Weather Clim. Dynam. Discuss., author comment AC1
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Reply on RC1

Jean-Philippe Baudouin et al.

Author comment on "Synoptic processes of winter precipitation in the Upper Indus Basin" by Jean-Philippe Baudouin et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-45-AC1, 2021

- Comment: "I strongly think 'cross-barrier' term to be replaced with orographic interaction in abstract and also later if there in manuscript too where ever it is meant for that purpose."

Answer: We agree that precipitation are triggered by "orographic interaction", however, we found this expression too generic as it includes all type of interaction between a mountain and the atmospheric flow. Here, we are specifically talking about the forced uplift of a moisture flow directed towards a mountain range, hence "cross-barrier moisture flow". In the text "cross-barrier" is always an adjective associated with "wind", "flow" or "moisture transport", and we don't see how " orographic interaction" would have the same meaning. In fact, "cross-barrier flow" is a relatively common expression in the literature on mountain meteorology (e.g. Mountain Meteorology: Fundamentals and Applications, Whiteman 2000, Mountain Weather Research and Forecasting, Bradley et al. 2021)

Changes:
l1. "is triggered by cross-barrier moisture transport" ☐ is triggered ***by orographic interaction and the forced uplift of a*** cross-barrier moisture ***flow***.
l. 17 "precipitation in the UIB is triggered by the forced up-lift of a moisture transport perpendicular to the mountain ranges" ☐ ***most of the*** precipitation in the UIB is triggered ***by orographic interaction, and more specifically***, by the forced up-lift of a moisture transport perpendicular to the mountain ranges.

- Comment: "Now the big question: all winter, if DJF, precipitation is not always by WDs. There is precipitation during to non-WDs days as well. In addition, there are WDs but at times they all don't precipitated. Authors are advised to distinctly make this clear."

Answer: You are right, we should have tackled this question in the conclusion. we also made minor changes to the introduction. However, "WD-days" is very dependent on the definition of a WD: e.g. location of maximum of vorticity, and associated threshold. Without these strict definitions, we showed here is that 1) the gradient over the IUB is more important than the location of the WD center, 2) the thermal structure/baroclinicity greatly modulates the strength of a WD. The two regressions (Fig1b and Fig5a) show an impressive good match that can only indicate that all the main processes leading to precipitation in winter have been considered. Finally, this work could be used to design better WD indices or WD definitions.

Changes:
l. 26 "Winter precipitation events in the UIB are related to the passing of extra-tropical, synoptic-scale, disturbances" ☐ Winter precipitation events in the UIB are ***in general*** related to the passing of extra-tropical, synoptic-scale, disturbances
l. 569-575: On the one hand, a PC regression demonstrated that cross-barrier wind is not only affected by how deep the upper geopotential anomaly of the WD is but also by the geopotential gradient east of the WD and by the position and intensity of the cold core of the WD. ***These characteristics could be used to explain why some WDs trigger very little precipitation.*** On the other hand, a quantile regression showed that the build up of moisture in the UIB is dependent on the history of the WD's development and track. The importance of these effects is reinforced by the approximately quadratic relationship that exists between precipitation and cross-barrier moisture transport at \SI{700}hPa and between cross-barrier wind and WD characteristics. The latter may also be related to non quasi-geostrophic and meso-scale effects, ***potentially unrelated to WDs*** (e.g. convection or frontal activity), which haven't been explored here but could constitute a topic for future investigations.

- Comment: "And I advise authors for a future study to take pressure; temp-moisture feedback mechanisms; vorticity together to determine that. As authors themselves are as well mentioning 'baroclinicity'."

Answer: we don't understand the comment and what "that" is referring to. And we do consider geopotential (better than sea level pressure in this context of heterogeneous terrain), temperature-moisture feedback and vorticity in this study.

- Comment: "And, to include impacts of gravity/Kelvin/Rossby waves too."

Answer: we do not see how gravity and Kelvin waves are relevant for precipitation in the UIB, but that may be due to a lack of knowledge on my part. As for Rossby waves, they are of utmost importance for the propagation of WDs, and particularly when investigating teleconnections.

- Comment: "Don't agree with statement 32-24: 'Despite the abundant interest, a precise and non case-specific understanding of the relationship between WD characteristics and precipitation variability is still lacking'. I would appreciate if authors reframe this sentence with mention of 'physical processes are still less understood'."

Changes:
l. 31-32: "Despite the abundant interest, a precise and non case-specific understanding of the relationship between WD characteristics and precipitation variability is still lacking." ☐ Despite the abundant interest, ***the general physical processes explaining*** the relationship between WD characteristics and precipitation variability ***are not well enough understood***.

- Comment: "In Fig. 1 I will suggest to include map of Indian subcontinent first as (a) so that most of the readers can geographically know the study region."

Changes: A new figure has been added, representing the relief of South Asia and the area of interest (Upper Indus Basin)

- Comment: "Sections 2 and 3 are perfectly fine in detailing the study region, data, methods etc. I still have reservation in using term 'cross-barrier', In fact this orographic interaction is the process which modulates the precipitation forming mechanisms."

Answer: cf. above

- Comment: "What is the rationale of mentioning it as cold core? Either provide or rename something else. Figure 3 (no lag) suggest it is warm core."

Answer: That's a key characteristic of the WDs, by opposition to the warm-core in tropical cyclone for example. The section itself is the rationale. Figure 3 clearly shows a cold core in the troposphere below the minimum of geopotential anomaly. There is indeed a warm anomaly in the lower stratosphere by adjustment, but that's not what we are interested in.

Changes:
l203: "The cold core" □ "The tropospheric cold core"

- Comment: "There is need of showing orgaphic interaction: as upslope flow along the windaord side and then sinking of the flow, ifonce cross the barrier."

Answer: To me this is just paraphrasing the sentence in the second paragraph of the introduction (l.23-24): "precipitation in the UIB is triggered by the forced up-lift of a moisture transport perpendicular to the mountain ranges". Sinking is not particularly important for the precipitation, although we do discuss in the result section. We think the reviewer might have understood cross-barrier wind as through the range, without changing altitude, which isn't physically possible.

- Comment: "Pls see work: western Disturbances: A review."

Answer: This paper is cited 13 times.