Review on “Western disturbances and climate variability: a review of recent developments” by Kieran M. R. Hunt et al. 2024

Thank you for giving me the opportunity to assess this comprehensive and well-written review article about western disturbances. I focused on the aspects related to moisture transport and isotope studies, which I found very well-summarized and inspiringly written. I have no problem with length since the well-thought-through structure helps the reader to orient efficiently. I have only few minor comments, mainly small questions on some phrasings.

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.

Abstract:

1) L. 7: “Recent studies…” -> what is the time period covered in this review?
Approximately ten years (i.e. since Dimri et al, 2015). We will add this in the revised abstract.

2) L. 8: “novel analysis techniques: mention automated tracking capabilities explicitly? Since this is mentioned in Section 2 as one of the key changes since the last review on the topic in 2015.
Thanks, this a good idea and we will include this in the revised abstract.

Introduction:

3) Fig. 1: add a black contour for topography? it could help to link to other Figs (such as Fig. 2).
Thank you for the suggestion. Given that this is already a very complicated figure, orography is already displayed using filled coloured contours, and there are already two sets of lines (rivers and national borders), we don’t think this would improve its clarity. However, following a suggestion from reviewer 2, we will add state borders to Fig 2.

4) L. 87: “a westward moving synoptic-scale trough” -> eastward-moving?
Correct – thanks for spotting this typo. We will correct this to eastward-moving in our revised manuscript.

5) L. 136: “inexpensive” sounds a bit inaccurate to me, the simulations are just getting comparably less expensive but high-resolution model simulations are still very expensive in terms of computational costs.
We agree and will rephrase this accordingly: “Firstly, recent studies are making increased use of high-resolution models, which are becoming cheaper to run,

both for regional climate modelling and numerical weather prediction."

6) L. 150: "rather **than**"
Agreed – we will fix this.

7) L. 159: "observed responses **in** the instrumental record". If it's really "observed responses to the instrumental record" that the authors mean, I don't understand the sentence.
The reviewer's suggestion is correct. We will fix this typo.

Section 2:

8) In my opinion Section 9.1 would be better placed in this section or after Section 3.
We agree, and has been requested by other reviewers. In our revised manuscript, we will move Sec 9.1 to a new Sec 3.6.

9) L. 179: put references in parentheses
Thanks for spotting this, we will fix these.

10) L. 179-182: so combining early reanalyses with WD track data has been done already before 2015, right? Maybe put this sentence before the important remark of the turning point around 2015 with the start of automated tracking algorithms to keep the story chronological.
Our phrasing was a bit misleading here. Mohanty et al (1998, 1999) used an Eulerian approach and showed simply that a passing WD was well represented. The first automated WD tracks didn't appear until Cannon et al (2015). We will clarify this in the revision.

11) L. 195: rephrase the first sentence: yes, of course, detection depends on detection but can you say more? Detection depends on the characteristics of interest and may therefore vary among algorithms?
Yes, we will add this suggested clarification.

12) L. 198: which characteristics are meant here? I would be careful when using characteristics because you seem to differentiate between characteristics i.e. properties of WDs in terms of circulation vs. impacts, i.e. surface weather-related hazards.
Thank you for this suggestion. The original sentence was: "Before 2015, Dimri (2013) used bandpass-filtered precipitation and outgoing longwave radiation (OLR) to build a composite analysis that first defined the characteristics and atmospheric circulation of a mean WD that lead to precipitation." We will revise this to: "Before 2015, Dimri (2013) used bandpass-filtered precipitation and outgoing longwave radiation (OLR) to build a composite analysis that first showed WDs leading to heavy precipitation were associated with strong southwesterly

moisture flux and deep convection."

13) L. 252: what is a weather distinct weather regime?
This was a typo – we will correct this to read "distinct weather regimes".

14) Section 2.2.: I like the bottom-up vs. top-down approach and my reading would benefit from a short introduction of these two approaches and what is meant by it at the beginning of the section. Is it event-based case studies (bottom up) vs. climatological composite analysis using tracking algorithms (top-down)?
Thank you. We will add the following text at the beginning of the section: "These can be broadly categorised into top-down and bottom-up approaches. Bottom-up approaches include case studies and Eulerian composites, often starting with the impacts of WDs and then working `upwards' to quantify the characteristics that drive these impacts. Top-down approaches use a prescribed WD characteristic (e.g., vorticity), often combined with Lagrangian compositing, then working `downwards' to quantify surface impacts."

15) Are WDs included in existing global climatologies of extratropical cyclones and cyclone-related features (i.e. WCBs)? E.g. in Wernli and Schwierz 2006 or Madonna et al. 2014.
Thank you for this interesting question. WDs do appear in these catalogues, though to a lesser extent in the W&S climatology, as they use SLP as their detection metric, and many WDs have very weak surface pressure signatures. We will include this near the beginning of our revised section 2.2: "Tracking has been done successfully for, e.g. extratropical cyclones (Dacre et al., 2012) and monsoon depressions (Hurley and Boos, 2015). In fact, depending on the detection criteria used, WDs appear in global climatologies of extratropical cyclone tracks (Wernli and Schwierz, 2006) and their features (e.g., warm conveyor belts Madonna et al., 2014). However, only recently have authors started to track WDs in reanalysis data."

16) L. 267: IMD has not been introduced as an abbreviation yet
Thank you – we will add that here.

17) L. 275: that seems also by design of most detection schemes since WDs are identified as eastward travelling, resp. the (probably very rare) westward travelling WDs are ignored?
Yes, this is a good point. While it is true that eastward movement is often specified as a tracking criterion, we have found that the final output is very insensitive to this choice. This is because flow configurations that advect upper- and mid-tropospheric vortices southward or westward in this region are extremely rare. We will clarify this in our revision: "All WDs originate from regions to the west of the Western Himalaya, or occasionally spin up *in situ*. This occurs largely because most WDs are advected along the subtropical jet, but also

because the flow configurations to advect systems westward or southward into the region virtually never occur."

18) L. 276: "also because any system propagating eastward…" -> you mean westward here, right?
Yes indeed – thank you for spotting this.

19) L. 425: why "above"?
This meant "in the text above", but we appreciate it is confusing and will remove it.

20) L. 427: by compositing you mean an Eulerian analysis of the typical circulation associated with WDs and their environment? I think this ought to be clarified. Moisture sources based on trajectory-based diagnostics can also be composited using on a series of precipitation events or WD events.
Yes, this is a good point – we indeed mean Eulerian and will clarify this.

21) L. 430: cite Dansgaard 1964 when defining the deuterium excess
Thank you for the suggestion, we will add this reference.

22) L. 432: the delta values are deviations of the mentioned ratio from a commonly agreed-upon standards representing ocean water. How about writing "where d2H is derived from the ratio of deuteriated water to the most abundant H216"? Also delta18O should be **\delta**.
Thank you, we will make these changes.

23) L. 432: both expressed as a deviation of the isotope ratio from a standard reference representing the isotope ratio of the mean ocean water.
Thank you, we will make this correction.

24) L. 425: missing space before reference
Thanks, will fix.

25) L. 435: this is not entirely correct: see Thurnherr et al. 2020 for a study of shipbased measurements of dexcess in oceanic regions with different SSTs. I think for the regional setting in this paper, one important point that can be made is that the deuterium excess shows different signatures for water vapour that has undergone continental recycling vs. originates from oceanic source regions. And, furthermore, isotope signals can help partitioning land-derived sources into soil evaporation and plant transpiration (see e.g. Aemisegger et al. 2014). Rather than a reliable measure of the moisture source location, isotopes are a tracer of moisture source conditions (i.e. processes that characterise the source).
Thank you for this information. We will revise this statement, hopefully capturing the reviewer's advice as intended: "D-excess shows different signatures for

different moisture sources – ocean evaporation, soil evaporation, and plant transpiration (Aemisegger et al., 2014). D-excess also tends to be higher in atmospheric water vapour that has evaporated from surfaces in less humid climates, and thus precipitation arising from moisture originating from different basins can have different D-excess values (Pfahl and Sodemann, 2014), but it can be hard to disentangle this signal from other drivers (Thurnherr et al., 2020). For example, the Mediterranean has D-excess values of around 22‰, much higher than the global average of 10‰ (Gat and Carmi, 1970; Natali et al., 2022) and higher than the Arabian Sea (Jeelani et al., 2017; Jeelani and Deshpande, 2017)."

26) L. 455: majority ->major?
Thank you for the suggestion. We do mean "majority" here, as it can be used as an adjective in this way.

27) L. 462: Here maybe a short statement on trajectory-based moisture source detection algorithms could be made. I.e. different techniques exist including Eulerian and Lagrangian approaches with each having their own specific limitations.
Thank you for the suggestion. Note that the trajectory-based approaches discussed here are Lagrangian – it is the moisture flux composites that are Eulerian. We will clarify this in the revision.
Slightly later in the section, we discuss the relative advantages and disadvantages of each technique: "Both isotope and trajectory methods are useful, but each have shortcomings that mean it is better to draw on results from both methods where possible. Trajectory methods give more precise results for moisture sources, and along-trajectory statistics like parcel humidity can be computed. However, large uncertainties can arise from the representation of orographic and boundary layer processes, both of which are crucial ingredients for WD precipitation. Indeed, the evaporative processes that increase parcel humidity are subgrid processes that are not necessarily well simulated in reanalyses. Further to this, trajectory calculations can be computationally expensive as large ensembles are required to reduce uncertainty. Isotope methods can therefore provide more accurate estimates of moisture partitioning, since they do not depend on small-scale processes being adequately represented by a reanalysis model. However, long time series are required to ensure a representative contribution from all moisture sources. Results from studies that depend on a single year of data, e.g. Lone et al. (2020) and Dar et al. (2021), must therefore be taken cautiously. Further work is needed with isotopic methods to better distinguish between Mediterranean and local recycling sources, both of which are associated with high D-excess (>20‰). Composite moisture flux analysis is more robust to orographic and subgrid processes, but by definition does not describe the entire distribution of possible sources, as back-trajectories can."

28) L. 490: calling for Eulerian moisture tracking methods (with numerical tracers for different sources) to be used in future studies?
Yes, we will add this in our revision: "Indeed, the evaporative processes that increase parcel humidity are subgrid processes that are not necessarily well simulated in reanalyses, and perhaps call for Eulerian moisture tracking methods with numerical tracers for different sources to be used in future studies."

Section 7

29) L. 1451: **of** the subtropical jet
Thank you, will fix.

30) L. 1453: and corroborates
Thank you, will fix.

31) L. 1485: misspelling of precipitation
Thank you, we will fix this and the other three(!) instances.

32) L. 1502: remove one that
Thank you, will fix.

33) L. 1540: **E**levation-dependent warming
Thank you for spotting this. Following comments from an earlier reviewer we are going to remove these paragraph headings.

Section 8:

34) L. 1750: "… each of the moisture sources…": why "each"? Does it imply that they are a priori clear? I would remove "each".
OK, we will remove this.

35) L. 1751: "… it is known **that**…"
Thanks, will fix.

36) L. 1752: "Do different ratios… -> source ratios?
Yes, we will add this.

37) L. 1756: is latitude really a WD impact? Or a WD property?
This is a good point, we will amend this in the revision: "Should we base a classification on WD impacts (e.g., precipitation), characteristics (e.g., latitude), or dynamics (e.g., upper-level vorticity or wind speed)?".

38) L. 1814: higher resolution than what? -> high resolution
Thanks, good point – we will fix this.

Summary:

39) I think the concluding section should be more than just a summary. It should put the review into the current context.
Thank you for the suggestion. As suggested by you and other reviewers, we will reframe the conclusions – including a table (below) that lists key statements and the confidence espoused in them by the literature.

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.

And with this in mind, it reads a bit strange to come back to the summary after the future research questions and challenges. One could imagine having one big Conclusion and summary section, which includes Section 8 about “Future research questions and challenges”.
We will adopt this suggestion, moving the summary to Sec 8.1 and the future research questions to Sec 8.2 (part of an overarching Sec 8, “Conclusions”).

40) Section 9.1 is out of place in my opinion. This is a sort of glossary remark or definition that should probably be placed much earlier in the paper. After the summary it comes very much as a surprise to me.
Agreed, and this has been suggested by other reviewers too. In the revised version, we will move this to the end of section 3.

41) I must emphasise that I really like Section 8, it is very inspiring, and I would have liked to finish reading the paper in this opening-view.
Thank you!

42) L. 1876: repetition of primarily
Thanks, although both were intended this does stick out. We will replace the former with “mostly”.