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Interactive Comment

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

Anonymous Referee #2

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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.

Major comments:

(1) Much of the language throughout focuses on the fact that gross fire emissions are

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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) 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.

(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.

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.

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

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-[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.

-[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.

-[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.

-[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.

-[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

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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.

-[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?

-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.

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