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Interactive comment on "Fire vs. fossil fuel: all CO_2 emissions are not created equal" *by* J.-S. Landry and H. D. Matthews

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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 CO_2 via CO_2 fertilisation that acts only at a global scale due to the fast mixing time of the atmosphere.

Major comments:

(1)

The fact that much of the carbon (not CO_2) 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.

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 CO_2 emitted by fire as compared to fossil fuel combustion" is simply wrong, which leads to significant confusion. The effect of the emitted CO_2 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 CO_2 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 CO_2 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.

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 "CO_2

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emissions" that is different, in the sense of "emitted CO_2", 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 CO_2 from wildfires and fossil-fuel combustion".

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: CO_2 emitted from fossil-fuel combustion changes radiative forcing in the atmosphere, and leads to CO_2 fertilisation on the land (leaving out the oceanic effects, like acidification). By contrast, wildfires lead rapid re-growth of vegetation leading to CO_2 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.

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 CO_2. This should be discussed. (Much of CO and CH_4 emitted will end up as CO_2, of course, but I think the point needs to be included).

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(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.

(3)

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

(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.

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).

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C5604/2015/bgd-12-C5604-2015-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 12, 15185, 2015.