Response to Reviewer 1

Manuscript: Fire across frontiers: Satellite-based investigation of climate-fire interactions in West Asia

We appreciate reviewer's time and effort on this manuscript and the constructive feedback on our work. Below, we respond to the following comments, and show that we will address these comments to strengthen this work.

1. Inadequate approach for correlation between fire and other factors

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.

Author's response: We thank the reviewer for raising this statistical concern. We agree that fire is a multi-factor process and that Spearman correlation does not capture non-monotonic relationship. However, we would like to respectfully defend the use of Spearman correlation as a screening measure to understand correlation between each factors and fire on a regional level. Spearman's correlation is widely used in many fire researches, while the limitation of this method has been acknowledged at the discussion. In the revision, we will more explicitly frame the analysis as a first-stage overview and the correlation and discuss the limitation of bivariate correlation in this paper. We hope this framing can address the concern for a paper that is primarily descriptive rather than causal.

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).

Author's response: First, we thank the reviewer mentioning the errors of the global Moran's I formula. We will correct it and redo the calculation based on the correct formula shown below:

$$\mathrm{I} = \frac{n}{S_0} \frac{\sum_{i=1}^{n} \sum_{j=1}^{n} w_{i,j} z_i z_j}{\sum_{i=1}^{n} {z_i}^2} \quad (1)$$

Where z_i is the deviation of fire count for grid cell i from the mean fire count across all cells. $w_{i,j}$ is the spatial weight between grid cell i and j. n represents the total number of grid cells and S_0 denotes the sum all spatial weights.

Second, we agree that the 10km grid cells are not independent observations, and the p-value we show might be inflated due to the large samples and nine SPEI variables. Therefore, we would like to revise the manuscript based on the following changes:

1. Apply a False discovery rate (FDR) correction using Benjamini-Hochberg procedure, so we ensure the results are robust without being so restrictive and lose important regional nuances.

2. Run the correlation per country: We will extend the Spearman correlation analysis to each region in this study (Tukey, Syria, Iraq, Iran, Israel, Lebanon, West Bank, Gaza). It can reduce the sample size and capture the spatial heterogeneity that we didn't capture from our initial analysis (one value represents the entire region).

2. WorldPop data justification

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).

Author's response: We agree that this is a very valid data concern, especially when we include the period of time when Syria and Iraq are experiencing the extreme demographic shifts and conflict-driven migrations. Using 2020 population snapshot can not reflect the population over this time period. In this revision, we will replace the static 2020 dataset with the latest annual WoroldPop dataset (https://hub.worldpop.org/geodata/listing?id=76) to try and capture the correlation between fire and population dynamics. Due to the data temporal coverage limitation (2000-2020), we plan to use 2020 population density data for 2021 and 2022 in our study.

3. AF/BA data detection mechanisms

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.

Author's response: We thank the reviewer for the comment on the explanation of the detection mechanism between MODIS active fire and burned area products. In this revision, we will explicitly clarify their fire detection approaches at section 2.2.1 (Under 2. Data and Method, and 2.2 Data). Regarding the potential detection limitation between cropland and forests fires: we agree that this is a limitation for our core findings that 74% of BA occurred on croplands. Many stubble burning fires are missed because they are often small, brief with moderate intensity, which make them even harder to be captured by 500m resolution burned area product. In this revision, we will test a small parcel of agricultural lands using MODIS BA, AF, and Sentinel-2 imagery to evaluate the accuracy of the fire products and add this result into our supplementary information. We will add this test result into section 4.7 Data and method limitation to explain this potential bias.

4. AF/BA data detection mechanisms

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.

Author's response: We appreciate the reviewer point this out and we agree the current justification is misleading. There are two contributions of AF in this paper: one is that we used the count of the AF data to visualize the normalized AF count by land cover and region (Figure 2), and the second one is that we aggregate the AF values into 10km grid and applied emerging hot spot analysis. The first part can tell reflect that AF's ability to detect small fires compared with BA; and the second part demonstrate AF spatial-temporal pattern in a regional level. However, we indeed did not clarify the two contributions and limitation of AF in this study.

In the revision, we will rewrite the AF justification in section 2.2.1 to clarify the difference between detection sensitivity and spatial resolution, and will add a statement in section 2.3.2 (Spatio-temporal hotspot analysis) acknowledging that aggregation to 10km allows us to see the regional pattern but limits the small and fragmented fires.

5. Concerns of the use of annual NDVI as a fuel proxy

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.

Author's response: We thank the reviewer for this detailed and well-grounded comment. We acknowledge that annual NDVI sums reflect overall greenness instead of the flammable fuel conditions. This poses a limitation of the current analysis. We appreciate the reviewer's suggestion to refine our approach and agree that EVI is a more robust vegetation index for this study area, as it is less sensitive to saturation in dense vegetation and uses the blue band to correct for atmospheric scattering caused by dust and aerosols. In addition, LAI can better capture the biophysical properties of vegetation relevant to fuel conditions. Therefore, in the revision, we will replace annual sum NDVI with seasonal MODIS EVI and LAI. We believe these updates will improve the ecological realism and robustness of our analysis.

6. Omitted variable bias: agricultural and socio-political drivers

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.

Author's response: We fully agree with this critique and acknowledge the limitation of the method and data in this paper. For population, we will update the data based on point 2. Apart from the population data, we will do the following revisions:

First, we will substantially strengthen the section 4.7 (Data and method limitations) to explicitly acknowledge the omitted variable problem and enumerate the agricultural drivers (crop type, harvest timing, fire management policy, conflict proxies) that would be needed for a more complete anthropogenic attribution. Second, we will revise the language in the discussion to avoid overstating conclusions about human influence that go beyond what our correlation model can support. Third, and most importantly, we will reframe the paper's contribution accordingly: rather than claiming that our analysis explains the human dimension of fire, we will position the results as empirical evidence that strongly motivates a focus on human and socio-political drivers in future work. The fire regime in the Middle East is complex and this paper only partially captures the environmental variables that affect the fire. We will clarify the narrative to make the aim, method and result more coherent.

7. Land cover data uncertainty

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.

Author's response: This is an important concern on the land cover data we use, which shows different results between MODIS Land cover (6% forests) and Turkish national forest data (30% forests). We understood that this data bias might result in uncertainty to out 74% burned area on cropland claim. Therefore, in the revision, we will compare the land cover classifications derived from the ESA CCI 300 m Land Cover product for selected years (2005, 2010, 2015, and 2020) to assess whether the dominant land cover associated with fire differs between the two products. We will specifically examine Turkey's forest cover extent and its overlap with the burned area dataset. If the ESA product provides a substantially more accurate classification and leads to different fire attribution proportions, we will update our analysis accordingly; if the two products yield broadly similar results for the fire-relevant classes, MODIS will remain the primary dataset but the ESA cross-validation result will be reported in supplementary material. By doing the land cover data comparison, we can then provide a better justification for the 74% burned area claim.

8. Topographic variables: 'microclimate' and 'fire spread' claims at 10km resolution

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 km² 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).

Author's response: We thank the reviewer for providing this comment. Our claims about microclimate and fire spread are not defensible at 10km resolution, and resampling the aspect, elevation, and slope to 10km also make the use of microclimate and its relationship with fire spread problematic as we did not consider the heterogeneous landscape at the study area. In this revision, we will remove the statement of microclimate at section 2.3.3 (topography data) and section 4.6 (Correlation patterns in fire dynamics), 4.7 (Data and method limitations) and replace them with language that accurately reflects what our aggregated topographic variables can capture: broad landscape condition, macro-scale terrain, and the general distribution of agricultural versus non-agricultural terrain. We will also add a note in the limitations section regarding the loss of information when aggregating slope and aspect across diverse mountain terrain such as the Zagros and Taurus ranges.

Figures 2 and 3: colour scheme

I highly recommend the authors change the colors in these plots

Author's response: We thank the reviewer for raising this point. We would, however, appreciate further clarification on this comment, as the current colour palette in Figures 2 and 3 was deliberately selected to meet colour vision accessibility standards, specifically to be distinguishable for readers with the most common forms of colour blindness (deuteranopia and protanopia). To balance the color inclusiveness and to visualize eight types of land cover classes, we chose these colors after testing different kind of combinations. We are not certain what aspect of the colour scheme the reviewer finds problematic or what alternative direction they have in mind. If the concern is about general visual clarity, contrast between specific land cover classes, or a different accessibility standard, we would be very grateful if the reviewer could indicate more specifically what change is being requested.

Toponyms: 'West Asia' and 'Türkiye'

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.

Author’s response:

On 'Middle East’ versus ‘West Asia’: We are very much aware of the colonial origins of the terms ‘Middle East’ and share the reviewer’s appreciation of using ‘West Asia’. This is why our current title use both terms together ‘Middle East (West Asia)’ and why we added the footnote to explain the details. We would, however, respectfully propose retaining this dual term rather than switching fully to ‘West Asia’ alone. Our main reason is that ‘Middle East’ remains its dominant terms in academic databases, policy discourse, and international organizations. If we only use ‘West Asia’ for the title and the entire article, there might be a risk to reduce its visibility for policymakers, fire managers, and researchers who are the key audiences. Therefore, we chose to use both terms to balance the awareness of the colonial contexts and science communication.

On 'Turkey' versus 'Türkiye': We appreciate the reviewer's note on this point and acknowledge that the name change was officially accepted by the United Nations in 2022. However, we would respectfully propose retaining 'Turkey' in the manuscript. we recognise the importance of the name change in its context, but 'Turkey' remains the established English exonym in the academic literature, including in an international leading journal Turkish Studies, which continues to use 'Turkey' in its articles. Both decisions on retaining 'Middle East' alongside 'West Asia', and the use of 'Turkey' reflect a between evolving terminology and established academic usage that serves our readership. Rather than an evasion of this issue, we hope the reviewer will agree with these considerations. We are of course open to guidance from the journal editors on this point, and will follow their style preference if it differs from our own.

We are confident that these revisions will address the reviewer's concerns and substantially strengthen the manuscript. We look forward to the opportunity to revise and resubmit.

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