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Atmospheric Chemistry and Physics Discussions

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

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We thank Yano for his stimulating comment (Yano, 2011). We do not believe that we could add too much on the "conflict between intellectual conservatism and intellectual radicalism", which has been rather permanent in Science, or on the fact that the mainstream has been rarely on the radical side, as well as on the numerous, unfortunate, consequences for the development of Science. However, we never claimed that " the well-accepted quasi-geostrophic (QG) theory should be abandoned because it is not consistent with generalized scale invariance (GSI)", but we have been arguing for a while that the growing amount of available atmospheric data do not support QG: GSI was indeed developed (Schertzer and Lovejoy, 1985) to fit the available data at the beginning of the 1980's, which already brought into question the relevance of QG.

Namely both the vertical and horizontal shears of the horizontal wind are scaling, but with a different exponent.

More recently, we pointed out (Lovejoy et al., 2009) that the last observational defence of QG is likely to be spurious. Therefore, contrary to Yano's comment, we do not believe that "it is fair to say that the observation supports the QG theory", or that we would not be "constrained by any particular objective observational fact". However, our "theoretical reply" (Schertzer et al., 2011a), which intended not to repeat the review of observational facts as done in (Lovejoy et al., 2009), was indeed sharply focused on two theoretical questions: (i) the theoretical limitations of QG, (ii) the possibility to derive dynamical equations for an anisotropic (+H_z)-dimensional turbulence. The former was indispensable to demonstrate the irrelevance of the claims by Lindborg et al. (2010) that QG could not be brought into question, the latter was in response to the fact, as emphasized by Yano (2010), that the conservatism in favor of QG could be nourished by the lack of a dynamical alternative. These two questions are not independent, the critical analysis of the QG derivation (sections 2-3 of (Schertzer et al., 2011a)) points out a major weakness of QG: it heavily relies on a linearization of the stretching vector.

At first, this linearization clearly shows that the QG approximation cannot be relevant at smaller scales, where the Earth's rotation no longer dominates the total vorticity, whereas the linearization is performed with respect to this term (supposed to be dominant). In other words the QG approximation is not uniform in scales and there is therefore no hint in this case that the" practical range of validity of a system derived under an asymptotic expansion could be much wider than the scale adopted for the original derivation" (Yano, 2011), as it may happen when the conditions of the asymptotic expansion are not crucial.

Secondly, the fact that the approximation is not uniform in scales also prevents the scaling of this approximation to be the same as that of the original equations. To satisfy Yano's request to demonstrate it, let us emphasize that the non trivial scaling of the fractional vorticity equations (Eqs.Âă30 of (Schertzer et al., 2011a) or Eqs.11-13 of (Schertzer et al., 2011b)) is fundamentally different from that of QG turbulence. Let us also emphasize that it is not at all "dogmatic", but on the contrary anti-dogmatic to look for a theoretical framework that is closer to a full set of observations, rather than to only fit a unique possibly/presumably spurious behaviour like the estimated spectrum k^{-3} on a restricted subrange. Furthermore, this is not a "subjective matter" to theoretically discuss the relevance of a linear approximation of a nonlinear equation! It would be a pity, when facing such a problem theorists would go on to disregard data on the one hand when they do not support "a well known theory..." and on the other hand would disregard a dynamical alternative to QG, after requesting it, claiming that it is pointless "without being constrained by any particular objective observational fact". Especially, when this alternative was motivated by observations. Furthermore, it is worth noting that the relationship between data and theories is much richer than a constraint. Indeed, it seems to be as in the precise case of GSI rather a dialogue between theories and data, e.g.:"scientific reasoning is a kind of dialogue between the possible and the actual,between what might be and what is in fact the case" (Medawar, 1969).

Concerning the basic properties of the fractional vorticity equations, we already concluded (Schertzer et al., 2011a) that "there are obviously many questions to be explored, e.g.: do their solutions exist for any value of the exponent H_z, does the theoretical value $H_z = 5/9$ play a special role?". However, there is no special value of H_z that corresponds to a linearization of the stretching vector, therefore to QG. Nevertheless, behind this negative reply to Yano's question, there is a more complex question. with respect to the singular limit $H_z \to 0$, and the dimension $D \to 2$. Indeed, for $H_z = 0$ the stretching vector vanishes and the vorticity becomes motion invariant, as well all its powers (Tur and Yanovsky, 1993). The linearization of the stretching vector becomes acceptable, although not uniformly with respect to scales, and one may wonder whether QG would correspond to this approximation $H_z \to 0$. We hope that Yano will accept that we do not yet provide the answer to this question in this short reply, in spite of its interest.

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