

## ***Interactive comment on* “The concurrence of Atmospheric Rivers and explosive cyclogenesis in the North Atlantic and North Pacific basins” by Jorge Eiras-Barca et al.**

### **Anonymous Referee #3**

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In this article the authors study the relationship between atmospheric rivers and explosive cyclogenesis through the objective identification of both large-scale features. The topic is interesting and within the scope of the journal. However, there are several details in the study and the interpretations of results that require attention before I can fully recommend the article for publication in ESD. These details are listed below in the specific comments followed by a list of purely technical correction.

### Specific comments

P1L14: I'm not convinced by the assertion that the occurrence of atmospheric rivers are characteristic features of baroclinic atmospheres. Are there atmospheric rivers on

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Mars?

P3L16-24: These lines are taken verbatim from Dettinger et al. (2015). Please rephrase or use quotation marks to indicate that you are using the words already published by another author. (To the editor: I didn't actively look for pieces of text taken without appropriate attribution from other sources.) On the other hand, I don't agree with the clarification on the terms warm conveyor belt (WCB), tropical moisture exports (TME) and ARs made by Dettinger et al. (2015). The main drawback in Dettinger et al.'s clarification is the lack of an explanation as how ARs are formed. In my opinion, they are the footprint of WCBs and possibly other frontal jets, which extract moisture from wetter regions (originally the tropics) to moisten drier regions. From this point of view ARs are a consequence of frontal dynamics. Dettinger et al. state that "[water] vapour is often transported to the WCB by an AR". However, a WCB is an air stream that develops as a consequence of the baroclinic development of a cyclone and frontal structure. Being an air stream, it's the WCB itself the entity that transports the moisture. The moisture would be present or absent depending on whether previous WCBs or other frontal jets transported it. Regarding TMEs, Knippertz and Wernli (2010, doi:10.1175/2009JCLI3333.1) explicitly included what was called AR in the set of TMEs. Therefore, all ARs are TMEs. The authors of the present paper seem to subscribe to this view at times: For instance, in P4L7-10, they seem to use the terms AR and TME as synonyms. All this is not to say that the authors should not be studying ARs. They provide a good definition of ARs (P3L11-13, however see also the comment to P7L20). However, if the authors are willing to enter the debate, this is a good opportunity to provide a better clarification of terms.

P5L16-17: Whether a trigger happens just prior to its effect or long before it is not something that can be guaranteed. Please rephrase.

P7L20: I'm not convinced there is a region with high IVT values extending from the Caribbean to the British Isles. This is precisely where the confusion in the interpretation of ARs arise as it is not IVT, but IWV what extends between these two locations in Fig.

1. Even the two AR-detection methods show that strong IVT is confined in its most southern and western extreme to  $30^{\circ} - 35^{\circ}\text{N}$  and about  $30^{\circ}\text{W}$ , whereas the Caribbean is a long way from this (around  $20^{\circ}\text{N}$ ,  $60^{\circ}\text{W}$ ). Please, rewrite this description.

P9L5-9: Is there really an increase? There is an increase between -36 h and -24 h but after that the lines are essentially flat. The lines in Fig. 3 must include error bars. This might reveal whether the increase is within the error or not. Also, please elaborate on the relationship of this increase and the frontal moisture convergence as it's not clear.

P9L11: What is a quasi-linear relationship? Even if it was a line, I don't see how it helps in the interpretation of results. This term also appears in P11L1.

P9L20-26: This part of the study produced the expected results, which is good, but it can go beyond that. What the composites are showing are the 80

P10L17: I don't see how your description goes beyond a statistical relationship. This is also stated in P11L16. However, to truly remove the statistical character the evolution of whole ARs would need to be studied too so that changes in cyclones can be related to changes in ARs.

Technical comments

P2L18: Delete 'that'.

P2L21: Delete 'the most'. Or how are you measuring the quality of being maritime?

P4L8: Delete 'the' in front of Ferreira et al.

P5L7: Use period instead of colon in  $0 : 75^{\circ} \times 0 : 75^{\circ}$ .

P5L9: It says '...lasting more than 24 hours'. Should it be less, i.e. '... lasting less than...'?

P5L13: I don't understand what the authors meant by 'attained'.

P5L14: Use 'rather than' instead of 'over'.

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P5L26: 'For' should not start with capital.

P5L27-P6L2: There is no need to give approximate figures. Give the actual percentages.

P6L7: Change 'Whereas' for 'While' and delete 'has'.

P6L12: Delete the second 'be'.

P6L15: Delete 'For' and start the sentence with 'Methods'.

P6L16: Delete ', they'

P6L17: '... combination of IWV and IVT estimated reanalysis datasets' is not clear. Please rewrite.

P6L23: Change 'cf.' to 'see'. Cf. indicates comparison, which is not the case here.

P7L1-2: Plural of radius is radii

P7L4: Delete 'et al.'

P7L4-5: Are Guan and Waliser (2015) also studying ERA-Interim to produce their dataset?

P7L7: Should it say 'world' rather than 'word'?

P8L24: I'm not sure what 'temporal association' means. Change to 'temporal coincidence'.

P9L1: The verb 'reduce' implies that it was once high and now it's low. Perhaps change to 'smaller'.

P9L27-28: What is a 36h wind-frame?

P10L15: Delete 'of this'.

P10L25: Delete 'reduced to'.

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P11L7: Should it say '-36 hours' rather than '-30 hours'?

P11L8: Change 'Afterword's' to 'Afterwards'.

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