

## On the potential of using smartphone sensors for wildfire hazard estimation

By Hofit Shachaf and co-workers.

**Summary:** This manuscript reports about the results of a study where smartphone data collected from the Weather Signal App are used to assess the humidity and temperature state of the near surface atmosphere. These results are then related to the potential to follow and forecast fire weather. This is illustrated for cases in Israel, Portugal and Spain. The use of citizens science and crowdsourcing that uses smartphone data records is evolving. Earlier its focus was very much on temperature, but this manuscript brings the field a step further by developing a methodology to also use humidity observations. Therefore I think the work is suitable for publication after some revisions.

**Recommendation:** Revisions needed

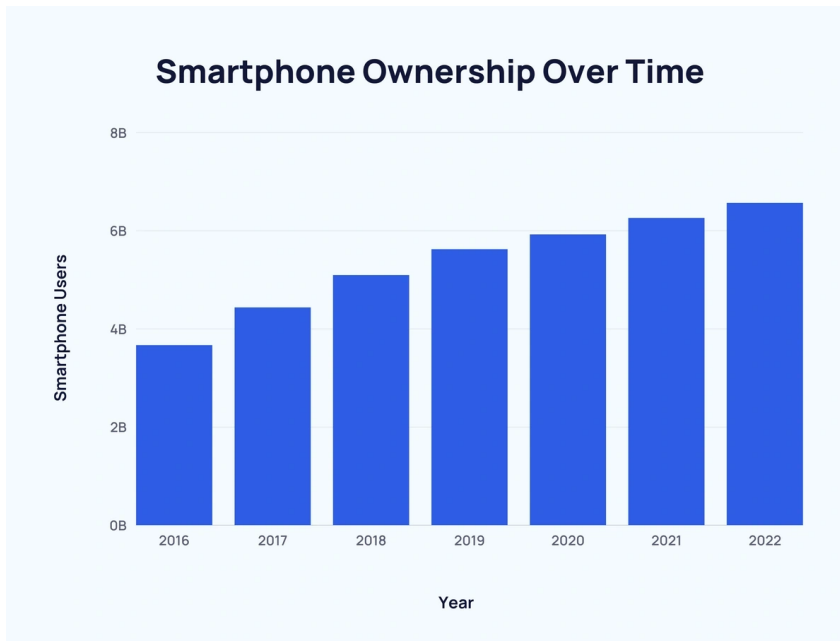
### Major remarks:

1. Ln 174: from here I get a little lost as reader. The manuscript is so far clear in the message that the goal of the paper is to use smartphone data to assess temperature and humidity to estimate wildfire risk. But from here it seems the reader is presented with lots of regressions between smartphone data between data from Spain, from Israel, from ERA5, from Portugal. Fitting local smartphone data records against ERA5 is risky since ERA5 is a rather coarse product (25 km), at least for this exercise. Hence landuse fractions are mixed, while both countries are at the coast. In addition, orography differences between the ERA5 grid cell and the local smartphone position can result in pressure and temperature differences. And Spain and Israel do have mountains. So why overall would one make so many correction formula's? If the goal is to show a correlation between smartphone data readings and professional products, then this can also be shown without fitting correction functions. Now it is very repetitive, while it is diluting the main message about the novelty of the method.
2. Discussion section: In my view the discussion section can be strengthened. Currently the authors are mainly bringing a very positive message about the potential for their method, but it is lacking critical notes that need to be ventilated, i.e.
  - a. It remains unclear why only data from the Samsung S4 has been used, and whether the method is also applicable to other smartphone models. The S4 model is very much outdated and hardly used anymore. The paper should discuss this deficiency. As far as I know smartphone manufacturers are on a track to reduce the number of sensors in their smartphone models, in order to fulfil customers needs to make the smartphones more robust against water.
  - b. The paper should be more critical on the aspect of using nearly live smartphone weather data to follow and nowcast fire risk. Personally I think is rather far fetched, and an ideal picture. As far as I know making weather data available for scientists involves quite some handwork for OpenSignal, so a live stream would not be possible in practise. Please discuss.
  - c. Related to point b, could the authors comment on the added value of their method compared to existing platforms like Weatherunderground.com, NetAtmo.com, and <https://wow.metoffice.gov.uk/> that distribute citizen science data from relatively dense networks, more or less live or semi-live. Are these platforms already offering all information needed for fire risk assessment?
  - d. The paper does not reflect so much that the dynamics of wild fires is much more complex than just indices that are presented. Pyrocumulus and pyrocumulonimbus clouds trigger very much their own meteorological conditions through feedbacks in cloud cover, solar radiation, up- and downdrafts, local winds. Moreover the wild fire risks also depend on the fuel amount and type on the surface and the sub-surface. Lots of recent knowledge has been developed about these dynamics. It would be good if the manuscript could reflect some of the complexity involved, and especially how these complexities would prohibit the use of the smartphone data in an meaningful way, or the other way around -i.e. whether the authors think there is also potential in the methods to follow individual Pyrocumulonimbus clouds.

Minor remarks on the next page

**Minor remarks:**

Ln 25: smartphone ownership growth is certainly not exponential anymore. See e.g. this graph (from <https://explodingtopics.com/blog/smartphone-stats>). Please reword.



Ln 52: Note temperature estimates were also made for contrasting urban form for Sao Paulo in Droste et al (2017; <https://journals.ametsoc.org/view/journals/atot/34/9/jtech-d-16-0150.1.xml>).

Ln 79: Please put the table caption above the table.

Ln 100: perhaps add a word or two to clarify that the IMS stations are the professional or reference stations.

Ln 123: The authors report here the sensitivity and accuracy of the sensors as provided by the manufacturer. In the recent study by Noyola et al. (<https://www.frontiersin.org/articles/10.3389/fenvs.2021.673937/full>), the authors have used the Samsung S4 in the field too, and have performed experiments to determine the response time of both the temperature and RH sensors. Likely there is useful information in that paper for the current study as well. In a practical setup the RH error appeared to be higher than reported in the current manuscript. The cold bias in Fig 2a is about similar to the one reported in Noyola et al.

Ln 164: The calibration was done also using a simple linear regression model. Could authors explain whether a linear regression on a conserved variable like specific humidity would perhaps have worked better? The relation between RH and temperature is highly non-linear (Clausius-Clapeyron), so perhaps a linear correction of RH might result in a biased outcome. Please comment.

Ln 170, Fig 3: please label the panels a and b and adjust the figure caption. In the panel on the right the y axis can start at RH =30% to show a better contrast between the two lines.

Fig 4: for the linear regression formula that is shown, please add in the caption that the temperature input should be in degrees C, not Kelvin.

Fig 4b,d: in panel b the  $R^2$  is in 2 decimals, while in panel d it is 1 decimal. Please make consistent, to respect the significance of the results.

Ln 217: note that the ECWMF model also has obvious biases in wintertime, often reporting warm biases too, so be careful with using ECMWF or ERA5 as "the truth" for these winter conditions and stable boundary layers.

Ln 240: the same RH at different temperatures results in very different VPD values. Please reword, since RH is a derived variable from conserved variables like water vapor pressure (at the surface) or specific humidity and temperature. Hence the temperature and the vapor pressure themselves govern what is the RH, not the other way around.

Ln 265: IMS has already been introduced before.

Ln 273: November 2013, 2015 and 2015. 2015 twice?

Ln 353: ...We are now attempting to monitor wind speed using smartphone horizontal pressure gradient data between different locations,... Please remove, this is somewhat speculative and I find this beyond the scope of the current paper. In addition wild fires trigger their own wind field/circulations so the wind field will not always follow the large scale field triggered by synoptic pressure gradients.

Acknowledgements: I miss a data availability statement.