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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.

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The concurrence of Atmospheric Rivers and explosive cyclogenesis in the North Atlantic and North Pacific basins Jorge Eiras-Barca, Alexandre M. Ramos, Joaquim G. Pinto, Ricardo M. Trigo, Margarida L.R. Liberato, Gonzalo Miguez-Macho

In this paper, the concurrence of atmospheric rivers and explosive cyclogenesis over the North Atlantic and North Pacific Basins is analysed using ERA-Interim reanalysis data for 1979-2011 (for the extended winter months ONDJFM). Atmospheric rivers are identified in concurrence with almost 80% of explosive deepening cyclones and only in about 40% of the cases of non-explosive cyclones. The Conclusion is offered that "The

above results strongly indicate that the presence of an AR near the developing cyclone is related with a higher probability of an explosive cyclogenesis occurrence. A detailed analysis of the time evolution of the high values of water vapour flux associated with the AR during the cyclone development phase leads us to hypothesize that this fact is a fingerprint of a physical mechanism that raises the odds of an explosive cyclogenesis occurrence and not merely a statistical relationship. ..... This insight can be potentially helpful to enhance the predictability of high impact weather associated with explosive cyclones and atmospheric rivers."

There are some minor errors like, for example: âĂć Pag.6 line 7 "Whereas for the Atlantic storm track has a clear SW-NE orientation is found, reaching values of 0.8 events"; either "has" or "is found" should be omitted; âĂć Pag.4 line 8 "...statistics changes over time (Table S1), as not all systems have the same life time."; "Table" is probably "Figure"; but overall the text is adequately written.

The object of the study is interesting and I believe the analysis can be extended to the role of localized flows of atmospheric water also in other types of adverse weather development. For example, in the analysis of the event which led to the disaster in Giampilieri (October 2009) a concentrated southerly flow of atmospheric water channelled between Sicily and Calabria was the source of intense precipitation which eventually caused the deadly landslide.

However, I have the feeling that the above mentioned conclusions are pushed too far with respect to the real achievements of the analysis reported in the paper: the simultaneous occurrence of different events is in itself no proof of a cause-effect relationship between them and, even less, of a predictive potential. My scepticism is based on personal experience in trying to identify "precursors" of relevant tropospheric developments. Specifically: âĂć in early seventies, following Ed Danielsen (1964,1968,1970), I participated in the search for correlation between tropospheric folding and alpine cyclogenesis(Nanni et al. 1975), but studies on the subject eventually revealed that although stratospheric air enhances signals (due to its very low density) it is too tenuous to exert

any real “forcing” on the troposphere below and, in fact, its analysis does not improve the prediction skill of intense Mediterranean cyclones; âĂć a few years later we went through a similar experience in studies concerning the relationship between stratospheric warming and blocking: sudden stratospheric warming eventually resulted to be the consequence and not the cause of tropospheric blocking.

In conclusion, my feeling is that the conclusive statements of the paper are too generic and I would suggest either to moderate the expectations or be more specific about the physical mechanism alluded to and the associated enhancement in the predictability of high impact weather associated with explosive cyclones.

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