I have reviewed this article with my limitations, I hope authors may find it helpful for improving the readability and scientific credentials.

Title: Western disturbances and climate variability: a review of recent developments

Author(s): Kieran M. R. Hunt et al.

MS No.: egusphere-2024-820

MS type: Review article

Recommendation: Accept with revision

This is a comprehensive review of WDs, authors have put meticulous efforts in this review research work by including all the available relevant research studies. This is surely useful for researchers interested in this field. However, I strongly feel simplified description will be more beneficial for new researchers to get crucial interest in the subject. It is clear that this review is more focussed on boreal winter time WDs, having baroclinic structure, basically 'frontal synoptic scale' in nature? Here the dynamical processes are dominant over thermodynamical. Though it is more confined to Himalayan regions, it also extends over central and western Indian regions. In my opinion this review of the past and present studies can be better structured (like IPCC report) where any scientific argument is categorised with low, medium and high confidence level. This may help in simplifying the description, otherwise it very confusing at each stage. The simplified description will enhance the readability as well as its scientific credentials. In deed this article should be accepted in this journal but with revision. Kindly find the line by line comments below.

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. This includes an IPCC-like confidence statement at the end of each section, which will then be summarised in our conclusions section.

Line 90: While describing western disturbances (in addition to it's interaction with summer monsoon systems) it would be more appropriate to distinguish it from typical summer monsoon synoptic systems in which the complex thermodynamics as well as dynamics plays a crucial role.
OK – we will add a sentence here drawing a contrast between WDs and the summer monsoon: "WDs differ considerably from monsoon low-pressure systems, the other

synoptic-scale vortex that regularly affects the subcontinent, whose development and propagation is driven by moist thermodynamics coupled to the mean monsoon flow."

Line 100: Along with Chevuturi and Dimri, 2016, you may like to refer Vellore et al. 2015/16

Thank you for drawing our attention to this reference. We will include it in our revision. (Reviewer is not clear exactly what paper they are referring to here, but we think it is "Monsoon-extratropical circulation interactions in Himalayan extreme rainfall" in Climate Dynamics)

Caption of Figure 3: It is Cold and dry 'air' advection?

Yes, though "air" is often conventionally omitted from such phrases. We will include it here for clarity.

Line no. 136: Firstly, recent studies …… increasingly ?????? high-resolution models, ….

The original sentence is "Firstly, recent studies have made use of increasingly inexpensive high-resolution models, both for regional climate modelling and numerical weather prediction." We will revise this to read: "Firstly, recent studies are making increased use of high-resolution models, which are becoming cheaper to run, both for…"

Line No. 140 and 145: Please Consider simplifying these statements.

The existing sentence on L140 is: "The large number of high-resolution experiments also serves as a primitive large ensemble -- as these models are able to capture processes more faithfully, experiments can more easily establish which physics schemes, forcings, and configurations are most important, collectively driving down the model uncertainty from which earlier studies suffered." We will revise this to: "The large number of high-resolution experiments also act as a primitive large ensemble. As these models better capture small-scale processes, experiments can more easily establish which physics schemes, forcings, and configurations are most important, reducing the model uncertainty from which earlier studies suffered."

The existing sentence on L145 is: "These developments have helped to link the physical processes of individual storms to the large-scale weather in which they are embedded, and to understand directly the influence of climate change on the statistical behaviour of WDs." We will revise this to: "These developments have helped to link the physical processes of individual storms to the larger weather systems they occur in, clarifying the direct influence of climate change on the statistical behaviour of WDs."

Line No. 160: Kindly include Vellore et al. 2015/16

Please refer to our response to an earlier comment regarding this reference. We will include it in the relevant section (3.5).

Line No. 191 and 192: These studies are 'more recent analyses'???? Sentence may be corrected.

Yes, 1999 and 2011 are more recent than 1947, 1956, and 1969. We will keep this sentence as it is.

Line No. 271: How WDs are different from Frontal system?

We agree that the difference between WDs and (we think the reviewer means) extratropical cyclones should be made clear. L271 is not the correct place for this, but we will include the following: "However, many features present in extratropical cyclones, such as frontal fractures, sting jets, and warm seclusions, have not yet been observed in WDs" at the beginning of Section 2.1. Also note the difference is already raised as an open question in Section 9 (q6).

Line 282,283: Sentence is not clear.

The sentence in question is: "There is a preference for cyclogenesis in regions of dynamical instability; typically downstream from mountain ranges, but also within the North Atlantic jet stream." We will rephrase this to: "Cyclogenesis tends to occur in areas of dynamic instability, often found downstream from mountain ranges or within the North Atlantic jet stream."

Line No. 344: Simplify the sentence for better readability describe how a negative correlation with ......?

The full sentence is "This is supported by Chand and Singh (2015), who found, when using satellite data to analyse a group of 10 WDs, that WD propagation speeds varied between 280 and 670 km day^{-1} and had a negative correlation with cloud-top height downstream, implying that WDs associated with stronger convection tended to propagate more quickly." We agree that this is quite long (and also contained a mistake) and will replace it with: "This is supported by Chand and Singh (2015), who used satellite data to find a negative correlation between WD propagation speed and downstream cloud-top height, implying that WDs associated with stronger convection tended to propagate more slowly. They also showed that WD propagation speeds vary substantially, from 280 and 670 km day^{-1}".

Line No. 349 – 351 : Do you mean baroclinicity?

No – but we appreciate this sentence could be more clearly worded: "In summary, the deep ascent ahead of WDs primarily occurs due to downstream upper-tropospheric divergence. This is supported by quasigeostrophic differential vorticity advection and mechanical uplift of induced lower-level southerlies as they interact with the orography."

Line No. 378 : Please correct the sentence for better readability.
The sentence is "The second WD spun up over northern Europe on Jan 22, before migrating southward and then propagated rapidly towards and then over the Western Himalaya, where it resulted in heavy precipitation." We will replace this with "The second WD spun up over northern Europe on Jan 22. It then migrated southward, before moving rapidly towards and then over the Western Himalaya, where it subsequently resulted in heavy precipitation."

Line No. 436-439: Sentence not clear.
The sentence here is: "However, this is complicated by fractionation – wherein rain preferentially forms from low D-excess water – further increasing the D-excess in moisture in air parcels that have been transported a long distance, orographically lifted, or even locally recycled (Kong et al., 2013)."
We will revise this to: "However, this is complicated by fractionation. Rain forms preferentially from low D-excess water, and so D-excess increases in moisture in air parcels that have been transported a long distance, orographically lifted, or even locally recycled (Kong et al., 2013)."

Line No. 440: Provide suitable references.
The sentence is "Ideally, therefore, the results of isotope analysis over the western Himalaya should be disambiguated with a complementary moisture trajectory or moisture flux analysis." This follows on from the previous sentence which discusses the uncertainties arising from fractionation. A reference is therefore not needed here.

Line No. 445: flawed????
It is unclear whether the reviewer is uncertain of the definition of "flawed" or its application to the list of references. The latter is clearly explained in the sentence itself: "…relying on only short sample periods or applying trajectory analysis either only to case studies or for whole seasons." A simple definition of "flawed" is thus provided here: *having a fundamental weakness or imperfection*.

Line No. 455: How significant is Mediterranean moisture?? here when it is not a majority moisture source?
Here we were quoting the conclusions sections of both papers. Jeelani et al (2017) does not explicitly quantify the Mediterranean contribution. Dar et al (2021) does in their Table 5, where they give the probabilities of each basin being the majority contributor for certain types of event. For the Mediterranean, they give values in the range of 20–30%, which we will include in our revision.

Line 475-485: In fact Section 2.4 is too confusing, you may kindly retain very relevant references?
The reviewer is here referring to the paragraph at the end of Section 2.4 which discusses how (Eulerian) moisture flux analysis can also be a useful tool in deducing moisture sources, alongside isotope-based or Lagrangian methods. We briefly

mention the recent results of Baudouin et al (2021), who examined these moisture pathways on seasonal timescales, before linking those results to earlier work on atmospheric rivers. We will rephrase, shorten, and try to improve clarity as follows: "Beyond isotope and trajectory methods, recent work by Baudouin et al. (2021) highlighted the potential use of composite moisture flux analyses in investigating precipitation moisture sources, with the caveat that such analysis only works on seasonal timescales or longer. They identified a mean moisture pathway between the Red Sea and the North Arabian Sea and showed that WDs transiently steer this pathway towards the western Himalaya and surrounding region. Results obtained using this method are very similar to those obtained from large-sample back-trajectory studies (e.g., Fig. 9). These pathways are analogous to the atmospheric rivers that are responsible for winter precipitation and flooding to the west, in Iran (Dezfuli, 2020; Dezfuli et al., 2021; Esfandiari and Lashkari, 2021). Atmospheric rivers have also been explicitly linked to the majority of winter precipitation variability and extremes over the western and central Himalayas (Rao et al., 2016; Thapa et al., 2018; Lyngwa et al., 2023), where composite analysis shows circulation that strongly resembles that of a WD. The altitude of these moisture pathways also appears to be important, with the largest moisture transport occurring between 850 and 700 hPa, a higher altitude than usual in the tropics (Baudouin et al., 2020b)."

Line No. 510: Figure 10: Caption- Is the percentile calculation based on entire time-seires or has been calculated on monthly basis.
As already stated in the caption, this is *overall* intensity percentile based on the full time series rather than monthly. If it were monthly, the deciles would all have the same size for a given month. We will clarify this in our revised caption.

Line No. 531: '..... associated with all winter WDs' What about other seasons?
The reviewer is here asking about our definition of "active" WDs. For this, we look at the daily precipitation over the Western Himalaya and surrounding region for all winter WDs, and take the top quartile of systems. There are several reasons we restrict this definition to winter. Firstly, it is consistent with earlier literature cited in this section (e.g., Datta and Gupta, 1967; Rao and Srinivasan, 1969; Chattopadhyay, 1970; Subbaramayya and Raju, 1982; etc), and this is, after all, a review paper. The vast majority of WDs occur in the winter months and the majority of their impacts are felt in this season. Secondly, we want to highlight the links between heavy precipitation in WDs and other WD characteristics. If we included monsoonal WDs in this, they would almost all by definition be active, since they can draw in monsoonal air masses and thus tend to precipitate much more heavily. Our section on variability would not then contrast strong and weak WDs, rather winter and summer WDs – which we already do in Section 2.3.3 and 3.5. Finally, a significant fraction of monsoonal WDs may arise as polar PV cutoff lows (see Sec 2.3.3, or Thomas et al., 2023) and may have different structure, characteristics, and behaviour. As these differences are not yet known (see Sec 8, Q10), we do not want to contaminate this overview with a small sample of potentially very different systems.

Line 535: why 350 hPa is being considered in analysis? please provide the supporting argument
This is the pressure level at which the average WD has its maximum vorticity (see Figure 8). We will clarify this with a footnote in the revision thus: "The choice of 350 hPa arises from Fig. 8, which shows composite WDs have their maximum vorticity at this pressure level."

Line no. 550: The difference between two studies is not understood here.
These references support the prior statement, which is that WD latitude can have a significant impact on WD characteristics and impacts. Both studies discuss this, in slightly different ways: Baudouin et al (2020b) show how WDs at different latitudes manipulate the mean moisture pathway (and hence precipitation) to different extents; Baudouin et al (2021) show how WDs at different latitudes encounter different orographic configurations, and hence varied thermodynamic environments. For the sake of brevity, we do not include these specific details in our manuscript.

Line no. 570: dynamical characteristics and categories are two separate issues?
Yes, categories typically discretise and label certain characteristics. Consider tropical cyclones in the North Atlantic – the characteristic is wind speed, but this is often discretised into five category bins (the Saffir-Simpson scale) which helps with public, operational, and even academic communication. Our point here is that no such system yet exists for WDs, and that developing one requires careful consideration given the complex relationship between WD characteristics and their impacts. We will slightly adjust the last sentence here for clarity, replacing "categorise" with "categorise or classify".

Line no. 642-643: This could be part of data and methodology?
This sentence discusses a shortcoming of one study that uses gauge data in NW India to assess the reliability of various publicly available gridded precipitation datasets. The flaw in this study is that they did not realise their gauge dataset was not independent from gridded gauge datasets that they rated highly. As this section is on evaluating precipitation datasets in the region, we believe it is appropriately placed. Note that as this is a review paper, we do not have a data and methodology section.

Line no. 665: is it supported by back trajectories etc?
Yes, although not in Jeelani and Deshpande (2017). We discuss this in much greater detail in Sec. 2.4, which we will reference here in the revision.

Line no. 670: any reference?
Thanks for the suggestion. We will add Kulkarni et al (2021; https://doi.org/10.1016/j.wasec.2021.100101) and Mukherji et al (2019; https://doi.org/10.1007/s10113-019-01484-w) here.

Line no. 702: This can be shifted to next section?
The reviewer is here referring to section 3.3.3 “crops and flora”. We believe they mean into the next subsection (3.4, “natural hazards and other impacts”) rather than the next section (4, “large-scale forcing and teleconnections”). We are happy to make the suggested change, so that “crops and flora” will be section 3.4.1 in the revision.

Line no. 753: ‘radiation fog’ – any reference?
Yes, the relevant reference, Patil et al (2020) is at the beginning of the previous sentence. The next sentence then clearly runs on from that “they show that… WDs… provided perfect conditions for radiation fog.”

Line no. 757: You mean blocking high?
It is not clear what the reviewer is attributing to a blocking high here. The sentences in question are: “Hingmire et al. (2019) also found a significant increase in foggy days from 1980 to 2013 using data from four major cities over the IGP (Delhi, Lucknow, Hissar, Amritsar). While this increasing trend in fog events may be explained by changes in WD activity, increasing the relative tendency of a solid substance to absorb moisture from its surrounding environment levels of pollution over the region and the increased moisture flux associated with WDs in a warming world may also play a role (Verma et al., 2022, see also Sec. 7.3).”

Line no. 795: what about sub continental blocking?
Yes, both Ratnam et al (2016) and Athira et al (2024) mention that coldwaves not associated with WDs appear to arise from blocking patterns. We already state this at the end of this paragraph: “Subsequent composite analysis linked normal coldwaves to WDs, but the intense coldwaves were found to be more commonly associated with omega blocking over Siberia (Athira et al., 2024).”

Line no. 869-874: How these past and recent studies are connected?
The inclusion of the Pisharoty and Desai reference here is in error. We will fix this and correct the sentence accordingly in the revision.

Line no. 880: Please be clear what you want impress upon.
It is not clear what the reviewer is requesting here. The sentences in question are: “This shift in seasonality was later confirmed by Hunt (2024), as we will discuss in Sec. 7.2. In fact, WDs can occur at any time of the year (hence their occasional interaction with the summer monsoon), but are usually most active between November and February (Fig. 10).” To improve clarity, we will replace “active” with “frequent” in our revised manuscript.

Line no. 992-994: I get lost between Agricultural applications and features over Indo-Gangetic plains
We’re not sure what the reviewer is asking for here. Firstly, there is no mention of the IGP or agriculture on L992-994. The nearest mention of either is the IGP on line

943, but that is to do with fog variability rather than agriculture. No other mention of the IGP in our manuscript references agriculture.

Line no. 944: What is fir tree? In this sentence
We believe the reviewer means L948. We will clarify this by revising the sentence thus: "This signature also appears in paleoclimate studies, with a positive NAO linked to increased precipitation over the Indus Basin in both fir tree -- a type of confiner – cellulose..."

Line no. 946-949: How this connected with WDs?
The paragraph in question refers to winter precipitation rather than WDs specifically – noting that studies have found a strong covariance with the NAO. As we state at the end of the introduction: "In some parts of this review, we have included additional papers that cover winter precipitation over the relevant region, as this can be a useful proxy for WD frequency and such papers can add useful evidence to the discussion."

Line no. 956: I am again lost here to connect with WDs.
Please see response to previous comment.

Line no. 991: Sudden jump to stratosphere? when ENSO relation itself is not clear?
In this paragraph, we are discussing possible reasons *why* the ENSO relationship is unclear. These studies fall into two groups. Firstly, we discuss those that investigate different flavours of ENSO (e.g., Central Pacific vs Eastern Pacific). Secondly, we discuss those that investigate the role of the QBO in modulating the effects of ENSO. This is not, therefore, a sudden jump to the stratosphere; rather a discussion of all the possible confounding factors in the ENSO-WH precipitation relationship.

Line no. 994: What is SSW? In this sentence?
SSW stands for sudden stratospheric warming. This is mentioned in the previous sentence but we appreciate we did not add the abbreviation in parentheses there and so it is easily missed. This will be corrected in the revised manuscript.

Line no. 1005: What is IWM in this sentence?
This stands for Indian winter monsoon. However, the inclusion here is in error and it will be removed in our revised manuscript.

Line no. 1019: Needs more attention.
We agree, this is why it is included as one of our future research questions (Sec 8, Q20).

Line no. 1034: Is it region specific? As it is not seen in case of summer monsoon convection over Western Ghats?
This appears to be true wherever convection and orography interact, since better representation of both intuitively leads to a better representation of their

interaction (e.g. Hohenegger et al, 2008 doi: 10.1127/0941-2948/2008/0303; Fosser et al, 2015 doi:10.1007/s00382-014-2242-1). This is also true for other parts of the Indian subcontinent (Willetts et al, 2016 doi:10.1002/qj.2991).

Line no. 1045: This may be true when dynamics is dominant in the weather system? The sentence in question is: “How important is the choice of convection scheme in simulating WDs?” We’re not sure what the reviewer is asking here, but since dynamics are important in all WDs (since they are upper-tropospheric lows that pass along the subtropical jet) it is not clear what contrast they want us to draw.

Line no. 1064-1065: This statement is irrelevant here.
We are happy to follow the reviewer’s discretion here and remove it.

Line no. 1067-1069: repeated statement.
This is true, we refer to Sarkar et al (2019) in the previous paragraph as well. The methodology of this paper is repeated and we will remove it in the revision.

Line no. 1070-1074: This sentence is not clear.
The original sentence reads: “This is because they are still capable of capturing much of the necessary local thermodynamics – Patil and Kumar (2017) demonstrated realistic CAPE and OLR behaviour in two WRF case studies -- as well as the synoptic-scale dynamics – Mannan et al (2017) demonstrated realistic precipitation even for the unusual situation of WDs passing over Bangladesh, where they draw on moisture flux from the Bay of Bengal.” We will revise this to: “This is because they are still capable of capturing much of the necessary local thermodynamics as well as the synoptic-scale dynamics (Mannan et al., 2017; Patil and Kumar, 2017)”.

Line no. 1076: Infact the local dynamics seems to play important role.
This is indeed true, as we discuss in Secs. 2.3 and 2.4. As the dynamics are invariably coupled to both convection and the orography, representation of these smaller-scale processes in models is crucial for accurate forecasting of WD impacts.

Line no. 1084-1089: very confusing statements, needs reformation.
The original passage reads: “Moving away from WRF, Laskar et al. (2015) comprehensively examined two cases of intense WDs that occurred during March 2015. Using output from the IMD operational model, the GFS, and local Doppler weather radars, they found that extreme precipitation associated with the WDs, linked to anomalous southerly moisture flux from the Arabian Sea, was undersimulated by the models due to their poor representation of deep convection. Dutta et al. (2022) showed that this negative wind bias in forecast WDs could be overcome by assimilating winds from Doppler radars in north India."

We will revise this to: “Apart from WRF studies, Laskar et al. (2015) conducted an in-depth analysis of two intense WDs occurring in March 2015 using data from the IMD operational model, the GFS, and local Doppler weather radars. They found that the models underestimated the extreme precipitation associated with these WDs due to a poor representation of deep convection, despite correctly modelling the strong southerly moisture flux from the Arabian Sea. Dutta et al. (2022) further showed that the negative wind bias in these forecasts could be reduced by assimilating wind data from Doppler radars in northern India."

Line no. 1100: How it is connected to WDs.
This was about the representation of WH precipitation in CMIP6 models. We will rewrite this sentence to clarify the link to WDs: "These results were extended for CMIP6 models by Meher and Das (2024), who argued that almost all CMIP models have different strengths and weaknesses in representing the range of mechanisms required to drive precipitation, including from WDs, over the Western Himalaya. They identified the representation of mid-latitude winds, choice of land-surface dataset, and choice of physical parameterisation schemes as important drivers of model skill."

Line no. 1115: Is it connected to WDs?
Yes – this is about the simulation of winter precipitation over the Hindu Kush and Karakoram in high resolution climate models. Most of that precipitation is provided by WDs. We will clarify that in the revision: "Indeed, higher resolution climate models do perform better: Iqbal et al. (2017) found that models of the CORDEX-SA experiment simulated winter precipitation across the Hindu-Kush and Karakoram – most of which is provided by WDs – well."

Line no. 1150: In fact, these early studies explored the qualitative analysis.
This is a good point, we will add this into the revision: "Before this, forecast verification was largely confined to qualitative case studies…"

Line no. 1164: This is a serious concern needs to be addressed appropriately.
Thank you – we agree. That is why this issue is mentioned in future research questions 22 and 23.

Line no. 1180: '…..context of WDs is left an important ….. ' This is a serious concern needs to be addressed appropriately.
Thank you – we agree. That is why the issue is mentioned in future research question 22.

Line no. 1190: Which is tract 1 in Figure 17??
Track 1 is labelled in blue in Fig 17 (see the legend directly underneath the map). This WD is particularly interesting as it highlights the large uncertainties that can arise in WD track forecasts from the jet moving either side of the Pamirs.

Line no. 1203: '….. sensitivity had to be reduced …..' Needs to be elaborated here.
Yes, we will do this. The original sentence is: "The modification was required

because the forecast output has daily sampling frequency and so, among other things, the sensitivity had to be reduced to mitigate incorrect linkages." We will revise this to: "The modification was required because the forecast output has daily sampling frequency. This included a reduction in the sensitivity of the detection algorithm which mitigates incorrect linkages by increasing the minimum vorticity threshold at which candidate WDs are detected -- which in turn reduces aliasing, false positives, and hence incorrect linkages."

Line no. 1230: '..... winter precipitation there is brought by WDs.' Sentence is not clear.
Please see our response below.

Line no.1234-1236: This statement is contrary to that of line no. 1230.
We agree this introduction was unclear. Following the advice of several reviewers, we have rewritten this to explain the caveats of interpreting WD activity from paleoclimate studies: "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."

Line no. 1269: This is very confusing.
The sentence in question is: "Kar and Quamar (2020) also argued for increased WDs in the early Holocene, although their technique could not readily distinguish between summer and winter precipitation." We will revise this to: "Kar and Quamar (2020) also supported increased WD frequency during the early Holocene, but their methodology was unable to clearly differentiate between summer and winter precipitation."

Line no. 1281:' … Paleoclimate modelling' It would be more appropriate to segregate observational and modelling studies
Thank you for the suggestion. We disagree for two reasons. Firstly, modelling studies make up only a small minority of studies discussed in this section; and secondly, for readability, we want to discuss the literature in chronological order of study period.

Line no. 1315: What is the confidence level here?
This is a good point, we used "probably" when in fact the confidence level is very high. We will remove this in our revision.

Line no. 1413: Section 7.2.1 Counting WDs - Very interesting section can be better presented - it is very complex at the moment
Thank you. Following your comment and one from reviewer 2, we will revise Sec 7.2.1. to be shorter and clearer.

Line no. 1463: it is Krishnan et al. 2019?
Yes, thanks for spotting this. This is different from the other Krishnan et al (2019), and was first published online in 2018 (though in a journal in 2019, which we will change this reference to).

Line no. 1471: No confidence?
Yes, as we discuss, the sign and significance of the trend varies with region, methodology, season, and study period. While we are able to disentangle some of these factors, we still have no confidence in the overall sign of the trend of WD frequency during the historical period. We will clarify that in the revision: "In summary, there is disagreement among recent studies on the sign and significance of the trend in WD frequency over the past 70 years. There is thus no confidence in the overall sign of the trend of WD frequency over the western Himalaya in the instrumental record.".

Line no. 1480: Here - The impact of climate forcing over the trend would be very interesting? Though may not have confidence level.
We agree, yet no study has attempted to disentangle the respective roles of interdecadal variability and climate forcing on WD trends. We will add this as a future research question: "28. What are the respective roles of interdecadal variability and climate change in recent observations of seasonal and regional trends in WD frequency?"

Line no. 1495: '…interdecadal variability' - There are lots of jumps from long-term trends to decadal scale trends?
No, the focus is indeed on long-term (climate trends). The difficulty in synthesising these studies arises from the fact there is a lot of decadal-scale variability. We mention this in the original manuscript on L1494: "Once datasets or regions with spurious behaviour are removed from the analysis, the key issue is decadal

variability -- meaning the results are sensitive to the choice of analysis period" and then explain in subsequent sentences. Essentially, any discussion of trends in WD behaviour must explain why those trends vary in sign and strength depending on the study, and the answer here is that many such studies are picking up decadal-scale trends from natural variability instead.

Line no. 1506-1508: Very difficult to understand this content.
The sentence is "Other studies have reported similar results for the Central Himalaya and Nepal Shrestha et al. (2019), states of north India (Rajasthan, Gujarat, Punjab Narayanan et al., 2016), Jammu (Khan et al., 2023) and Kashmir (Dar, 2023)." The only part we imagine the reviewer must not understand is the "similar results" part, which refers to the previous sentence. We will replace "similar results" with the more explicit "similar results – i.e. a weak trend dominated by interdecadal variability –".

Line no. 1528: '....which attributed to WDs' Is it the frequency of WDs?
Yes, we will clarify this, replacing "which they attributed to WDs" to "which they attributed to increased WD frequency".

Line no. 1530 : is it related to increased WD frequency?
It most likely is, since WDs are a major cause of convective storms in the region. However the authors did not explicitly make this link, and so neither did we. We will update this sentence in the revision thus: "Bhat et al. (2024) reported a significant and very large increase in reported pre-monsoon hailstorms in Kashmir between 2007 and 2022. This is likely due to WDs, as the predominant source of non-monsoonal convective activity in the region."

Line no. 1544: '..... surface levation.' What about lapse rate?
This sentence is the definition of elevation-dependent warming: "While the general decline in snowfall is attributed to a warming climate, the spatial variability is thought to be linked to elevation-dependent warming, where trends in near-surface warming increase as a function of surface elevation." Including discussion on lapse rate would not thus be relevant here, but we will add a clause later in the paragraph: "There are thought to be a number of important drivers, depending on season and location, with changes in albedo (Ghatak et al., 2014), snow depth, cloud cover (Duan and Wu, 2006), near-surface humidity (Rangwala et al., 2009), lapse rate (Qin et al., 2024), and radiative forcing (Palazzi et al., 2017) chief among them."

Line no. 1554: is it also supported by in-situ observations?
Yes, Li et al (2020), cited in this sentence, is based on surface meteorological stations with long records. We will clarify this in the revised manuscript.

Line no. 1576: '.... Anomaly' - you mean positive anomaly? If so mention it for better readability.
Thanks – we will add this.

Line no. 1580-1581: most closely and mostly closely? Correct the sentence.
Thank you, the “mostly closely” should read “more closely”. We will correct this.

Line no. 1581-1582: ‘….. particularly as a result of changing WD activity.’ Please explain how?
This follows from the line in the study cited in this sentence, Mehta et al (2021): “The glaciers in the study area (Suru River valley) are mostly nourished by the Western Disturbances (during the December, January, and February) with maximum solid precipitation, and melt during the ablation period (May–October).” It also follows from earlier arguments that interannual variance in WH/Karakoram winter precipitation is predominantly driven by WDs. We will rephrase this sentence accordingly: “Mehta et al (2021) showed that trends in glacial ablation are most closely associated with increasing temperature, but trends in glacial accumulation are more closely associated with increased winter precipitation, particularly due to WD activity, which they state is the primary source of glacier recharge in this region.”

Line no. 1588-1590: Please restructure the sentence for better clarity.
The original sentence was: “Despite these advances, it is clear that a great deal more research is needed on how climate change across the Himalayas, Karakoram and Hindu Kush will have downstream impacts on wetlands, agriculture, and ecosystems in general (Chettri et al, 2023).” We will rephrase this in the revision: “Despite these advances, further research is urgently needed to understand how climate change in the Himalayas, Karakoram, and Hindu Kush regions will affect downstream wetlands, agriculture, and ecosystems more broadly (Chettri et al, 2023).”

Line no. 1625: It would be more appropriate to summarise the contents here before proceeding further.
This section comprises two short paragraphs, and so we will add only a very brief summary: “There was thus no consensus on whether climate change would cause WD frequency to increase or decrease, and only low confidence that winter precipitation would increase.”

Line no. 1720: Though it is a comprehensive description of future projections, it would be more appropriate to classify this in near-future, mid-future and far-future. The uncertainty of near future projection say 2030 or 2040 could be very useful for various sectors.
Thank you for this suggestion, but this would require an advanced synthesis as many authors do not make these data available in their studies. As such, it is out of scope for this review, but we will include it in our revised future research questions: “33. There is also only a weak consensus on the projected future decrease of winter precipitation in the western Himalaya. Studies leveraging high-resolution models that are capable of resolving orographic feedbacks are needed to make more robust estimates of these changes, both in the near future and far future.”.

Line no. 1721: Section 8 Future research questions and challenges: This section is very well written.
Thank you very much.

Line no. 1819: In view of the above comments Section 9 Summary needs to be considerably improved for quantitative description and better readability.
Following this comment and your summary at the beginning, we will revise Section 9 (now Section 8.1) to include a table of all the key points synthesised in the review and the confidence level associated with them (see below). We will also make improvements to the clarity of the text.

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.

Line no. 1822: Again to remind that WD over the region of interest is Importantly a synoptic frontal type of system having baroclinic structure and dominance of dynamics.
Thank you for the suggestion. We will certainly include that WDs are baroclinic here. As we discuss in Sec. 2, and then again in future research question #6, only a few WDs have traditional frontal characteristics, so we will not include that here. It is not clear what is meant by "dominance of dynamics" here.

Line no. 1839: Indeed, Quantitative description may be more beneficial for readers.
As we mention, studies have not been able to agree on the relationship between ENSO and WDs, and thus we are not able to provide a sensible quantitative estimate here.

Line no. 1866: Yes the future scope of this study is well defined in this manuscript.
Thank you.