

Evaluation nhess-2018-149

General comments

The paper mainly presents a literature review about risk assessment for wildfire with a particular concern for the risk of fire of oil and gas facilities. The author is particularly interested by the region of Alberta Canada. To present the context, the author reproduces the maps of the main variables of interest (fire weather index, fire danger index, fire rate of spread, head fire intensity) for a particular fire occurred on May 1, 2016 for Canada as a whole, as reported by the Canadian fire authority. The author also presents the ignition probability map elaborated by the Canadian fire authority, for Alberta (not for Canada). Then the general model of risk estimation is presented, based on the wildfire probability estimation, using the ignition probability and the burn probability. Some empirical and semi empirical models are presented for burn probability, as examples. Then the author presents head fire intensity (HFI) models, in a review form. In fact the author will finally focus on the estimation of the area-fire or reaction intensity, which depends on HFI. The reaction intensity at a distance x from the flame center is the main variable of interest to study the risk for oil facilities because it is the explanatory variable of both the time to failure of the exposed vessel and the damage probability of the vessel. The model of the conditional damage probability is presented for a simplified and hypothetical case consisting of three tanks (domino effect scenarios).

While the paper is generally well written and well documented, I think that the literature review is too long in comparison to the scope of the paper and to the final objective which is the calculation of the conditional damage of the vessels.

The reader who is not aware about wildfire risk models may learn a lot about these models by reading the document. However, a reader who knows these models will have a deception especially that the case study is simplistic and with no application.

My detailed comments and questions are listed in the following:

- **Lines 65-67** this sentence should outline the methodology adopted in the cited paper. It is important to mention that high-hazard resolution and demographic projections were used under a business-as-usual scenario of greenhouse gas emissions. The referred paper diagnostics is that by 2100 2/3 of the European population will be affected annually by 2100. The author should correct the sentence to be close to what is said in the reference [https://www.thelancet.com/journals/lanplh/article/PIIS2542-5196\(17\)30082-7/fulltext](https://www.thelancet.com/journals/lanplh/article/PIIS2542-5196(17)30082-7/fulltext). A comment was addressed to this paper [https://www.thelancet.com/pdfs/journals/lanplh/PIIS2542-5196\(17\)30084-0.pdf](https://www.thelancet.com/pdfs/journals/lanplh/PIIS2542-5196(17)30084-0.pdf) outlying that people should be aware about the assumptions of the study (constant human vulnerability for the projection period, equal to present vulnerability). So the author should also take this in consideration. The method of estimating human costs is very important to notice. Finally I don't know if "affect" (the word used by the author) is the same as 'to be exposed' which is the terminology of Forzieri et al. (2017).
- **Lines 77-78.** I would say that the sentence is not related to the content of the referenced papers; the papers that are referenced are not dealing with the same scopes and methods; some are operational and others methodological. Some are about risk assessment and others about risk management. Some are about projections within climate change others are about assessment of observed fires in the field. Thus, they need to be considered separately; the sentence should be reformulated with emphasizing on the specificity of each referenced paper.

- **Line 79** The author should mention the document “fireSmart for the oil and gas industry”
http://www.enform.ca/files/Comms_Upl.../events_psc/2017/Speaker_p.../PSC_Wildfire_2017_May3_Connor_Wollis.pdf
- **Line 127.** What do author means by “in the absence of methodology to quantify the risk”? The sentence should be reformulated.
- Line 130 In Heymes et al. (2013) study the distance is 50 m. Is what the author means when using “such”?
- Line 146 “... fuel (trees...shrubs)”. This parenthesis should be presented earlier in the text, when using fuel for the first time.
- Line 155. The reference is without date.
- Line 156 In Figure 3a, the scale needs to be given.
- Lines 156-160 In the text, it must be clear (like in Line 176) that this was obtained by the source indicated under Figure 3a and 3b.
- Lines 167-173 the key word PROMETHEUS should be included in the text. Also it should be noticed that the model is based on Huygens principle of wave propagation.
- Lines 186-187 the author should briefly specify what are the main common points and differences between these models (Latham, Lawson, Anderson). Is the title of the paragraph complete or is it necessary to add “Canadian”?
- Line 192 A new paragraph should start from “Using the developed..”
- Line 194. Author need to justify considering severity in monetary units. Why “preferably” knowing that it is a lot of work and assumptions to convert direct damages in monetary units?
- Line 195 Author should use the word severity
- Line 198 Author needs to justify the choice of 400 m². Is it in linkage with the spatial scale evocated in the burning probability? See for example the glossary
<https://www.nrfirescience.org/sites/default/files/documents/ScottGlossaryWildlandFireTerms.pdf>
- Line 219 It should be noticed that satellite data were used in Jurdao et al. paper.
- Line 221 Drought code is not a parameter. Author need to reformulate.
- Lines 256-258. Do the cited Papadopoulos et Pavlidou (2010) review includes sophisticated models only? The sentences should be reformulated.
- Line 260 Author should justify why to detail about the model of Drossel et Schwabl which is older than others previously referenced.
- Line 263 Moore’s neighborhood. It should be referenced (it is well explained in
https://bib.irb.hr/datoteka/278897.Ljiljana_Bodrozic_ceepus2006_2.pdf)
- Line 271 author should give a reference for combining with Monte Carlo simulations. It is also needed to justify why using Monte Carlo simulations.
- Line 284 North should be indicated in Figure 6.
- Line 287 Do Monte Carlo simulations continue to be adopted in the literature?
- Line 288 –density of trees). Do author means only density of trees?
- Line 305 Un more recent paper by Cheney might be referenced for grass land case such as Cheney et al. (1998)
https://www.researchgate.net/publication/248885594_Prediction_of_Fire_Spread_in_Grasslands
- Line 320 Is there a difference between guidelines for British Colombia and Alberta? It is not said.
- Line 325 On which model is based the map of Figure 4b?. Is it different from the models presented in Eq. (5) and (6)? It is not clear. What is the scale (mesh) for this

map (Figure 4b)? Is downscaling needed for operational purposes? ‘Figure 4b can be used directly’ and what about Figure 9?

- Line 417. Maybe there is a mistake in the text because Q_x is an explanatory variable in Eq. (9). Maybe author means Eq. (8)?
- Line 415 what is the normal variable linked to p has to be linked to ϕ ?
- Line 450 the author should explain the novelty of the actual submission with respect to his previous paper (Khazad et al. 2018) https://www.researchgate.net/publication/325995779_Quantitative_assessment_of_wildfire_risk_in_oil_facilities
- Line 459 It is not clear from the text where the presented methodology is dynamic. In this paper author did not use up-dating of information. The sentence should be reformulated especially within the content in Line 462 (*can help*).
- Line 467. Do the author means that it is a perspective to use the model to optimize or rationalize the buffer zones? The word perspective is missing.