Atmos. Chem. Phys. Discuss., 11, C3431–C3435, 2011
www.atmos-chem-phys-discuss.net/11/C3431/2011/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 17 May 2011

This paper by Scherzer, Tchiguirinskaia, Lovejoy and Tuck is a comment on an earlier ACP paper by Lindorg et al (2010) (LTNCG) which in turn commented on a 2009 ACP paper by Lovejoy, Tuck, Scherzer and Hovde. (Let's call the latter LTSH.)

It seems to me that an indefinite sequence of comments on comments on comments is not very desirable. But the primary question with regard to this paper is does it enlighten the reader, are the arguments sound and are they presented in a reasonable way?

The context for all this is that LTSH suggested that the spatial scaling properties of aircraft measurements, e.g. of velocities, could be accounted for by distributions that had a single but different scaling exponents in the horizontal and in the vertical (both valid at all scales). (LTSH refer to this as 'anisotropic scaling'.) The well known change

of scaling seen in e.g. GASP data (usually presented as a change in slope of the spectrum) is accounted for by LTSH by arguing that the aircraft flight tracks are not exactly horizontal, so that what might appear as horizontal variation might actually be the effect of vertical displacments of the aircraft along its flight track (which therefore sample vertical variation). The latter effect would be expected to dominate at large horizontal scales. This is an interesting idea that deserved publication, but of course it also deserves scrutiny (especially when the scaling properties, anisotropic or not, are being hypothesised rather than predicted by any physical model).

LTNCG make two principal points. The first is that the scaling properties observed at large horizontal scales in the GASP and other aircraft data have been identified from other data sources where the variability of the height of the sampling point is not an issue and that these scaling properties have also been successfully simulated in different models. The second is that if one makes an order-of-magnitude estimate of the effect of variability in height on the spectrum (at large horizontal scales) then it is very small. The online comments on this paper from the referees essentially state that whilst the LTNCG paper is short (it is 2 pages in ACP) and does not include a complete deconstruction of the LTSH arguments it is nonetheless worthwhile in identifying points that need to be addressed further if the LTSH theory is to be accepted.

This brings us to the current paper. (Let's call it STLT.) This is 20 pages in ACPD format (so perhaps 10 in ACP format) so it is significantly more than a short comment. The claims in the abstract are (i) the paper shows that generalised scale invariance is indispensable to go beyond the limitations of quasi-geostrophic theory and (ii) that vorticity equations are derived in a space of fractional dimension less than 3 which seem to provide an interesting alternative to the quasi-geostrophic approximation and to quasi-geostrophic turbulence.

Section 2 presents with a full and detailed derivation of the quasi-geostrophic equations. The only justification for this is surely that some of this derivation is going to be re-examined/criticised in detail later in the paper. Examples of later criticism in Sec-

ACPD

11, C3431–C3435, 2011

tion 3 are: (p3308 l1-3) 'Furthermore, this type of approximation may hold over a given range of scales only if the smaller scale wave activity does not destroy the conditions of applicability of this approximation'; (p3309 l11) 'The first is that the QG approximation is fundamentally irrelevant to the mesoscale range ...'; (p3309 l25-28) 'This separation of scales between a 2-D regime and a 3-D regime, if it existed ...'; – all three of these comments are fair enough (or at least worthy of further consideration), but they don't seem to require the detail of Section 2 (just general knowledge of the QG equations which could be dealt with by suitable reference – e.g. to Vallis (2006).

In addition to mentioning the above points, Section 3 criticises LTNCG's reference to Tung and Orlando (2003). Perhaps this reference was poorly chosen, but it does not seem to me to be the most important point made in LTNCG's paper – that is that models can reproduce a kˆ{-3} spectrum (or something close to it) without the sampling subtleties mentioned above. Section 3 also states 'we can safely conclude – contrary to LTNCG – that there is neither theoretical nor model evidence in favour of two downward cascades'. My reading of LTNCG is that it does not mention cascades at all (whether or not some of the authors of LTNCG believe in cascades). So this comment seems irrelevant to LTNCG.

Section 4 applies scaling analysis and the principle of generalised scale invariance to the dynamical equations bearing in mind the distinction between vertical and horizontal. The latter implies that scaling factors for horizontal and vertical length scales may be different. Some of this section seems to be summarising arguments previously advanced in 1984 and 1985 by Schertzer and Lovejoy. The main substance of the section seems to be in deriving a new form (31) of the vorticity equation in which some of the standard terms are missing - in particular terms which normally appear in the 'stretching term' zeta . grad u on the right-hand side of the vorticity equation. In this term zeta is approximated, e.g. in the 1-component the term that would normally be the product of the 2-component of the vorticity multiplied by the 2-gradient of the 1-component of the velocity, has the 2-component of the vorticity approximated by minus the 1-gradient

ACPD

11, C3431–C3435, 2011

of the 3-component of the velocity. This is somewhat reminiscent of expressions of the vorticity equation under the hydrostatic approximation, but it is different to that and no useful discussion is given. Furthermore, as is admitted by the authors, the whole discussion is incomplete, since all that is specified are the terms in the vorticity equation that appear for a homogeneous fluid – there is no attempt to develop these into complete equations that contain the important baroclinic terms in the vorticity equation. So all that one gains from this is the idea that there might be novel approximate forms of the equation that are consistent with anisotropic scaling, but that is about it.

I do not see this paper as being of suitable standard to publish in ACP. As I have noted 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. Other material in section 3 seems to focus on rather minor points of LTNCG. The material in section 4 seems first to reproduce material on anisotropic scaling that has been set out elsewhere and then to use this material to make a very preliminary and incomplete proposal for a new approximate set of dynamical equations that allow anisotropic scaling.

The ongoing debate on how to interpret the spatial scaling properties of atmospheric observations is interesting, but I do not see this paper as contributing significantly to that debate. I 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.

Selected detailed comments:

p3302 l18: 'the present debate' – which debate? you need to explain what it is.

p3305 l9: 'infinitely' – don't see that this is needed. Surely most of us accept the principle that when a parameter is small (but non-zero) the resulting approximate equations may be a useful guide to the behaviour of the real system. (The 'may be a useful guide' of course has to be checked from time to time, as numerical or theoretical advances allow.)

ACPD

11, C3431–C3435, 2011

p3307 l19: 'restricted to large scales' – I suspect that 'restricted to small Rossby number' would be more precise – Check textbook derivations such as those in Vallis (2006).

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

ACPD

11, C3431–C3435, 2011