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Nonlinear Processes in Geophysics Discussions
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Interactive comment on "Multiscale statistical analysis of coronal solar activity" *by* D. Gamborino et al.

Anonymous Referee #1

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This study addresses the spatio-temporal dynamics of the solar corona from a interesting point of view, by decomposing coronal temperature maps over a given time interval into small sets of separable modes, similar to what had been pioneered in the 1990s in neutral fluid turbulence by N. Aubry, R. Lima, P. Holmes, G. Berkooz, and more.

However, this study looks much like a replication of [Futatani et al., Phys. Plasmas 16, 042506 (2009)] to a solar dataset, without properly taking into account the strong assumptions behind this dataset. In addition, the method is over-interpreted, thus leading to incorrect conclusions. Because there are several major issues here, I cannot recommend this manuscript for publication.

Here are some general comments

Your study heavily relies on the physically appealing concept of coronal temperature.

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Discussion paper

However, the temperature maps as provided by the method of Aschwanden et al. involve several strong assumptions. In particular, these temperature maps are assuming an isothermal plasma, which is a coarse approximation of what the true corona is; it certainly does not hold during flaring activity, and it is likely too to be incorrect too during transients, such as your heat fronts. In addition, flares cause artefacts such as pixel bleeding. Another good reason to be very careful when interpreting synoptic EUV images.

These limitations (which are mentioned in Aschwanden's article) should be considered very seriously before any physical interpretation can be given to these temperature maps. A first and obvious starting point would be to check the width of the temperature distribution, and discard all pixels for which the isothermality assumption does not hold.

Following this, I'm deeply concerned that the whole interpretation in Sections 3 to 5 remains purely speculative as long as these spurious effects are properly addressed, which may be quite challenging. Personally, I would refrain from using temperature maps at all, except for qualitative analysis, or for structures that are known to be approximately isothermal.

What is then the best observable ? Note that most studies consider log(T) rather than T because it is more convenient, and also because the distribution of T is assumed to be log-normal. Several of the properties of the SVD are optimal for datasets that have a normal distribution. For that reason, I would seriously consider working with log(T) rather than with T – assuming of course that the temperature can be used at all.

I also have major concerns regarding your interpretation of the SVD results. In line 178, you say that "there is some correlation between small scales and high frequencies". This is merely a consequence of the properties of the SVD, and has nothing to do with the physics. Whenever you diagonalise a covariance matrix (what the SVD does, in some way) whose values decay monotonically as you move away from the diagonal, then the eigenmodes (your topos and chronos) will be like Fourier modes, whose number of nodes will increase with the rank k. So, small wavenumbers will automatically be associated with small frequencies. Try to generate a surrogate dataset that has the same second order properties, and you'll get exactly the same results. For that same reason, there is no evidence whatsoever for a cascading (line 193). Incidentally, because the SVD exploits second order moments only, I would not recommend in problems in which phase coherence matters.

Later on (line 195) you introduce the scaling index gamma: this makes no sense because several of your topos and chronos don't have a clear characteristic size, or time scale. You may find values for <kappa> and <f>, but this does not prove that they make sense as they would, for example, for a wavelet basis. For example, for the rank 1 mode you capture the average background temperature, whose spatial or temporal scale is of no particular interest here, should definitely be excluded from your analysis. Several more details suggest that you're over-interpreting what the SVD is telling. I strongly recommend that you check your results carefully and test them, in particular by using surrogate data. This also applies to Figure 9, from which one cannot draw serious conclusions without knowing what the confidence intervals of the singular values and energy spectra are.

I would have many more comments, also regarding the analysis of the pdfs. Let me mention one more, in line 150: what is it you are calling a heat front ? Flares are intense events that generate various types of transients. So-called EIT waves have received considerable attention [Gallagher and Long, Space Science Reviews, 158 (2011), pp. 365–396] but they're not the only ones. Here, I strongly recommend that you put your analysis in context, and emphasize the novelty of your results in comparison to existing studies.

And now some more specific comments:

title: your title is misleading as you are not truly doing a multiscale analysis. The SVD does indeed separate different scales, but these are very loosely defined, and are in no way comparable to what truly multiresolution techniques, such as the wavelet decomposition, would give.

sun –> Sun

several citations do not show up correctly, e.g. line 24

line 17: obtained –> inferred

line 26: the POD is strictly identical to the SVD, not more general

line 27: many more studies have used the SVD, or variants thereof to investigate the spatio-temporal dynamics of the Sun. I would be good to mention some of them, and not focus only on the work by Vecchio et al.

line 33: topos and chronos are not a method, but just the names given to the spatial, resp. temporal modes obtained by applying the SVD to a spatiotemporal dataset, see [N. Aubry et al., Journal of Statistical Physics, 64 (1991), pp. 683–739]. BTW, in that context, the SVD is called biorthogonal decomposition.

line 33 the specific method: all these methods (POD, biorthogonal decomposition, SVD, EOF, PCA ...) are identical; what distinguishes them to some degree is the type of data they are applied to, or the preprocessing, but even that is not always true. So there is no point mentioning them as if they were different. Otherwise people keep on reinventing the wheel.

line 35: why is flaring activity interesting ? section 2.1 this part is clearly written, but quite mathematical, and devoid of a connection with the physics. It would help to say the your spatio-temporal wakefield gets decomposed into a finite series of separable modes of time and space, which, in addition, are orthonormal, etc.

line 88: mention at least the original work by Aubry, Lima, et al, who coined the words topos and chronos.

line 94: unfolding –> folding end of 2.1: again, what is the physical motivation behind working with A^{(r)} ?

line 119: why "requires significant emission of radiation" ?

line 120: what is the time span of your data set, and did you correct for solar rotation ? The latter point is *very* important because the properties of your SVD modes change if your spatial frame is moving.

line 131: For each wavelength there is a corresponding temperature: this is incorrect. Each wavelength is associated with a temperature distribution.

line 134: Do you mean that the filter response is associated with a DEM ?

line 135: note that there are alternate methods for inferring the temperature, such as [Guennou et al., Astrophysical Journal Supplement Series 203 (2012)], and [Dudok de Wit et al., Solar Physics, 283 (2012), pp. 31–47].

line 158: why that particular size of 32x32 ? Why not larger or smaller ? What is limiting the number of time steps ? Notice that since Nx*Nx » Nt, in your covariance matrix your ensemble average is done along the spatial dimension, and not along the more usual temporal one. This impacts your results, and should be addressed.

Fig. 4: what are there oblique stripes in all of your pictures, as if the plasma was moving sideways ? Since your spatial region is a square, it would make more sense to force its aspect ratio to 1.

line 175: why plot the absolute value ? and how should it better reveal periodicities ?

line 193: see general comments. There is no cascading whatsoever here.

line 199: the mode with rank 1 is just the average background. Usually, when analyzing a spatio-temporal wavefield that is quasi-stationary (as is the case here, as T stays around 10ˆ6 C), that first mode should be discarded since it doesn't tell us much about the dynamics. What matters is the variation on top of it.

line 208: T should also be indexed by t_i

line 210: for the reason explained just above, since you're interested by the dynamics only, you should start by subtracting the time-average from each pixel. This will affect the distribution of the singular values. The wording "energy" will then make much more sense as it truly describes the variance of the modes.

Fig. 10: please use symbols that can be read on B/W printouts.

Sections 4 and 5 : to be considered once the problem with these temperature maps has been addressed.

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