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Interactive Comment

Interactive comment on "The multifractal structure of satellite sea surface temperature maps can be used to obtain global maps of streamlines" *by* A. Turiel et al.

A. Turiel et al.

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We have already replied referee #1 in a previous comment; some of the questions he raised will be accounted in the next version of the paper. Here, we reply referee #2, who posted his report the day before closing the open discussion.

Reply to comments by Referee #2:

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General answer:

As a matter of fact, the paper is not written "in a hurry"; we just intended to make a brief and focused paper, not diverting the attention with complicated, technical details. We can increase the degree of detail of the arguments given inside the paper as much as you want, and in particular it is now plainly obvious to us the necessity of extending the introduction and explanation of the concept of divergence speed as a robust, adequate measure of the quality of singularity lines as streamline tracers, as far as it seems that this point is very unclear for both reviewers. In addition, a pointwise comparison of SST and singularity divergence speeds will be presented so that it will be clear to which extent singularity exponents are superior to SST as streamline tracers.

Also notice that, for the very first time in scientific literature, singularities are shown to act as a tracers. In (Turiel et al, 2005) and (Isern-Fontanet et al., 2007) this is conjectured; in (Turiel et al., 2008) singularities in very active areas are shown to correspond to level sets of altimetry-derived sea level anomalies with an error ranging between 10 and 20%. Here we have used a particular set of high-quality altimetry maps (only available for a period extending between 2002 and 2003) to show that altimetry-derived currents follow singularity lines within an average deviation of about 1 Km/day, that is, of around 1 cm/s, which is within the experimental error of these particular maps (between 1% and 10% of error), and the results hold on a global basis, including not only the most active regions but the less active ones.

The main concern of the reviewer correspond to the application of our method to model outputs in order to have a more transparent, quantitative way to verify the validity of the proposed technique. Our focus has intentionally been put on remote sensing data. Models can be more or less realistic, but they are still far from being real with respect to their ability to represent the dynamics of observed SST fields, despite of the improvements in present models. In that sense, the fact that singularity analysis works well in models is an interesting fact but does not prove anything. On the contrary, remote sensing-based evidence can be considered the reflection of a real behavior, as the data, although noisy, correspond to an observation of the real state. Thus, the reviewer's remark, although opportune, would produce a cascade of considerations that would lead the paper to an unmanageable size as we argue in the following.

First of all, in order to apply the MMF technique the fields produced by a model should verify to be multifractal in the microcanonical sense as it is verified by infrared and microwave SST images and to a less extent by altimetric fields .

Second, the singularity spectrum of each variable in a given model must be shown to correspond to the singularity spectrum derived from remote sensing data. In particular, this implies to obtain the singularity spectrum at different scales to show that it is scale invariant, and not only for the models, but also for remote sensing data, as the evidence on that presented so far just covers a rather small range of scales. Furthermore, the role of vertical velocities needs to be discussed, as we are performing 2D analysis. A dedicated study about the quality of singularities as tracers when vertical velocities become important is challenged because the difficulties to compare and/or measure such components.

Third, after exploring some model outputs, we have found that any of the studied models is multifractal (a remarkable thing, as not every signal is multifractal). We have analyzed the outputs of several different models in different configurations and regions:

- Five years of DieCast simulation in the Mediterranean (5m depth layer), provided by colleagues at IMEDEA

- One snapshot of MESO model of the whole Southern Ocean for the surface layer, provided by Robert Hallberg at GFDL (NOAA)

- Five years of Gent-Cane simulation of TIWs at the Pacific, surface layer, provided by Ragu Murtuggude (Univ of Maryland).

- Two years of Earth Simulator global simulation at 97 m depth, downloaded from the project webpage.

- Different snapshots of different configurations of OPA modelfor the Atlantic Subtropical gyre provided by colleagues at ICM.

In all these particular five instances, the singularities derived from temperature and

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salinity are very similar (although not always coincident) and trace the same structures. Additionally, the horizontal component of the velocity perfectly alineates with the singularity lines obtained from salinity and temperature. Singularity analysis seems to provide valid streamlines up to scales of 1/10 degree at least, but quality quantifiers are required. So a discussion on the merits and particularities of each model (advection schemes, forcings, etc) would be necessary, which we believe beyond the scope of the present paper. Just as an example, we intended to include in this final comment the singularity exponents obtained by MMF analysis of a temperature field taken from the Earth Simulator at 97m depth and the corresponding velocity field, which are the shallowest fields currently available from the model web site. Unfortunately the web-system does not allow this, so we ask the editor to kindly enable some way to upload this graphic file.

We have plans to present several results of all those analysis, but this is not a matter of a single paper but of at least two, apart from this one, because many things need to be discussed concerning models and the application of the Microcanonical Multifractal Formalism (MMF). Also notice that we would need to include the modelists (between six and ten persons) as authors of the present paper. This is not a major revision, this is a completely different work and arriving to that point we would prefer to retry the paper. Our present results are very conclusive, although the use of non-standard methodologies can have confused the referees and undoubtedly results must be put in value in the next version.

Answer to detailed comments:

1.- We agree, this will be reduced in the next version.

2.- A more detailed presentation of section 4 will be given in the next version.

Nowhere in the text it is said that figures 1 and 2 are quantitative; on the contrary, figure 1 is provided for illustration (as stated in the text) and figure 2 is just to provide a visual assessment (as stated in the text). Quantitative results are given in figures 3 to 5.

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It is quite evident that SSH maps cannot resolve the smallest scales at their nominal resolution, as they are generated by optimal interpolation of a set of traces. Hence, it is not surprising that singularities could reflect some small scale structures which are smoothened away in SSH. We will introduce a comment on this in the new version.

As commented before, section 4 will be expanded. Notice however that the meaning of having a value of 1-2 Km/day is commented in the paper, when we state that this is comparable with the experimental uncertainty in altimetry maps (which is obviously the minimum value that divergence speeds could take). In addition, it is explained that divergence speeds are measurements of the error. Nevertheless, all that requires a bit more of emphasis, to be introduced in the next version.

3.- As discussed above, including models (which are realistic but not real) would only complicate the discussion without providing further evidence to what is already presented (anyone could always say that the particular chosen models satisfy the observed property, but maybe others not). Thus we could add the results from the Earth Simulator model (as those shown in the figure above) but for us this would require an entire discussion on the effects of resolution, advection schemes, etc., model parameters and would introduce more questions than answers. We prefer not to include this here, and in fact the necessity of doing so for the sake of the discourse of this paper is not clear to us.

4.- Ok.

5.- We make our best, sorry for the inconveniences.

Interactive comment on Ocean Sci. Discuss., 6, 129, 2009.