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REPORT OF REFEREE

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TITLE:	Topology and geometry of the dark matter web

The manuscript contains the extensive report on several aspects related to the study of the formation and dynamics of cosmic mass distribution in terms of itss multistream character. The second author recognized in a seminal paper in 2012 that by means of tessellations it would be feasible to numerically compute and analyse the multistream structure of the cosmic mass distribution in phase-space, and that this is related directly to the character of the weblike patterns seen on large scales. The present study is part of a series of studies elaborating on this realization and seeks to study the geometrical and topological aspects of the multistream field on Megaparsec scales.

The central idea of the current manuscript is to use the (local) geometry of the multistream character of the flow field of the cosmic mass distribution to outline the spine of the cosmic web, as well as to identify the various components and objects in the cosmic web. To this end, the manuscript introduces the analysis in terms of the Hessian (eigenvalues) of the multistream field. This in itself being an original and interesting idea, it is even courageous given the fact that the multistream field consists of integer values and mathematically the idea of the Hessian of an integer value field is far from trivial.

Particularly noteworthy is the use of this geometric analysis of the multistream field to identify halos. It is an original idea, allowing the identification of haloes independent of assumptions on shape or density contrast, and provides an interesting alternative to existing halo identification schemes.

While therefore the study addresses and discusses in detail highly interesting and relevant aspects of the large scale multistream field, and as such certainly deserves publication in the Monthly Notices of the Royal Astronomical Society, the manuscript would profit from a substantial improvement in presentation.

1 Comments concerning presentation

In view of the latter I have the following three recommendations to improve the presentation:

1. The manuscript treats several issues that in my view would profit from a treatment in at least two separate papers. The title suggests the paper is about the topology and geometry of the dark matter cosmic web. The central idea of the manuscript is the use of the Hessian of the multistream field to infer geometric, structural and topological characteristics of the cosmic mass distribution. On the basis of this idea, the study extends itself to a range of subjects: the study of void region properties, percolation properties of the mulistream Hessian eigenvalues (which is the topology analysis alluded to in the title), and the identification of halos. For one paper this is a lot of diverse material. It takes away the focus of the manuscript and makes it hard to appreciate and study. I would strongly advise and encourage the authors to present these related but separate issues in two or even three different papers. It would strongly clarify the discussion and yield a clearer line of argument in the manuscript.

In this, one paper would focus on the idea of using the Hessian of the multistream field to analyze the mass distribution, possibly including the discussion on void region properties and percolation (topology). A separate paper would focus on the important but extensive aspect of halo identification.

2. The title of the manuscript suggests the paper focusses on the topology and geometry of the spatial pattern outlined by the cosmic web. Only following a detailed study of the paper it is becoming clear that it concerns the local geometry of the multistream field. Perhaps therefore the title of the manuscript would more clearly express the contents when stating "Topology and geometry of the multistream structure of the dark matter cosmic web". In connection to previous point (1), this would be a proper title for the discussion on the multistream geometry, void region properties and percolation.

3. There is a considerable level of redundancy and repetition throughout the text. To my impression the text could be shortened by removal of the redundancies, and at the same time increase its transparency.

2 Comments on scientific aspects

In addition to this recommendation for the overall arrangement of the manuscript, I would like to comment on several detailed issues in the manuscript:

1. Introduction and literature references on the cosmic web. The references in the introduction (such as pg. 2, first column) with respect to the existence and characteristics of the cosmic web are rather limited. I notice e.g. the absence of the key reference Bond et al. 1996, and later developments such as reported in e.g. reviews as van de Weygaert & Bond 2008, and publications such as Cautun et al. 2014, let alone references to state-of-the-art survey results (SDSS, 2MRS) and simulations (Millennium, Eagle, etc.). While the literature on the subject is by now extensive, a more representative body of references would be welcome, both in the introduction as well as in the main body of the manuscript.

2. While the idea of using the multistream field may be interesting, it may not be settled that the spatial structure of the multistream field is representative for the entire spatial region corresponding to the various weblike structures in the cosmic mass distribution. Partially relating to the argument above is the question of the extent of infall regions. Gravitationally they clearly belong to a region, but in general they correspond to larger regions than the multistream interior of an object. In this respect, note that even the turnaround region around a halo is vastly larger than the collapsed interior, let alone the entire gravitationally bound environment.

3. pg. 3, 2nd column, sect. 2.1: the statement that the first non-linear structures in the cosmic web have $n_{str} = 3$. That very much depends on the viewpoint, but in my view a partially contracted spherical mass peak that has not yet undergone full collapse will not have a multistream region while being highly nonlinear. In fact, we may even call a void a nonlinear object, when nonlinearity is interpreted in terms of how far the dynamics and evolution of the object deviates from linear evolution. In other words, such a statement without any further elaboration will call up confusion and needs to be better specified, justified or corrected.

4. pg. 4, 1st column, sect. 3: the statement "a single-stream flow implies that gravitationally bound structures have not yet formed" is not proper, confusing, or both. Structures may easily be gravitationally bound yet not having become nonlinear. Even primordial regions can be gravitationally bound.

5. The manuscript introduces the Hessian of the multistream field as a means to analyze its geometry. It would have been beneficial to have a more systematic argument and discussion on this. In this respect I would think in terms of eg. a related presentation of the cosmic web in the multistream field, as in figure 2, with that of the spatial outline of the eigenvalues, such as in figure 7. That is, for the same mass distribution, so that it will be more clear how the eigenvalues relate to the surrounding structure. I would certainly expect a more systematic discussion of the concept. It concerns an integer value field, and the Hessian of such a field - even when filtered - is a concept that makes a somewhat artificial impression. Perhaps a presentation in the context of a simple but illustrative situation would be helpful.

6. Taking the Hessian of the filtered - integer value - multistream field may be argued to represent a rather awkward operation. To this end, we may refer to its translation in Fourier space: it would correspond to a Fourier integral with as integrand the product of the filter function transform, the product $k_i k_j$ of the wavenumber components, and - most importantly - the Fourier transform of the integer value field n_{sc}, which will be beset by high-frequency ringing effects in Fourier space due to the sharp edges in the field. I am surprised this yields a stable outcome, as one might expect strong numerical artefacts. This surely deserves a more systematic discussion.

7. References/citations on voids. While the identity of voids is one of the main subjects of this paper, the discussion hardly addresses points of contention with other views on void structure, reflected in the absence of a number of relevant key studies on the subject is very limited. It is quite essential that these are mentioned, given the particular view forwarded in this manuscript on the identity of voids, which certainly is not a view that is generally supported. It would be by means of the contrast to these other views that the conclusions reached in the manuscript obtain more significance.

8. section 3, voids in the multistream field. Continuing the argument above, and contending that while one may certainly forward the thesis that voids are to be identified with monostream regions in the large scale flow field, there are ample reasons not to agree on this view.

[a.] There are numerous regions that are monostream but NOT voidlike. The manuscript exclusively identifies monostream regions with voids, while the large - often overdense - regions surrounding filaments and halos that consist of matter infalling towards these structures cannot, in my view, be considered as belonging to voids.

[b.] when looking at the distribution of mass or galaxies, the most straightforward definition is that they are the underdense regions in the mass distribution,

[c.] with the introduction of objective and mathematically solidly founded void identification, in particular by the watershed transform (Platen et al. 2007, Neyrinck 2008, Aragon-Calvo et al. 2011, Sutter et al. 2014, Cautun et al. 2016, and a score of others), the "observational view" has been quantified into a proper topological void definition by itself. Such regions are certainly not all percolating through space: they identify boundaries between voids with saddle points, as we do valleys with ridges centered on mountain passes. This is not only closer to what the eye perceives as a separate underdense region, it also corresponds to the dynamically based view that voids need to be identified with separate outflow - velocity divergence - regions (see eg. Aragon-Calvo & Szalay 2013, Sheth & van de Weygaert 2004).

[d.] unlike the multistream/monostream definition, such topological void identifications also take into account the accumulation of mass at a given location, ie. density, and prevents the identification of mildly dense or overdense regions surrounding voids with void interiors while they do not involve multistreaming flows.

9. Multistreaming and halo identification (section 5). The manuscript forwards the identification of halos with the regions surrounding a maximum in the multistream field. While very interesting, and providing an altnernative means of identifying halos, the text in the manuscript does not stress or emphasize sufficiently strong what the advantages are with respect to more conventional schemes.

10. Section 5 consists largely of a set of numerical results providing circumstantial evidence underpinning the identification of halos on the basis of the multistream criterion. I am missing a solid dynamical reasoning of why this would be a reasonable means of identifying halos. A concrete example for the contrary may illustrate why an argument would be in place: when describing a collapsing spherical halo (with a reasonable initial density profile), shell crossing would not happen until the final moment when the density reaches infinity at the very core. For most of its existence the mass accumulation would resemble a contracting mass concentration that would not have undergone shell crossing, while obviously identifiable as a gravitationally bound concentrated mass structure.

11. Halo identification and comparison: to understand the nature of using the multistream field towards halo identification, in comparison to the AHF and FOF halo finder, one would like to see not only a comparison of global properties such as the mass spectrum (fig. 16), but also individual halo comparisons.

[a.] it would be good to compare not only mass of the halos, but also the radii and, in particular, the concentration of the halos that were identified, as well as other relevant dynamical properties (binding energy, virial radius, etc.).

[b.] the one specific comparison of individual halos found by the multistream formalism, AHF and FOF is figure 16. The figure is quite confusing, and it would be better to present this in three different frames, each corresponding to the particles identified to belong to the halo by the different identifiers.

[c.] Figure 17 is also important for an assessment of the workings of the different halo identification procedures with respect to individual halos. It would be good to expand the size of these frames. The discussion at the end of sect. 5.3, pg. 13, would profit from a more in depth and halo-to-halo discussion (ie. examples more clearly shown).

12. When assessing the Venn diagrams in Figure 14, the overlap region between the AHF and FOF halo identifications reveal that in most of the cases the AHF halos are a subclass of the FOF identified halos: the number of AHF halos not identified by FOF only represents a minor ridge of the AHF circle. The situation is quite different for the multistream halos, both for 128^3 and $256^3 3$ halos there is a substantial fraction of multistream halos not identified as AHF or FOF. It would be good to have this more extensively commented and discussed on.

13. The discussion on the topology of the cosmic web is mainly restricted to percolation of structures. It would be good to be more explicit on this description, and at least mention other and more extensive topological characterizations of the (multiscale) mass distribution).

14. In the percolation analysis in section 6, such as presented in figure 19, the manuscript compares the percolation results of the multistream spine of the cosmic mass distribution with that of a CIC density field. This may hardly been considered a fair comparison. The CIC density averages the density over a cubic gridcell. It will therefore diminish any fine structure as well as anisotropic stucture, and in this way severely afflict the comparison with the multistream analysis. At least the analysis should seek to relate to a more refined density field measure in terms of mutiscale structure. Eg. something involving an adaptive density estimate by means of an SPH kernel or. However, most compellign would be a comparison with the density field estimates inferred from the phase-space structure itself, such as extensively discussed dby Abel et al. and presented in impressive visualizations by Kaehler et al. In a sense, the thesis forwarded is that the Hessian of the multistream field would be more sensitive than the density field. However, that should be discussed on a proper basis.

3 Comments on minor issues

A few minor issues that deserve attention are:

1. Throughout the text I often see the word "web" mentioned without the adjective "cosmic". As the word "web" is used in many different meanings, it would be appropriate to keep to the concept "cosmic web". I would like to ask the authors to take care this is changed throughout the manuscript.

2. pg. 2, 2nd column: Platen et al. 2007 is mentioned as reference that showed that DTFE is superior to CIC (and TSC). This is not entirely representative: Schaap 2007 and van de Weygaert & Schaap 2009 are the proper references for that. Platen et al. 2007 is the paper that introduced the watershed transform, in particular with respect to the identification of voids (and as such should be referenced, also in the discussion on pg. 16, 2nd column.

3. pg. 5, 1st column, sect. 3.2: the fact that Colberg et al. EVEN mention the existence of void haloes in several halo finder algorithms calls up 2 comments:

[**a.**] the text means several void finding algorithms (that was the subject of Colberg et al. 2008)

[**b.**] the remark appears to express surprise. This may be true for the void interpretation of the present study, but is certainly not strange in the context of other views of voids and void structure. In fact, there are targeted void galaxy survey campaigns (eg. Kreckel et al. 2011, 2012), and I presume the suggestion is not that these are based on a wrong interpretation of reality ?

4 Conclusion

This is a very interesting manuscript, focussing on an innovative and original development on the basis of the multistream structure of the cosmic mass distribution. I hope the comments above will be useful towards upgrading the manuscript, and I look forward to assessing the upgrade of the manuscript, hoping it may soon lead to publication in the Monthly Notices of the Royal Astronomical Society.