May 2nd 2021

ACP

Dear Sir, Madam

Review of: Lightning-ignited wildfires and long-continuing-current lightning in the Mediterranean Basin: Preferential meteorological conditions by Francisco J. Pérez-Invernón et al. (ACP-2021-125)

I have now completed the review of the paper by Pérez-Invernón et al. submitted for publication in ACP. This is a good, timely and comprehensive paper, that addresses an interdisciplinary topic with observational and analytical tools. The authors combine various data types from different platforms and conduct a thorough analysis aiming to distinguish and identify the types of meteorological conditions prevalent in Greece and the Iberian Peninsula which are conducive to producing lightning-ignited forest fires. The topic is highly relevant to the readership of ACP.

The graphs and tables are adequate, and the paper is well-organized, the language is fluent and clear (some typos here and there) and the overall quality of presentation is very good.

I have several that relate to the analysis and the conclusions, which I present for the authors to respond. They may require a minor revision of the manuscript before being accepted for publication.

Major Comments

1. Section 3.4.1 describes the methodology of obtaining LCC(>20 ms) from LIS data. However, it is entirely possible that these strokes are IC flashes and unrelated to LIW. Have the authors compared those events with ENTLN or WWLLN lightning data for specific storms that ignited wildfires? Were they really CG strokes? It would be more convincing if indeed Long Continuing Current strokes are also detected by ground systems and a correlation between brightness duration and actual peak-current or energy is obtained. [With this in mind, could it be that LCC(>20 ms) strokes are superbolts? (Holzworeth et a., 2019)]. Are the authors able to define the multiplicity of flashes with LCC(>20 ms)?
2. In trying to reconcile the dynamical and microphysical structure of thunderstorms that ignite fires compared with those that do not, there seems to be a contradiction (or at least, inconsistency) between the depth of storms as defined by their average CTH reported by satellites (Figure 12) and the fact that on average they exhibit slower updrafts (Figures 10) or faster (Figure 11). This fact also seems at odds with the statement (line 338) that the instability is higher for clouds that produce fire-igniting strikes compared with those that do not. This is also mentioned in section 3.2.1 with regards to CAPE values (line 360) where fire igniting lightning in the Iberian Peninsula have lower CAPE values compared with the climatological media.

It is a well-known fact that supersaturation closely depends on the vertical velocity (see Rogers and Yau, 3rd edition 1989, chap. 6) and so one would expect that slower updrafts will result in less activated CCN, less droplets and fewer ice crystals, all leading to a reduced efficiency in charging. Can the authors elucidate this mismatch between dynamics and microphysics?

3. Lines 455-468: The geographical distribution of LCC (>20 ms) with Cloud Base Height (CBH > 2km) as presented in Figure 15 shows that they are produced mainly over land, and not as stated in the text over the ocean and in coastal areas, even when the total lightning is over land (line 465). This is in contrast with the cited Holzworth et al (JGR, 2019) paper and with Fullekrug et al. (Ann. Geophys., 20, 133–137, 2002) that showed intense lightning (or super-bolts) to be occurring over oceans and near coasts. At least this is what this reviewer sees in the upper panel of Figure 15. Am I missing something here? If the most intense lightning indeed occurs in coastal areas and above sea water, how can they be the ones that ignite forest fires? This seemingly contradictory results is actually discussed in lines 469-474. Further explanation is needed.
4. The distinction between storms that produce lightning with LCC (>20 ms) and those that produce only LCC (>10 ms) and "normal" ones is not entirely clear to me. Let us suppose that there was just 1 flash with a long continuing current – does this qualify the storm to be included in the statistics? Or is there a threshold of some number of such flashes? After all, lightning discharge processes are (almost) entirely random and it can well be that a storm has all the "ingredients" needed to produce LLC (>20 ms) and still does not. This randomness is partially manifested in the seasonal ratio as described by Figure 16, which is higher in winter. Nevertheless, winter thunderstorms produce fewer flashes and are generally less deep and so (in line with comments #1) may not be ideal for generating such flashes.
5. The weakest part of the paper is the concept of the "transition phase" discussed in section 3.4.3 (and also in lines 602-604). The definition is somewhat unclear, and is unrelated to the typical microphysics and dynamical evolution of thunderstorms. If there is a clear change from a low-flash rate to a high-flash rate regime (or vice-versa) prior to the occurrence of LCC(>20 ms) strokes then we should see specific quantitative values describing these phases of the storm. For example, Emersic et al. (MWR, 2011) defined 3 distinct periods of lightning activity, and related them to the charge structure (see their Figures 4 and 5). Lang et al. (BAMS, 2004) showed how the flash-rate evolves as a function of time while differentiating between IC and CG strokes along the storm's life cycle. It is unclear how LCC(>20 ms) strokes are distributed as a function of time and if (and how) they are related to cloud microphysics. Either give more information or delete this section.
6. In lines 469-479 the authors discuss the comparison between LIW maps and LCC(>20 ms) maps. There are several places where we see fires, but no strong lightning. What can be the interpretation of these fire events? Is there a possibility that those fires had been ignited by "regular" strokes, or those with shorter CC? It seems that the selection of 20ms threshold is arbitrary, and

actually there may be episodes that even shorter strokes can ignite fires (for example if the forest was dry or already deteriorated).

7. The discussion about forecasting the potential for LIW (lines 565-575) may benefit from including the concept of the Lightning Potential Index (LPI; Yair et al., JGR 2010). This parameter was later developed into the Dynamic Lightning Index by Lynn et al. (WAF, 2012). Perhaps simulating LIW events and "calibrating" the LPI values against the occurrence of LCC(>20 ms) will improve forecast capabilities in operational models.