**Biogeosciences
Discussions**

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Interactive comment on "The influence of vegetation, fire spread and fire behaviour on biomass burning and trace gas emissions: results from a process-based model" *by* K. Thonicke et al.

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This is important and necessary work and certainly conceptually a step forward in global fire modelling. However, I think that both model SPITFIRE and this paper are not ready. So, the modelling part of the paper contains errors and many wrong statements. There is a serious flaw in simulation of fuel moisture which excludes further using of SPITFIRE within DGVMs at the moment. Presented validation and model evaluation do not support claim that spatial and temporal of fires are presented realistically. I recommend a major and serious revision of SPITFIRE model and the paper before possible publication in "Biogeosciences". Bellow is my comments which may help to

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authors in major revision.

Modelling part.

2.2 Basic equation.

Basic equation, despite of being taken from Reg-FIRM (see equation 1 Venevsky, et.al.) and then reworded, contains error. So Pd is the product of expected number of fires per day per area (i.e. numfires*ha-1*d-1) multiplied by mean fire area in ha and this product (units are numfires * d-1) is named as the probability. However, the probability is unitless. As a result equation 2 is also wrong because it has ha * d-1 on the left side and numfires * ha *d-1 on the right side. Here I think you should just take equation 1 from Reg-FIRM and not to reinvent something new.

2.2.1 Ignition events.

Lightning ignitions.

The statement that 0.2 from all lightning flashes are cloud-to-ground is not anyhow justified (and wrong). This ratio depends on many factors, the simplest is dependence from latitude (see Price and Rind, 1993 in GRL). The ratio is changing from 0.25 to 0.35, it is not equal to 0.2 (see the same paper). The statement that "Interannual variability in lightning is small in comparison to the seasonal variations" is not justified (and wrong – so for example in Alberta the number of observed cloud-to-ground flashes in 1989 was approximately 80 000 and in 1994 - 160 000 see [Wierzchowki et al., 2002]. This rather wrong statement is an effort to mask your inability to simulate interannual variability of number of cloud-to-ground flashes and interannual variability of number of lightning ignitions. This statement should be removed and it should be clearly stated that you cannot simulate interannual variability of number of lightning fires/ignitions.

Human ignitions.

Basic equation describing number of ignitions, equation 3, is taken from Venevsky et.al. 2002 (see equation 10), so should be referred properly. The statement that a new for-

mulation of dependency of number of ignitions by population density (see equation 4 for K(Pd)) "is more realistic than the function introduced by Venevsky et al. (2002), which allows the modelled number of human-caused ignition events to increase indefinitely with increasing population" is irrelevant (and wrong), because population density of any species (including humans) cannot grow to infinity even theoretically. This statement should be removed. The only prove that the new formulation is better can be a direct comparison of old and new formulation against observed data, for example for number of human induced fires in Spain, where the old formulation was shown to be working (see Venevsky, et.al., 2002). Of course, any other dataset with observations can be used for these purposes. The statement that "Estimates of the constant a(ND) were obtained by an inverse method, using data on numbers of human-caused fires and population densities for various regions" is unjustified. What is this "an inverse method"? What did you invert and how? Your number of fires, like in Reg-FIRM, depends as on Fire Danger Index, sensitive to weather data, so it is unlikely that you can make inversion so easily, accounting that stochastic generator is used for precipitation in LPJ DGVM (see Gerten, 2004). Anyway, you should give values and geographical distribution of a(Nd), in order the other people can replicate your model/results. What is ND in your equation 3 by the way? In Reg-FIRM (Venevsky et.al.) it is the number of day, so if you copy an equation, please, also copy an explanation to the equation.

2.2.2 Fuel moisture content/2.2.3 Fire danger.

Equation 5 (simplified Nesterov Insex) with method of Tdew calculation is taken directly from paper of Venevsky, et al., (see equations 2 and 3 with explanations) and should be referred properly. Because, the original Nesterov Index (NI) was slightly simplified, you cannot go around citation of Venevsky et.al. Equation 6 is very interesting and in fact is a key equation for SPITFIRE, as it provides direct dependence of number of fires and areas burnt from climate and vegetation. At first glance I quite liked it, however, after looking more closely I found that it sets large conceptual problems and hinders significantly applicability of SPITFIRE in DGCVMs and absolutely not justified.

I THINK THAT THIS EQUATION IN ITS RECENT FORM AND PRESENTATION IS NOT ACCEPTABLE. Indeed,

a) in physical sense it is wrong because on the right side it contains wo fuel moisture, which is supposed to be a physical value with units either m3 of H20/ m3 fuel, or kg of H2O/ kg of fuel (it is not clear from equation 7 if it should be volumetric or mass relative fuel moisture, because moisture of extinction is not anyhow described in the paper – see comment of Reviewer 1). On the left side on the contrary there is an exponential function which does not have obvious physical units (the units under exponential look like something kg*m-1 *grad C*grad C).

b) even if we ignore the violated physical units of this equation, we can see that by its design it is senseless and not robust to provide realistic values for w0. Indeed, under the exponent in the right part of equation we have product of simplified Nesterov Index (with a range 0-6000) and the value of (a1*w1+a2*w2+a3*w3)/(w1+w2+w3), where wi are densities of fuels of different classes in kg*m-2 and "The values of ai applied to the three fuel classes are in inverse proportion to their surface area-to-volume (SAV) ratios". Unfortunately, these key ai values are not described any further and not presented in any Tables. By sense they should be vegetation (PFT) specific. Taking into account that values of NI have an order of hundreds to thousands under the exponential, to get relative fuel moisture w0 >0.5 (moderately wet fuel), the values of ai should be in order 0.001 to 0.0001. The imaginative cubic fuel particle with SAV equal to 1000 to 10000 required, will have the side 0.006 to 0.0006 (m) (side is calculated for cube as 6/SAV). This size is too small even for fine fuel particles. The requirement to have more wet fuel, say with 0.7-0.8 relative fuel moisture moves our fuel particles to the size of dust and even almost nanoparticles! Thus we came to visible nonsense in the design of this equation. Besides, from calculation point of view we have multiplication of very small value ai with high relative possible error, to another value wi with unknown relative possible error, to another value NI with unknown relative possible error. Afterwards we take an exponent from this product, blowing up relative possible error for fuel

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moisture w0, FDI and as an end effect to area burnt and number of fires which linearly depend from FDI. Thus, equation 6 by design can bring high relative error and cannot provide robust results.

c) For practical application within DGVMs of equation 6 one needs to know "quantities of the three fuel classes (gCm−2) (1-h, 10-h and 100-h fuels)". To my knowledge no DGVM provides these values. LPJ DGVM also does not calculate them. If you know how to calculate quantities of the three fuel classes (gCm−2) (they should probably be PFT specific?) and you included it already it in LPJ DGVM, please, describe it. Because, this is a serious flaw in modelling part of SPITFIRE, I suggest redoing this key part. I see three possible ways how proceed. First way: you get rid of equations 6 and 7 and return to the original old formulation of FDI provided by Reg-FIRM (Venevsky, et al, equation 7), which does not have listed problems. Second way: you put some dimension coefficients where it is needed in equation 6 of SPITFIRE, collect observed data for fuel moisture and quantities of the three fuel classes (gCm−2) (1-h, 10-h and 100-h fuels) for all or some PFTs and make non-linear regressions, to get these coefficients before and inside the exponential in the left part of equation 6. Third way: you construct new equation for fuel moisture, which includes NI, based on some physical reasoning, and prove it against the observed data. YOU MUST FIX THIS PROBLEM BEFORE CLAIMING THAT SPITFIRE IS READY, BECAUSE SIMULATING OF PHYSICAL VALUE, HERE FUEL MOISTURE, WITHOUT PHYSICAL REASONING AND WITHOUT SUPPORT OF FIELD DATA IS IMPOSSIBLE.

2.2.4 Rate of spread (ROS)

Classical Rothermel equation combined Canadian Forest Service method is used to estimate ROS. Some 12 equations are used to estimate ROS from wind speed, fuel moisture and bulk density of fuel in SPITFIRE in line with the Canadian methods. Reg-FIRM (Venevsky, et.al., 2002) uses as well Rothermel equation, but in simplified form, so calculation of ROS from wind speed, soil moisture (proxy for fuel moisture) and bulk density of fuel requires only one equation. The Canadian approach is undoubtedly

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showed to perform well for boreal zone for landscape level fires. However, an application of this approach for other zones at global scale and at coarse resolution is questionable sensu model benefits-computational costs. Direct comparison of simplified approach of Reg-FIRM and complicated approach of SIPTFIRE for ROS estimation is desirable and can be done on example of area burnt in Spain, because computational effort suggested by SPITFIRE for calculation of ROS and area burnt will hinder possible use of SPITFIRE by other models. Equation 13 for calculation of fire duration is very strange. Where and/or how did you get it? Indeed according to equation 13, if fire danger index (FDI) is 0 (no fire possible) fire duration is 4/6 min (40 seconds), if FDI=1 (highest fire potential) fire duration is 240 minutes (4 hours). For boreal zone (Russia, Canada) typical duration of wildfire is 12-24 hours. Please, delete this equation for fire duration and find something more acceptable. BTW you need to limit in the model your fire duration to 4 hours, may indicate that you have some problems with ROS calculations, which I suspect may be explained by incorrect estimate of fuel moisture – the fuel is probably almost always dry in your model (see my previous paragraph)

2.2.6 and 2.2.5

These parts are most interesting in the paper. Representation of post-fire mortality in Glob-FIRM (Thonicke, et al) and Reg-FIRM was too simplistic I believe. So, application of Byram approach to estimate fire intensity and afterwards cambial damage and post-fire mortality is an important step in advancing of global fire models. What is required for this paper, however, is to validate these new process descriptions within SPITFIRE against observed fire intensity (remote sensing products are available –see Wooster et al) and against post-fire mortality (field observations exist - elsewhere).

Model evaluation

Figure 2a must be deleted from the paper. The number of fires presented in this map is incorrect (4 to 10 times higher) and thus, contains misleading information. This can be easily seen on example of Spain (compare with Moreno data) where average number

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of fires is typically less than 0.02 km-2*yr-1 and in the case of SPITFIRE produced Figure 2a it is between 0.08 and 0.16 km-2*yr-1

Validation is almost non-existent in this paper. So, the claim in abstract that "Overall SPITFIRE produces realistic simulations of spatial and temporal patterns of fire under modern conditions" is not confirmed by presented materials. It must be deleted in case of "temporal patterns" completely, because no validation against observed time series is presented. In case of spatial patterns the validation is not-sufficient. So geographical comparison in the style "model shows fires and RS product shows fires in the grid cell" presented as a major validation for spatial patterns in Fig. 5 and 6 is not convincing for process-oriented model. The only proper comparison model/observations for Siberia presented in Fig.7 for fraction of burnt area does not say in favour of SPITFIRE – fires are not simulated in Southern Siberian boreal forests where they are pretty usual.

The only graph with quantitative data Figure 4, where observed against simulated fire length season is presented, is unclear, because what is the fire length season in SPIT-FIRE is not determined in the paper.

Editing

Generally, I agree with the reviewer 1 that SPITFIRE conceptually is follow-up and an extension of Reg-FIRM to global scale, which would be better to mention in the paper. The authors may be aware that SPITFIRE is not the only follow-up and extension of Reg-FIRM to global scale. The first full development of Reg-FIRM to global scale within DGVM named SEVER-FIRE appeared already in year 2005 (see invited talk of Venevsky, Patra, Maksyutov and Inoue at International Carbon Dioxide Conference 7, Boulder, USA, 2005 www.esrl.noaa.gov/gmd/icdc7/proceedings/talks/venevsky.ppt). SEVER-FIRE could be also mentioned in the paper. This, however, is the matter of taste and scientific politeness, so I do not insist on it.

The part presenting the results of simulation with SPITFIRE is far too long in comparison with obtained results. This part should be shortened 2-3 times.

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