

Interactive comment on “Quasi-geostrophic turbulence and generalized scale invariance, a theoretical reply” by D. Schertzer et al.

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Received and published: 21 July 2011

[acpd, online, hvmath]copernicus_{discussions}

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A reply to the supplementary comment by Yano D. Schertzer et al.

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We thank Dr. Yano for his second comment (Yano, 2011). We do not believe there is any misunderstanding on what follows:

- 1) There are many examples of asymptotic expansions that are finally valid on a domain larger than that initially thought.
- 2) QG approximation (rather than QG theory) was constructed without regard to scaling laws. We indeed emphasised the fact that it was derived in the framework of a scale analysis, not with the help of a scaling analysis.

However, many examples do not make a rule -neither Olver (1974) nor Bender and Orszag (1978) claim it-, especially when there is no empirical evidence to support it. We do not understand how to conclude from (2) that “in this very respect, it is pointless to criticise that QG is *inconsistent* with the scaling behaviour of the atmosphere” and to be satisfied by the general fact that “all the theories have limitations, so does QG”. One may note that we did not use the expression *inconsistent*, but we argued that “the

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growing amount of available atmospheric data do not support QG” (Schertzer et al., 2011c). We theoretically pointed out that the scale range of QG applicability could be much narrower than previously thought. Contrary to Yano’s claim, it is not obvious that “QG [approximation] can explain the k^{-3} -law”, because this seemingly horizontal spectral slope estimate is likely to be biased by vertical fluctuations (Lovejoy et al., 2009). Furthermore, contrary to Yano’s claim that “the contributing authors do not point out any *self-inconsistency* of the QG theory”, we have been arguing at length (Schertzer et al., 2011a,b,c) on the *inconsistency* of the linearization of the stretching vector, which is a key ingredient of QG.

We do not agree on the idea that the “importance of the power law in atmospheric physics has to be clearly demonstrated in a convincing manner for a majority of meteorologists”, because what is at stake in the current debate since its beginning is not the existence of power law statistics of atmospheric dynamics, but the precise value of power law exponents and the corresponding physical mechanisms. This is the core of the present debate and disagreement with Lindborg et al. (2010).

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