Response to Referees

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1 Referee #1

This is an interesting paper, bringing non-modal analysis to bear upon a problem of plasma turbulence. Non-modal analysis has been used very effectively in hydrodynamics. Its use in plasma physics is a relatively recent trend, and holds the promise of providing new perspectives on old problems. This paper, which includes theory as well as a strong experimental connection, is consistent with the criteria for publication in PRL. Having said that, we would like to request the authors to address the following questions:

1) After advocating the use of non-modal structures rather than linear eigenmodes for the entirety of the paper, the author use a time-scale derived from linear eigenmodes (see page 4, paragraph 2). Furthermore, they use the frequency for the n=1 mode instead of that for the n=0 mode(s), with no apparent justification (other than "nonlinear coupling). It would seem much more consistent to use the time-scale for the nonlinear instability they talk about earlier (although this would mean perhaps a loss of predictability that they are very keen on). To us, this appears to be the weakest link in the paper. Perhaps, if they present this paper as a study of the turbulence properties instead, they could give information on how the curves in Figure 3 change with the choice of the linear time- scale. It would seem that this would be much more useful to the reader in assessing the power of the non-modal analysis, rather than presenting just two choices, and claiming predictability.

Response: First we would like to thank the referee for the careful reading of the paper and the nice review. As for the first comment, we believe that this is a valid criticism. We thought a lot about this before submitting the first manuscript and tried a few different linear time scales. The one we chose worked best, but it is difficult to justify it otherwise. We agree that this was the weakest link of the paper. We also thought that the suggestion of presenting several choices of linear time scales was a good one, so this is what we've done. We've presented four different choices of linear time scale and compared the results to each other. We have also softened our language on predictive capability and presented our results as a way of understanding the turbulent growth rate through linear non-modal calculations with the ultimate goal of achieving prediction. Overall, we have modified the manuscript quite a bit to make these changes, including changing the last two figures.

2) What closure assumption is used to write equation (5)?

Response: If by closure, you mean an approximation of the nonlinearity, then we do not use any closure to write equation 5. It is an exact equation that can be derived from the model equations [1, 2]. We have also now written it differently to make it simpler and more transparent we hope.

3) On page 2, last paragraph, there is a puzzling statement: "Perhaps one of the most useful statistical properties that we can predict is the turbulent growth rate spectrum. Do they actually have a measurement of the turbulent growth rate spectrum? Since predictability is one of their prime concerns, what measurable transport property does the growth rate spectrum relate to?

Response: We acknowledge that this was too cryptic a statement. In it's place, we have inserted the following, "The procedure ultimately produces growth rate spectra that can be used to predict turbulent properties such as saturation levels and transport rates through mixing length arguments." Then, as before, we proceed to calculate the growth rate spectrum from the nonlinear simulation (shown in Figure 1a) and also calculate it from our non-modal linear procedure.

4) We think a log scale should be used for Figure 2. On the scale used in the paper, it is impossible to determine if the growth rate at short times is somewhat similar to $G_{max}(t)$ or quite different. If similar, the potential for formulating fully analytic models is more promising.

Response: We have changed Figure 2 so that we do not show $G_{max}(t)$ any longer. The growth rate of the ensemble of curves is not close to the growth rate of $G_{max}(t)$. $G_{max}(t)$ grows so quickly, and it isn't clear how one would use it in a procedure like ours because the points along it require linear evolution from very specific initial conditions, and every point comes from a different initial condition.

5) Minor point: Figure 3 is confusing simply because the important curves are not sufficiently well-delineated. For example, the turbulent growth rate spectrum should be bold (or something equivalent) to distinguish it from the others.

Response: This figure is also much different now, but we have tried to make all of the curves distinguishable by using different colors along with symbols so that they are even distinguishable in black and white.

2 Referee #2

1) I find this manuscript somewhat confusing, especially when the authors try to cast it as a non-modal analysis. For non-modal stability analysis of a linear system,

$$\frac{d\mathbf{u}}{dt} = \mathbf{A}\mathbf{u} \tag{1}$$

I would expect the prediction of the short time behavior of some norm of $\mathbf{u}$ *through the mathematical analysis of* $\mathbf{A}$ *by methods other than the standard eigenmode decomposition, e.g., by methods based on the* ϵ*-pseudospectrum and/or numerical range.*

If I understand correctly, the method presented here starts by dropping the nonlinear terms in the equations of motion of the reduced Braginskii 2-fluid model. The resulting linear system is numerically simulated up to a time τ_{nl} *at which the system is then re-randomized whatever it means. A growth rate* γ_{TR} *defined and computed using data between* $t = 0$ *and* $t = \tau_{nl}$ *then constitutes the prediction by this method. So it seems that the non-modal method described in this manuscript is basically a numerical simulation of the linearized equations of motion. While prediction of turbulent properties by numerical simulations can be useful, it probably has less impact than a theoretical prediction (which is admittedly much more difficult).*

Response: First, we would like to thank this referee for the careful reading and insights into the paper. As for the question over 'non-modal analysis', we contend that our method (especially with revisions in the new manuscript) is a form of 'non-modal analysis'. That is, we use only linear calculations to find growth rates without using normal modes (eigenvalues and eigenvectors). While most non-modal analysis in the literature seems to center around the calculation of the *epsilon*-pseudospectrum, much also includes the calculation of $||e^{\mathbf{A}t}||$ and $||e^{\mathbf{A}t}\mathbf{u}(0)||$ with various optimal $\mathbf{u}(0)$ vectors, i.e. to get the numerical range and numerical abscissa. More importantly, however, it would be helpful to have a more theoretical prediction that doesn't require simulation. In that regard, we have come across a method [3] that can replicate our numerical simulations with a simple calculation that looks much more like a typical non-modal calculation. See Eqs. 9 and 10 and Figure 2. With this new change, we believe it is very clear that what we are doing is non-modal analysis and that we are not simply using linear simulations.

2) For the procedure described above, there are several things I do not fully understand. First of all, if γ_{TR} is only computed using data for $0 < t < \tau_{nl}$, how does the re-randomization at $t = \tau_{nl}$ come into play? I am probably missing something here.

Response: We agree that this was a confusing way to word our procedure. We therefore have removed this statement. At the beginning of the manuscript we offer this description of our model: " We model the turbulent steady state as a series of processes: (1) the turbulence starts as a spatially random state, (2) linear transient growth deterministically amplifies the turbulent energy (or decrease it in wavenumber ranges where linear damping dominates), (3) nonlinear transfer sets in at a specified timescale, terminating the transient growth process and re-randomizing the turbulent state (at which point the cycle repeats)." This is not the procedure we use to do the actual calculation, but just our simplified description of the turbulent model.
As for the actual procedure, we state: "In practice, we implement this model by starting with an ensemble of random initial conditions, which we evolve linearly for a time τ_{nl}, and then take the time and ensemble averaged growth rate of these curves."
Also, we use the non-modal calculation discussed above, added more equations, and replaced Figure 2 to make our procedure more clear.

3) Secondly, the various growth rates used here are defined as

$$\gamma(m,n) = \frac{Q(m,n) + D(m,n)}{E(m,n)} \tag{2}$$

whereas the evolution of the energy for mode (m, n) is given by

$$\frac{dE(m,n)}{dt} = Q(m,n) + D(m,n) + \sum_{m',n'} T(m,m',n,n') \tag{3}$$

What is the reason of ignoring T in the definition of γ? Similarly, what is the meaning/significance of plotting $E(m,n,t)$ with nonlinearities turned off? $\sum_{m',n'} T(m,m',n,n')$ is non-zero and contributes to $\frac{dE(m,n)}{dt}$ even though $\sum_{m,m',n,n'} T(m,m',n,n') = 0$. Would it be more informative to compare γ_{TR} to $\frac{dE(m,n)}{dt}$ instead of the $\gamma_e(m,n)$ defined in the manuscript (which ignores contribution from T)?

Response: Note that we have changed our notation in the text, and added some more equations and discussions of this so that this issue should now be clear. But we will explain here anyway.
The reason why T is missing from the definition of γ is because $\frac{dE(m,n)}{dt} = Q(m,n) + D(m,n) + \sum_{m',n'} T(m,m',n,n')$ is virtually zero when the turbulence is in steady state (by definition of steady state). It may not be zero at a given time, but it is zero on average, and thus not suitable for use in defining the growth rate spectrum, which is a time-averaged property (see Figure 1a for the average and the spread in the growth rate). If we were to include T in the definition, the turbulent growth rate curves in Figure 1a would be zero with some finite spread.
More informatively, the growth rate we use is a measure, by wavenumber, of the rate at which the fluctuations take energy from the equilibrium gradients (or return energy if the growth rate is negative). This comes purely from the linear terms. The nonlinear terms transfer energy between different fluctuation 'modes', but do not affect the overall energy budget. So the picture is that certain modes or structures (those with positive γ) inject energy from the equilibrium gradients into the fluctuations through the linear terms. Those modes then non-linearly transfer energy to other modes in an overall conservative way (like a cascade). When energy is nonlinearly transferred into modes with negative growth rate, that energy is dissipated. In the case of normal linear operators, the unstable linear eigenvectors inject energy, the nonlinearities transfer that energy to stable linear eigenvectors which then dissipate the energy. For normal operators, one would expect this growth rate to be pretty similar to the fastest growing eigenmode growth rate, though it could be smaller. But it certainly cannot be larger. We have added and

modified quite a bit of text and added a reference [4], which uses this growth rate definition.
Finally, the reason we plot $E(m, n, t)$ with the nonlinearities turned off is to show that the linear terms alone cause (transient) growth of the fluctuations despite the fact that the $n = 0$ mode numbers contain no unstable eigenmodes. It illustrates that the turbulent structures inject energy into the fluctuations in the short term.

4) I also feel that the clarity of the present manuscript suffers because of the page limitation of PRL, a lot of the symbols are not defined (e.g. v_E, N_r) making it difficult for the general audience. Together with the concerns raised above, I am afraid I cannot recommend the publication of the present manuscript in the PRL.

Response: We have now defined all symbols and removed unnecessary parts of the paper and replaced them with better text. We hope the major changes have made the manuscript significantly better and easier to understand.

References

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[4] P. W. Terry, D. A. Baver, and S. Gupta. Role of stable eigenmodes in saturated local plasma turbulence. *Phys. Plasmas*, 13:022307, 2006.