The manuscript titled "Fire weather index data under historical and SSP projections in CMIP6 from 1850 to 2100" presents a global dataset of FWI changes over the long term, using all CMIP6 simulations. The data could be used to evaluate the impacts of climate change on fire danger. Although I appreciate the significant effort the authors have made in processing the CMIP6 data, calculating the FWI, and estimating the model agreement in different regions, I have some major reservations about the study in its current form, which are detailed below.

Major Comments:

1. I would argue that it is a convention to use the RH_{min} (rather than RH_{mean}) to calculate the fire weather index, as per previous studies (Van Wagner, 1987; Vitolo et al., 2019; Abatzoglou et al., 2019). Replacing RHmin with RHmean resulted in large decreases in DMC, FFMC, and FWI by 30-35% (Fig. 6). Especially, using RH_{mean} influence the monthly variations of DMC and FWI, which is expected as there are much larger differences in RH_{min} than RH_{mean} between the wet and dry seasons. The author chose to use RH_{mean} as "because daily minimum relative humidity is not provided for many CMIP6 runs, reducing the total number of runs from 1486 to 1321 (Line 303-304)". I suggest that 1321 CMIP6 runs with RH_{min} would be adequate for conducting FWI predictions.

2. The manuscript needs to highlight the novel aspects of the FWI dataset, especially compared to the one produced by Abatzoglou et al. (2019), which generally describes FWI in the same period (1860-2099). What new information could be obtained using the CMIP6 ensembles?

3. The manuscript requires further explanation of the methods, validation, interpretation of the results, and discussions of the data limitations. Specifically, the authors need to clarify the following:
    1) Why do we need to use the "day length" and "drying factor" adjustments? How do these adjustments play a role in different seasons? The authors need to provide equations to describe the adjustment explicitly.
    2) How is this FWI product compared with other FWI products (Vitolo et al., 2019; Abatzoglou et al., 2019)? A comprehensive evaluation is needed in the historical period to conduct future analyses.
    3) As we know, increases in FWI could not be translated to the burned area change directly. Then how could we interpret the increase in FWI? Please extend more discussions in section 3.5 than just providing the numbers. Also, why do we see less agreement in FWI changes in boreal Asia, boreal America, African tropical forests, and India?
    4) Clarify the data limitations for readers who will use this data for analyses.

Comments on figures:

Figure 3:

1) I suggest that the authors show the changes in the fire season of the Southern Land which has a greater difference, for example, January 1st instead of July 1st. They could exchange Figure A2 and Figure 3.

2) Does the shaded area in Figs. (g-l) show ±1 standard deviation for historical only or both historical and SSP585. The shaded areas get so overlapped with each other and confuse me what can be learned from the figure. And there is no interpretation of this standard deviation range.

Figure 4: Again. I suggest exchanging Figure 4 and Figure A3 because the regions (Southern Land) showing large differences are in the wet season on July 1st. Therefore, we need to focus on fire season changes.

Figures 7 & 8:

1) Can you explain why you use 1851-1900 as a reference year? Is it better to use more recent years (e.g., 2000-2020) when the observations of most fire regimes are available?
2) The authors need to clarify how they calculate the number of days with extreme fire weather, the length of fire season, and the seasonal average of the FWI ***at different GWLs***. For example, are you using the number of days above 95-th percentile of the FWI ***over 1851-1900*** to calculate *fwixd* at different GWLs?

Minor Comments:

1. Line 130: what are the benefits of using this day length adjustment?
2. Line 133: it would be helpful to explain how the day length parameter varies across different seasons and whether it only affects fire season.
3. Line 137: I am curious about the reason for considering potential ET. Can you provide an additional explanation?
4. Line 205: Please refrain from using "correct" here –it is unclear whether adding the adjustments would improve the FWI prediction or not
5. Line 205-210: In Fig. 3, this is a clear seasonable pattern in DMC in the Northern land and Tropical land, but not in the Southern land. Can you explain why?
6. Line 214: Why is there a decrease in the range of FWI/DMC?
7. Line 248-249: Could you rephrase the sentence where you mention "one month before the observed extreme in the differences in DC"?
8. Line 253: "However, the FWI presents higher sensitivities to changes in FFMC than to DMC, and even more to DC": This sentence is confusing: not clear if the sensitivity of FWI to DC is higher or lower than FFMC. I think Dowdy et al. show FFMC > DMC > DC. Please rephrase here.
9. Line 255: Please avoid using the word "correcting"
10. Line 343-344: "It concerns the length of the fire season, the annual maxima, and the seasonal average of the FWI, but not the number of days with extreme fire weather that continue to show an increasing trend in these regions". I still see an increase in the number of days with extreme fire weather (second row of Figure 7). Is my understanding incorrect?

Reference:

Abatzoglou, J. T., Williams, A. P., and Barbero, R.: Global Emergence of Anthropogenic Climate Change in Fire Weather Indices, Geophys Res Lett, 46, 326-336, https://doi.org/10.1029/2018GL080959, 2019.

Van Wagner, C.: Development and structure of the Canadian forest fire weather index system, 1987.
Vitolo, C., Di Giuseppe, F., Krzeminski, B., and San-Miguel-Ayanz, J.: A 1980–2018 global fire danger re-analysis dataset for the Canadian Fire Weather Indices, Scientific Data, 6, 190032, 10.1038/sdata.2019.32, 2019.