This manuscript presents an excellent and thorough review of the current state of understanding of western disturbances. It is well structured and quite easy to follow while including substantial insights. A similar review was carried out in 2015 and much research has been published in the intervening years. The manuscript describes this more recent research in detail and also briefly discusses what was understood in 2015 for context. The manuscript makes a substantial contribution mainly by bringing together existing knowledge to increase overall understanding of the subject and formulating coherent plans for future research; there is also new material and new presentation of previously published material. The manuscript is very long: my personal recommendation is that its length should not be reduced (subject to any formal length limits imposed by the journal) as all of the material is of interest and relevance and any repitition serves to improve its clarity; it is also fairly well packaged into sections for those who are interested primarily in one aspect of western disturbances research. I note that it is longer than all of the 5 existing published review articles in WCD but of a similar length to the longest one.

I recommend publication subject to satisfactorally addressing the following issues/questions. Numbers without other context refer to line numbers in the manuscript.
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. We would especially like to thank this reviewer for taking time to check our interpretation of the many references within this review!

87: This is at first confusing as on line 94 they are described as moving eastwards. Do you mean westwards relative to the jet? In that case I suggest changing what is inside the parenthetical dashes to something like "a synoptic-scale trough moving westward relative to the subtropical westerly jet , in which it is embedded"
Thanks for spotting this typo. This should read eastward and will be corrected in our revision.

A general question that I thought might come up for a reader relatively new to western disturbances (more on reading Section 2 but perhaps could be addressed in Section 1) is whether WDs occur by definition in this particular part of South Asia (and perhaps similar phenomena occur elsewhere but have different names) or if they can only occur in this region (e.g., due to the unique orography of the Himalaya and what is to the west of it).
This is a very good question. Dynamically, they sit somewhere between extratropical cyclones and (mid-latitude) upper-tropospheric troughs – but it is through the combination of both proximity to mountains and maritime moisture supply that allows them to make such an impact. We are not aware that systems of this nature exist in other regions, but are happy to be corrected if any of the reviewers or editor is aware of any. We will add the following text to section 1: “Thus, the dynamics of

WDs sit somewhere between extratropical cyclones and mid-latitude upper-tropospheric troughs. Their unique proximity to both mountains and a maritime moisture source, however, leads to a unique array of impacts that means such storms are not found in other regions."

173-174: Can you explain how this is shown by Dimri (2004)?
Good catch – this was the wrong reference. We will update this with the correct reference (Dimri, 2008) in the revision.

I felt it might be worth clarifying that Figure 6 refers to all times rather than just during WDs.
Agreed, we will make this change: "Computed for all days (not just WDs) in winter months (December to March) using CloudSAT data."

344-345: Naively I'd associate a higher cloud top height with more convection -- can you explain how a negative correlation of speed with cloud top height implies that WDs associated with stronger convection tend to propagate more quickly?
This was a mistake – we will correct this to say that deeper convection is associated with more slowly propagating WDs.

Figure 8: is the mean for the anomaly 10 days either side of the WD?
Yes – that's correct. We will clarify this in the revised caption: "The anomalies are computed against a 20-day mean centred on the WD event."

401: It is not clear from Figure 8 how there is a northwestward tilt with height.
We agree that this is quite subtle in the composite. You can see it best in the PV field (upper-level PV maximum is several degrees to the west of the centre) and meridional winds, where the zero isotach starts at about +4° at the surface and finishes at about -4° at the tropopause. We will note this evidence in the revised manuscript.

435: Where does Pfahl & Sodemann (2014) say that colder climates lead to higher d values? I could only find a positive relationship between SST and d.
Agreed – and thanks for spotting this. We will make the appropriate correction here (removing the reference to air temperature).

Section 2.4: One question I had when reading this, was how much the methods rely on modelling, and whether they do extensive sampling of the isotopic ratios of collected precipitation. Obviously one can read the references but a sentence clarifying this in general might be interesting.
Good suggestion. In general, studies use isotope analysis to make a first guess and then support that with trajectory analysis. We will mention this in the end-of-section summary: "Many of the studies discussed in this section use both: typically making a first guess of moisture source using isotope analysis, and then supporting their hypothesis with trajectory analysis."

568-569: Is it more correct to say that Javed et al. (2023) thresholds on vorticity? (I realise this is directly dependent on wind speed though!)
No, they stratify on 300-400 hPa wind speed (see Table 1 of their paper).

580-581: I don't know if you are trying to say that these proportions of active WDs are surprisingly low, but if so the manuscript itself earlier defines active as only the top quarter!
Good point! Our choice of 25% was actually based on these studies, which we will clarify in the revision: "This leaves 25% of our WD population defined as active WDs, a relatively small number, but consistent with earlier definitions (Mohanty et al., 1998; Dimri, 2006)." So yes, perhaps a surprisingly small fraction associated with heavy precipitation, but it serves as a benchmark threshold nonetheless.

Section 3.4.1: Is it fair to say that this is particularly uncertain (compared with other topics discussed in the manuscript)? So the overall message is there is strong evidence that WDs affect fog, but in what way is as yet quite unclear?
We think this is a reasonable summary, and will add the following to the summary at the end of the section: "In summary, there is strong evidence that WDs affect the frequency of fog events over north India, but the exact nature of the relationship and its driving mechanism are as yet unclear."

596: Can you point out where Bamzai & Shukla (1999) and Liu & Yanai (2002) say this?
Good spot. We have revisited the interpretation given in Dimri's earlier review. We will use a more appropriate reference in the revised version (Biemans et al., 2019) and remove the part on delayed summer monsoon onsets.

774-775: Does the reference to Patnaik et al. (2024) relate to their mention of increased PDNC during WDs (so less pollution during WDs?)?
I think PDNC is the percentage error of their lidar compared to satellite observations but this is a good catch regardless. Their results are unclear (you could argue there's a small reduction in N_2O, but it's very small) and we will remove this reference in our revised manuscript.

809-810: Why does their box 1 not cover the high-strike-intensity area to the west?
Good question. Perhaps as this region is in Pakistan rather than India, which is the focus of their study, but such speculation is out of the scope of this review!

Figure 15: Please define the acronyms and what the signs mean in the caption.
Yes, we will do this.

901,904: Roy (2006) reports negative correlations with PDO and ENSO over India as a whole so presumably Western Himalaya is an exception to this? Is this based on their Figure 4?
Yes, that is correct. They report positive loading (and hence, in their case, negative

correlation) over most of peninsular and central India, but the sign is reversed in both the northwest and northeast, hence a positive correlation.

958: Is this based on the positive correlation between NAO and temperature during cold periods (their Figure 7b) and cold periods being linked with increased winter precipitation?
Yes, that is correct. The authors use this interpretation themselves in their own Fig 6c, where they associate cold reconstructed temperatures with strong winter precipitation and vice versa.

965: Are you suggesting that paleoclimate studies are less reliable because the historical climate is more difficult to observe?
I think the reviewer means that the paleoclimate is more difficult to observe? If so, then yes – in part. But it may also be that the relationship between the NAO and winter precipitation has changed, as we have seen for the ENSO-summer monsoon teleconnection over the last century. We can clarify this if needed, but I'm not sure it's necessary here.

1087-1088: Is the bias weak (as in small) or negative (as in winds being too weak)?
The latter. We will rewrite this more clearly in our revision: "negative wind bias".

1131: Seems odd that heavy precipitation is not mentioned in the list of hazards, although I accept you want to emphasise the less commonly considered ones.
Agreed. We will add it.

Figure 16: Should these be accessible from the website given in the caption? I was not able to find them easily.
We agree, unfortunately the IMD does not have a standard archive of their historical weather warnings to reference. Perhaps the easiest option is their official X/Twitter account, which we will also reference in revised figure caption.

1219: I would argue that the issue of how to forecast them (i.e., shorter range) is of similar importance (see also lines 1862-1866).
Agreed, we will rephrase this in the revision to "One of the most important questions on WDs is how they respond to climate change"

Figure 19: Does the horizontal axis increase into the future or into the past?
This is a paleoclimate figure, so larger numbers indicate deeper into the past, as is typical in that discipline. We will add this to the caption for clarity.

1329: Don't these studies look at somewhat later periods than 3500 to 1500 years ago?
Our original statement was not well phrased, "this period" refers to the late Holocene, not specifically 3500-1500 years ago. We will correct this. However, the

cited studies here do span the late Holocene: Kotlia et al (2012) covers the last 400 years; Sanwal et al (2013) the last 1800; and Kotlia et al (2015) the last 4000.

1383: Wasn't the link to global warming in Munz et al. (2017) with the weakening IWM?
Yes, that is one of the key results of that paper. However, in the final paragraph of their results section, they discuss the weakened teleconnection and hypothesise that arises due to a GHG-driven increase in the strength of local circulation.

1396-1397: What makes these two different from the other studies in blue in Figure 20?
They were published prior to (and thus included in) the last WD review, so we do not cover them in as much detail as the other studies. However, for completeness, they are included in Fig 20.

1416: "Earlier studies have suggested a decline in WD frequency": is this based on the two black minus signs (and no black plus signs) amongst the blue studies from Figure 20?
Not quite, this is based on the trends from (blue) studies from 2015 or earlier (two grey and one black minus; one grey plus). We will clarify this in the revision.

Figure 20: Presumably the different shades of blue/red/green are just to differentiate the studies and don't have any other meaning?
Correct. We will update the caption to clarify this: "Different shades of blue, red, and green are used only to differentiate between studies."

1494-1495: Looks like something has gone wrong with the text here so that the meaning is not clear.
Thank you – looks like some text got deleted here. We will revise this to: "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. This was highlighted by Baudouin et al (2020), who found a regional minimum in winter precipitation between 1995 and 2010."

Figure 22: What do the grey contours (that are not very clearly visible) represent?
These have the values as the filled contours, plus an extra set for zero. We agree this is quite messy and will remove them in our revision.

And can one tell from these panels where the gauges are for each dataset?
Unfortunately not. Authors of the IMD dataset do not make the gauge data location available (except through figures in their paper). The APHRODITE authors similarly only make gauge location available through their paper figures, although they do release a gridded gauge density product. Unfortunately, density is not easily included in this figure.

1519-1520: Does this mean that we know which of Pai et al. (2013) and Chauhan et al. (2022) provide the correct interpretation?
The disagreement is between Nageswararao et al (2016) and Chauhan et al (2022). Pai et al (2013) is the dataset both used. Having re-read both studies, we realise they largely agree and that the trends vary by region. We will therefore rewrite this section to improve clarity and better bring out the role of interdecadal variability:

"The role of decadal-scale variability is most clearly highlighted by opposing results of long- and short-term studies. Using gridded gauge data from 1901 until present, both Nageswararao et al. (2016) and Chauhan et al. (2022) found generally positive trends in winter precipitation over north India. However, as in our Fig. 22, those studies that have measured their trends over comparatively short periods (~40 years) (Shekhar et al., 2010; Zaz et al., 2019; Ullah et al., 2022; Abbas et al., 2023; Safdar et al., 2023) instead report a significant decline in winter precipitation across north India and Pakistan. These studies are therefore likely to be detecting a mode of interdecadal variability. Long term studies of aridity during the rabi season (i.e. the winter months) have also indicated a trend towards wetter conditions over northern Pakistan in the regions typically affected by WDs (Ahmed et al., 2018, 2019), although this too appears to be subject to significant interdecadal variability (Ullah et al., 2022).

"Those studies reporting declining trends in winter precipitation typically invoke declining WD frequency and shifts in subtropical jet position as the cause, as did Gunturu and Kumar (2021), who argued that a recent decline in WDs has been responsible for reduced cloud cover and increased fog over the recent decades."

1541,1556: The formatting implied by the bold text is not clear here.
Yes, these were supposed to be new subheadings but EGU journals cap to three levels. We will remove these two bold subheadings.

1692-1693: Meher & Das (2022): is this based on their Figure 5 (standard deviation)? Are you arguing that an increased standard deviation implies an increased mean?
Yes, although we believe that this is a reasonable assumption as precipitation tends to follow a gamma distribution, we will clarify this in the revised text thus: "Among these, Midhuna and Dimri (2022) and Meher and Das (2022) also reported changing seasonality, finding a relatively larger increase in mean precipitation during the late winter (February) and spring (March and April) respectively, and thus providing further evidence for a lengthening of the active WD season due to climate change."

Figure 23: It might be helpful to move this forward a bit, nearer to where it is first referred to in the text.
We agree, but at this stage the location of figures is mostly set by LaTeX. We will encourage it forward, but ultimately this can be fixed by typesetters during production.

Figure 24: Which study are the solid coloured lines from?
There is a mistake in the caption – thanks for pointing it out. We will fix this (Hunt 2019b – coloured lines for CMIP5 RCPs and black line for CMIP5 historical).

1833: Does this imply that extreme WD precipitation rarely occurs in the core WD season? Or could this interaction occur over a timescale of a few months?
No, such events are comparatively rare (and interactions with the monsoon occur more-or-less simultaneously). Note the original text reads “rainfall”, not “precipitation”.

I would also like to make the following typographical recommendations/comments.

103: I would suggest changing "avalanches" --> "and avalanches" to improve comprehension of the sentence here.
Agreed, we will make this change.

150: "rather" --> "rather than"
Agreed, we will make this change.

251: The word "weather" appears twice: I think this is a mistake?
Agreed, we will make this change.

336: Could change "greater" --> "higher" just to make absolutely clear that you don't mean higher pressures (and thus lower altitudes).
Agreed, we will make this change.

432: One of the "delta"s has not been rendered correctly.
Both seem to be ok in the PDF version of the manuscript given to reviewers. Perhaps the issue is that we write 8δ, which we now change to 8·δ for clarity.

483: "occur" --> "occurring".
Thank you, we will fix this.

1005: Please define IWM here.
This shouldn’t be here – we will remove this.

1333: "differential": do you mean "different"?
Our intention with “differential” here was to say that the studies in question compared speleothems in different locations. But we agree this is confusing and will remove it.

1345: Please define LIA here

This is the “Little Ice Age”, which we will explain in the revision.

1451: "if" --> "of"
Thanks, will fix this.

1783-1784: MJO is listed twice.
Thanks, will fix this.