008. Ecosystem disturbance, carbon, and climate. Science 321, 652–653.

Scholze, M., Knorr, W., Arnell, N. W., Prentice, I. C., 2006. A climate-change risk analysis for world ecosystems. P. Natl. Acad. Sci. USA 103, 13116–13120.

Seiler, W., Crutzen, P. J., 1980. Estimates of gross and net fluxes of carbon between the biosphere and the atmosphere from biomass burning. Climatic Change 2, 207–247.

Thonicke, K., Spessa, A., Prentice, I. C., Harrison, S. P., Dong, L., et al., 2010. The influence of vegetation, fire spread and fire behaviour on biomass burning and trace gas emissions: results from a process-based model. Biogeosciences 7, 1991–2011.

Thonicke, K., Venevsky, S., Sitch, S., Cramer, W., 2001. The role of fire disturbance for global vegetation dynamics: coupling fire into a Dynamic Global Vegetation Model. Global Ecol. Biogeogr. 10, 661–677.

van der Werf, G. R., Randerson, J. T., Collatz, G. J., Giglio, L., 2003. Carbon emissions from fires in tropical and subtropical ecosystems. Glob. Change Biol. 9, 547–562.

van der Werf, G. R., Randerson, J. T., Giglio, L., Collatz, G. J., Mu, M., et al., 2010. Global fire emissions and the contribution of deforestation, savanna, forest, agricultural, and peat fires (1997–2009). Atmos. Chem. Phys. 10, 11707–11735.

Ward, D. S., Kloster, S., Mahowald, N. M., Rogers, B. M., Randerson, J. T., et al., 2012. The changing radiative forcing of fires: global model estimates for past, present and future. Atmos. Chem. Phys. 12, 10857–10886.

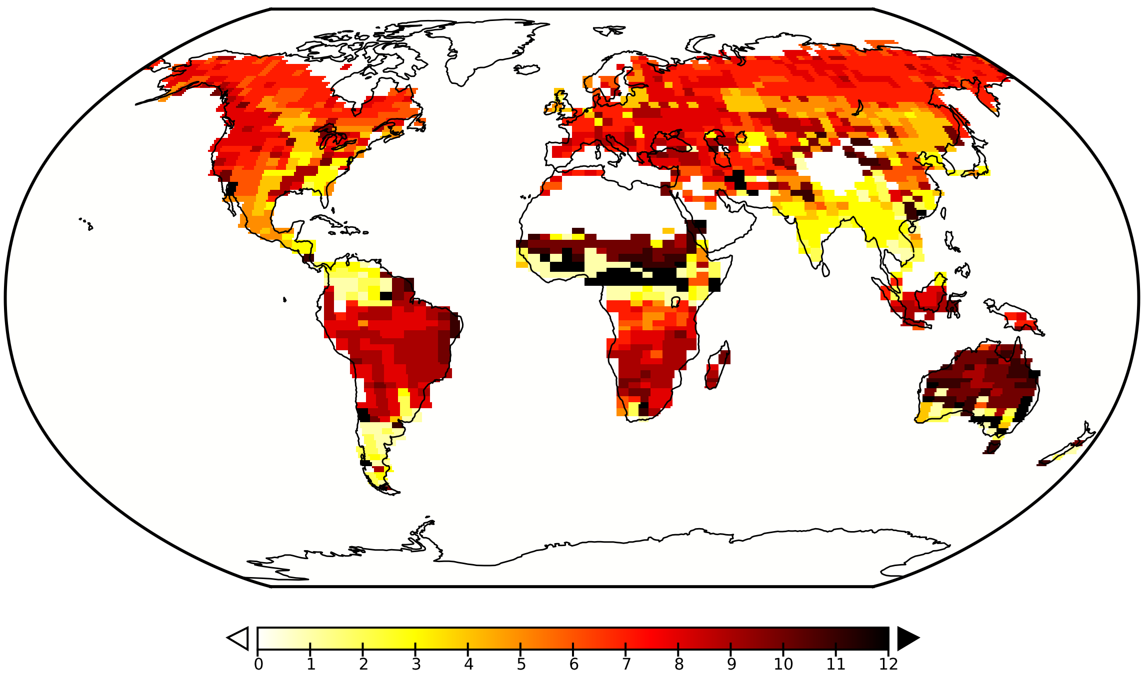

Figure R1. "Fire cells" used in the fire simulations. Numbers from 1 to 12 give the month of the year when fire occurs, whereas number 0 corresponds to grid cells without fire.