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Natural Hazards and Earth System Sciences
Discussions

# *Interactive comment on* "Forest fire danger rating in complex topography – results from a case study in the Bavarian Alps in autumn 2011" *by* C. Schunk et al.

**Anonymous Referee #2**

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This article tried to find appropriate fire danger index from the results of a case study. In total four fire danger indices including the Angstrom index, McArthur's Forest Fire Danger Index(FFDI) FFDC, and DMC are introduced and applied to complex topographical area, and discussed the effectiveness of fire danger indices. Also addressed the appropriate time-scale for optimized index. Overall arguments are reasonable escept for some argument. However I think the manuscript can be highly improved if the authors make additional address and add further analysis for quantitative discussion. I recommend this manuscript to be published in NHESS in major-revised form (not in present form). Please see the following comments for revising the article.



Major Comments: 1)Brief summary with equations of four fire danger rating would be required. I.e., current 4 fire danger indices should be discussed in terms of their definitions in the manuscript, and explanation of their meteorological quantities as functional variables would be needed. Then it could be addressed why some indices such as DMC respond slowly with time in comparison with others.

2) In connection with 1), among 4 fire danger indices, some index shows slow response time, and this might be due to the employed time scale of meterological variables designed for representing cumulative measurement (i.e., 3-4 previous days observation such as effective humidity instead of relative humidity by itself). For this case, probably one-hour high time resolution might not be meaningful. Authors should justify the importance of hourly interval time interval for this case in more careful and quantitative manner, possibly against expert observations and real fire occurrence data (described in the manuscript).

3) The authors also wanted to point out the importance of existing inversion layer for fire danger indices (in Abstract and Section 4). Reading through this manuscript, the rationale of temperature inversion existence vs. Fire danger indices is quite confusing. For example, as shown in Fig. 4, surface atmosphere is observed as saturation, but inversion looks not so relevant to the saturation. It might be understood that authors wanted to point out the occurrence of inversion can generate relatively stronger diurnal temperature variation, and thus wetting fuel is certainly affecting under the condition of saturation (or semi-saturation). As a result, it should be resulting to lower fire danger index values during the time of 1200 and 1400 eventually. However, I guess inversion should be treated from the standpoint of atmospheric stability, rather than from atmospheric saturation. Clarification of this issue would be essential.

Other comments

-Abstract "This drought was caused by a persistent high pressure system, inducing a pronounced temperature inversion with cool, humid conditions ..."

Is that true that drought is connected with humid condition? Seems like higher persistent high pressure is reasonable but if there were no quantitative evidence, at least reference would be required here. But again temperature inversion is quite common phenomena during the night time, not thought to be relevant to drought.

-p 8, '....the temperature-dew point spread increased until reaching a maximum of 20 41°C..." I can not find any elevation with the temperature more than 10C

-In Fig. 4, just one single profile from sounding data at just one single night time hour was presented. More detailed interpretation regarding inversion occurrences on this matter would be required.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 1383, 2013.