

# Referee Report

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## *General comments to the author*

The idea of the authors is interesting and would be of some use for the fire management in the study area but is not very innovative. This paper has two major difficulties: (1) reduced the temporal dimension of the fire data; (2) does not unequivocally demonstrate the usefulness of the results obtained. I'm sorry but, at this stage, the manuscript is very far from being accepted for publication. There are a lot to explain and too many corrections to make. Please see my "Some more specific comments", mainly focused until the first subsection of the section of results, but referring to each and every sentence of this subsection.

## *Some more specific comments*

1. (Page 3, line 7) It seems that the authors are using an em dash. Please replace it by an en dash and include a space character between the numbers and the dash; Do the same for periods of time (e.g. in page 13, line 19, please replace "2003-2004" by "2003 – 2004") and elsewhere in the manuscript;
2. (Page 5, line 29 and page 13, line 9) The FWI codes and indices do not have a large-scale character but are being computed with large scale data; Please see (page 19, lines 7 – 8) in the conclusions where this is clear;
3. (Page 6, line 15) Why do you use the ( $1.875^{\circ} \times 1.92^{\circ}$ ) NCEP reanalysis product and no other with better spatial resolution?
4. (Page 6, lines 23 – 24) Why do the authors "only used fire occurrence (number of fires) to represent of fire activity in relation to fire weather indices in the study area"? Burned area is not also an important aspect of fire activity and the most immediate consequence of fire? In many biomes, including boreal forests, larger burned area are due to a relatively small number of fires. In addition, can you obtain the exact number of fires from the MODIS data?
5. (Page 6, lines 25 – 27) So, you are saying the number of fires is only dependent on weather conditions? or, can you assure that all human initiated fires were excluded from the analysis? In this case, how was this performed? Please see page 16, lines 8 – 10) in the "Discussion" where is written that "more than 87 % of fires in boreal Russia are human-caused"; I suggest a different/better explanation for the decision of using the MODIS and the "number of fires" as a measure of fire activity.
6. (Page 7, lines 10 – 11) According to the Köppen–Geiger climate classification system "extremely continental" is not a main type of climate (see for example Kottek et al. 2006 or Peel et al. 2007). This is a three letter classification scheme where the first (A to E) define the main type of climate, the second is determined

by the precipitation regime and the third by the temperature. The “extremely continental” is a subtype (identified by the third letter “d” of the group D (Continental/microthermal or snow climates), indicative of three or fewer months with mean temperatures above 10 °C and a coldest month temperature below - 38°C;

7. (Page 7, lines 16 – 17) In this study, is there any difference between wildfire and forest fire? This sentence may be confusing and should be rewritten;
8. (Page 7, line 19) The title of section 2.2 should be changed because satellite data is not necessarily different from weather data, i.e., weather data may include satellite data;
9. (Page 7, lines 20 – 25) The authors should explain how the 8 day MODIS active fire product (MOD14A2) was used in this study, namely to compute the monthly number of fires and to produce, for example, the Table 1;
10. (Page 8, lines 1 – 8) There is some repetition in the introduction; In my opinion, the detailed description of the data and methods should be moved to section 2;
11. (Page 8, lines 9 – 10) The spatial resolution is unnecessarily repeated;
12. (Page 8, lines 20 – 23) What is the definitions of burn trend? Why use trend and tendencies in the same sentence? This sentence should be rewritten;
13. (Page 8, line 24 to Page 9, line 16) The FWI system is very well known and described in the literature; if the authors decide to present additional aspects of the indices and codes of the CFFDRS, citations/references are needed; if not the citation to a few studies (already in the list of references) are sufficient;
14. (Page 9, lines 19 – 24) Repetition;
15. (Page 12, lines 5 – 6) Please, explain the reader why WCS without normalization to the single wavelet power spectrum, this can produce mis-interpreting the relationship between two time series;
16. (Page 13, lines 11 – 12) What was the test used? The (numerical) results must be presented;
17. (Page 13, lines 11 – 12) Why? What are the potential consequences/limitations?
18. (Page 13, lines 18 – 19) Which results are on the basis of this sentence (Table 1, Figure 2, and/or Figure 4)? A mention to any of these Table/Figures must be included? What are the (mathematical/statistical) definitions of “severe” and “critical”? Without fire statistics for other periods, how can the reader know/confirm that “Wildfires in south central Siberia region were found to be severe for the last 14 years”? What are the meaning of defining the “critical periods” as “2003–2004, 2006–2008, and 2010–2012”? This means “2003 and 2004; 2006, 2007 and 2008; and 2010, 2011 and 2012” or the transition (some months in 2003 and other months of 2004, ...? The second option cannot be true because of Figure 3 and the following paragraph (page 13, line 24 to page 14, line 3); the first either, because of the results presented in Table 1 and Figure 4 (assuming that the missing values in Table 1 for the months of January, February, November and December are negligible in comparison with the value for the other months). For example, according to this table, both the annual number of fires and burnt area in 2004 (844 fires and 346 km<sup>2</sup> burnt) where much smaller, just about 20% of the number of fires and 7% of the burnt area in 2002 (3956 fires and 4844

km<sup>2</sup> burnt), not considered a “critical” year! Results for 2010 are slightly higher but also much smaller than for 2002; why include 2004 in the list of the critical periods but exclude 2013 when occur much more fires and burnt area (970 fires and 1266 km<sup>2</sup> burnt)? This must be clarified and changes must include the conclusions (page 19, lines 1 – 2);

19. (Page 13, lines 19 – 22) Results presented in this sentence cannot be easily drawn solely from Figure 2; According to this figure, the highest value of the number of fires were obtained for 2008 while the highest values of the FWI indices was never on 2008; the year with the second highest number of fires was 2003 and the indices with an higher value in this year are DMC and BUI; most of the indices present higher values in the last years (2010, 2011 and 2012) when, according to Figure 4, the number of fires where only in “relatively” high in 2012 and 2011 but much smaller than in 2003 and 2006; in fact, values for the great generality of the indices were very small for 2006; Apparently, the indices were only able to rate adequately the fire danger in 2003!
20. (Page 13, line 23 to page 14, line 3) What is the number of fires and burnt area in the months not considered in Table 1? What is the need/objective to define “non-regular” seasons, using the same names of the calendar seasons and/but with different durations (Spring 4 months, summer and winter both with 2 months)? If June was considered a summer month (as usual) the percentages of number of fires will be very different; if these seasons are fire seasons, what was the criteria used to define them? Please see Conclusions (page 19, lines 5 – 6) where the reader may be confused; What is and why was not present and discussed the intra-annual variability of the burnt area? This is particularly important because it seems that there are “large burned areas (> 1000 ha) within the study area”, help to understand the option of “only used fire occurrence (number of fires) to represent of fire activity in relation to fire weather indices in the study area” and help to determines the relevancy of this study and its findings; the reason cannot be because the “correlation between FWI components and burned areas was poor and varied depending on ecozone” (page 18, lines 20 – 21);
21. (Page 14, lines 1 – 3) “In the primary fire season, May was the most severe month of fire activity accounted for 48 % of total fires during fire season. Two other peaks of fire activity were in July and September annually (Fig. 3)”. For me, it does not make much sense to define 3 consecutive fire seasons (with decreasing “magnitude”), two of them with just 2 months and, in addition, identify fire peaks in these two fire seasons; In my opinion you only have 1 fire season with a prominent peak in May with almost 50% of total number of fires;
22. (Page 14, lines 4 – 8) I believe that the authors have three options: (1) remove this sentence from the manuscript, (2) include a citations/reference for these results ; or (3) describe the data and the methodology used to obtain these results;
23. (Page 14, lines 9 – 10) “In general, higher number of fires resulted in larger burned area, except for fires in 2007 and 2008 (Fig. 4)” This kind of sentences cannot be accepted! What is the meaning of “In general”? It means in any place of the world? It means that the fires in the study regions have approximately the same size? How

can this sentence result from the analysis of Fig. 4? Why the exception of 2007 and 2008? Why not 2002 and 2009?

24. (Page 14, lines 9 – 12) “Visual interpretation of fire activity during this period, almost all fires occurred in the flat and low elevation areas that resulted in the high rate of fire spread and thus larger burned patches even small number of fires”. The authors cannot expect that such a sentence be accepted easily. The reader do not have the possibility to verify the obtained results. Once again the authors have three options: (1) remove this sentence from the manuscript, (2) include a citations/reference for these results; or (3) describe the data and the methodology used to obtain these results; the same type of sentence appear in the discussion (e.g., Page 18, lines 14 – 16);
25. (Page 14, lines 12 – 15) “Both fire occurrence and burned area data showed a cyclic pattern of about 4–5 years interval in south central Siberia region with the severe fire/burn years in 2003, 2008 and 2012 (Fig. 4)” what was the methodology used to obtain these results? What is the (statistical) significance?
26. (Page 16, lines 8 – 10) For me, it is not clear why the fact that “more than 87 % of fires in boreal Russia are human-caused” is consistent “with the spring-dominated fire season found in this study” even with the potential explanations provided in the Discussion section (Page 16, lines 10 – 16);
27. (Page 16, line 19) Please present a definition of burn trend which is not shown in Figure 4;

#### Tables and figures

28. Table 1 needs much work. It is not clear what is shown. Only after some calculations it is possible to find that the monthly values are of the number of fires; replace “ *Fire*” by “ *Number of fires (#)*” ; replace “ *Monthly*” by “ ”, Total or Sum; in fact the intra-annual variability of the burnt area is not shown and the caption should be changed;
29. Please include the latitude and longitude ranges in Figure 1.
30. Used the same name for the NCEP reanalysis product in both Figure 1 and Figure 2 “reanalysis-2”;
31. In Figure 3, what is the definition of “Trend of fire occurrence”? Please replace “Number of fire” by “Number of fires”;
32. Figure 4 is not necessary because exactly the same information is presented in Figure 7;

#### *Technical corrections*

1. Do not start a figure caption (e.g., of Figure 2 and Figure 3) with a numeral;
2. Used the same name (“NCEP reanalysis-2”) for the NCEP reanalysis product in the captions of both Figure 1 and Figure 2.

3. To be coherent, please uniform the reference “de Groot et al. 2012” or “De Groot et a. 2012” in the text (line 3, page 4 and line 10, page 16) and in the reference list (page 21, line 4);
4. (Page 7, line 20) Replace “tile h23v03and tile h23v03” by “tile h23v03 and tile h23v03”;

### *Manuscript Evaluation Criteria*

Principal Criteria	Excellent (1)	Good (2)	Fair (3)	Poor (4)
<b>Scientific Significance:</b> Does the manuscript represent a substantial contribution to the understanding of natural hazards and their consequences (new concepts, ideas, methods, or data)?				×
<b>Scientific Quality:</b> Are the scientific and/or technical approaches and the applied methods valid? Are the results discussed in an appropriate and balanced way (clarity of concepts and discussion, consideration of related work, including appropriate references)?				×
<b>Presentation Quality:</b> Are the scientific data, results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of technical and English language, simplicity of the language)?			×	

## *Access Review (Quick Report), Peer-Review & Interactive Public Discussion (NHESSD)*

Manuscripts submitted to NHESS at first undergo a rapid access peer-review (initial manuscript evaluation), which is not meant to be a full scientific review but to identify and sort out manuscripts with obvious major deficiencies in view of the main principal evaluation criteria.

Manuscripts rated 4 (poor) in any of the principal criteria are normally rejected without further review and discussion. Manuscripts rated 1-3 (excellent-fair) in all criteria are normally published on the Natural Hazards and Earth System Sciences Discussions (NHESSD) website, the discussion forum of NHESS, where they are subject to full peer-review and Interactive Public Discussion.

In the full review and interactive discussion the referees and other interested members of the scientific and technical communities are asked to take into account all of the following aspects:

1. Does the paper address relevant scientific and/or technical questions within the scope of NHESS?

No

2. Does the paper present new data and/or novel concepts, ideas, tools, methods or results?

No

3. Are these up to international standards?

No

4. Are the scientific methods and assumptions valid and outlined clearly?

Only partially

5. Are the results sufficient to support the interpretations and the conclusions?

Only partially

6. Does the author reach substantial conclusions?

No

7. Is the description of the data used, the methods used, the experiments and calculations made, and the results obtained sufficiently complete and accurate to allow their reproduction by fellow scientists (traceability of results)?

No

8. Does the title clearly and unambiguously reflect the contents of the paper?

No

9. Does the abstract provide a concise, complete and unambiguous summary of the work done and the results obtained?

No

10. Are the title and the abstract pertinent, and easy to understand to a wide and diversified audience?

Yes

11. Are mathematical formulae, symbols, abbreviations and units correctly defined and used? If the formulae, symbols or abbreviations are numerous, are there tables or appendixes listing them?

Yes

12. Is the size, quality and readability of each figure adequate to the type and quantity of data presented?

No

13. Does the author give proper credit to previous and/or related work, and does he/she indicate clearly his/her own contribution?

Yes

14. Are the number and quality of the references appropriate?

Yes

15. Are the references accessible by fellow scientists?

Yes, in general.

16. Is the overall presentation well structured, clear and easy to understand by a wide and general audience?

No

17. Is the length of the paper adequate, too long or too short?

Adequate

18. Is there any part of the paper (title, abstract, main text, formulae, symbols, figures and their captions, tables, list of references, appendixes) that needs to be clarified, reduced, added, combined, or eliminated?

Yes



19. Is the technical language precise and understandable by fellow scientists?

Yes

20. Is the English language of good quality, fluent, simple and easy to read and understand by a wide and diversified audience?

Acceptable

21. Is the amount and quality of supplementary material (if any) appropriate?

N/A

### *Referee decision*

The manuscript, in its current form, cannot be accepted for publication. A major revision (possibly including some modeling effort) is required before it can be reviewed again.