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

J.-I. Yano

jun-ichi.yano@zmaw.de

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In the present contribution, I clearly see a conflict of intellectual conservatism and intellectual radicalism. The intellectual conservatism insists on maintaining the basic theories as much as possible even when defects are found in them as long as they are not considered to be fundamental. On the other hand, intellectual radicalism emphasizes importance of always looking for new theories based on new discoveries.

The present authors clearly stand to the side of the intellectual radicalism, in conflict with majority of conservative scientists. Probably, deepest irony here may be that the observational evidence of atmospheric fractality has been already established for decades for now. However, this fact is hardly accepted as anything important in mainstream meteorology even today. I believe, the contributing scientists should consider

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seriously why that is the case.

The authors insist that the well-accepted quasi-geostrophic (QG) theory should be abandoned because it is not consistent with generalized scale invariance (GSI) found in atmospheric fractality. Probably, this could be listed as a weakness of the QG theory, however, I personally doubt whether this constitutes the reason for totally abandoning this theory.

After carefully presenting the derivation of the QG system in Sec. 2, the authors state in the beginning of Sec. 3 that "The fact that the domain of validity of the QG approximation is *a priori* restricted to large scales does not prevent the possibility of studying the scaling behavior of its solutions on a wider scale range."

This point could not overemphasized in the context of the present debate. 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. Our experience tells that in fact, the QG theory appears to be applicable to much wider scales than it can be formally applied.

Immediately following this lead sentence, the authors add: "However, it should not be forgotten that the results obtained could be quite different from a direct scaling analysis of the original equation." This is indeed a just statement, but the point must be clearly demonstrated.

Note that the main issue of the debate here is the observed spectrum slope of k^{-3} . As the authors openly admit, the QG can explain this observed spectrum shape, thus it is fair to say that the observation supports the QG theory here.

The structure of the QG theory may not be consistent with a general framework of GSI, but this is more of a dogmatic issue than anything else. By comparing QG and GSI, judging which principle is more fundamental is a subjective matter without being constrained by any particular objective observational fact.

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Finally, I am glad to see that they have proposed an alternative dynamical description(their Eq. 30) of the atmospheric flows consistent with GSI, partially responding to my earlier request (Yano 2010). I am looking forward to see further elucidations of this system developed, presenting us what kind of different conclusions we can derived by using this new system instead of the QG theory. Whether they predict something clearly unknown from the QG theory or not would be the main question.

I would also like to know more basic properties of this new system (what kind of conservation laws they follows). The most fundamentally, it should be clarified, whether the traditional QG theory is contained as a special case obtained by taking the QG scale from this new system.

Reference:

Yano, J.-I., 2010: Interactive comment on "Why anisotropic turbulence matters: another reply" by S. Lovejoy et al. *Atmos. Chem. Phys. Discuss.*, **10**, C1625–C1629.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 11, 3301, 2011.

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