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

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A reply to the interactive comment by Anonymous Referee #2 on " Quasi-geostrophic turbulence and generalized scale invariance, a theoretical reply " by D. Schertzer et al

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We thank the referee for his detailed comments, which were very helpful to improve our manuscript independently from the fact that we often disagree with their criticisms. We indeed believe that the latter correspond to important misunderstandings of our paper. We therefore revised our text to eliminate possible sources of misunderstandings, as well as to provide a terser and more consistent paper.

1 Reply to general comments

The referee complains against a possible "indefinite sequence of comments on comments on comments", whereas, as indicated in its title, our paper (Schertzer et al., 2011a), STLT hereafter, is a "theoretical reply" to the comment by Lindborg et al. (2010), LTNCG hereafter, on the paper by Lovejoy et al. (2009a), LTSH hereafter.

This is nothing else than a usual procedure. More precisely, ACP being a discussion journal provides two distinct possibilities for a reply to a comment. We chose the peer-reviewed one because the debate with LTNCG bears on very fundamental issues and not on technical details. However, this yielded a standalone paper that has much more original results (see below) than a usual reply to a comment.

We appreciate that the referee considers that the main argument of LTSH (i.e. airplanes sample the vertical fluctuations instead of the horizontal ones) “is an interesting idea that deserved publication”. However, she/he should have mentioned that LTSH went beyond this idea and pointed out that the empirical spectral slope value is closer to 2.2 (like for buoyancy subrange) than to 3 (like for the enstrophy inertial subrange of 2D turbulence). This result gave therefore some credence to the relevance of the model of atmospheric turbulence of dimension D=23/9 that we have argued for a while. This is rather opposed to the referee’s claim that “the scaling properties, anisotropic or not, are being hypothesised rather than predicted by any physical model” unless if “physical” is understood in the restrictive framework of PDE’s (of integer order). But in this case, it is difficult to understand why the referee rather ignores our claim already stated in our abstract that the obtained “vorticity equations in a space of (fractional) dimension $D = 2 + H_z$ $(0 \leq Hz \leq 1)$ [...] seem to be an interesting dynamical alternative to the quasi-geostrophic approximation and turbulence”. Furthermore, this question of scaling and physical modelling was the focus of the interactive discussion we had with Yano ((Yano, 2011) and our reply), which seems also to have been disregarded. We therefore included some elements of this discussion in the revised manuscript to avoid any misunderstanding of this type.

We are surprised that the referee did not notice in LTNCG both claims discussed in STLT. It is indeed difficult to make more “important points” or stronger claims than to state “One does not need to go into any further technical details to see that the hypothesis of Lovejoy et al. is unreasonable” and to conclude “Thus, there are strong theoretical arguments supporting a k^{-3}-spectrum at synoptic scales”. Our paper aims

to show that the latter is not at all obvious and that therefore the hypothesis of LTSH becomes quite reasonable. This does not preclude the fact that the obtained fractional vorticity equations may have much wider consequences, as now briefly pointed out. The same comments apply to the seemingly unnoticed claim of LTNCG that “limitations [of this theory] have been relaxed in many of the modern models of atmospheric turbulence”. As a consequence, it would be inconsistent to claim on the one hand that scaling laws are “not predicted by a physical model” and on the other hand to disregard a partial differential system generating these scaling laws!

Unfortunately, the summary of STLT presented by the referee seems to go along this contradiction. For instance, the referee objects to the necessity of a detailed discussion of the quasi-geostrophic (QG) approximation (our section 2), whereas we explained in our introduction its necessity (i) to better evaluate the limitations of the quasi-geostrophic turbulence (QGT, our section 3) and (ii) to be able to derive an alternative (our section 4). It is indeed amazing that the referee did not report at all our main argument, which is the fundamental link between Sections 2 to 4, i.e. the problem of the linearization of the stretching vector. More precisely, this linearization is done to obtain QG (Section 2) and it put into question the relevance of QGT (Section 3), while the alternative is built up on the preservation of the nonlinear character of the stretching vector (Section 4). Same comments apply to the seemingly unnoticed question of going from scale analysis (to derive QG, Section 2) to scaling analysis (to build up an alternative, Section 4). As a consequence, we improve the introduction to emphasise that both questions unify our reply.

Presumably due to previous misunderstandings of our reply, the reading of section 4 by the referee seems to have been a bit random. For instance, although the question of anisotropic scaling of atmospheric dynamics goes back to the 1980’s, the corresponding early results (Schertzer and Lovejoy, 1985) were only used to illustrate the original approach developed in this section rather than to be summarised, as claimed by the referee. We indeed believe that it is the first time that the pullback transform is used to systematically study the effect of space contraction/dilations on given partial differential equations. Secondly, when applied to the vorticity equations, we obtain an original partial differential system (Eq. 31 of STLT, or Eqs 11-13 in our author comment (Schertzer et al., 2011b), hereafter STLTb). The originality of both the methodology and its application is surprisingly disregarded by the referee.

Although we insisted, especially in STLTb, upon the fact that this partial differential system is not an approximation of the vorticity equations, but generates a subset of solutions of the original equations, the referee understood that these equations are "a new form (31) of the vorticity equation in which some of the standard terms are missing". Furthermore, the referee claims that this would be particularly the case for the stretching vector, which she/he evokes for the first time, whereas we emphasised that this vector is on the contrary fully preserved, but its contributions to the material derivative of the vorticity are split in an anisotropic manner. There is therefore no reminiscence of the vorticity equation under the hydrostatic approximation and it would be pointless to discuss this issue. This simple fact precludes any relevance to the referee's claim that "as is admitted by the authors, the whole discussion is incomplete". Obviously what we admitted was quite different: after having derived in an original manner a new set of differential equations, the full discussion of their properties remains beyond the scope of STLT. In this respect, one may note that two decades separated the derivation of the QG approximation and the uncovering of the statistics of its solutions.

Contrary to the referee's claims, Eq. 31 are obviously valid for a barotropic flow, not only for a homogeneous flow, and the baroclinic case is straightforward. It was indeed mentioned right after Eq. 11: "the baroclinic vector b is of second order in a quasi-baratropic flow", we now recall that by its mathematical definition it is precisely zero for a barotropic flow. Furthermore, as pointed out in STLT, there is there is no difficulty to add the pullback of the baroclinic vector (Eq. 32) on the right hand side of Eqs.30 to obtain the corresponding baroclinic equations. Although our reply remains focused on the (barotropic) QG approximation, the corresponding baroclinic equations are included in

the revised version to avoid any misunderstanding on the extent of the methodology.

A long series of misunderstandings and contradictions are again repeated by the referee in a second summary of STLT, which shows that the referee seems to have missed most of the significance of our paper and concludes by the rejection of STLT. For the same reasons, the tentative suggestions are unfortunately not directly relevant, although we did not hesitate to indirectly use them, as already mentioned. For instance, we did our best to make as terse as possible section 2, although we disagree with the referee's claim that "the detailed discussion of the quasi-geostrophic equation, e.g. in section 3, is not sufficient to justify the long and detailed derivation in section 2", in particular because this derivation is also needed for section 4.

The referee claims to "encourage the authors to concentrate on developing their mathematical and physical ideas (e.g. those in section 4) further and more completely, rather than pursuing further the polemical style chosen in this paper", but there is no evidence that the referee paid any attention to STLTb, where the authors already improved the derivation and the physical presentation of the fractional vorticity equations. We would have also appreciated to have a few examples of the polemical style that we are supposed to have chosen. These encouragements leave us therefore very interrogative.

We hope that our detailed replies, as well as the revised version of our paper, will help the referee to better appreciate the content of our paper.

2 Replies to minor or detailed comments

Because the referee raises a question on the use of the term cascade that might not satisfy some participants to the debate, we clarify the fact that both the expressions inertial (sub-) range and cascade denote the same physical phenomenon, although with a slightly different emphasis on the possible underlaying dynamical mechanisms.

Due to the fact that STLT is focused on the fundamental question of anisotropic scaling and dynamics, we did not want to reproduce the comments by Lovejoy et al. (2009b) on the back-of-the-envelope calculation performed by Lindborg et al. (2010) to evaluate the importance of the non-horizontality of isobars.

As suggested by the referee, we clarified the meaning of small parameter values and large scales.

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