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REMARKS ON THE CLUMP THEORY

By

John A. Krommes

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REMARKS ON THE CLUMP THEORY*

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ABSTRACT

Further details are provided of a soon-to-be published dialog [*Phys. Fluids* 29 (July, 1986)] which discussed the role of the small scales in fluid clump theory. It is argued that the approximation of the clump lifetime which is compatible with exponentially rapid separation of adjacent orbits is inappropriate for the description of the dynamically important large scales. Various other remarks are made relating to the analytic treatment of strong drift-wave-like turbulence.

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Recently I have published[1] some comments on the recent work of Terry and Diamond on plasma edge turbulence,[2] to which Terry and Diamond have replied.[3] This dialog centers not so much on the details of edge turbulence *per se*, but rather on the interpretation and validity of the so-called clump algorithm which Terry and Diamond employed. Unfortunately, the brevity of Refs. 1 and 3 may make this dialog difficult to appreciate for anyone not intimately familiar with the somewhat arcane subject of clump theory. The purpose of the present work (which, unfortunately, also suffers from a length constraint) is to expand on the remarks in Ref. 1, and to discuss and to answer the points which Terry and Diamond made in Ref. 3.

The fundamental point which I was trying to make in Ref. 1 was that the interpretation of "clump" which follows from a *literal* reading of Ref. 2 and earlier literature can be misleading and should be approached with great caution. I also wished to point out that it is considerably premature to attach quantitative significance to detailed algebraic results which emerge from the clump theory, especially when it is employed to describe the dynamically important scales of a plasma fluid. Given the nature of scientific research and the intrinsic complexity of plasma applications in general, such caveats are not, of course, unusual. They imply not that the work in question is without value, but just that one should be cautious when interpreting and generalizing the results.

It is impossible to proceed without stating precisely what I mean by the "clump algorithm" and by the term "small scales". By the former, what I mean here and in Ref. 1 is the *approximate* theory of the "two-point" correlation function as described first in Ref. 4.

Characteristic of this theory is the use of relative diffusion operators in the description of the small scales, a particular logarithmic form for the so-called clump lifetime, and a very approximate inversion of the spectral balance equation in terms of that lifetime. I specifically do *not* mean to embrace by "clump theory" the hole theory that Dupree has been developing over the last several years,[5] which attempts to deal with aspects of coherent structures, nor do I mean an "exact" theory (which, of course, includes all possible physics, at all scales) or general relations such as long wavelength conservation laws.[6] (The denotative power of "clump" is substantially lost if it is used just as a synonym for a fluctuation at an arbitrary wavelength.) Clump theory as it has been used in practice is a very specific approximation.

In particular, the issue of hole theory, which Terry and Diamond raise in Ref. 3, is irrelevant to the remarks in Ref. 1. (This is not to say that there is not much to be learned, both conceptually and practically, from such a theory.)

As far as the small scales are concerned, I mean, in the notation of Ref. 1, fluctuations with wave numbers k satisfying $k \ll k_0$, where k_0 is a characteristic production wave number. In the drift wave problem, $k_0 \sim \rho_s^{-1}$. As I read Ref. 3, I find that I could resolve a good deal of the conflict between the remarks in Ref. 1 and in Ref. 3 if in Ref. 3 I interpreted the phrase "small scales" to mean scales of order the production scale. However, that is not the interpretation of "small" which is clearly stated in Ref. 1. In fact, the issue is: To what extent is it reasonable to describe the production scales in terms of concepts and approximations which are *a priori* valid only at scales very much smaller than the production scales? Terry and Diamond seem to suggest that this is valid *in detail.* I argue that such a description is certainly not quantitatively accurate, and that its qualitative content is probably limited to only that of the simplest dimensional balances, in the absence of a proof which has not yet appeared in the literature.

Terry and Diamond state[3] that in Ref. 1 I erroneously attempt to "extrapolate results and intuition of homogeneous Navier-Stokes turbulence ... to the more complicated case of dissipative drift-wave turbulence ... ." In fact, the word "homogeneous" never appears in Ref. 1. Let us discuss, therefore, the extent to which my remarks in fact depended on such an assumption.

As is well known, drift wave turbulence, in general, is closely analogous to the problem of shear flow turbulence in neutral fluids,[7] the mean flow velocity in the latter being analogous to the diamagnetic drift in the former. It must be noted that shear flow turbulence *can* be homogeneous, at least in an interesting theoretical idealization, if the gradient in the mean flow is taken to be constant in all space.[8,9] Similarly, a very common approximation in drift wave turbulence theory is to model the mean density gradient as constant in space. Thus, drift wave turbulence can also be homogeneous (in the directions perpendicular to the magnetic field). Such flows are not *isotropic*, however; presumably, Terry and Diamond meant to use the phrase "isotropic Navier-Stokes turbulence" in the above quotation. (Isotropy implies homogeneity.) Now since everybody will agree that scales of order the production scales are anisotropic, the discussion focuses on whether the small scales are isotropic—and whether or not that makes any difference to the remarks in Ref. 1. Guided by a number of discussions in the literature,[7,10] I do believe that it is a useful approximation to consider the asymptotically small scales as isotropic. It should be recalled that in even the simplest model of fully developed isotropic turbulence hides an element of anisotropy. The usual thought experiment for realizing the long wavelength injection of energy in the isotropic case employs a macroscopic grid or other some such very anisotropic object. Nevertheless, the presence of such macroscopic anisotropy has not prevented several generations of theorists from discussing the scalar, isotropic Kolmogorov (1941) spectrum.[11] As various workers have discussed, it is true that macroscopic anisotropy does manifest itself in the so-called intermittency corrections[12–14,15] to the K41 spectrum, but these are important primarily for the higher order spectral functions and, in any event, represent a refinement far beyond the level of the very approximate clump theory under discussion here.[†] Thus, at the level of the rather general discussion I wished

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[†] My point of view is that the clump algorithm falls within the general scope of the direct-interaction approximation (DIA), being in fact much more primitive than the DIA. There is a slight subtlety here, since the clump theory employs Markovian and many further approximations which strictly do not follow from the DIA. In the same sense that a simple wave number cutoff on the fluid DIA restores Galilean invariance, an effect formally beyond all orders in vertex renormalization (see the references in Ref. 15), one might argue that the clump algorithm attempts to capture physics beyond that described by the DIA; one often sees "clumps" being discussed in the same context as intermittency. However, the cut-off DIA does not describe intermittency; inasmuch as the clump theory is built around diffusion coefficients, it does not seem that it can plausibly address that issue either.

We may also note a related remark by Boutros-Ghali and Dupree[6] which seems confused. They conclude their discussion of clump-related renormalizations by saying (last paragraph of p. 1855) "if by the direct interaction approximation ... we understand a scheme which iterates the coherent response only then [the proper solution of the balance equation which describes the incoherent response] will break down." In fact, the DIA has been unambiguously defined for all quadratically nonlinear equations by Martin, Siggia, and Rose[16] and by Kraichnan's earlier pioneering work[15]; it does contain the incoherent response[15] in a description which, though approximate, is far more detailed that that of the clump theory.

to give in Ref. 1, isotropy of the small scales would seem to be an issue of secondary importance. Details of the short wavelength spectrum might affect the actual value of the clump lifetime (*i.e.*, the size of the Batchelor constant), but the question of the degree to which it is valid to extrapolate the asymptotic clump lifetime formula back to the production scales is much more general than such details.

Terry and Diamond state[3] that a difference between drift wave turbulence and homogeneous, isotropic turbulence is that in the former "[t]urbulence is driven at all wave numbers k present in the fluctuation spectrum." Now by "driven", one must understand an energy flow directly from the outside world, for if one interprets "driven" to mean "excited", then this statement is equally true (or untrue) for both classes of turbulence. Certainly all wave numbers are excited in both cases, just by virtue of nonlinearity. The question is how a fluctuation at a particular wave number gets predominantly excited—by a reactive coupling, possibly a cascade, from other wave numbers, or by an instability driven directly by the long wavelength mean flow. Terry and Diamond seem to imply that the latter is true for all wave numbers in the drift wave case. However, this seems like an overstatement, since at least according to linear theory sufficiently short wavelength modes ($k_\perp \rho_s \gg 1$) are stable. The picture of direct drive at all wave numbers seems at variance with various standard discussions of injection into a turbulent fluid. Leslie,[9] for example, summarizes properties of various "engineering" flows which are quite analogous to the drift wave problem. It is clear from such reviews that there exist high Reynolds number flows in which the production and dissipation wave numbers are cleanly separated. Also, Batchelor's discussion of the convective subrange,[17] to which I referred in Ref. 1, posits the injection of scalar variance at wave numbers small compared to those in the convective subrange. Now my point is not that I believe one can solve drift wave problems by blindly taking over a potpourri of results from all of the neutral fluid problems which have been discussed to date. Turbulence in a strongly magnetized plasma is rather unique, both in terms of the general physics and in terms of annoying practical details involving the geometry of the magnetic field. (Thus, Terry and Diamond use in Ref. 2 the ballooning representation, which may or may not ever be useful for the description of turbulence in neutral fluids.) However, the thrust of the discussion in Ref. 1 is that at the general conceptual level it seemed that the arguments of Terry and Diamond[2] could be applied as well to the neutral fluid as to the drift wave, but that if one did so he encountered difficulties.

It is true, as Terry and Diamond state,[2,3] that drift wave turbulence supports a collective wave spectrum which is absent in the isotropic case. Indeed, this makes the physics much richer and the theory much more difficult; however, in Ref. 1 I did not object to the treatment of the collective spectrum.

Let us now turn to the discussion by Terry and Diamond in Ref. 3 of the spectral balance equation. Indeed, at some sufficiently abstract level, the form of this equation is "inevitable". Probably the most complete general expression of the classical balance equation was given by Martin, Siggia, and Rose,[16] who provided a functional differential equation (admittedly formal) for its determination. Of course, the MSR formulas had important antecedents in the work of Kraichnan (*cf.* Ref. 18 or Ref. 19) and the Schwinger formulation of quantum field theory. (See the references in Ref. 16.) The structure of the

MSR formulas[†] has been discussed in light of their applications to plasmas by Krommes[20,15] and DuBois.[21] Consistently, Terry and Diamond do not reference any of these works. Since it is difficult to believe that they are unaware of this published research, one is led to conclude that they believe it is irrelevant to the issues under discussion. Presumably, they do not argue with the formulas themselves, but question whether such formulas can be reduced to ones useful in practice. However, with this interpretation in mind, one becomes progressively more confused as he pursues a detailed study of the nuances in Ref. 3. Terry and Diamond point out that the balance equation in its general form is "intractable from any practical point of view", and that approximations are needed. I consider such remarks to be obvious (otherwise, I would see no need to engage in long discussions[15] of the structure and ramifications of the direct-interaction *approximation* and further simplifications). But surely approximate, practical formulas must be compatible with the general relations which must be true. In fact, the entire thrust of my remarks in Ref. 1 was that since many of the details of the small and of the larger scales *have* been extensively discussed and worked out for relevant problems in fluid dynamics, inasmuch as the problem at hand also involves turbulent fluid motions, the burden of proof is on the author of a new approximation[‡] to show that it is either more accurate or more useful

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[†] One can note (probably in vain) an unfortunate choice of nomenclature in the plasma clump literature, namely the distinction between a "one-point" and a "two-point" theory. In the clump literature, "one-point" denotes a theory of the response function, "two-point" denotes the spectral balance equation at *equal* times. This is a complete transposition of the meanings natural in field theory. There,[16,15] the response function is clearly a *two*-time, two-space-point object (it is related to the dielectric function). Also, the spectral balance equation at *one* time is the specialization of the *two*-time equation for the two-space-point correlation function. It is the *one*-time spectral equation which, by symmetrization, contains the interesting cross terms whose relevance to relative diffusion Dupree has stressed.[4]

[‡] In fact, the fundamental algorithm appears to have been first applied to a fluid problem by Dupree.[22] However, Terry and Diamond do not reference that work.

than the previous ones. Of course, it should be physically correct as well! In this context, one rationale put forth by Terry and Diamond for using the clump theory is particularly interesting: they cite[3] "the relatively simple and intuitive physical picture it provides". Now certainly simplicity and heuristic appeal are to be strived for. But nothing has been gained, and likely much has been lost, if such pictures are achieved at the price of incorrect physics. The concern which prompted me to write Ref. 1 was that a picture of "shielded clumps", whatever its utility for understanding the very smallest scales may be, seems to be a misleading model of the dynamically and energetically important scales, as in this picture one seems to lose sight of the triad interactions which justifiably dominate other discussions of nonlinear fluctuation-fluctuation coupling in strong turbulence.[†]

Continuing the last remark, I wish to expand on the the discussion in Ref. 1 on the delicate balance between the input term and the output term, and on the relevance of the work described in Ref. 23. Stemming, no doubt, from the structure of, and intuition behind, the kinetic theory of the weakly coupled, discrete plasma, there seems to be a tendency, manifest in Ref. 2, to assume that the dielectric function is more or less known while focusing on, as a separate issue, approximations to the "incoherent noise", or input term. This is very dangerous in a theory of strong turbulence. The work described in

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[†] A reasonable question is whether my concerns should not apply as well to the velocity space problems which Dupree discussed initially[4] as to the fluid problems. In fact, inasmuch as the velocity space problems are also in a strong turbulence regime, I see no immediate reason why my concerns should not apply to those problems. However, the physics in velocity space is much richer, hence more subtle. Also, I prefer to be specific, not abstract, in the discussions, and it seems most clear to focus on the fluid problems, since the parallel neutral fluid literature is so widely known. Furthermore, the definitive tone of Ref. 2 suggests that the theory has evolved to a state much more highly developed than the early work of Dupree,[4,22] who clearly considered his work to be very approximate.

Ref. 23 represents an extreme, but nevertheless relevant, limit (guiding center plasma) in which it can be seen clearly that the input and the output terms must be treated on equal footing, as important cancellations can occur between them. Similar results were discussed earlier by Similon,[24] and can be found many places in the extensive fluid literature (*cf.* Ref. 25) Of course, when all terms are of order unity, as they nominally are in discussions of the energy-containing modes, it is extremely difficult to proceed analytically. When one nevertheless does so, a careful statement of the justification and likely precision of the result would seem called for. It was my hope, unfortunately unfulfilled, that my comments in Ref. 1 would elicit some discussion on this matter.

I will now remark on the five points which Terry and Diamond singled out for specific discussion in their reply to Ref. 1. The numbering follows Ref. 3, to which the reader should refer.

(i) As I discussed earlier, I do not believe that the dynamics of the *small* scales is significantly different in the two models. Obviously, the details of energy injection in the production range can be very different in the case of isotropic as opposed to anisotropic turbulence. They come much more to the fore in the drift wave case. Inasmuch as they do, they deserve to be treated with care, if one is to take seriously results more specific than very gross balances and scaling. That is just the point of Ref. 1.

(ii) The "related difficulties" to which I alluded refer to the way in which the incoherent correlations are approximated. The proper description of the incoherent fluctuations is required at any Reynolds number, large or small.

(iii) The remarks of Terry and Diamond here are quite correct. However, they miss my point, which was that while the clump theory makes a reasonable prediction for the small scale spectrum, it is too crude to provide a quantitatively satisfactory description of the interaction of the production and longer wavelength modes among themselves. These are the dynamically important interactions.[†]

(iv) Terry and Diamond seem here to equate the scales of density blobs with the small scales. I repeat that in Ref. 1 the small scales were defined by $k \ll k_0 \sim \rho_s^{-1}$.

(v) To state that "analogies between uncorrelated particles and exponential divergence are incorrect" is to misunderstand the intent and sense of the analogy I was drawing. The contrast is not between "uncorrelated" and "correlated", but rather between the treatments of the small scales and the large scales. The notion of "uncorrelated" entered the discussion only for the discrete particle side of the analogy; it arose there because that is how one most physically does the calculation of the short wavelength fluctuations of a weakly coupled plasma. To state in the baldest possible terms the complaint I was trying to make, one does not, in either a plasma or in any other statistical dynamical system, compute the numerator [*cf.* Eq. (4) of Ref. 1] of the balance equation (the "incoherent noise") at *short* wavelengths, then shield it with the dielectric and expect to obtain a result valid at *long* wavelengths. What one gets thereby are the true fluctuations *at the same short wavelengths with which he started.* To obtain the fluctuations at the long

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[†] Although it is peripheral to the discussion in the present manuscript, it is worth remarking that any theory which purports to predict the spectral details of a system which exhibits stochastic instability carries a rather great burden: If it relies on a general mechanism (exponential orbit divergence), it should have something to say about many other well-known stochastic systems, including the very simple ones such as the standard map or the Lorenz system. This is not excluded, but it represents an interesting challenge for the advocates of clump theory.

> wavelengths, one must approximate the balance equation at the long wavelengths. The question is: In such a long wavelength approximation, does somehow the clump lifetime *computed from the asymptotically short wavelength approximation* emerge in a way dynamically important for the large scales? In the absence of a specific argument to the contrary, this seems physically implausible.

With regard to the "remarkable agreement" between theory and data to which Terry and Diamond allude, a large part of any such agreement is forced on one just by general dimensional considerations, and I do not question here that part of the work. The more substantive question is whether there is a more detailed agreement uniquely traceable to the specific clump theory employed—*e.g.*, to the detailed physics of the small scales and the specific form of the clump lifetime. This is by no means clear to me. I do not believe it has been argued in the literature; further discussion by Terry and Diamond would be very desirable.

In their concluding remarks, Terry and Diamond offer their opinion of the class of second order closures popular (with appropriate caveats) in the theory of neutral fluid turbulence. They state that "[s]uch equations would require numerical solution (a ludicrous thought, since the *exact* equations could be so solved) and would be relatively devoid of physical insight." The only possible interpretation of a remark such as this[†] is that they believe that (their) statistical closure theory for strongly turbulent fluids and plasmas has matured to the same level of quantitative predictive power as, say, the linearized Vlasov

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[†] Webster[26] defines "ludicrous" as "1: amusing ... through obvious absurdity ... 2: meriting derisive laughter or scorn as absurdly inept, false, or foolish."

equation. This is certainly not my view, not only of the clump theory but of any other known closure, and it is difficult to find support for such a strong statement anywhere in the literature. To brand as "ludicrous" (a) such excellent pioneering work on the comparisons between numerical solutions of the exact dynamics and predictions of various closures as, for example, that of Kraichnan[27] or Herring,[28] or (b) the recent and ongoing work on efficient numerical algorithms,[29,30] is to dismiss out of hand the foundations of the subject and to attempt to reduce it from a scientific discipline to witchcraft. It is true that the advantage in computational speed enjoyed by isotropic closures over direct flow simulations is lost when anisotropy must be considered (as I am well aware[31]). It must be remembered, however, that there is a compensating advantage: analytic closures, whether numerically or analytically solved, afford one the opportunity of examining in isolation the role of specific parts of the nonlinear interactions, by turning them on or off.[†] Such opportunity is lost in the direct numerical approach; there, turning off the nonlinearity reduces the problem to triviality. Nevertheless, I will not argue here dogmatically for extremely expensive numerical solutions of complicated, yet obviously incomplete, statistical closures. I do believe that, to the extent that such can be done, they should be pursued, as the results gained can only add to our presently very inadequate understanding of the rich, intricate physics of plasma turbulence. However, it may well be that the proper way of analyzing turbulence in realistic practical situations is through a combination of direct flow simulation and analytic scaling theory; the recent dramatic advances in computing power appear to make such a program feasible. The danger of a highly analytic approach to such difficult

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[†] For example, one decomposition (more practically useful for phase space problems than for fluids) is into the so-called diffusion and polarization parts[15,23] of the turbulent collision operator.

problems is that the results, obscured by formalism, will be believed unquestioningly. It is not that I eschew analysis. I do, however, feel that it is a great misfortune for two fields as closely related as are strong fluid turbulence and strong plasma turbulence to develop in isolation. It is my hope that the dialog which I initiated with Ref. 1 will aid in clarifying some of the connections between these disciplines. Both fields can only benefit from such clarification.

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