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COMMENTS ON "THEORY OF DISSIPATIVE DENSITY-GRADIENT-DRIVEN TURBULENCE IN THE TOKAMAK EDGE" [PHYS. FLUIDS 28, 1419 (1985)]

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Comments on "Theory of dissipative density-gradient-driven turbulence in the tokamak edge" [Phys. Fluids 28, 1419 (1985)]

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Recently, Terry and Diamond[1] (TD) presented a detailed study of tokamak edge turbulence. Using a nonlinear fluid model, they generalized and employed the "clump" algorithm[2] to determine properties of the turbulent steady-state. The present work represents an attempt to understand the physical basis, consistency, and quantitative accuracy of that procedure (as applied specifically by TD to a *fluid* model). My conclusion is that the fundamental physics picture proposed by TD is, in its emphasis on the small scales, in fundamental disagreement with well-established facts about the small-scale behavior of turbulent fluids. A weaker statement, that[1] "the stationary turbulent state is ... a 'soup' comprised of waves and eddy-like blobs" is certainly correct, and the authors correctly emphasize, as have many others,[3] the importance of mode coupling. However, the authors' specific mathematical calculation, with its emphasis on a particular definition of clump lifetime which involves exponential divergence of adjacent trajectories, goes beyond this weak statement in a way which seems unjustified.

As TD noted, the actual Reynolds number R for laboratory experiments is $O(1)$; in such a regime, little more than order of magnitude estimates and certain parameter dependences can be trusted from an analytic theory of the variety presented by TD. Such estimates are, nevertheless, very useful, and TD do not claim quantitative results for that regime. However, they also studied asymptotic regimes in considerable detail in order that "the basic physics ... [be] illuminated." It is important to understand whether the physics picture which has emerged from that study is accurate. (My following remarks will apply directly to the high R regime; although I believe there are related difficulties with the small R regime, I will not discuss those here.)

In this and all of the earlier works[2,4,5] on clump theory, repeated reference to the "small scales" is made, and a central role is played by the calculation of the "clump" lifetime $\tau_{cl}(\vec{\rho})$ of those scales and its Fourier transform $\tau_{cl}(k)$. Here $\vec{\rho}$ is a relative position vector [(y_-, η_-) in the present case] and $\tau_{cl}(k)$ can be written $\tau_{cl}(k) = \tau_0 A(k)$, where τ_0 is an effective eddy circulation time of scales of order the production scale $L_0 \equiv k_0^{-1}$, and TD found $A(k) \doteq (2\pi/k^2)[1 - J_0(2k/k_0)]$. The authors computed this lifetime by analysis of the mean-squared exponential divergence of trajectories initially separated by a distance small compared to L_0 (equivalently, by approximate solution of the spectral balance equation in the same limit). They suggested the picture of "collective resonances ... driven by *emission* from *localized* ... blobs" whose lifetime is τ_{cl}. (Italics added.) Now there are, of course, fluctuations on all scales, and these are all coupled. In very general terms, that coupling can indeed be described in part as an emission process. However, the authors' words and the particular way in which the calculation of τ_{cl} was performed force one to conclude that in the authors' picture a clump is something very specific: a very small blob, points on whose boundaries actually diverge exponentially, which assumes a dominant, driving role in the turbulence. However, such a picture contradicts well-established facts about the physics of the small scales in turbulent cascades, as I will discuss.

As we proceed, it will be useful for the reader to keep in mind the following question: *Would the true calculation of steady-state* macroscopic *quantities such as total fluctuation levels, transport coefficients, etc. be, for practical purposes, any different if the calculation of the exponential orbit divergence were completely excised from the analysis?* I stress this question, whose answer I believe is "no" (contrary to the scenario of TD), in order to emphasize that I am objecting here primarily to the physical picture which asserts that shielded *small scale* clumps *drive* the turbulence. A related issue is whether the theory treats the dynamics of the larger scales in enough dynamical detail to be believed in any more detail than the constraints of dimensional analysis and scaling theory.[6] I am skeptical of this as well, but space precludes a detailed discussion.

First, it is worth pointing out that the x space method[1,2,5] of computing τ_{cl} provides[5] an alternative, approximate calculation of Batchelor's well-known k^{-1} convective range of a passive scalar.[7,8] Recall that the convective range arises from the quasi-steady straining, by *large-scale* ($k \lesssim k_0$) *motions*, of structures of very much smaller scale, and that in generalizations of that result to advection of scalar quantities coupled self-consistently back to the advecting velocity field (cf. the vorticity in two-dimensional fluids and the resultant enstrophy inertial range[9]) it can be verified[10] that asymptotically the small scales react negligibly on the large, energy-containing scales. Now it is clear from the calculations of TD that the role of Batchelor's passive scalar is played there by the nonadiabatic electron density δH. Given this, there can be no significant quarrel with the asymptotic wave number dependence of the result

$$\mathcal{H}(k) \equiv \langle \delta H \delta H \rangle (k) \sim \tau_{cl}(k) S \tag{1a}$$

$$\sim S/k^2 D \quad (k \gg k_0). \tag{1b}$$

[The quantity S, named the "source" by TD, has been called the "production" term in fluid mechanics[11]; that terminology avoids any confusion with external injection. S is the production rate of scalar variance (of electron density). Also, note that TD called the right-hand side of (1) the *incoherent* part of $\mathcal{H}$ and thus wrote $\tilde{\mathcal{H}}$ rather than $\mathcal{H}$ on the left-hand side of (1). This usage leads to confusion, and will be discussed below.]

In the present problem, δH is not completely passive: electron density fluctuations affect the total potential, and vice versa. The clump algorithm represents one proposed way of determining properties of the resulting self-consistent steady-state. Thus, in the method the result (1) is not used in the asymptotic convective range $k \gg k_0$, where it applies; rather, the long wavelength behavior of (1a) is taken literally and used in an essential way to determine the steady-state spectral balance. That is, the authors write [cf. Eq. (101) of TD]

$$S = \sum_{k,\omega} \Gamma(k,\omega)\, \tilde{\mathcal{H}}(k), \tag{2}$$

where Γ is a weighting factor closely related to the nonlinear growth rate. TD have determined Γ correctly, and if $\tilde{\mathcal{H}}$ is taken to be the true incoherent noise,[3] Eq. (2) is correct.

However, TD now replace $\tilde{\mathcal{H}}$ by the right-hand side of (1a). Then, upon multiplying (1a) by Γ, summing over modes to construct S, and cancelling the nonzero production rate from both sides, they are left with the result

$$1 = \sum_{k,\omega} \Gamma(k,\omega)\, \tau_{cl}(k), \tag{3}$$

which they interpret as the steady-state spectral balance.

Equation (3) is dimensionally correct. However, noting the specific appearance of τ_{cl} (as computed by TD), I must object that this procedure seems flawed. Mathematically, the method uses in an essential way the *long* wavelength extrapolation of the spectral density of *small* scale fluctuations [it is readily verified that (3) is dominated by the wave numbers $k \lesssim k_0$, and TD note that "the most significant ... emission [arises] from the large-scale blobs"], whereas that density, which has been derived solely from the theory of exponential orbit divergence or of quasi-steady straining, is valid only at asymptotically large wave numbers.[6] More importantly, it asserts physically that the small scale fluctuations *drive* the turbulence, whereas in fact they are parasitic, *driven* by the straining action of the large scales.

It would be consistent to use (1b) to determine the total production rate due to the small scales by summing (2) over just the small scales; however, that result would be small. Alternatively, the total production rate would be effectively unchanged by excising from (2) the small scales; then, however, the proper $\tilde{\mathcal{H}}$ to use in (2) would have to be determined by explicitly calculating the interaction of the production scales among themselves.

The invalid wave number extrapolation may be illustrated with a discrete particle analogy. Consider a collection of fictitious charged particles whose interaction potential is $G(k/k_D)/k^2$, where the dimensionless function $G(\kappa)$ is not specified except that $G(\kappa > 1) \sim 1$. (Equivalently, imagine replacing the Poisson equation at long wavelengths by some more complicated equation.) Suppose we desire the spectrum $\mathcal{E}(k) \doteq \langle \delta E^2(k) \rangle$ in thermal equilibrium, for weak coupling, and for $k \lesssim k_D$. We know that for $k \gg k_D$ the particles may be assumed to be uncorrelated; standard calculations then lead us to conclude that $\mathcal{E}(k) \sim G(\infty)/(k\lambda_D)^2 \sim (k\lambda_D)^{-2}$ for $k/k_D \to \infty$. If we followed the clump algorithm at this point, we would then be (mis-)led to conclude that the true spectrum, including the effects of correlations (shielding) would be $\mathcal{E}(k) \sim [(k\lambda_D)^2\epsilon(k)]^{-1} = [G + (k\lambda_D)^2]^{-1}$. [Here $\epsilon(k)$ is the static dielectric, which, with the postulated interaction, takes the form $\epsilon(k) = 1 + G/(k\lambda_D)^2$. *This is incorrect; the true answer, which from the Fluctuation-*Dissipation Theorem is $\mathcal{E} \propto 1 - \epsilon^{-1}(k)$, involves an additional multiplicative factor of G. Since G is arbitrary, this can be as different from the clump answer as one pleases. The specific analogies to the calculation of TD are: "uncorrelated" $\to$ exponential divergence; $k_D \to k_0$; $G(k) \to$ the complicated mode coupling effects at and below the production wave numbers.

The intent of the clump algorithm is to take approximate account of the self-consistent generation of the so-called "incoherent noise," loosely interpreted as a mode-coupling effect. In plasma physics, the original expressions for the noise were developed on intuitive grounds, and have been very useful in various phase space problems.[2,12] However, in a *strongly turbulent plasma fluid where mode coupling dominates and the important dy-*namical scales may be all of the same order, it is not clear that the original notions, based partly on ideas of shielded test particles, apply. Fortunately, we now know formally exact expressions,[3,13-15] and understand[3] the relation of those to various renormalizations, such as the direct-interaction approximation[16] or the test field method (TFM),[17] known to be useful for fluid problems. The general theory does have the general structure

$$\mathcal{N}_{k,\omega} = \frac{\tilde{\mathcal{N}}_{k,\omega}}{|\epsilon(k,\omega)|^2} \equiv \frac{\mathcal{N}}{\mathcal{D}}. \tag{4}$$

The real question, though, is whether or not this form is useful. It is useful in the theory of weakly coupled, discrete plasma because both $\mathcal{N}$ and $\mathcal{D}$ can be computed from a theory of zeroth order in the fluctuations. However, for strong turbulence both $\mathcal{N}$ and $\mathcal{D}$ contain important, related terms of nonlinear order. For example, if TD's construction (98) is to be consistent with the general theory (Ref. 3, sec. 5.5.9), their incoherent correlation $\tilde{\mathcal{N}}_{k,\omega}$ *must* (one has no choice) be identified with the Martin-Siggia-Rose[13] mass operator component Σ_{--}—the "input" term of Kraichnan.[18] In general, this term does not in itself determine the k spectrum. Rather, as is clear from Kraichnan's analysis of the relevant transfer function in the convective range [(Ref. 18, Eqs. (6.4) and (6.7)] and more generally, the role of the input term (the incoherent noise) is to participate in a delicate balance (there can be near cancellations) with the output term (the nonlinear damping, closely related to the *dielectric function*[19]) in order to ensure steady-state transfer of scalar variance. There are exceptional cases (when the Fluctuation-Dissipation Theorem holds) when the $\tilde{\mathcal{N}}_k$ at all k's can be inferred from $\tilde{\mathcal{N}}_{k,\omega}$. In general, however, for $k \lesssim k_0$ the balances just indicated must be dealt with explicitly. It seems misleading to call the numerator of (4) a "clump" for arbitrary (k,ω).

At the level of gross balances, the results of TD would be as correct if they had merely written $\tau_{cl} \sim \tau_0$. If the purpose of the discussion of TD is to argue that mode coupling effects are important, I agree wholeheartedly. However, the picture and concomitant mathematics of the turbulence as a sea of shielded clumps, where clumps are small scale structures whose boundaries diverge exponentially rapidly, seem to be qualitatively unfounded. The quantitative accuracy of their conclusions for predictions of macroscopic quantities such as transport coefficients is difficult to assess. However, since so little of the physics of the important scales has been included in dynamical detail, it seems that caution should be exercised about any results more detailed than those which follow from the (reasonably stringent) constraints of dimensional analysis and scaling theory.[6]

I consider the calculation of TD to be an honest attack on an extremely difficult problem. Since I have objected to some of the details, a fair question is to ask what I would do instead. The answer is that if I wanted to study the self-consistent renormalized mode-coupling of the scales $k \lesssim k_0$ analytically and to obtain wave number dependences and numerical coefficients, I would study a second-order Markovian closure such as the TFM. Such methods handle the mode coupling in a consistent way; they make a specific prediction, different from (1a), for the $\tilde{\mathcal{N}}$ in (3). [The TFM is also consistent with the simplest (no intermittency) spectral properties of the asymptotically small scales, if one should desire that information.] However, there is no guarantee that results thereby gained would be correct in detail either, unless there were substantial confirmation from direct numerical integration of the equations of motion.

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