The manuscript presents a regional assessment of vegetation fire dynamics for West Asia from 2001 to 2022. Utilizing MODIS active fire (AF) and burned area (BA) products, the authors investigate spatial and temporal fire patterns and employ Spearman rank correlation to explore relationships with climate (SPEI), topography, and population density. While the study addresses an important topic in a highly vulnerable and understudied region, with volatile human activity, there are several major methodological and statistical issues that limit the reliability of the current findings.

General Notes:

1. Use of Spearman rank correlation for evaluations is inadequate since it ignores interactions (fire requires ignition, fuel, and specific weather concurrently. A bivariate correlation looks at these in a vacuum. It cannot capture that population density might only increase fire risk under specific dry-season SPEI conditions. Also, as the authors admit in their discussion, the relationship between population density and fire is often non-monotonic (e.g., it peaks at intermediate densities and drops in highly urbanized areas). Spearman rank only tests for monotonic (strictly increasing or decreasing) relationships, making it the wrong tool for the job. And the data points (10 km grid cells) are not independent in satellite data (unlike some models). They are contiguous, and since the authors already proved the data is highly clustered (Moran's I, although their results may be erroneous due to the error in the printed equation in the manuscript (see below comment), running a Spearman correlation without accounting for spatial autocorrelation violates basic statistical assumptions and artificially inflates significance (the p-values).

   On the selection of correlation variables, while the inclusion of climatic, topographic, and anthropogenic variables is conceptually sound, the specific metrics chosen for the Spearman rank correlation limit the validity of the results. The inclusion of nine different permutations of SPEI (wet/dry x mean/median/max/min, plus SPEI-12) introduces a severe multiple comparisons problem. Testing this many permutations on an *N* of 63,730 almost guarantees statistically significant *p*-values by random chance. The authors should justify this variable selection or apply a statistical correction (e.g., Bonferroni).

2. The study relies on a static 2020 WorldPop population density map to represent human influence over a 22-year period (2001-2022). Given the extreme demographic shifts, conflict-driven migrations, and urban expansion in regions like Syria and Iraq during this exact timeframe, using a single static snapshot from 2020 introduces severe chronological misalignment and undermines the assessment of anthropogenic drivers, especially considering the increasing human activity in the region within the last decade (and with that fire activity, please see the following comment).
3. The authors mainly rely on MCD64A1 dataset which is by now a scientific standard in the wildfire community for burned area analysis. They compensate for resolution using the MCD14ML (AF) dataset. The burned area (BA) product has a spatial resolution of 500m. The AF product has a spatial resolution of 1 km, but it can detect thermal anomalies, and therefore smaller fires, under optimal conditions and if they are high intensity (and may not leave a big enough burn scar to be registered by the 500m BA product). The authors seem to be confusing spatial resolution with sub-pixel thermal sensitivity here. I recommend an explicit discussion of how the 500m detection threshold might be skewing the ratio of, specifically cropland fires (stubble burning) versus forest fires, and whether this alters their core conclusions about the regional fire regime.

   Also, the manuscript highlights significant regional anomalies (e.g., the conflict affecting Syria and Iraq in 2019) and attributes these to a complex mix of climate and conflict. However, the discussion would benefit greatly from benchmarking these regional trends against recent comprehensive global synthesis reports, specifically the State of Wildfires annual reports (e.g., 2023-2024 and 2024-2025). These reports utilize the same underlying satellite networks (MODIS, VIIRS, CAMS) but provide important attribution analyses on how climate change is shifting the odds of extreme fire weather and amplifying burned areas. Integrating these findings into the discussion would strengthen the manuscript by grounding its localized regional anomalies within the broader, shifting global fire regime.
4. The authors aggregate all fire data into 10km x 10km grid cells for their emerging hotspot analyses and Mann-Kendall trend detection. While this aggregation is a standard and appropriate methodology for regional trend analysis (such as Mann-

Kendall and EHA), doing so contradicts the authors' earlier justification that they utilized the AF product for its high spatial resolution to capture small fires. I highly recommend the authors revise this justification to accurately reflect AF's actual contribution (sub-pixel thermal sensitivity), and clearly address how aggregating to a 10km grid limits their ability to make claims about small, fragmented agricultural fires.

5. The authors' use of the annual sum of NDVI to represent fuel accumulation. There are several structural and ecological issues with this metric, and it is misaligned with the region's fire dynamics (and also complex topography/ecoregions). In dryland and agricultural systems fire risk is driven mainly by fuel curing, desiccation, and post-harvest stubble, not total annual greenness. Phenological metrics (e.g., seasonal NDVI decline, or peak-to-trough differences) are required to capture actual fire susceptibility.

   The study area encompasses a massive gradient of ecoregions. NDVI is known to saturate over dense canopies. Given regional heterogeneity, utilizing the Enhanced Vegetation Index (EVI) would provide a more robust baseline by mitigating canopy saturation and atmospheric noise (e.g., dust).

   The authors attempt to link their vegetation metric to "fuel accumulation". Modern fire attribution studies (such as those led by World Weather Attribution) increasingly rely on biophysical variables like Leaf Area Index (LAI) to measure physical fuel load and evapotranspirative demand far more accurately than a purely spectral "greenness" index like NDVI. The authors should justify their reliance on annual NDVI or consider updating their fuel proxy to align with current state of the art fire modelling practices.
6. Given the manuscript's core conclusion that 74% of BA occurs on croplands and is heavily influenced by human activity and conflict, the correlation model suffers from omitted variable bias. Relying solely on static population density to capture 'human influence' while omitting agricultural drivers (e.g., crop type, harvest timing, proximity to infrastructure) or socio-political data disconnects the statistical analysis from the paper's own narrative conclusions.
7. The authors justify using the MODIS land cover product – despite acknowledging it misclassifies Turkish forests by 24% – by claiming it is the only continuous dataset available. The authors should consider cross-validating their land cover claims with

alternative continuous datasets (such as the 300m ESA CCI Global Land Cover product) to ensure that the 74% cropland claim is an ecological reality rather than an artifact of MODIS misclassification. Such comparison will also highlight any potential misclassification on the Turkish forests (which would constitute an important part of the forest density in the study area) for 2005, 2010, and 2015.

Specific Notes:

In Section 2.2.3, the authors state they used a 30m ASTER DEM to extract elevation, slope, and aspect to account for microclimate, fuel moisture, and fire spread. However, they subsequently aggregate this data to a 10km x 10km spatial resolution. Assessing microclimate or fire spread dynamics is physically impossible at a 10km resolution. Furthermore, aggregating topographic variables – especially 'aspect' – across a 100 km^2 grid cell in mountainous terrain (such as the Zagros or Taurus mountains) mathematically obscures the actual terrain (e.g., averaging North and South slopes yields a mathematically meaningless East-facing average). The authors should remove claims regarding 'microclimate' and 'fire spread' and acknowledge that their 10km topographic variables represent macro-geographical landscape position rather than localized physical drivers. Additionally, claims about climate-fire interactions are overstated given that wind, temperature, and relative humidity were omitted in favour of a single drought index (SPEI).

Equation 1 (Global Moran's I): The equation as is written in the manuscript is erroneous. The denominator of the second term is written as the sum of indices. It should be the sum of squared deviations. If the authors actually used the formula printed in the manuscript, this would yield erroneous results, and it must be corrected.

Figures 2,3: I highly recommend the authors change the colors in these plots.

On toponyms: While I appreciate the authors' recognition of the geopolitical (in fact colonial) reference of the term Middle East, for a paper submitted to an EGU journal, I highly recommend them to change the title's geographical reference to West Asia: e.g., *Fire across frontiers: Satellite-based investigation of climate-fire interactions in West Asia*. This will also save them the footnote explanation. As some social science scholars will ask, "the middle of whose East". This will also require the authors to change the text from ME to WA, but I strongly believe it will be worth it. The study area's perimeters are already specified in 2.1.

Also, minor but important in the regional context: Turkey is now being written in its Turkish version "Türkiye", and scholars from the region are required to spell out the Turkish name in their articles. While the authors of this paper are not bound by this regulation, they may consider applying it.