Review of EGUsphere 2024-820 "Western disturbances and climate variability: a review of recent developments" by Kieran M. R. Hunt et al.

Synopsis:

This review paper by Hunt et al. aims at providing an overview of recent research on the topic of western disturbances (WDs) over the Indian subcontinent. The review addresses many aspects such as the structure and dynamics of WDs, natural hazards associated with them, their predictability and their response to climate change. Overall, the paper is well written and it covers many aspects. However, the paper appears to be overly detailed in places and it is difficult for the reader to identify the essential points. Accordingly, I suggest some points for revision before the paper can be published.
We would like to thank the reviewer for their positive assessment of our manuscript, and for their detailed comments, which we respond to point-by-point in red below. Planned revisions to our manuscript will be highlighted in blue.

Comments:

1) My major comment is that several subsections have a 'list' structure summarizing one result after another (e.g., 4.3, 4.2, 3.4.1, 7.1.3).
We will completely rewrite Sec 3.4.1 (which will be 3.4.2 in the revision) to provide a shorter and clearer narrative and thus to better synthesise the lists. We will also do this for 4.2, 4.3, and 7.1.3 as requested, as well as several other sections requested by other reviewers.

Thus, the storyline of the review does not necessarily become clear and it may be helpful to decide on a consistent conceptual framework for each subsection early in the paper. For example, a brief statement at the beginning of each subsection could describe our current understanding of a certain aspect of WDs and indicate the confidence that the research community has. The statement could then be followed by a more detailed summary. Still, this summary should not simply list all studies but for example comment on the confidence that we have or explain why several aspects are still uncertain.
We thank the reviewer for this comment. We will go through the paper and add short summary statements with IPCC-style measures of confidence, using their calibrated uncertainty language, throughout.

We will explain this in the introduction: “Each section starts with a summary of older literature, to orientate the reader and to provide context for the newer research. Each section then concludes with a short summary statement, including a measure of confidence in the main points, following the IPCC calibrated uncertainty language. These statements are summarised in Sec. 8.1.”

Then, in Sec. 2.2: "Thus, there is **high confidence** (*robust evidence, medium agreement*) that tracking algorithms have improved our understanding of WDs, but low agreement on how such algorithms should be implemented."

In Sec. 2.3: "There is **medium confidence** (*medium evidence, medium agreement*) that WD cyclogenesis occurs primarily in regions of dynamic instability, i.e. over oceans and downstream from mountain ranges. There is **very high confidence** (*robust evidence, high agreement*) that WD intensification arises from baroclinic instability which can be further amplified through coupling with moist or orographic processes. There is **very high confidence** (*robust evidence, high agreement*) that WDs primarily impact the Western Himalaya and surrounding regions – incuding the Karakoram, Hindu Kush, foothills, and to some extent the plains of Pakistan and north India. There is **very high confidence** (*robust evidence, high agreement*) in the dynamical structure of WDs: a mid- to upper-tropospheric vorticity maximum with deep ascent ahead of the vortex centre."

In Sec. 2.4: "There is **high confidence** (*robust evidence, medium agreement*) that WDs primarily draw moisture from the Arabian Sea, with some also coming from the Mediterranean and Eurasian lakes."

In Sec. 2.5: "There is **high confidence** (*medium evidence, high agreement*) that WDs occur most frequently in the winter months (December to March) but can occur at any time of year, including during the summer monsoon."

In Sec. 2.6: "There is **high confidence** (*medium evidence, high agreement*) that WDs exhibit high variability across a range of their characteristics, including intensity, precipitation, latitude, propagation speed, and lifetime."

In Sec. 3.2: "There is **low confidence** (*medium evidence, low agreement*) in the fraction of winter precipitation that WDs provide to the Western Himlayas and surrounding regions, but there is **high confidence** (medium evidence, high agreement) that it is at least 50%."

In Sec 3.3: "There is thus **very high confidence** (*robust evidence, high agreement*) that WDs play a vital role in recharging glaciers and snowpack during the winter months, and are thus crucial for water security in the Indus and Ganges river basins."

In Sec. 3.4: "There is **medium confidence** (*limited evidence, high agreement*) that WD rainfall is important for rabi crop growing and **high confidence** (*limited evidence, high agreement*) that hailstorms and heavy snow brought by WDs can damage crops. There is very **high confidence** (*robust evidence, high agreement*) that WDs provide conditions conducive to widespread fog. There is **medium confidence** (*limited evidence, high agreement*) that rainfall and near-surface winds brought by WDs temporarily reduce pollution levels. There is **high confidence** (*medium evidence, high agreement*) that some WDs lead to coldwaves. There is also **high confidence** (*medium evidence, high agreement*) that WDs are the primary source of pre-monsoon lightning across north India. There is **medium confidence** (*limited evidence, high agreement*) that landslides in the Western Himalaya are frequently triggered by WDs, but only **very low confidence** (*limited evidence, low agreement*) that they lead to avalanches."

In Sec 3.5: "There is **very high confidence** (*robust evidence, high agreement*) that the interaction between WDs and the summer monsoon often leads to very heavy rainfall."

In Sec. 4: "There is **high confidence** (*robust evidence, medium agreement*) that WD frequency and intensity increase during positive phases of the NAO, but **low confidence** (*limited evidence, medium agreement*) that WD frequency increases during EL Ni\~no. There is very high confidence (*robust evidence, high agreement*), however, that winter precipitation is greater over the Western Himalaya during El Ni\~no. There is **very low confidence** (*limited evidence, low agreement*) that a positive IOD increases WD frequency."

In Sec. 5: "There is **high confidence** (*robust evidence, medium agreement*) that simulations of WDs are mostly insensitive to the choice of parameterisation scheme, but also **high confidence** (*medium evidence, high agreement*) that the choice of land surface scheme is important. There is very **high confidence** (*robust evidence, high agreement*) that WD simulations improve with increased model resolution."

In Sec. 6: "There is thus, at present, **very low confidence** (*limited evidence, low agreement*) that WD tracks can be skilfully forecast in operational models at any lead time."

In Sec 7.1: "There is **medium confidence** (*limited evidence, high agreement*) that WD frequency was higher than present during most of the Late Pleistocene (60--12 ka) and much lower than present during the Early Holocene (12--8 ka). There is **very high confidence** (*robust evidence, high agreement*) that WD frequency was lower than present during the Mid Holocene (8--4 ka) and **medium confidence** (*robust evidence, low agreement*) that it was much lower during the 4.2-ka event. There is **high confidence** (*medium evidence, high agreement*) that WD frequency was lower during the Roman Warm Period (2.5--1.9 ka) and Medieval Warm Period (1.5--0.7 ka), and **very high confidence** (*robust evidence, high agreement*) that WD frequency was higher during the Little Ice Age (0.7--0.2 ka)."

In Sec 7.2.1: "There is **medium confidence** (*robust evidence, low agreement*) that there is no clear trend in WD frequency or intensity over the instrumental period."

In Sec 7.2.2: "There is, therefore, **high confidence** (*robust evidence, medium agreement*) that winter precipitation has declined over the Western Himalaya, Hindu Kush, and Karakoram during the instrumental period."

In Sec 7.3.1: “Therefore, there is **very low confidence** (*limited evidence, low agreement*) that future WD frequency will decline due to climate change.”

In Sec 7.3.2: “There is **high confidence** (*robust evidence, medium agreement*) that climate change will cause winter precipitation to increase over the Western Himalaya but decrease in the foothills. There is **very high confidence** (robust evidence, high agreement) that climate change will cause the ratio of snowfall to rainfall across the region in winter to decrease.”

It would also be helpful to provide a table in the paper where key points are listed together with a measure of confidence. I would expect that the paper will receive greater visibility if key points are directly visible instead of being hidden in the text. We agree, and will add a table summarising key points and their measures of confidence (as fully explained in our response to the comment above) in our revised conclusion (Sec. 8.1). Please see below.

Statement	Confidence	Section
Tracking algorithms are a useful tool for understanding WDs.	high	2.2
WD cyclogenesis mostly occurs over ocean or downstream from mountain ranges.	medium	2.3.1
WDs intensify through baroclinic instability, sometimes with moist or orographic coupling.	very high	2.3.2
WDs primarily affect the Western Himalaya and surrounding mountain ranges.	very high	2.3.3
WDs have mid- to upper-tropospheric vorticity maxima with ascent ahead of their centre.	very high	2.3.4
The Arabian Sea is the primary moisture source for WD precipitation.	high	2.4
WDs are most frequent between December and March but can occur at any time of year.	high	2.5
There is large variance in most WD characteristics, such as lifetime, intensity, and latitude.	high	2.6
WDs provide the majority of winter precipitation to the Western Himalaya and surrounding area.	high	3.2
By recharging glaciers and the snowpack, WDs are vital for regional water security.	very high	3.3
Rabi crops rely on WD rainfall.	medium	3.4.1
Heavy hail or snow from WDs can damage crops.	high	3.4.1
WDs provide conditions conducive to widespread fog.	very high	3.4.2
WDs reduce pollution levels through increased rainfall and near-surface winds.	medium	3.4.2
WDs can cause coldwaves over north India.	high	3.4.3
WDs are the primary cause of pre-monsoon lightning over north India.	high	3.4.4
Landslides in the Western Himalaya are often triggered by WDs.	medium	3.4.5
WDs can trigger avalanches in the Western Himalaya.	very low	3.4.5
The interaction between WDs and the summer monsoon often leads to very heavy rainfall.	very high	3.5
A positive phase of the NAO leads to increased WD frequency and intensity.	very high	4.2
A positive phase of the AO leads to increased WD frequency.	high	4.2
El Niño leads to increased WD frequency.	low	4.3
El Niño leads to increased seasonal precipitation over the Western Himalaya.	very high	4.3
A positive phase of the IOD leads to increased WD frequency.	very low	4.4
Simulations of WDs are mostly insensitive to the choice of parameterisation schemes.	high	5
Simulations of WDs are sensitive to the choice of land surface dataset and parameterisation.	high	5
Increasing model resolution considerably improves simulations of WDs and their impacts.	very high	5
WD tracks can be skilfully forecast in operational models.	very low	6
WD frequency was higher during most of the Late Pleistocene (60–12 ka).	medium	7.1.1
WD frequency was much lower during the Early Holocene (12–8 ka).	medium	7.1.2
WD frequency was lower during the Mid Holocene (8–4 ka).	very high	7.1.3
WD frequency was lower during the Roman (2.5–1.9 ka) and Medieval (1.5–0.7 ka) Warm Periods.	high	7.1.4
WD frequency was higher during the Little Ice Age (0.7–0.2 ka).	very high	7.1.4
There is no clear trend in WD frequency during the instrumental period.	medium	7.2.1
Winter precipitation over the Western Himalaya has declined in recent decades	high	7.2.2
Climate change will cause WD frequency to decline.	very low	7.3.1
Climate change will cause winter precipitation to increase over the Western Himalaya	high	7.3.2
Climate change will cause winter precipitation to decrease along the Himalayan foothills	high	7.3.2
Climate change will cause the ratio of snowfall to rainfall to decrease	very high	7.3.2

Table 2. Summary of the key statements that have emerged from WD literature in the last decade, along with the confidence in those statements (following the IPCC definitions of confidence) and section in which the relevant studies can be found.

Overall, I would hope the paper to become more concise once the subsections with a 'list' structure have been revised.

Thank you, please see our response to your first comment, above.

2) Even for state-of-the-art reanalysis data sets, there are uncertainties on the fraction of precipitation that can be attributed to WDs. Accordingly, it seems to be even harder to quantify the fraction of precipitation associated with WDs for past or future climate states. Still, quite often the authors refer to studies establishing a link between precipitation and WDs. For such statements, it would be important to

explain at least briefly how the authors come to the conclusion that a clear link between WDs and precipitation exists even if the WDs have not been identified objectively in some cases (e.g., l. 1568, l. 1581, l. 1664).

We agree that this is not sufficiently well discussed in the text. Following this suggestion, and that of reviewer 1, we will add the following to the beginning of our paleoclimate section: "Paleoclimate research has become increasingly popular over the last few decades, especially as more advanced proxy techniques have been developed and refined. For precipitation, these include speleothems, marine and lake sediments, tree rings, and pollen analysis. As we discussed in Sec. 3.2.2, present-day WDs are responsible for the majority of total winter precipitation over the Western Himalaya and surrounding region and likely – through changes in WD frequency and intensity – the majority of its interannual variability as well. For these reasons, precipitation is often used in paleoclimate studies as a proxy for WD activity over the Western Himalaya. However, there are several important sources of uncertainty that arise with this approach. Firstly, the relative contributions of winter precipitation (i.e., WDs) and summer precipitation (i.e., the monsoon) to the annual total may change over time. However, this uncertainty can largely be removed by quantifying the d-excess of the sample studied (see Sec. 2.4). Secondly, as mentioned above, some winter precipitation variability must arise from non-WD sources, the primary source of which is cloudbursts. The fraction is unknown, but probably small, and may also have varied over long time periods. Thirdly, analyses often make do with proxies from winter precipitation dominated areas nearby (e.g., Iran, central Asia), and extrapolate the result to the study area (e.g. Petrie and Weeks, 2018). Thus, while we can be reasonably confident that long-term changes in winter precipitation are related to changes in WD activity, we must bear these caveats in mind when discussing the results of the paleoclimate studies that follow."

l. 92: Can you comment here or later on what processes lead to the WD cyclogenesis?

Yes, we will add the following sentence here in the revision: "They then propagate as troughs embedded within the subtropical westerly jet until they reach South Asia (Singh et al., 1971)."

l. 136-142: In principle, I agree. But what about the fact that WDs travel along distance before they reach India/that they are embedded in the large-scale weather? Wouldn't this require global models?

Yes – some studies do require global models. However, many WD modelling studies tend to focus on their impacts (as we mention later in Sec. 5) and thus tend to be high-resolution limited-area models forced at the boundaries by reanalyses. We will revise the wording here to make this distinction clearer: "Research on WDs, especially of their impacts, benefits from these developments, not only because the models are now often convection-resolving…".

l. 169: Please specify that it is cyclonic potential vorticity anomalies.
Yes, will do.

l. 175: What about diabatic processes? I assume these also play a crucial role in the development of mid-tropospheric PV anomalies of WDs.
Yes, this is certainly correct (as we discuss later in Sec 2.3.2) although not much covered in earlier research. We will add that, including a new reference, here: "As deep troughs, WDs are associated with high vorticity in the mid-troposphere which can be further enhanced through orographic interaction and diabatic heating (Rao, 2003; Hara et al., 2004; Dimri and Chevuturi, 2014b)."

l. 229 and elsewhere: Please double-check whether all acronyms have been introduced before their first use.
Thank you for this comment – we have identified all unique abbreviations in the text using regular expressions and will ensure the first instance of each is explained in the revision. We will introduce meteorological terms (e.g. NAO, CAPE) in the text itself and use footnotes for product names (e.g. IMDAA, APHRODITE), as these can be lengthy and reduce readability.

l. 230: Are you referring to the layer mean relative vorticity between 450 to 300 hPa.
Yes, this is correct. We will include this in the revision.

l. 225 & l. 232: The study regions of WDs differ. Would it be an important future step to agree on one region across the the WD science community? Likewise, the minimum lifetime seems to be quite variable. Also concerning this aspect, a critical discussion of the different criteria would be well suited in a review paper (see also comment on Fig. 5).
We agree such a discussion would be valuable and will add the following: "The reader will have noticed that each of these studies uses different criteria -- both capture regions and minimum track lengths or durations -- to filter their WDs. This is because no standard yet exists, and so authors typically choose their capture regions to reflect the impacts they want to investigate. This makes intercomparison between studies challenging, as even basic statistics such as frequency are sensitive to these choices. Therefore, based on the discussion in this section, we propose basic criteria that could be adopted in future WD tracking studies in order to standardise the results.

Firstly, rather than a capture region, where the choice of longitudinal extent can have a significant impact on the characteristics of the WDs in the final catalogue, we propose simply that WDs must cross 70°E (to the east of almost all genesis areas, see Sec. 2.3.1; and to the west of the regions of greatest impact), and do so between 20°N (to filter out tropical systems) and 50°N (to filter out polar systems, but retain northward-tracking WDs that can still have an impact over the Karakoram or Pamirs). We also propose a minimum track duration of 48 hours to filter out transient systems; but no minimum track length, as WD genesis can occasionally be very close to the Western Himalaya."

l. 239-252: Though I appreciate the authors attempt to list existing techniques to approximate WD frequency and related statistics, it may be more important to the reader to understand what the implications of the different techniques are. For example, what fraction of uncertainties in WD statistics (number per year, speed etc.) can be attributed to different tracking techniques.
Thank you. We agree that such a discussion would be very valuable. However, the uncertainties in WD statistics are sensitive not only to the choice of these techniques but also to thresholds used (e.g., the spectral power/variance must exceed some value for it to be considered a WD day). Therefore, to quantify these uncertainties would require replicating each of these studies, which we deem to be beyond the scope of this review. We will add the following sentences in the revised version of the manuscript explaining this: "This wide range of indirect techniques leads to a wide range of estimates of WD statistics, such as frequency. Quantifying these uncertainties is made significantly more challenging by the sensitivity of each method to the cutoff thresholds used to define WD activity (e.g., variance), and so they are difficult to compare directly."

l. 277: Is it really the case that disturbances are blocked by the Tibetean Plateau? For example, taking relative vorticity at 400-300 hPa, I'd be surprised if the systems were blocked by the Tibetean Plateau. Is it not rather the case that flow configurations advecting disturbances southward do hardly occur? Or are you referring here to disturbances near the surface?
WDs can be blocked by the Tibetan Plateau (although this phrasing is slightly misleading – rather the jet is only stable on either side of the TP, not on top of it). However, you are indeed correct in that this is actually an issue of flow configuration, and we will correct this accordingly in our revision.

l. 292: Diabatic heating can also occur over a prolonged period and with considerable latent heat release in stratiform precipitation (e.g., in WCBs). So, is it a necessary condition for the intensification that WDs are associated with convective precipitation?
This is a good question and one that does not appear to have been addressed in previous literature. There is no reason that stratiform precipitation couldn't also play the same role, and as we see in Fig 6, stratiform clouds are not uncommon along the foothills in the winter. We will add the following sentence: "Stratiform clouds are also common along the foothills (Fig. 6) and therefore may also play a role in WD intensification, but this has not yet been investigated."

l. 304: Through which process does latent heat release increase the strength of the upper-level (cyclonic) PV anomaly? If the latent heat release occurs in the mid-troposphere, it would rather lead to an anticyclonic PV anomaly in the upper

troposphere, i.e., reduce the strength of the upper-level (cyclonic) PV anomaly. Perhaps you can also refer to lower and upper troposphere, instead of lower and upper level.
This is a good point and arises from an earlier misreading of Para et al (2020). The original sentence reads: "However, Para et al (2020) used a case study of extreme precipitation over Jammu and Kashmir in 2017 to show that the broad quasigeostrophic ascent can be coupled with convection, as latent heat release increases the strength of the upper-level PV anomaly." We will revise this to:
"However, Para et al (2020) used a case study of extreme precipitation over Jammu and Kashmir in 2017 to show that WD circulation can be coupled with convection, as latent heat release increases the strength of the lower-tropospheric PV anomaly."

l. 342: Are you referring to divergence in the upper- or lower troposphere? Also, I found it very difficult to follow the line of arguments here. If there is a negative correlation between propagation speed and cloud-top height, would this not mean that systems with low cloud-top height propagate faster, and those with high cloud-top heights propagate slower? This at least would be dynamically understandable: Systems with high cloud-top heights are associated with stronger upper-tropospheric divergence and a corresponding divergent outflow which would be directed against the eastward propagation of the WD. If it is the case that WDs with stronger convection tend to propagate more quickly: Is this because of diabatically generated lower-tropospheric cyclonic PV similar to diabatic Rossby waves/vortices?
This was an error in the original manuscript. We meant to say that the correlation is positive and will fix this in the revision. We will also include the dynamic reasoning provided.

l. 359: The quasi-geostrophic ascent could also occur before the moisture reaching the orography. Is this also observed?
Yes – as is the convection. We will rephrase this sentence to make this clear:
"Orography is not required for WDs to precipitate, as neither the convection nor QG forcing depend on it."

l. 370: It reads as if there is "orographic instability" which is certaintly not meant here. Please clarify.
We will remove the parenthetical here.

l. 408: Please clarify trough which process the transport of ozone is happening since a PV anomaly per-se does not necessarily lead to stratosphere-troposhere exchange.
Good question. According to the original paper (Satheesh Chandran et al., 2022), "The equatorward intrusion of high PV associated with the western disturbance (Figure 8d) facilitates the transport of mid-latitude lower stratospheric air deep into the tropical latitudes. Associated with this intrusion, a strong subsidence is observed in the eastward side and upwelling in the westward side (Figure 8c)." We will clarify

this in our revision: “As in extratropical cyclones, the deep PV anomaly associated with WDs -- which extends to the tropopause -- can result in substantial transport of ozone through advection of mid-latitude stratospheric air into the tropical upper-troposphere (Satheesh Chandran et al., 2022).”

l. 439: How exactly could a moisture flux analysis supplement the isotope analysis? Moisture flux analysis provides only a Eulerian viewpoint and does therefore not provide information on the actual moisture source.
This is certainly true for individual events, but the Eulerian approach does work for seasonal composites. We will remove the usage here, and then clarify its inclusion at L475.

l. 475: See previous comment.
We will clarify that moisture flux is valid in this context only on longer timescales: “Beyond isotope and trajectory methods, recent work by Baudouin et al (2021) highlighted the utility of composite moisture flux analyses in investigating precipitation moisture sources, with the caveat that such analysis only works on seasonal timescales or longer.”

l. 620: Is this a general statement or specific to the region affected by WDs?
It probably does hold generally, but was intended for the study region. We will clarify accordingly in the revision.

l. 655: That the percentage of precipitation attributed to WDs varies substantially between studies calls for a consistent approach when matching WDs and rainfall. Have there been approaches where the distance at which rainfall is still attributed to a WD is based on objective criteria such as the Rossby radius of deformation? A critical discussion would be worthwhile here.
This is a good question. Unfortunately, the studies discussed here are the only one that have attempted to quantify the attribution fraction. The methods used in these studies (either taking a fixed radius or all precipitation on the day – i.e. an infinite radius) follow earlier studies attempting to do the same for monsoon depressions. My own (unpublished) experience is that the answer is relatively insensitive to the choice of radius, if sufficiently large, since almost all precipitation that falls on WD days is due to the environment created by the WD itself. The other main source of winter precipitation in this region is cloudbursts, which are either triggered by WDs or occur independently on other days.

The reason, we believe, that Midhuna et al (2020) only managed to explain 20% of the monthly precipitation *variance* is because they *only* considered whether precipitation fell on a WD day, when in fact there is also a strong correlation between WD intensity and precipitation, as we discuss in Sec 2.6.

What is clear, then, is that WDs cause a majority of total winter precipitation in the Western Himalaya and surrounding regions, with a conservative lower bound of

55% and an upper bound of over 90%. What remains unknown, though, is the fraction of precipitation variance for which WDs are responsible across different timescales.

We will rephrase and extend the final paragraph to reflect these points: “These results suggest that the true value of the seasonal winter precipitation contributed by WDs is likely to be somewhere between the values stated by Hunt et al. (2019a) and Midhuna et al. (2020), but it is certainly a majority. The uncertainty likely arises not from the choice of attribution radius – to which attribution fraction has been found to be relatively insensitive for other types of system (e.g. Hunt and Fletcher, 2019) – but from the method used to detect WDs. This is because the other main source of winter precipitation in this region is cloudbursts -- very intense but short-lived thunderstorms that drive highly localised extremely heavy precipitation. Cloudbursts are either triggered by WDs or occur independently on other days (Singh and Thapliyal, 2022; Dimri et al., 2023), and so almost all precipitation on WD days arises from the environment created by the WD. What remains unknown, however, is the fraction of precipitation variance across different timescales for which WDs are responsible. There is evidence that this is probably a large majority on intraseasonal timescales: firstly, because WDs are responsible for most of the seasonal total and their frequency and intensity varies substantially between different years (Sec. 2.6); and secondly, because WDs are responsible for 90% of extreme winter precipitation events over the Western Himalaya and northern Pakistan (falling to only 20% in the summer). Further work is needed to quantify this explicitly, and in so doing validate results from climate and paleoclimate studies that infer changes in WD activity directly from precipitation”.

l. 730: I am wondering whether the heading of section 3.4 would be a better one for Section 3. Precipitation can lead to natural hazards so the separation between precipitation and other natural hazards seems a bit arbitrary.
We agree and will make this change.

l. 739: What is the reason for the cool ground?
Winter. The overnight lows in Delhi and Chandigarh both average about 7°C, both with record lows around freezing. We will clarify this by adding “wintry” in the revision.

l. 750: Are there already insights on why the boundary layer turbulence is suppressed in the rear sector of WDs? Is it due to descending air masses causing an inversion layer that prevents the downward mixing of momentum.
This is likely one of the reasons, although it is not stated in Patil et al (2020) (they attribute it to surface cooling) and we couldn’t find it in earlier literature either. We will add a short statement in the revision to reflect this: “Patil et al (2020) attributed this reduced turbulence to surface cooling induced by the WD, but it may also be

due to descending air masses causing an inversion layer that prevents the downward mixing of momentum."

l. 758: Though I am not an expert in this field, I would expect that the increasing pollution is the most important factor.
Please see our response to the next comment.

l. 770: This somewhat confirms my previous statement.
Agreed. We will include this in the revised version of the manuscript (bearing in mind that this section will also be revised considerably): "Fog has increased significantly over north India in recent decades (Hingmire et al., 2019), as much as doubling since 1970 (Srivastava et al., 2016). Most studies agree that this is primarily due to increased aerosol loading and urban expansion (Smith et al., 2022; Hingmire et al., 2022; Verma et al., 2022). However, WDs may also play a role – a recent reduction in WD frequency, and hence weaker near-surface winds, increased radiative cooling, and reduced precipitation, has also been linked to increased pollution over north India, both in models (Paulot et al., 2022) and observations (Gunturu and Kumar, 2021; Xie et al., 2023)."

l. 735-786: This section needs to be revised substantially. It currently reads as a collection of literature, but due to the partly opposing research results it is difficult to develop a conceptual picture. A different approach would be to rather summarize the findings about which we are certain and then to mention the uncertainties which still need to be quantified.
Thank you for this suggestion. Following this and your first major comment, we have revised and shortened this section substantially.

l. 805: Can you explain what is meant by "nor'westers"? It reads like a phenomenon associated with strong winds, but this would not fit to the section dealing with lightning and hailstorms.
These are squally northwesterly winds that impact Bengal and the surrounding region. We will add a sentence in the revision that states they are associated with both hail and lightning: "Pre-monsoon storms can also occur near the Himalayan foothills in northeast India and Bangladesh, where they are associated with nor'westers, known locally as *kalbaisakhi* (Roy and Chatterji, 1929; Das et al., 2014). These kalbaisakhi often bring heavy hail and lightning (Midhya et al., 2021)".

l. 890: A further shortcoming might be that the role of lower-level PV maxima and upper-level PV maxima has not been quantified yet. The concept of piecewise potential vorticity inversion would be one diagnostic to assess the role of different PV anomalies.
Agreed, we will add this in the revised manuscript: "Another shortcoming was that the roles of lower- and upper-tropospheric PV maxima were not quantified. Further work might use piecewise PV inversion to assess the role of different PV anomalies."

l. 898: Though I agree that SAM, NAO rectify onto SSTs with similar patterns to those shown here, it is still questionable whether a physical link exists. For example, through which physical process would SAM be connected to the WD occurrence frequency? This aspect definitely needs some explanation.
This is a good question. We already discuss in the text potential causal mechanisms linking the NAO to WD frequency, e.g., "A composite analysis by Hunt and Zaz (2021) found that winters with a strongly positive NAO resulted in a stronger subtropical jet, which in turn forced more frequent and more intense WDs, driving increased winter precipitation over the Western Himalaya and Indus Basin" and "[Attada et al, (2019)] showed that warmer winters there are associated with negative phases of the NAO and AO, which decrease the upper-level meridional temperature gradient, weakening the subtropical jet and decreasing WD frequency."

An extended discussion of the SAM is out of scope for this review, as no studies have explicitly linked it to winter weather in our region of interest. However, there is literature linking the SAM to variability in the summer monsoon, and we will add a very brief discussion of that to the section summary in our revision: "Despite its distance from the subcontinent, there does appear to be a link between the SAM and the both the East Asian and South Asian summer monsoons (Pal et al., 2017; Fogt and Marshall, 2020), and so a connection with the winter weather of these regions is also plausible."

l. 1012-1019: These lines should not be part of section 4.4 since they summarize Section 4 in total.
Agreed. As there is no way to leave a subsection without creating a new one, we will use a blank line to separate this summary in the revision.

l. 1122: Could you explicitly state which land surface datasets were found to yield superior results? Or do you only want to state that the representation of WDs is more sensitive to the land surface dataset than to model parametrisations.
Although a discussion of which land surface dataset is "best" would be interesting, our purpose here is the latter. Generally, newer datasets tend to perform better, but as the cited studies only test different pairs, this would probably require further research.

l. 1165: Are these short-range forecasts deterministic? If so, can you comment on the added value of ensemble forecasts and this is one way forward?
Yes, the short-range forecasts produced by the IMD (from which they draw these warnings) are deterministic, either from a 3-km WRF model or a T1534 (12 km) version of the GFS. We will add a short comment on this in the revision (at the end of this subsection): "These warnings are derived from deterministic short-range operational forecasts, and so one approach to improve skill may be to use an ensemble forecast – which are often better at capturing extremes – instead

(Boucher et al., 2011). However, a full analysis of warning bulletins and nowcasts in the context of WDs is left as an important topic for future work."

l. 1200: Extended range and subseasonal forecasts often use hybrid approaches combining statistical and dynamical models. Have there been insights on whether statistical models for the occurrence of WDs are useful? For example, though their connections to ENSO, NAO etc. there could be valuable predictors on this longer time scale.
This is a very good point. These kinds of forecasts exist for the summer monsoon, but not, to the best of our knowledge, for winter precipitation or WD activity. We will note this both here (Sec 6.4) and in Sec 8 as an important avenue for future research: "One such avenue might be the use of hybrid statistical-dynamical forecasts, such as those already used for the summer monsoon (e.g. Rajeevan et al., 2007), which could leverage additional predictability offered by teleconnections to ENSO, NAO, and other large-scale modes of variability."

Section 7: Overall, Section 7 needs to be shortened. On several occasions the link to WDs is not clear and it is difficult to synthesize all the given information to form a consistent picture.
Thank you for the suggestion. In the revision, we will rewrite sections 7.1 and 7.2.1 to shorten them, make the links to WDs clear, and synthesise the key themes of the relevant literature.

Further, it is not clearly explained how WDs are linked to precipitation in paleo-climate studies. Such explanation is necessary given the difficulty of identifying WDs even in state-of-the-art reanalysis data sets.
Thank you for raising this point. Please see our response to your major comment #2.

l. 1220: Could you include an initial hypothesis on why a response of WDs to climate change is expected?
Yes, we will add the following to the beginning of our revised Sec 7: "As the climate warms, we expect changes to WD dynamics -- as inhomogeneous upper-tropospheric warming modifies the subtropical jet. We also expect thermodynamic changes to WDs, as warmer near-surface conditions modify static stability and increase atmospheric moisture content."

l. 1541: The "elevation-dependent warming" can presumably be removed.
Thank you, this was errant subsection labelling that we will remove in the revision.

Fig. 2: Please include state borders.
We use orographic contours here primarily as that is the strongest control on mean precipitation patterns. However, we appreciate state borders are more familiar to most readers and will include them in our revision (see figure below).

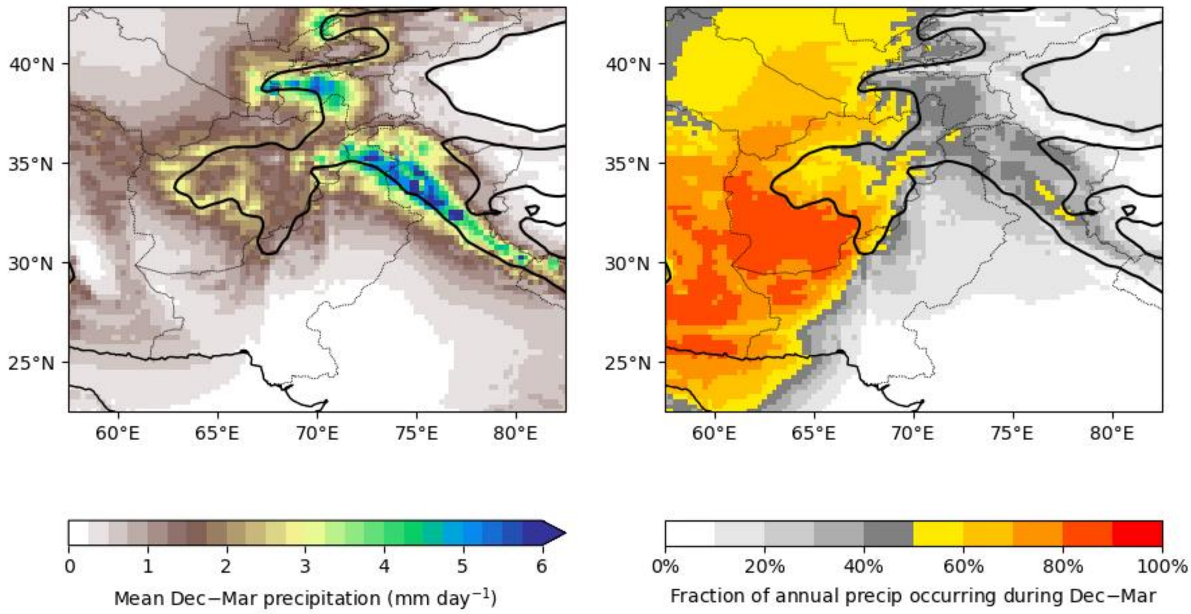

Fig. 3: WDs are tilted northwestward with height. Is there a reason for not showing this tilt?

Yes – this is a good point. We decided not to show the tilt because it is relatively slight (see Fig 8 and related discussion) and the schematic figure is already quite complicated.

Fig. 5: To my understanding a "commonly used WD track capture region" does not really exist in literature and the definition of regions varies from paper to paper. Please reconsider the formulation.

This is correct. We will rewrite the caption so that it reads "The black box indicates the WD capture region used in some tracking studies (60—80°E, 20--36.5°N). The dotted box shows the extension to 42.5°N which has also been used in some more recent studies."

Fig. 10: Given the stronger jet during winter, it is probably not too surprising that the WDs are stronger in winter. If the intensity was normalized with the seasonal mean vorticity, would this show intense WDs also during summer?

Yes, by the definition of normalisation, this would indeed be the case if we normalised summer WDs by the summer mean WD vorticity.

References:

Keller, J. H., and Coauthors, 2019: The Extratropical Transition of Tropical Cyclones. Part II: Interaction with the Midlatitude Flow, Downstream Impacts, and Implications for Predictability. Mon. Wea. Rev., 147, 1077–1106, https://doi.org/10.1175/MWR-D-

17-0329.1.
Thank you for this suggestion. We will add it to Sec. 6.3: "This is also compounded by the fact that WDs likely modulate the jet downstream, further degrading predictability of high impact weather, as has been shown for extratropical cyclones (Keller et al., 2019)."