We reiterate our sincere thanks to all reviewers, especially the two that have followed up below. Our responses are again given point-by-point in red, with material changes to the manuscript denoted in blue.

Response to reviewer 2 (report 3)

I would like to congratulate the authors on their careful and comprehensive revisions. My previous concerns were addressed in a suitable manner and I am convinced that the review article will be of great interest to the WCD readership. Before publication I still have some minor remarks that should be addressed:

l. 130: As a midlatitude dynamicist, I find it difficult to separate between dynamics of extratropical cyclones and upper-tropospheric troughts. In my view troughs and extratropical cyclones are intricately linked. So, does the statement mean that the dynamics of WDs share the same distribution of processes as those associated with extratropical cyclones?
Yes, this is correct – while WDs do share some features with extratropical cyclones, they are often missing others (e.g. clear frontal zones). As such, we argue that they sit on the continuum between upper-level troughs (e.g. Rossby waves) and ETCs. Either way, what makes WDs unique is their environment, as we state in L124-5: "Their unique proximity to both mountains and a maritime moisture source, however, leads to a unique array of impacts that are not found in other regions."

l. 1041: It would be beneficial to include a suitable reference to where the concept of piecewise PV inversion is explained.
We agree and have added a reference to Davis (1992) and Bracegirdle and Grey (2009) in L964, where we first discuss PV inversion.

l. 1415: Please clarify that the Keller et al. 2019 paper deals with tropical cyclones undergoing extratropical transition but not with extratropical cyclones per-se.
We assume the reviewer means the Keller et al. (2019) reference on L1277 (since it doesn't appear elsewhere). We have made the suggested change.

Response to reviewer 3 (report 4)

I would like to thank the authors for their comprehensive response to my comments and their thorough review of the manuscript based on the comments of all the reviewers. There were two minor points to make from the authors' responses:

Line 88: this is now "an eastward".
Thank you, we have made this correction.

Lines 123—124: "Key characteristics of the WD, along with its environment, are summarised in Fig. 3" – would it read better if this sentence was kept to being the last one of the subsection (i.e., so that the two new sentences are inserted before it)?
We agree and have made this change.

I feel that the paper is almost ready for progressing to typesetting from my point of view, but I would first like to follow up some more of the citations. Ideally, when reviewing a regular article, a reviewer will have a good general overview of the relevant literature themselves and will only need to follow up a relatively small number of citations. However, I think for such a long and wide-ranging review article such as this one it is not feasible to find reviewers who would not need to follow up a huge number of citations, making the exercise impossible within the normal constraints of performing the role in the margins of a regular scientific job. Therefore, in my initial review I took a statistical approach to following up the citations, by checking in detail roughly 8% of them (chosen as far as possible at random); this is of course in addition to reading through the manuscript and having in mind my general knowledge of the literature (which of course varies in its extent for the different parts of the paper!). My thinking was that, if all reviewers take the same approach, and no errors/discrepancies are found (or only a very small number are found and corrected), then one can be reasonably confident that the total number of discrepancies is very small. Five such discrepancies were found and corrected just from my check of 8%; it is of course true that they were all fairly minor and do not change the overall conclusions of the paper so I don't think this is a major concern. However, since the review article is intended to act as a reference, synthesising the available literature, it is important to ensure that it provides as accurate a representation of previous work as possible. I would therefore suggest that the authors go through the citations carefully and ensure that there are no discrepancies as far as they can; additionally, I have repeated my "statistical" exercise for this second review and followed up 10% of the citations (spread evenly throughout the paper). Most of these I can see are correct, and for the remaining ones it would be useful if the authors could address my concerns listed here: it may well be that in most cases I have misinterpreted or not found the relevant statement/plot in the paper.
We thank the reviewer again for their attention to detail, which has certainly made the manuscript more robust. As we hopefully demonstrate below, the discrepancies identified in this round only uncover one material error and two overconfident syntheses.

Line 231: Is the interpretation that some of the extratropical cyclones in Wernli & Schwierz (2006) fulfil the same criteria used to define WDs but do not reach as far south/east? Is this something demonstrated in the article or something you have followed up with the data set?
Wernli and Schwierz (2006) used closed contours of MSLP to track extratropical cyclones. This does capture some WDs, but only those with a sufficiently pronounced surface low (which is not all systems, as we state in L121: "WDs [are] usually associated with a weaker surface low"). That WDs, as a distinct phenomenon, are present in the W&S dataset is visible in their Figure 4.

Line 485: I could not find where D-excess values explicitly in the Arabian Sea were referred to by Jeelani & Deshpande (2017). Is this based on interpreting D-excess values from some of the regions they mention as having a source in the Arabian Sea?
Yes, that is correct – the authors directly attribute high D-excess in moisture sources to their Arabian Sea origin.

Line 592: Where do Riley et al. (2021) mention feeble/weak WDs in particular?
Our sentence "Although feeble WDs are only associated with light precipitation, they are sufficiently frequent that they comprise a large fraction of the total seasonal precipitation (Riley et al, 2021)" is deduced from their Fig 5, where they show that only about 40% of all WD precipitation arises from 90% percentile precipitation events.

Line 660: Don't Thayyen et al. (2013) investigate an August flooding case (rather than pre-monsoon)?
This is indeed an error. This should be Thayyen et al. (2010), which we have now corrected.

Line 834: Hingmire et al. (2022) look at future change; where do they say that the recent increase in fog is due to increased aerosol loading and urban expansion?
We made this statement based on the following sentences in Hingmire et al (2022): "Figures 2 and 3 show the time series of daily fog fractions... for the study period 1981–2018 using the station observations.... One can clearly see fog fractions are predominantly below 0.5 during the pre-1997 period compared to post-1997 period, which is referred by Syed et al. (2012) and Kutty et al. (2020) as a regime shift in fog variability in the IGP region (Fig. 2). The causes for the regime shift in fog variability are still not clearly understood, however these studies rest their conjectures on the increasing anthropogenic aerosols as well as other regional and global changes in environmental parameters due to global warming." We have modified our statement slightly to reflect this uncertainty and the new Kutty et al (2020) reference: "Most studies agree that this is probably primarily due to increased aerosol loading and urban expansion (Kutty et al., 2020; Smith et al., 2022; Hingmire et al., 2022; Verma et al., 2022)."

Lines 1127-9: Patil & Kumar (2016) only show maps for the best model don't they, so how can you describe the biases in this detail for all experiments? And I would say that their Experiment 5 led to some fairly substantial improvements in the precipitation RMSEs (their Table 4), but overall "They found only a low sensitivity to the choice of microphysics scheme." is probably a fair statement to make!
Our original statement was made on the grounds that their best-performing experiment demonstrated these biases, and the RMSEs of precipitation and circulation were so similar with the other experiments that they most probably suffered from the same bias. However, we accept that this was overly confident and so we've revised our text slightly to read: "Even their best-performing experiment showed a southwesterly lower-tropospheric wind bias over the Western Himalaya, leading to significant biases in the location of heavy precipitation."

Lines 1220-1: Das et al. (2003) do recommend using 10 km spacing to improve forecasts but I couldn't find where they explicitly demonstrate that the biases can be significantly reduced by dynamical downscaling.
Again, we agree our statement was overly confident – it was based on a combination of them running a downscaling experiment (10-km mesoscale model forced at the boundaries by an 80-km global model) and then subsequently recommending use of the 10-km model. We have adjusted our phrasing to reflect the reviewer's comment: "Das et al. (2003) showed that such forecasts could be improved by dynamical downscaling – in their case using NCMRWF operational forecasts to drive a 10 km nested model."

Lines 1363-6: It seems strange that the same authors would make contradicting statements in different works (I wasn't able to access the book reference).
We agree this is surprising, but it is indeed the case.
Phartiyal et al. (2022): "The westerly [i.e., WDs] dominates in the beginning of Holocene".
Phartiyal and Nag (2022): "Recurrence of ISM dominance is seen from ~12 ka to Early Holocene." and "Early Holocene Khalling Glacial Stage suggests strong ISM [over Ladakh] driven by global insolation maxima"

Line 1426: Where does Lone et al. (2022b) state that it was warm and dry for these particular years (maybe I got mixed up converting from their "years since the present" value to CE/BCE!)?

We derive these claims from their Figure 3, which shows substantial decreases in ice accumulation (rightmost column) during these periods (~2.5kya and ~1.5-0.5 kya) as well as a marked reduction in WD activity, especially in the latter.

Lines 1472-3: Where do Singh et al. (2015) mention the elevation dependence?
They do not, explicitly, but this inference can be made by comparing their Table 2 (containing station locations and elevations) and Table 5 (containing trends in daily snowfall, precipitation days, etc). There is a negative correlation between daily snowfall trend and elevation.