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What Is the Matter with High-Reynolds-Number Turbulence?

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I wish to discuss high-Reynolds-number turbulence briefly. The intent is to get across the broad idea that there is new physics to be learnt by exploring flows at higher and higher Reynolds numbers. While the facts presented here are not subjective to the best of my knowledge, their interpretation may be less so. I am aware that these notes might be open to criticism on several counts and so, gentle reader, I beg your forbearance in advance. If they make you think a little differently, the notes will have served their intended purpose adequately.

1. Is there a unique fully turbulent state at all, sufficiently high, Reynolds numbers?

The issue here is the conventional wisdom that there exists a unique entity known as the fully turbulent state at all Reynolds numbers which are sufficiently high. I choose to discuss it in terms specifically of the flow past a circular cylinder – although this restriction is by no means necessary.

a. The frequency parameter in the wake of a circular cylinder

Fix the cylinder (diameter D) in the usual way in a standard wind tunnel or water tunnel in which a uniform flow (velocity U) can be created, and imagine that the Reynolds number Re (= UD/ν, where ν is the kinematic viscosity of the fluid) can be increased indefinitely.[1] At low Reynolds numbers, the vortex 'shedding' behind the cylinder is ordered. A velocity probe placed at a modest distance of order 10D behind the cylinder will show a clean periodic signal. The flow becomes less ordered at higher Reynolds numbers, but the power spectral density of the probe-output will show a peak corresponding to the characteristic frequency of the large-scale flow in the downstream vicinity of the cylinder. Plot this frequency, suitably normalized, as a function of the Reynolds number.

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[1] In practice, experiments over a wide range of Reynolds numbers are not made using the same cylinder in the same flow facility by increasing the flow speed continuously. One resorts to various combinations of cylinder diameter, fluid viscosity and flow facilities - which means that the precise flow configuration does *not* remain the same in all experiments. It is generally believed that the Reynolds number is the only important parameter for all incompressible speeds provided that the secondary parameters satisfy the following conditions: (a) The aspect ratio L/D is 'sufficiently' large for the 'end-effects' to be negligible; (b) the disturbance level in the fluid stream remains small, and (c) the flow facility in which the cylinder is housed is large enough for the flow around the cylinder to be immune to the presence of 'walls'. However, these features control flow development in the immediate vicinity of bifurcations.

Results from two experiments are shown in figure 1; those below an Re of 2.10^4 were obtained by me using standard hot-wire anemometry while those at higher Re were obtained by Schewe (1983) from the spectral analysis of the fluctuating lift on the cylinder – which itself is produced by the fluctuating flow field. In both instances, accurately identifying the dominant frequency was easy and subject to little error. Much data that exist in the low-Reynolds-number range are consistent with those presented here; reliable data in the high-Reynolds-number regime are quite rare, however.

An overall feature of figure 1 is the presence of several distinct power-laws. The largely two-dimensional 'vortex-shedding' regime, Re < 150, is characterized by the exponent 4/3. The subsequent change in slope coincides with the onset of strong three-dimensionalities in the flow field, and appears to be unity for an extended regime[2] up to about 2.10^5. From qualitative criteria that the flow is strongly three-dimensional and the spectral density becomes increasingly broad with increasing Reynolds number (in fact according to an expected power law), the flow behind the cylinder can be legitimately called turbulent.

The boundary layer on the cylinder remains laminar up to this point, and further transitions in figure 1 are due to changes in the boundary layer structure. The near-discontinuity seen at a Reynolds number around 2.10^5 is associated with the formation of a separation bubble (that is, separation and quick reattachment of the boundary layer) on one side of the cylinder surface; the boundary layer on the other side still remains attached. At a somewhat higher Reynolds number, a separation bubble is formed also on the other side (with an accompanying jump in the frequency parameter) and the symmetry is restored.[3] A power-law variation with an exponent of 0.95 occurs beyond this Reynolds number; the deviation of this exponent from unity may or may not be significant. Finally, there is the transition associated with the boundary layer on the cylinder becoming turbulent, and a power-law with a different exponent close to 3/2 ensues.[4]

I present these data in this particular fashion to demonstrate that there is in fact a dominating physical phenomenon associated with each of the power-law regimes, and that this conclusion may be quite general. It should also be mentioned that another gobal quantity of interest, namely the drag coefficient, shows power laws (see, for example, figure 1.4 from Schlichting 1968) that are consistent

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[2] Separate least-square fits to my data and to Schewe's yield slopes that differ by about 5%, but the significance of this observation is uncertain because the secondary parameters mentioned earlier differ in the two flows. It should, however, be noted that there are other indications that some subtle changes occur within this Reynolds number regime. For instance, the data on the drag coefficient shows a change of magnitude from one constant level to another, slightly different, value. So do spectral amplitudes.

[3] The frequency jumps consequent to the formation of the separation bubbles on the two sides of the cylinder are in the ratio of the golden mean (Schewe 1985).

[4] An important challenge is to explain these various scaling exponents. At the least, this requires a good qualitative understanding of the interaction of various flow scales involved. We have a simple picture in mind which explains – after a fashion – these scaling exponents.

with the physical changes occurring in the flow. Conventional thinking would be that the last power-law of figure 1 would mark the end of possible transitions and that, if one continued measurement at higher Reynolds numbers, one would continue to follow the last power-law regime indefinitely. While no one has previously looked at the frequency parameter in this way and made this precise statement, the feeling that no more qualitative changes are possible – because the boundary layer on the cylinder in this last regime is turbulent – is ingrained in text books and other places. I propose that more, in fact many more, transitions are likely.

I can cite no direct evidence to support my claim, but will provide indirect evidence by showing that further increase in Reynolds number is accompanied by qualitative changes in the turbulent boundary layer on the cylinder. Because of the intimate relation between the structure of the boundary layer on the cylinder and the flow behind it, I will infer that more transitions might occur.

b. The turbulent boundary layer

For illustrative purposes, consider the turbulent boundary layer on a flat plate housed in a wind tunnel or water tunnel whose width is very large compared to the nominal thickness of the boundary layer. Let us look at the first-order quantity such as skin-friction coefficient C_f (or, in a suitably normalized way, the drag force on the plate). Figure 2 compiles data from two sources: Wieghardt & Tillmann (1944) and Fiore (1977). (Much existing data has also been examined, and the conclusion drawn below is supported by most of them; the primary reason for restricting to two sources in figure 2 is to avoid the clutter.) Concentrate first on Wieghardt's data: We see that there is a well-defined power-law with an exponent of about $-1/4$ for all momentum-thickness Reynolds numbers, R_θ, below about 6000. Thereafter, a change appears to occur in the power-law exponent. Unfortunately, Wieghardt's (or similarly good) data incompressible flows do not extend to high enough Reynolds numbers to enable unambiguous determination of this latter exponent*.

This compels me to use data from compressible boundary layers. Fiore's data at a Mach number of about 3 cover very large Reynolds numbers but the Mach number effects on the skin friction are hard to assess accurately. To a first order, Schlichting (1968, Chapter 23) discusses the effect of compressibility. A more recent discussion is due to Gaudet (1986). In simple terms, the net effect is to require the 'compressible data' on skin friction and R_θ to be multiplied by Mach-number-dependent

* It might seem that a reasonable option is to look for friction data in pipe flows in which higher Reynolds numbers than in boundary layer flows have often been attained: Since the friction data behave very similarly in the two flows, our knowledge from pipe flows can be translated to boundary layers without discomfort. Nikuradse's (1932) experiments in smooth pipes show that the $-1/4$ power law characteristic of low Reynolds number turbulent flow gets modified beyond a Reynolds number (based on the pipe diameter) of about 40,000. Some data in Helium II by Walstrom et al. (1988) are consistent with this conclusion. Unfortunately, while the pipe flow data do confirm that an important change in the C_f -Re relation occurs beyond a certain Re, they too do not extend to high enough Reynolds numbers to determine the exact nature of this change. Incidentally, if the Reynolds numbers in pipe flows and boundary layers are expressed using comparable length and velocity scales, the quarter-power variation ceases to hold at about the Reynolds number in the two cases.

factors. This procedure is known to work under certain circumstances (see Glaudet's paper), but there may still be some lingering uncertainty. I have myself tried variations on the existing corrections and found that existing uncertainties have no signficant effect in a log-log plot. Also, note that the Mach number in Fiore's experiments does not vary much; and so, whether or not the applied correction is precisely right, the observed skin-friction variation is likely to be primarily a Reynolds number effect, and the deduced power-law is likely to be robust[@].

Fiore's data after this simple manipulation are plotted in figure 2. The conclusion is that two different power-laws apply[5] in two Reynolds number regimes for the skin-friction variation in boundary layers on *smooth* surfaces: One for $R_\theta < 6000$ for which the exponent is about $-1/4$ and another for higher Reynolds numbers for which the exponent is about[#] -0.13. We have for a long time known that the wake component changes its character from being Reynolds-number-dependent to a constant at precisely this Reynolds number. I tentatively argue that these and other changes around this Reynolds number are reflections of a readjustment of the importance of different scales in the flow[**].

At even higher Reynolds numbers, given a fixed surface finish, the plate becomes essentially rough because the characteristic viscous length, with which the roughness height should be compared, becomes progressively smaller as the Reynolds number increases. The understanding of the roughness effect is at best moderate but, from pipe flow experiments for which the data are more systematic (Nikuradse 1933), I have found that the asymptotic skin-friction coefficient depends on the roughness height according to the power-law

$$C_{f(asymp)} \sim (R/k)^{-0.3} \tag{1}$$

@ There is another direct measurement of the skin friction at a truly high Reynolds number (Zakky et al. 1978). The measurement, albeit on a cylindrical surface and at a Mach number of 0.64, should be reasonably good representation of the type of data we are after. This value is decidedly lower than Fiore's data at the same Reynolds number - whether or not compressibility corrections are applied. However, only a single measurement is reported by Zakky et al. and we do not know too many details about the measurement. It would be useful to resolve why the differences exist and which set of data represents the 'standard'.

5 Because of the uncertainties to which attention has already been drawn, I do not claim that this conclusion is fool-proof. In fact, an inevitable conclusion that follows from this exercise is that properly planned experiments at high Reynolds numbers are absolutely essential for a better understanding even of basic quantities such as skin-friction. It may seem odd that I should stress this self-evident fact, but I have in the past encountered strong opinions that nothing fundamentally different occurs between low-Reynolds-number boundary layers and high-Reynolds-number ones.

The fact that the two exponents are in the ratio of about 2 does not seem to have any intrinsic significance.

** There is, of course, the objection that the power-laws are a red herring and that Prandtl's skin-friction law extends to all Reynolds numbers. Based on such a notion, Schlichting (1968, page 574) stated that 'measurements with higher Reynolds numbers are, therefore, not required.' Perhaps so. Let me note two facts, however: First, the skin-friction law for pipe flows due to Prandtl (and especially its variants for the boundary layer as discussed, for example on pages 321+ of Monin & Yaglom 1971, volume 1), are quite empirical even beyond the logarithmic variation of the mean velocity on which they are based. Secondly, a power-law variation of the mean velocity distribution, to which a power-law for the skin friction can be related in some fashion, is based on solid grounds and especially appropriate to low-Reynolds-number boundary layers.

where R is the pipe radius and k is the roughness height[6].

At even higher Reynolds numbers, what happens is unclear. Presumably, first, the drag on the roughness elements will cease to be completely a consequence of the pressure drag, and some Reynolds number dependence will be reintroduced. The boundary layers on each of the roughness elements will become transitional and finally turbulent, as in the low-Reynolds-number part of figure 2. Subsequently, these boundary layers change to the high-Reynolds-number variety, and so on. Many further 'transitions' are therefore possible.

c. Return to the cylinder problem

I shall now think that the type of changes one may encounter in the flat-plate boundary layer are equally likely to manifest in the boundary layer on the cylinder surface. Simple estimates suggest that, even at the highest Reynolds number covered in figure 1, the boundary layer on the cylinder is in the low Reynolds number mode of figure 2. If other structural changes are due in the boundary layer at higher Reynolds numbers, it stands to reason that the flow behind the cylinder will correspondingly undergo further transitions. The process may, in fact, continue[%] *indefinitely!*

The points made here are no doubt based on somewhat tenuous connections, but I hope that they will not be taken as facetious; they are meant to suggest that, with increasing Reynolds numbers, a continuum of transitions may be possible in turbulent flows. This, in my view, leaves open the possibility that there might be no such entity as the unique, fully turbulent, state. If so, it raises many questions. In particular, it suggests that any scaling relation one may find will be valid only within a finite range of the Reynolds number. Does this restriction apply to the energy dissipation rate? If it does, what do we know about the scaling laws of this important parameter?[++] Does it apply also the Kolmogorov-type scaling? If so, in which of the myriad scaling regimes possible in a given flow will the Kolmogorov scaling hold?

2. The Kolmogorov-type universality

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[6] Another challenge is to explain the various power-laws to which I have made reference in the skin-friction behavior of the boundary layer. I have shown in unpublished work that the exponent in (1) can be related to the two exponents $-1/4$ and -0.13 by means of simple change of scales, and so explaining one of these exponents is equivalent to explaining all the others. Some beginnings in this direction have been made.

[%] I have concentrated my attention on the near-field. There are experiments to suggest that the flow downstream is not unique (Wygnanski et al. 1986). If this is true, my arguments become even more relevant.

[++] So far as I have been able to determine, the energy dissipation rate integrated in the outer part of the boundary layer, when normalized on the outer variables, has different values in the smooth-wall and rough-wall boundary layers. This conclusion is quite distinct from the general belief that the outer part of the flow is independent of the inner structure. In the current view expressed here, this simply means that the energy dissipation rate, when scaled on the outer variables, shows different magnitudes in different power-law regimes of the boundary layer structure.

As a brief reminder, let me note that the basic notion due to Kolmogorov (1941) is that the 'cascading' of kinetic energy from large scales to smaller ones occurs faster with decreasing scale-size and that this transfer, which is assumed to be local in wave-number space, is accompanied by the statistical decoupling of the scales involved. In particular, scales which are small compared to the large scale L are assumed, in a statistical sense, to be spatially and temporally homogeneous and isotropic, and that they do not directly depend on the large scale properties of the flow. Roughly, this is the notion of local isotropy which is basic to Kolmogorov's two 'similarity hypotheses'. The first of these hypotheses is that small scales of turbulence are completely controlled by the average energy dissipation rate, $<\varepsilon>$, and the kinematic viscosity, ν, of the fluid. This implies a universal functional form for the statistical properties of small scales. For example, the turbulent energy spectral density $E(\kappa)$, where $E(\kappa)d\kappa$ is the energy contained in a spherical wave number shell of size κ and thickness $d\kappa$, is of the form

$$E(\kappa) = (\nu<\varepsilon>)1/2\ F(\kappa\eta) \qquad (2)$$

where F is a universal function. The second similarity hypothesis states that, at Reynolds numbers which are so high that there exists a sub-range of scales (the so-called inertial sub-range) such that $\eta << r << L$ (which are neither dissipative nor energy-containing), the only controlling parameter is the scale-to-scale energy transfer rate. Since these scales do not dissipate, the average rate of energy transfer they experience is identical in magnitude to the energy dissipation rate $<\varepsilon>$ occurring at smaller scales of order η. It follows from dimensional reasoning that $E(\kappa)$ assumes the shape

$$E(\kappa) = \alpha<\varepsilon>2/3\kappa^{-5/3}, \qquad (3)$$

with α as a universal constant.

Corrections to the 5/3 exponent in (3) have been proposed because of the intermittency of ε; such corrections to (2) are not hard to propose. These corrections appear to be important for high-order statistical quantities with which I shall not concern myself here. The correction for second order statistics, if it exists, is believed to be small, and I shall proceed on this basis.

Kolmogorov's theory is meant to hold for 'sufficiently high' Reynolds numbers, irrespective of the precise scaling domain and the precise flow. I argue that the quality of proof that exists for (2) and (3) leaves a lot to be desired. As an illustration, I offer in figure 3 the data collected by Monin & Yaglom (1971, volume 2) to support universal scaling of the small-scale fluctuations in turbulence. While these measurements do confirm that there is a rough similarity on the basis of Kolmogorov scales, they are not sufficiently accurate to determine whether there are systematic changes as a function of Reynolds number, of the flow configuration and the like. How can one detect such changes if the 'scatter' is so large that at $\kappa\eta = 1$, say, the various measurements of figure 4 differ among themselves by almost two orders of magnitude?[7] To a skeptic, it appears as if the collapse of the data in the dissipation range, such as it is, occurs only because the spectral density rolls off extremely rapidly. This unsatisfactory circumstance may well be due to uncertainties in measurements, in which case we still have ahead of us the task of providing more satisfactory data; my point is that the 'scatter' may well hide some important differences from one flow to another and from one high-Reynolds-number regime to another. Until better measurements become available, it appears safe to conclude that universal scaling *a la* Kolmogorov, which is supposed to apply equally well to all possible scaling ranges in a given flow and to all flows occurring in Nature and technology, is still an open question. I submit that a systematic exploration of the sort of questions raised here remains to be made, but hasten to add that the approximate validity of the Kolmogorov scaling is not the point at issue.

Another point: The fact that the C_f-Re relations are different in different Reynolds number regimes suggests that the scaling of the mean energy dissipation rate might also be different. If this is so, it has important implications to our basic thinking about turbulence.

I can continue to elaborate on these points, but choose not to do so here. Some of it has recently been discussed elsewhere (Sreenivasan 1991), where I have also pointed out the serious difficulties that one encounters in applying Kolmogorov-type arguments to passive scalar fields advected by turbulence. In particular, I argued on the basis of existing measurements that local isotropy, if it applies at all, is valid only at such extraordinarily high Reynolds numbers as to be of no relevance to Earth! The one piece of evidence worth repeating here is that the skewness of the temperature derivative, which should be zero for isotropy, is of the order unity even in geophysical flows where the Reynolds number is rather high. To clinch the issue, note that the sign of the skewness has been shown to be directly related to product of the signs of the *mean* velocity and *mean* temperature derivatives. The details are discussed in the reference cited above.

3. Conclusion

I have put forth the possibility that there is new physics to be discovered at higher and higher Reynolds numbers, and that this new physics may well bear on basic and practically useful quantities such as skin-friction and heat transfer. I have further tried to show that very little is known precisely and definitively in high-Reynolds-number turbulence and that extrapolation from low-Reynolds-number experience may not always be quite valid. Indeed, the applicability of basic notions such as Kolmogorov's scaling and local isotropy in the dissipative range remains to be established satisfactorily. It is not my claim that the latter are incorrect, but that such powerful ideas deserve better and more solid vindication. True progress, which may or may not bear on applications immediately, is

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[7] If one selects data only from experimentalists whose data collection and data processing practices are respectable, the scatter in figure 4 diminishes, but the differences are still quite conspicuous and large; they cannot be attributed totally to problems such as finite probe-length. Issues such as the hot-wire calibrationprocedure, absolute levels of turbulence, etc. become critical.

possible only by means of accurate measurements at high Reynolds numbers. This, of course, requires appropriately bigger and better flow facilities for experimental research. If you are reminded of the arguments made in high-energy physics, the analogy is not accidental.

Acknowledgements

I am thankful to Albert Libchaber, Leo Kadanoff, Jim McMichael and Steve Orszag with whom I have had conversations that touch upon the contents of this note directly or indirectly. Russ Donnelly brought to my attention the data of Walstrom et al. and assured me that higher-Reynolds-number measurements do not exist even in Helium II. Similarly, Lex Smits referred me to the high-Reynolds-number data of Zakky et al., and made useful remarks on a preliminary draft. To them all, I am thankful. Needless to say, I am fully responsible for all my blunders.

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Figure captions

Figure 1: The frequency parameter as a function of Reynolds number in the wake of circular cylinders. The cluster of points between slopes of 1.0 and 0.95 consists of two jumps corresponding to the formation of separation bubbles on the two sides of the cylinder. Note the hysterisis effect when the boundary layer on the cylinder undergoes transition to turbulence.

Figure 2: The skin-friction data from Wieghardt (unfilled squares) and Fiore (filled diamonds). See text for details. All indications are strong that Fiore's flat-plate was smooth.

Figure 3: This is a copy of figure 76c from Monin & Yaglom (1971). The data are from 19 different flows studied by different authors. The flows range from wakes, jets, grid turbulence and pipe flows at low to moderate Reynolds numbers created in laboratories to a tidal channel flow at high Reynolds number. Our point is not that there is no universality, but that an important concept such as this deserves better proof.

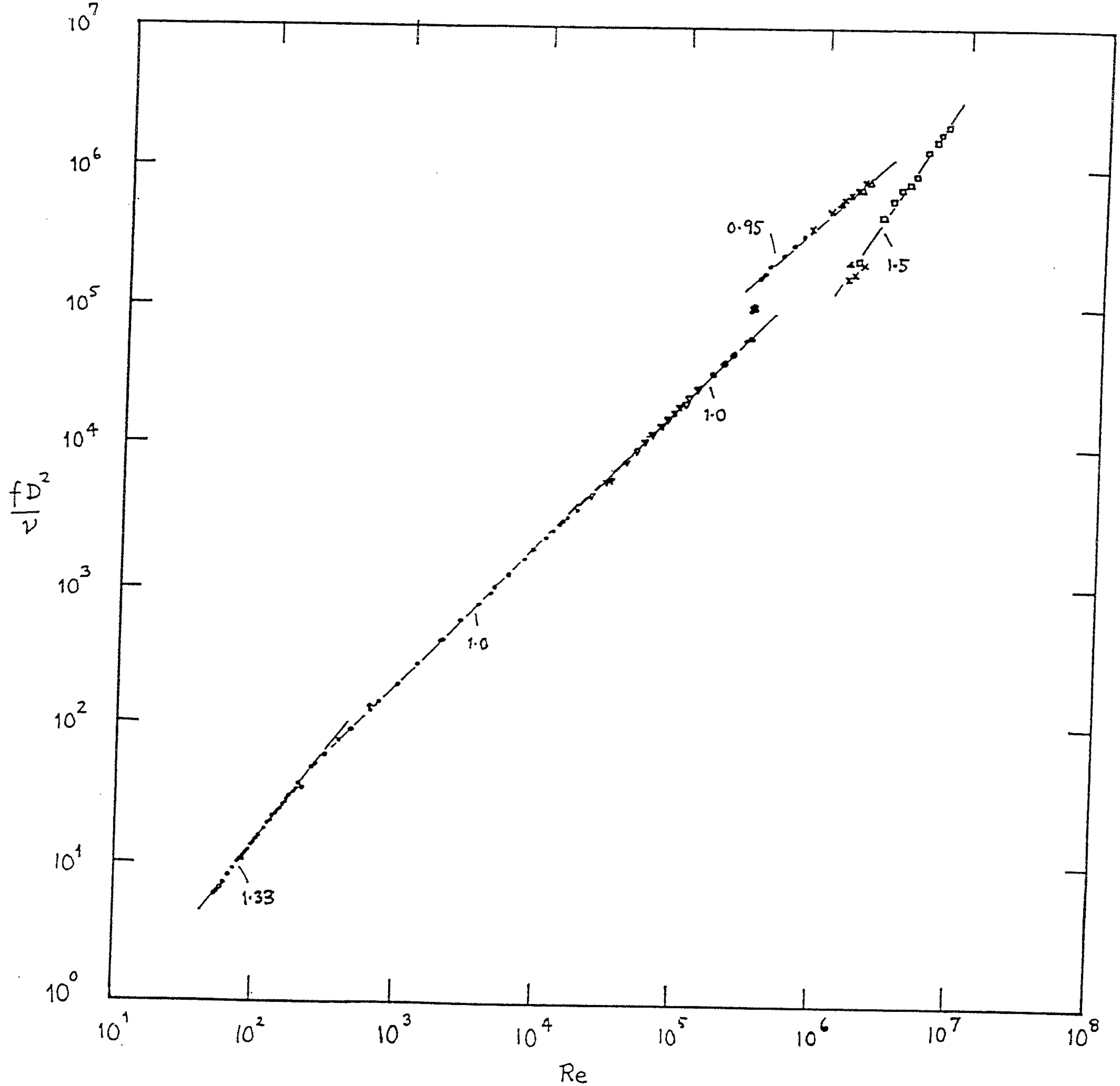
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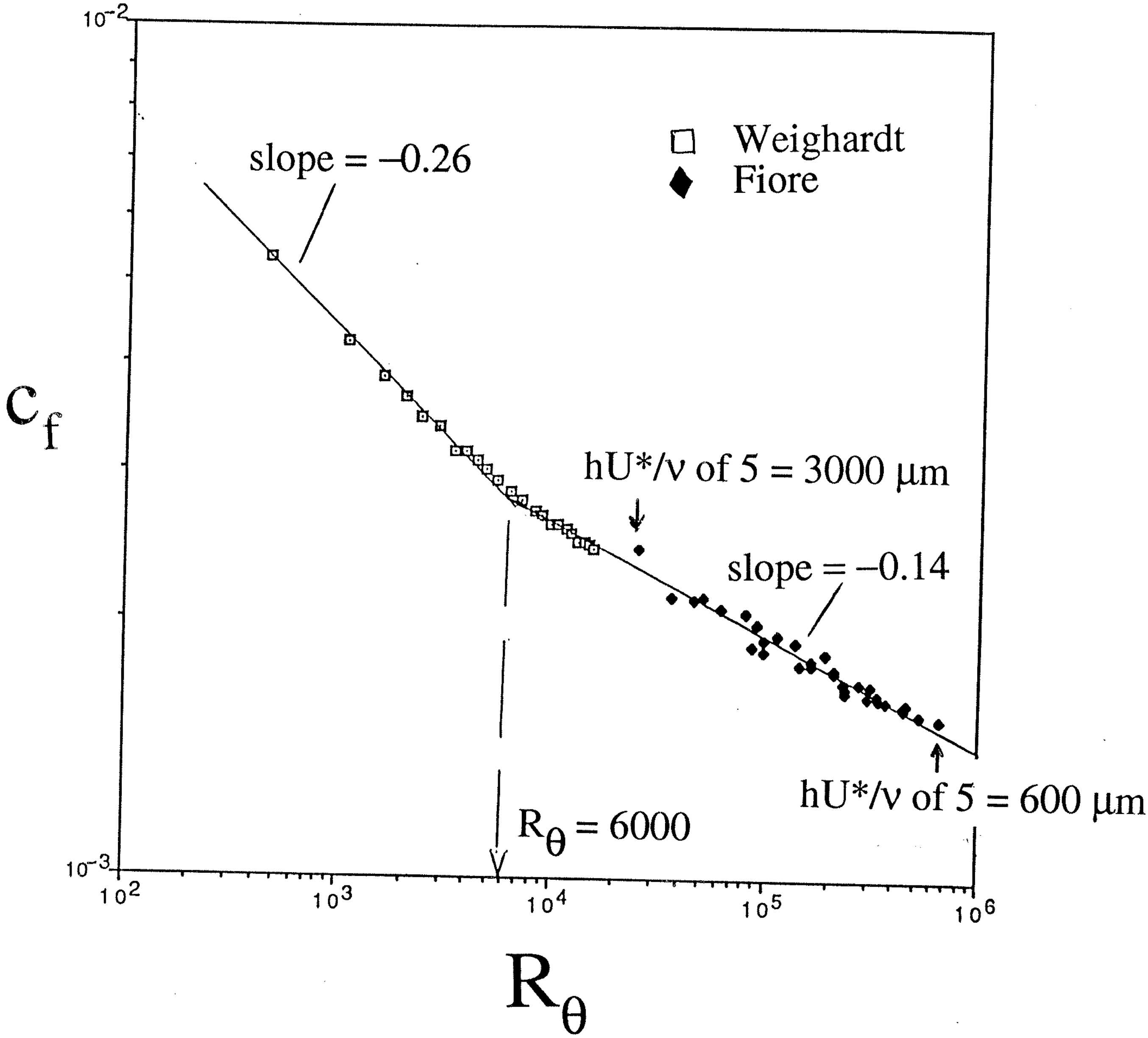
Figure 1



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R_θ = 6000

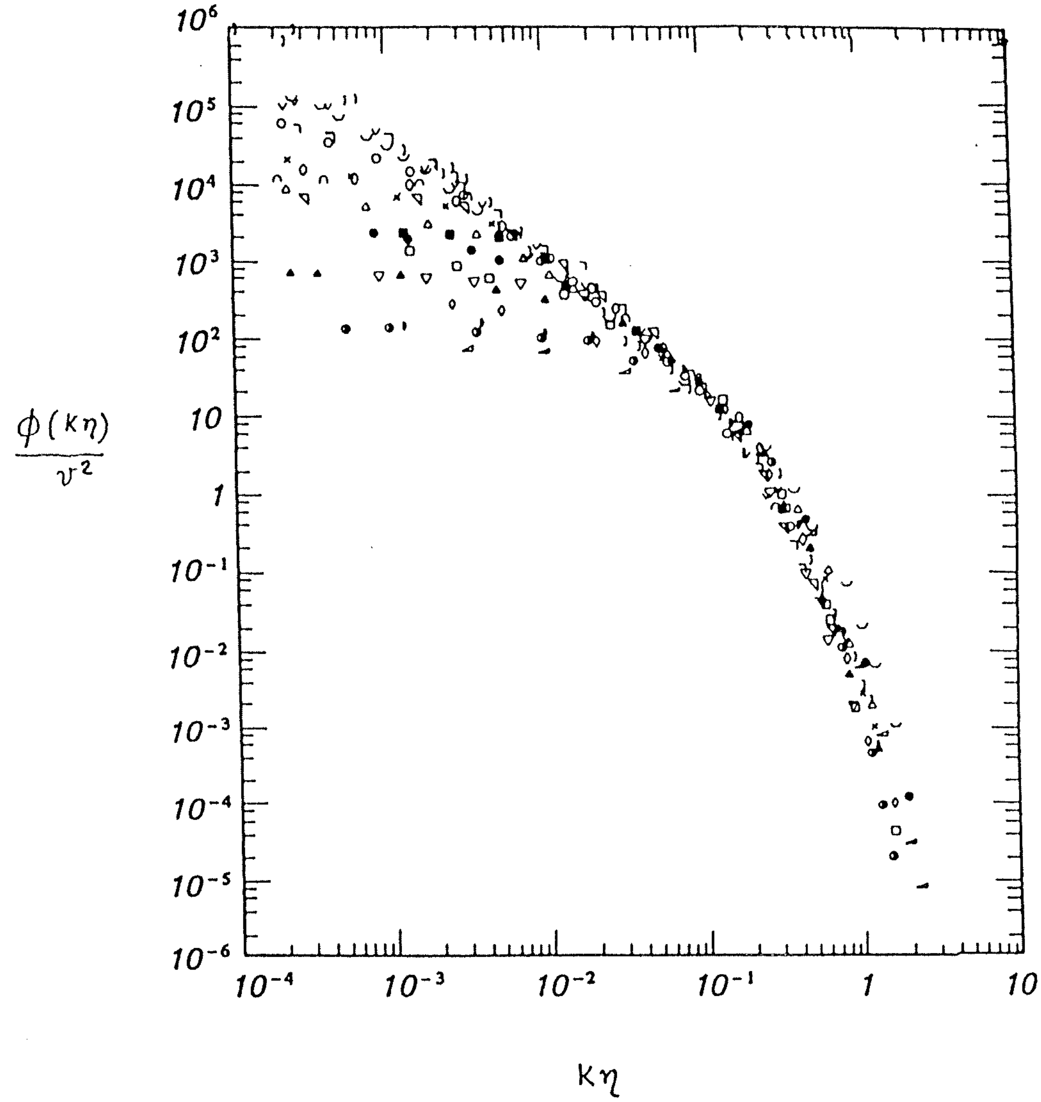
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figure 3