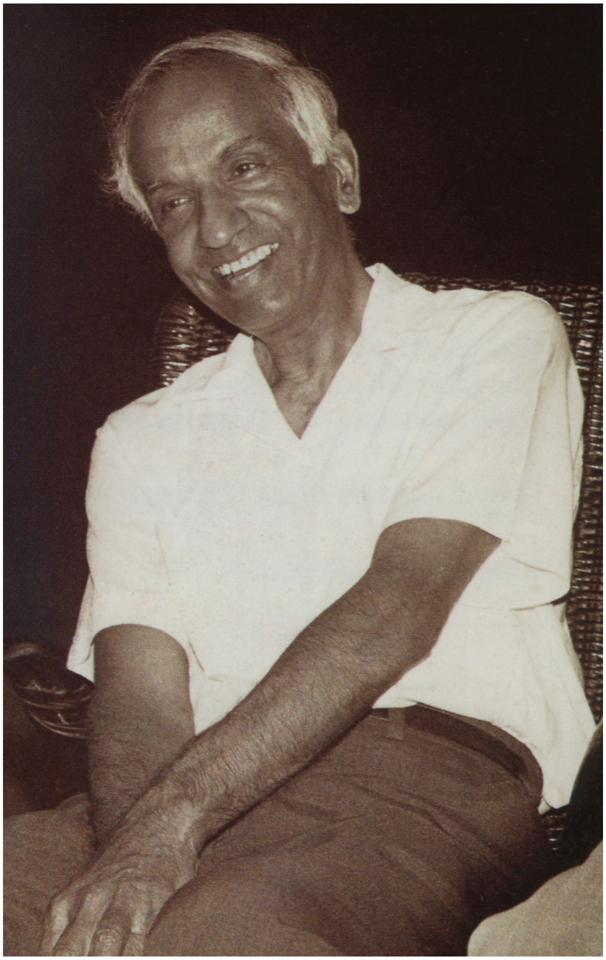

Subrahmanyan Chandrasekhar

ANNUAL REVIEWS

Annual Review of Fluid Mechanics

Chandrasekhar's Fluid Dynamics

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S. Chandrasekhar, hydrodynamic and hydromagnetic stability, fluid turbulence, MHD turbulence, history of science

Abstract

Subrahmanyan Chandrasekhar (1910–1995) is justly famous for his lasting contributions to topics such as white dwarfs and black holes (which led to his Nobel Prize), stellar structure and dynamics, general relativity, and other facets of astrophysics. He also devoted some dozen or so of his prime years to fluid dynamics, especially stability and turbulence, and made important contributions. Yet in most assessments of his science, far less attention is paid to his fluid dynamics work because it is dwarfed by other, more prominent work. Even within the fluid dynamics community, his extensive research on turbulence and other problems of fluid dynamics is not well known. This review is a brief assessment of that work. After a few biographical remarks, I recapitulate and assess the essential parts of this work, putting my remarks in the context of times and people with whom Chandrasekhar interacted. I offer a few comments in perspective on how he came to work on turbulence and stability problems, on how he viewed science as an aesthetic activity, and on how one's place in history gets defined.

On one occasion, now more than 50 years ago, Milne reminded me that posterity, in time, will give us all our true measure and assign to each of us our due and humble place; and in the end it is the judgment of posterity that really matters. And Milne further added: He really succeeds who perseveres according to his lights, unaffected by fortune, good or bad. And it is well to remember that there is in general no correlation between the judgment of posterity and the judgment of contemporaries.

—S. Chandrasekhar (Srinivasan 1996, p. xiii)

1. INTRODUCTION

Subrahmanyan Chandrasekhar, known to many as Chandra, particularly in the West, was born on the 19th of October, 1910 (19-10-1910, as the British style would represent it, making it unforgettable). His birthplace was Lahore, then part of British India. Parenthetically, two other Nobel Laureates were born within a 100-mile radius from Lahore, not too distant in time: Har Gobind Khorana (1922–2011), who won the 1968 Prize in Physiology or Medicine for protein synthesis, and Abdus Salam (1926–1996), who won the 1979 Prize in Physics for electroweak unification. Chandra's father was stationed at that time in Lahore as Deputy Auditor General of the Northwestern Railways of India, but the family soon settled in Madras (now Chennai), the cosmopolitan city closest to Chandra's ancestral home in the Tanjavur district of Tamilnadu. His family placed great emphasis on learning and scholarship, and his father's younger brother, Sir C.V. Raman, won the 1930 Nobel Prize in Physics for his discovery of the Raman effect. Chandra's siblings as well as his nieces and nephews form an illustrious group.

Chandra received his schooling at home until the age of 12, and after four years of high school, he joined the Presidency College in Madras. He completed a bachelor of science in physics with honors at 19 and traveled soon after to Cambridge, England, for postgraduate study on a Government of India scholarship. While on this voyage, he developed a theory about the nature of stars, which questioned the common scientific notion of the time that all stars, after burning up their fuel, became faint, planet-sized remnants known as white dwarfs. Assuming the relativistic equations of state to hold, Chandra determined that stars with masses greater than about 1.4 times the solar mass, now known as the Chandrasekhar limit, must evolve to states different from white dwarfs. This is the theory for which he was awarded the Nobel Prize in 1983, some 50 years later (see **Figure 1**). Now we know that massive stars explode as supernovae and eventually collapse into neutron stars or black holes, which are objects of enormous density.

Why it took so long for the Nobel Prize to be awarded is a topic of much discussion in the literature, some of which is sensitively and wonderfully recounted by Wali (1991). Briefly, it is well known that the distinguished astronomer Sir Arthur Eddington ridiculed Chandra's theory by calling it a reductio ad absurdum in the Royal Astronomical Society meeting of January 11, 1935. Other eminent scientists at Cambridge and elsewhere supported Chandra privately but would not break ranks with Eddington in public, and the admiration that Chandra himself had, and continued to have, for Eddington would not allow him to engage the latter in public debate. Miller (2005) wrote, "Chandra's discovery might well have transformed and accelerated developments in both physics and astrophysics in the 1930s. Instead, Eddington's heavy-handed intervention lent weighty support to the conservative community astrophysicists, who steadfastly refused even to consider the idea that stars might collapse to nothing. As a result, Chandra's work was almost forgotten" (p. 150). In essence, the fog that had descended on Chandra's theory took a long time to be lifted. It is hard to overestimate the impact of Eddington's rejection of Chandra's theory on the latter's scientific and personal life.

In 1936, Chandra moved to the United States and joined the Yerkes Observatory at Williams Bay in Wisconsin (the Department of Astronomy and Astrophysics at the University of Chicago

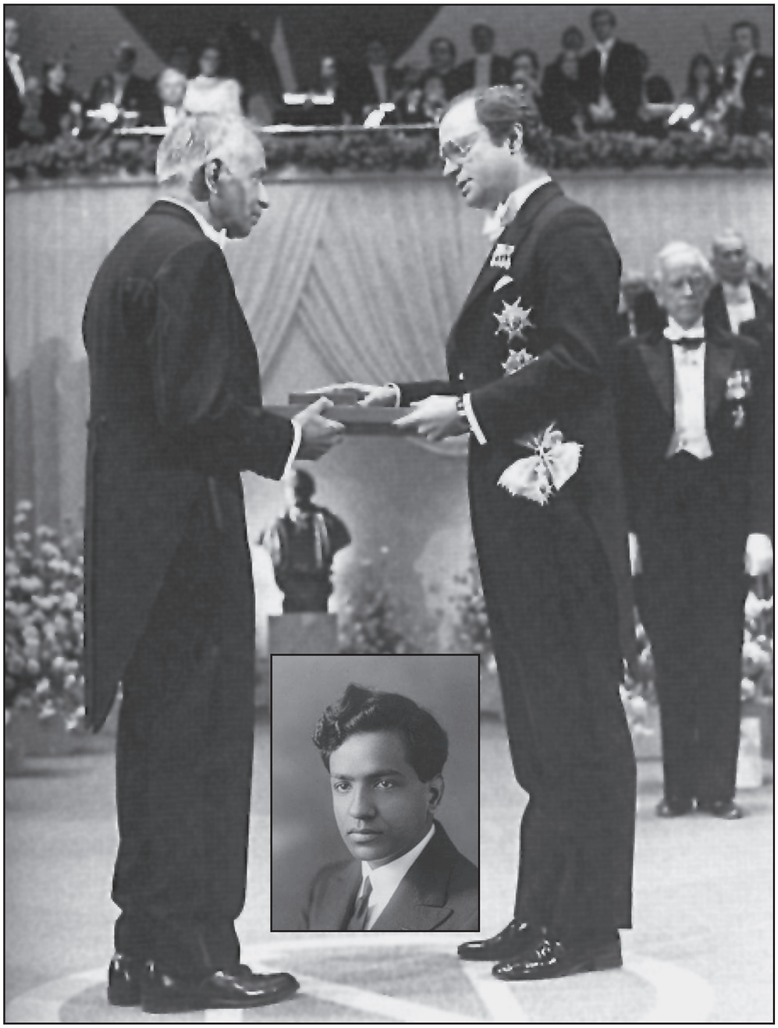


Figure 1

Chandrasekhar receiving the Nobel Prize in Physics from King Gustav of Sweden on December 10, 1983. (*Inset*) Chandra roughly around the time he did his Nobel Prize work, some 50 years earlier. Photos printed with permission from the Special Collections Research Center, University of Chicago Library.

resided there at that time). In 1948, he moved to the main campus in Chicago, where he became one of its most prestigious professors, held in awe by his students and colleagues alike. Although perhaps expressing the reverent end of opinions on Chandra, Osterbrock (1996) recalled, "A few thought of him as a god; most recognized him as an exceptional human being" (p. 229). Chandra was gentle on most occasions but could be quite tough or difficult on some. Osterbrock (1996) presented the following picture: "Chandra could be very demanding, dictatorial, sometimes even insulting with students he did not know well, although in his own mind he was simply trying to impress them with the necessity of more study to achieve understanding. . . . Chandra drove more than one student out of Yerkes Observatory, but surely not everyone who came there should expect to get a degree automatically, he would have replied" (p. 204). He was the Morton D. Hull Distinguished Service Professor from 1952 until 1986 when he became emeritus professor. He continued to work in Chicago until his death in 1995.

Chandra worked in several areas of physics at different periods of his long scientific career, spanning some 65 years. He developed a working style in which he would give up a field altogether once he took stock of it in the form of a monograph and start afresh in another, almost like a novice. In his own words,

> There have been seven such periods in my life: [1] stellar structure, including the theory of white dwarfs (1929–1939); [2] stellar dynamics, including the theory of Brownian motion (1938–1943); [3] the theory of radiative transfer, including the theory of stellar atmospheres and the quantum theory of the negative ion of hydrogen and the theory of planetary atmospheres, including the theory of the illumination and the polarization of the sunlit sky (1943–1950); [4] hydrodynamic and hydromagnetic stability, including the theory of the Rayleigh–Bénard convection (1952–1961); [5] the equilibrium and the stability of ellipsoidal figures of equilibrium, partly in collaboration with Norman R. Lebovitz (1961–1968); [6] the general theory of relativity and relativistic astrophysics (1962–1971); and [7] the mathematical theory of black holes (1974–1983). (Chandrasekhar 1983a)

Chandra did not explicitly include turbulence in item 4, but he invested a good part of the dozen or so of his fluid dynamics years on it, as we see below in Section 2.

Chandra was scientifically active for a dozen or so years (1983–1995) after making the above statement, during which he worked on the theory of colliding gravitational waves, on nonradial perturbations of relativistic stars, and, finally, as a labor of love, on *Newton's* Principia *for the Common Reader* (Chandrasekhar 1995), published just a few weeks before his death. During all these years, he immersed himself in research and teaching and supervised some 50 PhD students [some half-dozen of them in fluid dynamics, particularly in hydromagnetics, or magnetohydrodynamics (MHD) as it is now better known]. More than 100,000 of his scientific books have been sold; his article on Brownian motion (Chandrasekhar 1943a), an interest that was triggered by its relevance to the theory of stellar evolution, has alone been cited about 6,500 times (Web of Science by Clarivate Analytics). He received many accolades for his work from all over the world, including the US National Medal of Science (1966), the Padma Vibhushan from the Government of India (1968), the Copley Medal of the Royal Society (1984), and, as already stated, the 1983 Nobel Prize in Physics for his work on item 1 in the quotation above. Besides the well-known Chandrasekhar limit on white dwarfs, Chandra's name is attached to the Chandra X-ray Observatory (the NASA telescope specially designed to detect X-ray emissions from very hot regions of the universe), Chandrasekhar friction (the loss of momentum and kinetic energy of moving bodies through gravitational interactions with surrounding matter), the Chandrasekhar number (a dimensionless quantity used in magnetic convection to represent the ratio of the Lorentz force to the viscous force), and the Chandrasekhar virial equations (a hierarchy of moment equations of the Euler equations), among others. For some 20 years, Chandra also served as the managing editor of *The Astrophysical Journal* and built it up to be the leading journal in the field.

Why was Chandra motivated to work in fluid dynamics, a subject in which he had no formal pedigree? What did he accomplish, and how much of it is lasting (with the hindsight of some 50–60 years)? What were his interactions with his contemporaries, his moments of glory and agony? What was the larger perspective that drove his science? This article is an attempt to address these questions. Given the constraints of space, however, it cannot do full justice to Chandra's prolific work or to his extraordinary personality.

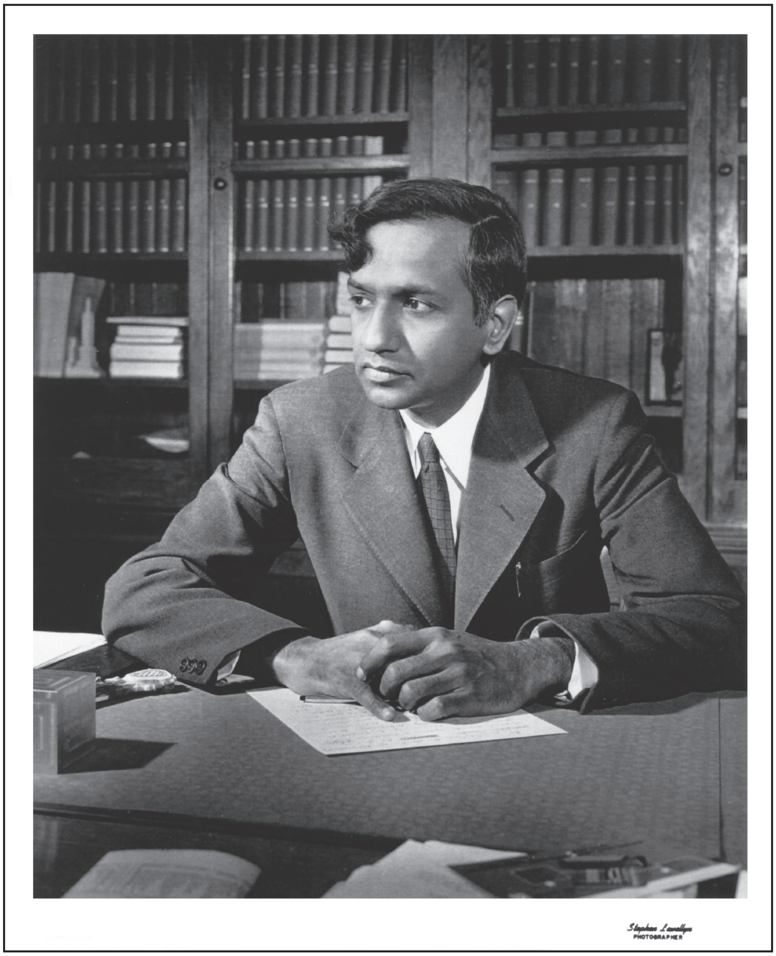


Figure 2

Chandrasekhar around the time he was engaged in his fluid dynamics work. Photo printed with permission from the Special Collections Research Center, University of Chicago Library.

A quandary for any biographer of Chandra is the enormity of the primary material available, some of it penned by Chandra himself, preserved at the Special Collections Research Center at the University of Chicago Library. Also available is a wealth of secondary material written by his family, friends, colleagues, and admirers. Many of the quotes below are from the correspondence stored in the University of Chicago archives. Other quotes are from Chandra's autobiographical diary that Wali (2011) published after Chandra's death. **Figure 2** is a photograph of Chandra around the time he was engaged in his fluid dynamics work.

2. CHANDRA'S TURBULENCE RESEARCH

Chandra's interest in turbulence was kindled in the late 1940s when he began to realize that "we cannot expect to incorporate the concept of turbulence in any essential manner without a basic physical theory of the phenomenon of turbulence itself" (Wali 2011, p. 5); he should be given much credit for raising awareness among astrophysicists to turbulence. This was the time when the works of G.I. Taylor, T. von Kármán, and L. Howarth had been well digested; A.N. Kolmogorov's paradigm was made available to the Western world by G.K. Batchelor at Cambridge University, whose own substantial contributions on homogeneous turbulence were being made at a rapid pace; and W. Heisenberg had reentered the field for a few years because of postwar circumstances. This section summarizes the work that arose from Chandra's interest in the subject. (C.F. von Weizsäcker wrote an important paper side by side with Heisenberg's, but Chandra did not comment on it—although, in an unrelated context, he spoke very highly of Weizsäcker as a scientist.)

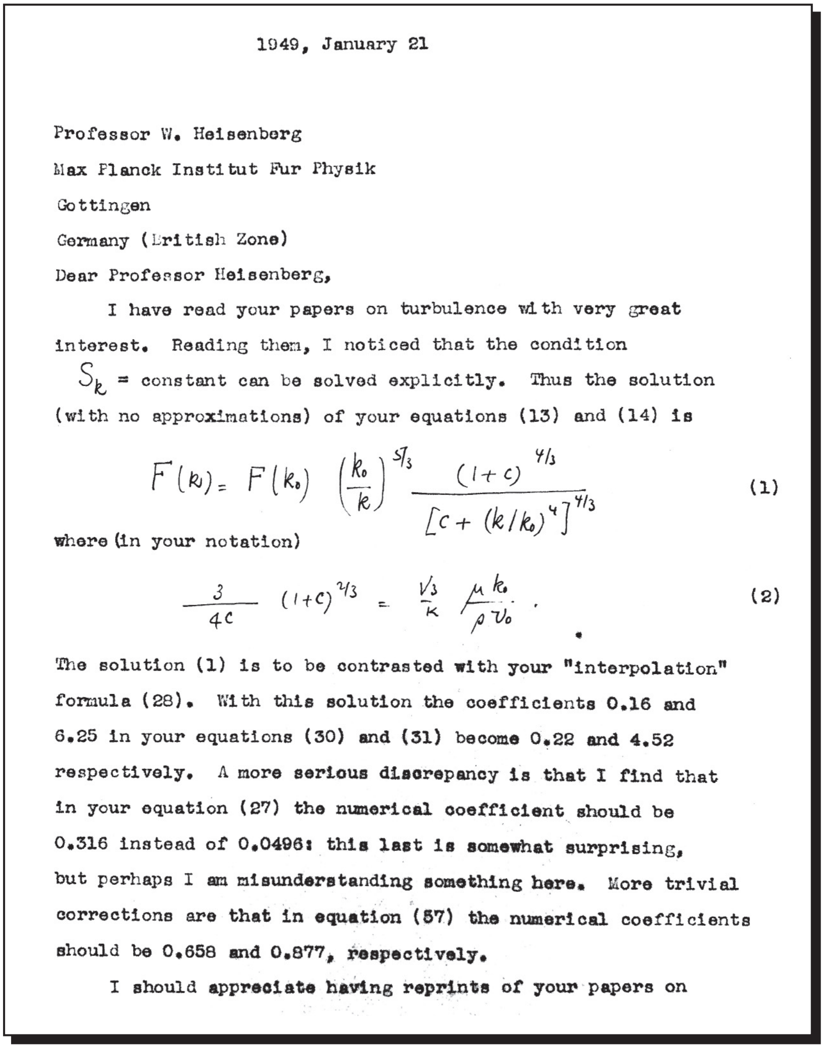
2.1. Heisenberg's Similarity Theory

Chandra's first foray into turbulence (Chandrasekhar 1949a,b,c) was built on Heisenberg's wartime work, for whom hydrodynamic turbulence was a subject of long-standing interest, and whose pioneering thesis on the stability of parallel shear flow and turbulence was published in 1924. Heisenberg (1948) assumed the energy transfer across wave numbers in isotropic turbulence to be governed by a wave number–dependent eddy viscosity that can be written, by dimensional arguments akin to those of Kolmogorov (1941), as $\alpha \int_\kappa^\infty \frac{d\kappa}{\kappa^{3/2}} \sqrt{E(\kappa)}$, where α is a constant and $E(\kappa)$ is the energy spectral density in wave number κ [i.e., $\int dk\, E(k)$ is the kinetic energy]. He derived a closed-form integro-differential equation for $E(\kappa)$; here and elsewhere, we use Batchelor's (1953) notation. The equation can be written (with ν as the kinematic viscosity) in the form:

$$\epsilon = 2\left[\nu + \alpha \int_\kappa^\infty \frac{d\kappa''}{\kappa''^{3/2}} \sqrt{E(\kappa'')}\right] \times \int_0^\kappa E(\kappa')\kappa'^2\, d\kappa'. \qquad 1.$$

Heisenberg did not solve Equation 1 but obtained, in the appropriate limit of the inertial range, the Kolmogorov-Obukhov-Onsager-Weizsäcker result, $E(\kappa) \sim \kappa^{-5/3}$; he deduced $E(k) \sim \kappa^{-7}$ in the far-dissipation range. Chandra solved the integral equation explicitly for the stationary case and, for the decaying case, integrated it numerically by first reducing it to a differential equation in similarity variables (Chandrasekhar 1949c); readers are referred to figure 7.10 in Batchelor (1953). He wrote about his results to Heisenberg (see **Figure 3**), who charmingly stated, in a letter on January 27, 1949, that he was "quite ashamed that I have not seen the solution myself," and pointed out that Batchelor and J.C. Rotta in Göttingen had been working on the same problem. Chandra's initial enthusiasm for Heisenberg's work was moderated when he learned from J. von Neumann, in a colloquium that Chandra gave at Princeton in the spring of 1949, that the κ^{-7} power law in the far-dissipation range did not have experimental support.

A slight digression is useful as the background for later comments. After learning from Heisenberg about Batchelor's work, Chandra promptly wrote to the latter (the first letter seems to have been lost, and the earliest letter in existence is dated March 25, 1949). Thus began an intense and frequent correspondence, with letters sent sometimes less than a week apart, some three dozen pairs in all, most of which are available in the archives of the University of Chicago; this correspondence lasted until 1952 when the draft of Batchelor's influential monograph, the *Theory of Homogeneous Turbulence*, was completed (Batchelor 1953). Later correspondence was sporadic and insubstantial. During the years leading up to about 1952, Chandra had varying levels of discussions on turbulence with others such as J.M. Burgers, E. Hopf, R.H. Kraichnan, T.D. Lee, C.C. Lin, W.V.R. Malkus, and G.I. Taylor, but the bulk of the correspondence was with Batchelor. The two were supportive of each other in several respects—not to be discussed here since it transcends their scientific work. In particular, in the letter of January 2, 1951, Batchelor wrote, "I know of very few schools where turbulence is considered seriously from the theoretical side. I guess that



1949, January 21

Professor W. Heisenberg

Max Planck Institut Fur Physik

Gottingen

Germany (British Zone)

Dear Professor Heisenberg,

I have read your papers on turbulence with very great interest. Reading them, I noticed that the condition S_k = constant can be solved explicitly. Thus the solution (with no approximations) of your equations (13) and (14) is

$$F(k) = F(k_0)\left(\frac{k_0}{k}\right)^{5/3} \frac{(1+c)^{4/3}}{[c+(k/k_0)^4]^{4/3}} \qquad (1)$$

where (in your notation)

$$\frac{3}{4c}(1+c)^{2/3} = \frac{V_3}{K}\,\frac{\mu k_0}{\rho v_0}. \qquad (2)$$

The solution (1) is to be contrasted with your "interpolation" formula (28). With this solution the coefficients 0.16 and 6.25 in your equations (30) and (31) become 0.22 and 4.52 respectively. A more serious discrepancy is that I find that in your equation (27) the numerical coefficient should be 0.316 instead of 0.0496: this last is somewhat surprising, but perhaps I am misunderstanding something here. More trivial corrections are that in equation (57) the numerical coefficients should be 0.658 and 0.877, respectively.

I should appreciate having reprints of your papers on

Figure 3

Chandra's first letter to Heisenberg announcing the analytical solution to the latter's equation. Chandra seemingly kept a meticulous copy of all the letters he wrote to others and their responses. The letter is a carbon copy with the equations filled by hand. He kept up this practice until facsimiles became common. Photo printed with permission from the Special Collections Research Center, University of Chicago Library.

you are going as far as you can on a relatively exact basis, and this, to my mind, is the kind of development needed at the present time" (emphasis original).

2.2. Axisymmetric Turbulence

Axisymmetric turbulence is the state of turbulence for which the statistics display symmetry around a preferred direction. Batchelor (1946) had already applied Robertson's (1940) theory of invariants. Starting from there, Chandra observed (Chandrasekhar 1950a) that the second-order symmetrical solenoidal tensor can be written explicitly in terms of two arbitrary scalar functions that can be derived from gauge invariance of a skew tensor. This elementary observation was powerful in tensor manipulations and led Chandra to express axisymmetric tensors in terms of two defining scalar quantities (instead of four in Batchelor's treatment—he said in a letter on December 11, 1949, that he was "rueful for not spotting [it]"). Chandra derived equations for these two scalar functions, essentially bringing the axisymmetric theory (Chandrasekhar 1950a, equations 118 and 119) up to the level of the Kármán–Howarth equation for isotropic turbulence (von Kármán & Howarth 1938). We may recall briefly that von Kármán and Howarth derived the equation for the defining scalar function in the two-point double-correlation tensor for isotropic turbulence in terms of the scalar function defining the two-point triple-correlation tensor. In Chandrasekhar (1950b), Chandra worked out the final period of axisymmetric turbulence to the same level of completion as Batchelor & Townsend (1948) had for isotropic turbulence. In short, this tour de force work completed the analytical theory of axisymmetric turbulence as far as possible without making explicit dynamical assumptions.

2.3. Density Fluctuations in Compressible Turbulence

Chandrasekhar (1951a) derived an expression for the correlation between instantaneous fluctuations at two points and obtained an invariant of motion, which is analogous to the Loitsiansky invariant in the theory of homogeneous turbulence of an incompressible fluid. Using the quasi-Gaussian approximation (more about this below), he was able to relate the properties of density–density correlation to the defining scalar for the two-point velocity correlation function. Finally, for small turbulent Mach numbers, he obtained an expression for the speed of propagation of density fluctuations (Chandrasekhar 1951a, equation 52).

2.4. Effect of Turbulence on the Jeans Criterion

The Jeans stability criterion concerns the collapse of interstellar gas clouds and subsequent star formation in our galaxy. The collapse occurs when the internal gas pressure is weak and cannot prevent gravitational attraction. All scales larger than the so-called Jeans length are unstable to gravitational collapse. Chandrasekhar (1951d) argued the case for including turbulence and used his theory of density fluctuations in isotropic turbulence to generalize the Jeans criterion for stability.

2.5. Magnetohydrodynamic Turbulence

As another endeavor (Chandrasekhar 1951b), Chandra extended the theory of isotropic turbulence to magnetohydrodynamics. MHD turbulence, which describes the motion of an electrically conducting, magnetized fluid, is strictly applicable only to the regime dominated by collisions of charged particles but often provides a useful guide to the behavior of magnetized plasmas in the collisionless limit as well. Incompressible MHD is described by two solenoidal vector fields, the velocity and the magnetic field intensity, which must satisfy the Navier–Stokes and induction equations. For this reason, MHD turbulence is richer than the hydrodynamic case and offers a greater variety of solutions. For Chandra, this presented no particular obstacle. He said in Chandrasekhar (1951b), "Indeed, it will appear that the inclusion of electromagnetic forces does not introduce any essential difficulty which is not already present in our understanding of ordinary turbulence" (p. 435). Chandra's major contributions were to (*a*) use the theory of invariants, following Batchelor (1946), to obtain the forms of the (joint) double and triple correlations of the velocity and magnetic fields; (*b*) derive the equivalent of the Kármán–Howarth equation for

MHD turbulence; (*c*) deduce equations for the dissipation of turbulent and magnetic energy; and (*d*) show that MHD turbulence permits the definition of expressions analogous to the Loitsiansky invariant in hydrodynamic turbulence, based on similar assumptions on the behavior at large separation distances of the second-order correlation functions. Chandra noted the similarity between the equations for magnetic induction and hydrodynamic vorticity and explored the analogy further by defining a vector potential for the magnetic field, analogous to the hydrodynamic case. Chandra's papers (Chandrasekhar 1951a,b) contain nuggets that have not been explored fully [e.g., Shivamoggi (1999) discovered a second Loitsiansky invariant associated with the large magnetic eddy characteristics].

In the next paper in the sequence (Chandrasekhar 1951c), Chandra extended the analysis to include pressure into the second-order tensor correlations of the velocity and magnetic field. Using the quasi-Gaussian assumption, he showed how to express these tensors in terms of the scalar functions of the velocity and magnetic field correlations.

2.6. Convective Turbulence

By restricting attention to the fluid far from the boundaries, Chandrasekhar (1952b) treated turbulence in convection as approximately homogeneous and axisymmetric (with gravity providing the preferred direction) and used his theory of axisymmetric vectors and tensors to characterize multipoint correlations between various field quantities. He developed a closed system of equations for the defining scalars when the nonlinear terms in the equations of motion and heat conduction can be neglected (analogous in spirit to the final period of decay). One point of contention with Batchelor was this: Should such a flow be called turbulence at all or simply a superposition of noninteracting modes of random amplitudes? Chandra simply stuck to his point of view (although the appendix to the paper seems to have been added in response to Batchelor's criticism).

2.7. Chandra's Discontent with Work on Turbulence

At this point, Chandra had authored some dozen papers on turbulence, all of them containing solid results, and he began to feel that that the first outlines of a physical theory were just emerging. Yet he remarked that some of this work had led him merely to "cut my teeth into the subject" (Wali 2011, p. 23) and was dissatisfied with the overall progress (below we see why); he then began to focus more on the topic of stability, a subject in which he had already made considerable progress (see Section 3). In the spring of 1954, Chandra gave a lecture on turbulence, again at Princeton. He later recalled that his colloquium was "moderately 'frivolous' and cutting cruel jokes about the 'superstitions' of the subject and the prevalent complacency in spite of the lack of any really rational theory" (Wali 2011, p. 34).

We should ask why Chandra was down on turbulence at that time, given that he had been engaged in it very productively until about two years prior, and others assigned a high place for his contributions. Indeed, in a letter on October 2, 1949, Batchelor wrote, "I can't imagine how you manage to produce such a large piece of work in the incredibly short time of a month or two." But their correspondence was becoming a bit strident. Batchelor recurrently admonished Chandra on terminology and the physical picture and was entirely aware of (and worried about) the cumulative effect that his comments may have produced. He noted on August 26, 1951, "My remarks seem always to be critical and I hope you do not find them captious; I imagine that critical remarks are useful when they are understood to be given in a proper spirit." At some point, Chandra did think that these remarks had indeed become sterile and pompous. In hindsight, Batchelor's comments are entirely reasonable for those who grew up in his scientific tradition, but one can imagine why someone else, brought up in a different tradition, might not have shared that same burden. What mattered to Chandra was what the equations revealed; everything else was superstition and complacency.

Further, when Chandra began, with the encouragement of N.F. Mott (perhaps among others), to consider writing a monograph on the subject titled "Statistical Theory of Turbulence," he sought Batchelor's opinion in a letter on September 5, 1950. Batchelor responded on the 24th of September: "Your note that you are preparing a monograph on 'The Statistical Theory of Turbulence' embarrasses me considerably, as I am half-way through such a book myself for Cambridge University Press. Our treatments are bound to differ but I feel that the subject matters of the two books are likely to be fairly similar . . . It is just unfortunate that we had the same idea at the same time." Batchelor later regretted that he had expressed his opinion poorly, but it was clear that another book besides his own, covering similar topics, was one too many for him. Chandra promptly gave up the idea by generously remarking that it was, after all, Batchelor's work that inspired his own; indeed, Chandra took off on several fronts that Batchelor had left unexplored or partially explored. Chandra's lecture notes were compiled by E.A. Spiegel many years later under the title *The Theory of Turbulence: Subrahmanyan Chandrasekhar's 1954 Lectures* (Spiegel 2011). These notes contain the standard development found in Batchelor's book (except that they use Fourier series methods in contrast to Batchelor's Fourier–Stieltjes methods), new insights from Fermi and von Neumann on alternative derivations of Kolmogorov's (1941) result, some applications of the Kármán–Howarth equation, and, finally, a discussion of a dynamical theory that Chandra developed; this is described below.

The second and more important reason for Chandra's dismay was his thinking that the progress he had made was formally kinematic and did not touch the central dynamical problem. Thus, he pushed aside turbulence from the front of his occupations around the middle of 1951. However, as he would later recall, Martin Schwarzschild, who was present at the 1954 Princeton colloquium cited above, expressly did not like his frivolity and conveyed it to him privately after the talk; in any event, he asked Chandra what he was going to do about it. Thus began Chandra's second engagement with the subject. With renewed focus, he began in the summer of 1954 a series of seminars on the subject. It was then that it occurred to him that one might choose the set of moment equations by considering the correlations at two different points in space and at two different instances of time. In Chandrasekhar (1955a, p. 4), one finds:

> A description in terms of E(k) only (or Q(r) only) would be complete only if there were no phase relationships between the different Fourier components of the velocity field. But this is not the case. Phase relationships must exist: without them there would be no exchange of energy between the different Fourier components which is, after all, the essence of the phenomenon of turbulence. A theory, albeit an approximate theory, must incorporate in itself some element which describes these phase relationships; without such an element the theory would lack the means of accounting for the essence of the phenomenon. It would appear that by introducing the correlations in the velocity components at two different points and at two different times, we can incorporate features which are the result of these phase relationships.

Chandra developed this theme in two papers on hydrodynamic turbulence (Chandrasekhar 1955a, 1956) and in a sequel to the first on MHD turbulence (Chandrasekhar 1955b). The first paper promptly appeared in *Proceedings of the Royal Society A*. It was clear that he was excited: In writing to Taylor on August 28, 1955, right after submitting the second paper, he said: "I feel that in this way the theory I began in my first paper has met with the real test of a deductive theory. Perhaps I am exaggerating but I am rather excited about the developments and feel moderately anxious for the paper to be published fairly promptly." To his dismay, however, the second paper was rejected by the Royal Society with "a most discourteous report." The referee subsequently withdrew some of his "more blatant remarks" as a result of Chandra's restraint in the face of what he regarded as "insulting behavior." He withdrew the paper and published it in *Physical Review* (Chandrasekhar 1956).

For some time, Chandra continued to correspond on the second paper with the referee and with Burgers, Heisenberg, Kraichnan, Lin, Taylor, von Neumann, and others. Both the rejection by Royal Society (which, rightly or wrongly, he attributed to the "English school") and the negative reaction to the paper by a few others (especially Kraichnan) stung Chandra. Soon after the paper was rejected, he sent a note on November 15, 1955, to Heisenberg (who had already made encouraging remarks about it) complaining about the referee report. In part, the letter said,

> Meanwhile, the Royal Society has rejected the paper on the basis of referees' report which among other things calls the paper "fallacious" and "of no value." I have tried to be as critical as I can, but I cannot see that there is anything unsound in what I have said in Sections 2 and 3 of the paper. These are the sections to which the referees have objected. If you have had a chance to examine those sections, I shall be most grateful for any criticisms you may have. The referees of my paper have used rude language; but they have not stated arguments with substance. If I have gone astray I should like to know where; and I should appreciate any comments you have on these general ideas.

It is not clear how or whether Heisenberg responded. Chandra complained similarly to Lin, especially about the letters he had been receiving from Kraichnan, who, he said, was flooding him with "reports almost every week." Lin was sympathetic to Chandra's unease with Kraichnan.

What was Chandra's work about and to what did these people just mentioned, accomplished in their own right, object strenuously enough to cause him distress? A brief description of the hydrodynamic part is useful. The substance of Chandra's objection to Kolmogorov's universality theory in the inertial range was that the large scales would have to appear through boundary conditions at infinity, which would invariably render the theory nonuniversal. The notion that the large scale would make its presence felt in the inertial range has been accepted since and has been at the heart of much work that has followed (see, e.g., Frisch 1995, Sreenivasan & Antonia 1997). Instead of the standard structure functions (Kolmogorov 1941), Chandra sought to remedy the situation by considering $\chi = \partial\psi(r,t)/\partial r$, with $\psi(r,t) = \overline{(u' - u'')^2}$ and u' and u'' denoting the velocities in the x-direction (say) at two points on the x-axis separated by a distance r and at times separated by an interval t. When Kolmogorov similarity principles are applied to χ, it obeys the form

$$\chi = (\epsilon^3/\nu)^{1/4} X\,[r(\epsilon/\nu^3)^{1/4}, t(\epsilon/\nu)^{1/2}], \qquad 2.$$

where X is a universal function of the arguments specified, and ϵ and ν are the energy dissipation rate and kinematic viscosity, respectively. In the limit of zero viscosity (or infinite Reynolds number), it is easy to see that ν disappears only when X takes the special form

$$X \to r^{-1/3}\sigma(t/r^{2/3}). \qquad 3.$$

The boundary conditions on $\sigma(x)$ are that $\sigma = \sigma^*(>0)$ and $\mathrm{d}\sigma/\mathrm{d}x = 0$ at $x = 0$ and that σ tends to zero as x tends to infinity. Chandra's point was that this would not introduce the large scale into the theory for χ (unlike that for ψ), making it a more likely candidate for universality. He set up a differential equation for σ and solved it numerically (modulo some scale factors). It is in setting up this equation that he used the quasi-Gaussian assumption.

Given his enormous devotion to clarity of expression, it is surprising that Chandra had made several poorly considered statements in his first paper. For example, he initially regarded symmetry to prevail in χ with respect to time t, a point he later withdrew in private correspondence with

Burgers; he had not commented in any of his papers on the difficulties in assuming homogeneity and stationarity simultaneously, which occupied a good part of what he had to defend. The concept that stationarity could be maintained by forcing turbulence at the large scale *à la* Kraichnan was still unfamiliar to many at that time. But the most serious weakness of the theory that was persistently raised was the use of the quasi-Gaussian assumption: For the boundary conditions applicable to Chandra's problem, Kraichnan (1957) demonstrated that realizability [i.e., everywhere nonnegativity in the function space of $\hat{u}(\mathbf{x}, \mathbf{t})$, where the hat denotes the Fourier-transformed velocity] would not hold; Kraichnan drove this point home also in various ways in several letters to Chandra (see below). Lin, too, voiced similar criticism, although far more gently.

As is well known, the quasi-Gaussian approximation for simultaneously measured velocity statistics was introduced for homogeneous turbulence by Millionshchikov (1941) and explored by Heisenberg (1948), Obukhov (1949), and Batchelor (1951)—as well as by Chandrasekhar (1951c). Although its shortcomings were voiced, for example, by Batchelor (1951) and Proudman & Reid (1954), it was used as a plausible model on the basis of which specific results could be derived. Kraichnan (1957) particularly pointed out that the two-time version under the quasi-Gaussian approximation led to the conclusion of a net-positive flow of energy by nonlinear interactions to all wave numbers, without there being a negative contribution even for distant wave numbers—and this cannot be correct since the task of nonlinear interactions is to distribute energy among wave numbers without dissipating it. Kraichnan was certainly aware that any particular application of this approximation could be benign despite this problem of principle, but of course, Chandra was interested in matters of principle. The defects of the quasi-Gaussian approximation became quite crystallized in the literature when Ogura (1963) showed that the energy spectrum assumes substantially negative values right in the middle of the energy-containing range.

Subsequently, Kraichnan, Wyld, Edwards, Herring, and others (see Leslie 1973) produced a new class of theories that did not suffer from the shortcoming of realizability, although they had other defects. These theories did not hold the $-5/3$ spectrum to be sacrosanct: For instance, Kraichnan (1959) spent considerable effort making just the point that the $-5/3$ power had no serious experimental support, claiming further that the $-3/2$ power of his direct interaction approximation theory was quite reasonable. However, Grant et al.'s (1962) measurements of a tidal channel at very high Reynolds numbers put an end to that line of argument, and faith in the $-5/3$ spectrum was restored shortly thereafter. A good part of Kraichnan's own effort in subsequent years, and that of others such as McComb (2014), was oriented toward making this new class of theories compatible with the $\kappa^{-5/3}$ spectrum. In particular, primarily as a result of Kraichnan's work, it became clear that the effect of large-scale sweeping is to invalidate the self-similarity assumption for two-time correlation functions.

Another brief comment on the subsequent development is in order. Starting from the failure of the quasi-Gaussian assumption, Orszag (1970) (and later work) developed the so-called eddy-damped quasi-normal Markovian model, which seems to have served several important roles in turbulence modeling (see Sagaut & Cambon 2008). While the important effects of phase relationships cannot be incorporated as easily as Chandra assumed for space–time correlations, understanding its limitations has since led to new developments, but of the sort in which Chandra would not likely have taken any interest.

For completion, we may note another flaw in Chandra's work and, in fact, in all the work of that time: It failed to recognize that the strong fluctuations in turbulence render it a multiscale problem for which a simple scaling function, as in critical phenomena, does not seem to exist. Furthermore, strong dissipation fluctuations mean that the effective boundary conditions would have to be modified in a theory such as Chandra's, much as the presence of strong fluctuations in the wall region of a turbulent boundary layer alters the effective boundary conditions in large-eddy simulation methods. Although our understanding of the problem has been continually improving, we are not yet in a position to come up with a full theory.

Returning to Chandra, it is clear that he was quite disappointed by the reactions his work engendered. As he would recollect years later, "It was about the most frustrating in my entire experience." Thus, his interest in turbulence as a subject of scholarly study came to an abrupt end. One may speculate that this abrupt end (which he seems to have occasionally regretted) is part of the reason that his turbulence work does not receive as much attention. Buried in this general inattention is his first-rate work on kinematics of hydrodynamic and hydromagnetic turbulence. However, I will not develop that theme here. The **Supplemental Appendix** lists Chandra's important papers on turbulence.

Supplemental Material >

3. HYDRODYNAMIC AND HYDROMAGNETIC STABILITY

By the fall of 1951, Chandra was beginning to feel that he had gone as far as possible on the formal development of turbulence (although, as described earlier, he did return to the subject), and he began to focus on problems of hydrodynamic and hydromagnetic stability. His interest began with the calculation of the effects of inhibition of convection instability by magnetic fields. At different times until 1960, he wrote about 50 substantial papers on the subject (see the **Supplemental Appendix**), covering some 600 pages of densely written material, and it is impossible to summarize all of them adequately. Fortunately, it is not necessary to do so because his monumental book of 654 pages on this subject was published by Oxford University Press (Chandrasekhar 1961) and has been reprinted several times, including in a low-cost Dover edition. During these years, Chandra carried on scientific correspondence with a number of rising stars, including Bill Reid, Paul Roberts, Norman Lebovitz, Russell Donnelly, Dave Fultz, Peter Vandervoort, Yoshinari Nakagawa, and Eugene Parker, among others. At least to me, however, this correspondence does not reveal as much about the person as his correspondence on turbulence does, so in this section I mostly focus on the work itself.

In his book, Chandra considered thermal convection with and without rotation, and with and without a superimposed magnetic field; Couette flows with and without magnetic fields; Rayleigh–Taylor (RT) and Kelvin–Helmholtz (KH) instabilities; gravitational equilibrium and instability; the onset of thermal instability in fluid spheres and spherical shells; and other miscellaneous stability problems of jets, cylinders, and gravitational masses. In any one topic, hydromagnetic instability for instance, he considered all possible orientations with respect to gravity of the rotation axis and the direction of the magnetic field.

Several questions arise: How much original work does the book contain? How well is the material presented? What drove him to consider a plethora of permutations, essentially using similar tools? How was the book received when it was published, and what is in it for today's students of fluid dynamics, some 50-plus years after its publication? What aspects of this rich subject did he not include? These are some questions I now address in an intertwined way.

Even though part of the book was based on his own work, almost all the numerical work was redone for the book, a certain amount of new material was worked out, and clarifications were produced on topics such as Boussinesq approximation, vorticity theorems, the Taylor–Proudman theorem, the treatment of toroidal and poloidal functions, and wave propagation in rotating systems. Thus, the book brought a considerable unity of approach to the treatment of many of the typical problems in hydrodynamic and hydromagnetic stability. For the theoretician that he clearly was, Chandra displayed no snobbishness with regard to integrating rigorous analysis with numerical calculations and recourse to experiment, as the problem required. Chandra aimed for his book to possess "a certain logical structure with symmetry and pattern" (Wali 2011, p. 46), and that is what he produced.

L.N. Howard, a distinguished fluid dynamicist himself, had this to say about Chandra's book (Howard 1962, p. 158):

> This extensive and impressive work is devoted to a number of topics in stability theory, selected principally from those to which the author has made so many contributions, both personally and by the guidance and inspiration he has given to the work of his students . . . The presentation throughout is systematic and thorough and mostly authoritative . . . . The systematic theoretical treatment, the compact presentation of the results of many difficult numerical calculations, the discussion of experimental results and the extensive bibliography make this an extremely useful book for reference purposes—one which will be wanted in the library by all, and on the desk by many, of those whose work is connected with hydrodynamic or hydromagnetic stability.

The comprehensiveness of Chandra's book elicited the following reaction by another reviewer (Gillis 1962, p. 58):

> It is now at least half a century since it became clear to applied mathematicians that it would henceforth be prudent, before ever publishing any of their research, to check whether it had not already been done by Rayleigh. The time has come to amend this rule to read "Rayleigh or Chandrasekhar." The latter's newest book, representing only one facet of his many-sided work, will stand for a long time as a text on problems and methods, a reference work of results, and a monument to the scientific power and erudition of its author.

It is well to remember that Chandra was highly active for more than 30 years and covered several more areas after this statement about his "many-sided work" was made. In a certain sense, Chandra's book closed a chapter on linear stability on a variety of problems, and further work has taken off on nonlinear stability, which has relied much more on numerical work.

Chandra's work on stability can be usefully separated into his original contributions and those that he "merely" extended and recast more elegantly. I put quotes around "merely" because, in science at large, elegant reformulation of a problem in itself results in further important developments—and many have discovered new insights in Chandra's formal development of known earlier work (see comments below). It is clear that Chandra shaped the study of how magnetic fields and rotation affect flow stability in a variety of configurations.

3.1. Rotating Convection and Magnetoconvection

The standard problem in thermal convection, the so-called Rayleigh–Bénard stability, concerns the evolution of a fluid layer of constant density placed between two closely spaced horizontal plates maintained at a constant temperature gradient against gravity, and the buoyant force that drives convection is provided by the thermal expansion coefficient. Chandra beautifully laid out this problem in the early part of his book. Naturally because he came to fluid dynamics from astrophysics, where rotation and the magnetic field are both important, and where the gravity, rotation axis, and the direction of the field could all be in different orientations, Chandra devoted the first eight chapters of his book, accounting for slightly less than half of its size, to a variety of problems illustrated by the following titles and subtitles: the thermal instability of a layer of fluid heated from below, the effect of rotation, the effect of a magnetic field, the combined effect of rotation and magnetic field, the case of the magnetic field and gravity acting in different directions, combinations of rigid and free boundaries, etc. One should not confuse the variety of problems Chandra considered for a lack of any particular discernment on his part, and he reworked most major aspects of earlier work.

Both rotation and the magnetic field inhibit convection (that is, the critical Rayleigh number increases, as does the critical wave number, with rotation and with the imposed field strength), but for different physical reasons. An inviscid flow is stable against convective instability because of the Taylor–Proudman theorem, according to which all slow motions in a rotating system are two-dimensional and resist the tendency to overturn. Detailed calculations show that the instability in convection appears as an overstability with oscillatory motion, and the convection system is overstable at small Prandtl numbers but unstable at large Prandtl numbers. There is no comparable effect to Taylor–Proudman's theorem in the magnetic field. However, as Chandra illustrated, the inhibition of convection occurs because a vertical magnetic field drives the fluid layer essentially to the Taylor–Proudman state. The relative magnitude of the Lorentz force created by the impressed field (only the component along gravity matters) to viscous forces is expressed in terms of what is now known as the Chandrasekhar number Q, given by

$$Q = \frac{B^2 H^2}{\mu_o \rho \nu \lambda}, \qquad 4.$$

where B is the impressed magnetic field strength, H is the height between the plates of the convection apparatus, μ_o is the magnetic permeability, ρ is the fluid density, ν is its kinematic viscosity, and λ is the magnetic diffusivity. (The Hartmann number, Q^2, is named after J. Hartmann, whose work from 1937 obviously predated Chandra's.) Subsequent theoretical predictions (Chandrasekhar 1952a, 1954) were verified experimentally by his Chicago colleagues, primarily D. Fultz and Y. Nakagawa (and their collaborators).

3.2. Couette Flows

The flow between concentric cylinders, the classic example of Couette flows, was studied experimentally and theoretically by Taylor (1923). Rereading that paper, one is struck by how accurately Taylor understood the significance of the work and how, in that respect, one cannot do much better even today with the hindsight of some 95 years. One is also struck by the enormously complicated algebra that Taylor employed in solving the sixth-order differential equation. Chandra met Taylor in the winter of 1951 in Berkeley, which may well have motivated him to think about Taylor–Couette flows. Chandra's contribution was to bring to the problem the stability machinery that was developed after Taylor (1923); from his autobiographical remarks (Wali 2011), it is clear that it took Chandra about two years of thinking, off and on, to complete the work. Chapter VII is the result.

There are two new parts of Chandra's contributions. First, he considered (see, e.g., Chandrasekhar 1953) more general cases of Couette flow (chapter VIII), for example, those with axial pressure gradients. The superposition of the axial flow over the rotational flow introduces new elements; for instance, the critical Taylor number increases with the Reynolds number of the axial flow, a prediction that was verified in experiments by Donnelly & Fultz (1960). Second, Chandra considered the stability of the Couette flow containing a conducting fluid in the presence of an imposed magnetic field—axially, azimuthally, and in combination, with and without viscosity (chapter IX). While the magnetic field lines tend to stabilize the flow, their detailed effects are different in each case, for which one should indeed consult Chandrasekhar (1961). Comparisons with the mercury experiments of Donnelly & Ozima (1960) led Chandra to conclude that "the experiments amply confirm the broad aspects of the theoretical predictions" (Chandrasekhar 1961, p. 426).

3.3. Rayleigh–Taylor Instability

RT instability (chapter X) occurs when a fluid layer of heavier density accelerates into a fluid layer of lower density. This instability is important in astrophysical contexts such as Type II supernovae; it is the same instability that deforms the mixing interface in inertial fusion. Chandra did not state that he was driven by any such applications, and he presented with no fanfare the solution for the effects of vertical and horizontal magnetic fields; he found it possible to include surface tension effects as well. It can be expected, again, that the magnetic tension inhibits instability. The effect of the vertical field is essentially akin to the surface tension effect that inhibits short wavelengths most drastically, so that, unlike the nonmagnetic case, the growth rates do not increase without bound with decreasing wavelength; the large wavelengths are not affected.

3.4. Kelvin–Helmholtz Instability and the Rest of the Book

In many astrophysical flows—for example, soon after the onset of the RT instability—fluids of different densities will flow past each other; this is when the KH instability sets in (unlike the RT instability, which sets in when the fluid layers are at rest). Chandra studied the effect of the magnetic field, again parallel and perpendicular to the flow (chapter XI). The results are similar to the RT case: The tension of the magnetic field lines inhibits instability so that, unlike the nonmagnetic case, the relative velocity between the two streams has to exceed the Alfvén speed before instability can set in. (An Alfvén wave balances the inertia provided by ion mass density and the restoring effect of the magnetic tension.)

I now mention the last three chapters of the book without much comment: Chapter XII on the stability of jets and cylinders includes methods for handling pinch problems of interest in the thermonuclear context (see also a brief mention of the pinch problem at the end of Section 4); chapter XIII on the gravitational equilibrium and stability are of great interest in cosmological problems; and, finally, chapter XIV concerns a general variational formulation of the stability problem.

Taking a dispassionate view of the book today, it appears to have two major virtues. First, with admitted exaggeration, everything of interest in linear stability of classical hydrodynamic and hydromagnetic stability can be found in the book. Many problems of stability are discussed using the same style and the same techniques, so that if a new student of stability masters the techniques once, then gaining entry into all other problems is easy (despite some quaint terminology). As was pointed out by Gillis (1962), the modern computing power of today can be used to take certain MHD problems discussed in the book to a higher level of sophistication. But one must add that the problem of the stability of the viscous shear flow was excluded by design because Lin's (1955) book had just covered it. Other problems that were excluded, perhaps because the astrophysical context was lurking so strongly in the background, are atmospheric phenomena such as internal gravity waves, baroclinic instability, and Rossby waves. These omissions made the book of lesser interest to engineers and atmospheric scientists.

It should also be pointed out that Chandra did not devote much time to speculating about the contexts in which his theories might find applications. That simply was not his style. To take perhaps an extreme example, he showed that Couette flows with the magnetic field along the axis of rotation, contained between cylinders of radius R_1 and $R_2 > R_1$ rotating with angular velocities of Ω_1 and Ω_2, respectively, are unstable when $R_2^2\Omega_2 < R_1^2\Omega_1$. He did the analysis, it would seem, without any ostensible reason. But the result is important because it was thought until then that magnetic fields would only stabilize a fluid system. Although Velikhov (1959) obtained the same result independently, it was left for Balbus & Hawley (1991) to demonstrate that this instability, now known as magnetorotational instability (MRI), arises in the context of accretion as a subject of astrophysical study. They clarified that accretion cannot be explained by molecular viscosity alone because it is far too weak, just as in other large-scale fluid flows; it needs MRI in accretion disks. For more discussion, readers are referred to Brandenburg (2011).

A typical reader of the book may see it as a masterly account of many stability problems of fluid dynamical and astrophysical relevance, written in leisure and quiet. But a look at Chandra's autobiographical remarks (see Wali 2011) shows the enormous pressure under which he operated. He had committed to a deadline of the spring of 1960 to deliver the manuscript to the publisher and was racing against time to meet it. A few selected quotes may describe the frenzy of the three weeks before the deadline (Wali 2011, pp. 50–52):

> Only three weeks were now left . . . Starting Chapter XIII under extreme pressure, I realized that the virial theorem should have to be formulated in tensor form. The existing treatments had many loopholes and were quite unsatisfactory. I developed a whole new approach . . . I had to organize all the figures . . . When all this was finished, I was so tired that I decided to go to New York to give my invited talk to the American Mathematical Society. On returning from New York, the weekend and Monday were spent on various sections of the book which had been incomplete . . . It was finally on Tuesday morning that I started on Chapter XIV . . . I actually thought I would abandon the idea of having a Chapter XIV. I knew this would disappoint Donna [Chandra's secretary] and so I decided that I would start on the chapter anyway . . .The theory was fully worked out by late Wednesday evening; and I wrote up a first draft before going to bed. Early on Thursday morning, I started my second draft. By noon I was ready for the *n*th draft. (By this time, I was in a constant state of nausea.) . . . It was finally completed by 9:30 p.m. I called Donna at that time and she came over to start typing the last chapter. Most of Friday was occupied by filling in the formulae . . . Early on Saturday morning, Norman Lebovitz drove us to O'Hare . . . In London the following day, April 24, the manuscript was handed over to Mr. Wood of the Clarendon Press.

(Chandra did take a four-month "break" in the fall of 1961 in India, but delivered some seventy lectures at various academic and research institutions.)

Returning to the review of Chandra's monograph, Howard (1962) noted that the book contained a few misleading statements ("Even Homer nods," he said on p. 152) and gave two examples: the first on the subtle interpretation of the Taylor–Proudman theorem in thermal and magnetic convection (I have basically described Chandra's interpretation above) and the second on the interpretation of Rayleigh's criterion for instability in Couette flows. It is amusing that Chandra's corrections in later editions did not consider Howard's suggestions. His criticisms notwithstanding, Howard made sure that the generosity of his review was in no doubt: "These criticisms apply to only a very small fraction of the work and by no means affect the conclusion that as a whole this book is a most valuable contribution" (p. 160).

4. OTHER RELATED AREAS OF RESEARCH

Chandra worked on different areas related to fluid mechanics at different times during the period under purview. Only a brief reference will be made to them here.

4.1. Wartime Work on Shock Waves

Chandra's first concrete introduction to hydrodynamics occurred during the war. Like all young people of the time, he was deeply affected by the Pearl Harbor attack in 1941 and wanted to contribute to the war effort. But he was not a US citizen at that time (he became one only in 1953 when the Immigration Act allowing Asians to become US citizens was passed), so there were serious security hurdles. As a British citizen, however, he received clearance to be a civilian consultant at the Ballistics Research Laboratories of the Army Ordnance Department at Aberdeen, Maryland. Oswald Welden of the Institute for Advanced Study at Princeton was a consultant to the Ballistics Laboratory and recruited, besides Chandra, researchers from Brown, SUNY (State University of New York) Buffalo, Caltech, Duke, Harvard, Michigan State, Princeton, Purdue, Wisconsin, and WPI (Worcester Polytechnic Institute). Von Neumann, whom Chandra had come to know during the visit to the Institute of Advanced Study earlier in 1941, apparently tried to persuade him to join Los Alamos, but Chandra was unwilling to go there; he was, in fact, quite intimidated by the racial prejudice of the South during those days. (Even his appointment at Chicago was met with considerable racial overtones up to the decanal level and was made possible only by the intervention of the enlightened university president at the time, R.M. Hutchins.) Chandra joined a group led by R.H. Kent, an expert on ballistics, and arranged his life by spending three weeks at Aberdeen, going back to Williams Bay and lecturing there for three weeks, and then returning to Aberdeen for three weeks—a routine he kept up for about two and a half years. The knowledge of hydrodynamics that he acquired during those years was to come in handy later in his astrophysics work. Chandra's results on the decay and reflection of shock waves can be found in Chandrasekhar (1943b,c).

4.2. Helium II

Sometime in 1955, following the advice of L. Onsager, R.J. Donnelly, who had joined the University of Chicago, began to get Chandra interested in helium II (He II). Chandra was not keen to delve into the complexity of the microscopic description of He II (which at that time was clear only to a few inspired physicists such as Landau, Onsager, and Feynman), but he saw that he could make a useful contribution on the basis of the two-fluid macroscopic description of He II. He thus began to work on the stability of He II in the Taylor–Couette system by making two different assumptions on the mutual friction between the normal and the superfluid components. The two papers that resulted were published simultaneously in the *Proceedings of the Royal Society A* in February 1957. The first paper with Donnelly (Chandrasekhar & Donnelly 1957) examined the hydrodynamic instability of He II between rotating cylinders and evaluated critical Taylor numbers for both assumptions on mutual friction. Detailed experiments were not available at that time, but the comparisons made with existing sketchy ones showed good agreement. The second paper (Chandrasekhar 1957) attended to the more technical details of stability calculations.

4.3. Plasma Physics

Plasma is the material of stellar interiors, atmospheres, and the interstellar gas and was thus of interest to Chandra, given his astrophysical background. In the early 1950s, plasma physics and the confinement of ionized gas by magnetic fields was coming to the fore, and the hope of producing clean and limitless energy through fusion was high on the scientific radar. Despite some milestone achievements, the hope still remains to be realized. During the summers of 1956 and 1957, Chandra finally ventured into Los Alamos and began to learn plasma physics. There, he collaborated with A. Kaufman and K.M. Watson on the perturbation solutions of the collisionless Boltzmann equation for calculating the dynamical stability of the collisionless plasma confined in an axial magnetic field, as well as with N.C. Metropolis on the numerical integration of the equations of hydromagnetic turbulence. During the fall and winter of 1957, Chandra gave a two-quarter course on plasma physics in Chicago. These lectures were later published, somewhat to Chandra's displeasure, by his student S.K. Trehan (Chandrasekhar & Trehan 1960). Finally, when Chandra was a visitor at La Jolla in the summer of 1958, M.N. Rosenbluth got him interested in the theory of the stability of the magnetic pinch.

5. PERSPECTIVE AND SUMMARY

Following Chandrasekhar (1996), we invoke T.S. Eliot's (1932) remark on Shakespeare: "We may say confidently that the full meaning of any one of his plays is not in itself alone, but in that play in the order in which it was written, in its relation to all of Shakespeare's other plays, earlier and later: we must know all of Shakespeare's works in order to know any of it" (p. 170). In this sense, it is useful to examine, if only briefly, Chandra's other periods just before and after the one on stability and turbulence, his motivations for entering a field in which he had no formal upbringing, and his own sense of accomplishment.

A brief reference to Chandra's Cambridge days was made above in the Introduction. In his early twenties, away from home for the first time in a distant land, homesick and tired of the cold climate (as can be seen in letters to his father), rejected by a person he admired greatly, Chandra was clearly distressed. He just kept working on problems he found interesting and consolidated the results in the form of two books (periods 1 and 2). When he started working on radiation (period 3, which I have deliberately avoided discussing here), he had left behind the influence and personal debt he felt for the stars of his days in stellar dynamics, such as Eddington and Edward Milne. He felt, for the first time, that he was on his own, no longer "intimidated by bigger people in front of me" (Weart 1977). And the subject seemed to carry him forward naturally. He felt that his standing was secure and that he could act with freedom scientifically: His two earlier books on stellar structure and dynamics were becoming standard references, and he had just then been elected a Fellow of the Royal Society in 1944 at the age of 34. In his own words, those were his happiest days as a scientist.

After completing his work on radiative transfer in the fall of 1948, brimming with the confidence of a relatively young person of considerable accomplishment, Chandra began to explore a new area to work on, one in which problems "will find their solutions only a decade or two later" (Wali 2011, p. 21), and settled firmly on turbulence; he was not looking for instant success (recall this article's opening statement). It was clear to him that many interesting problems in astrophysics could not be solved unless turbulence was understood better. In his so-called Monday evening seminars, he began to discuss the works of Taylor, von Kármán, Howarth, Kolmogorov, Batchelor, and Heisenberg. Of the dozen or so years he spent on fluid dynamical problems, during which he enjoyed associations with many young stars (already mentioned in the beginning of Section 3), he had two episodes of intense activity on turbulence, roughly 1948–1951 and 1954–1956, which have already been described, along with how he gave up the idea of writing a book on the subject.

By the late 1950s, Chandra felt that he had accomplished a substantial amount in the area of stability and began work on his book (Chandrasekhar 1961) for which he spent inordinate efforts synthesizing and, where necessary, redoing some of his own earlier calculations. The book on stability is true to his attitude and style of work, which he characterized in the following words:

> After the early preparatory years, my work has followed a certain pattern motivated, principally, by quest after perspectives. In practise, this quest has consisted in my choosing (after trials and tribulations) a certain area which appears amenable to cultivation and compatible with my taste, abilities, and temperament. And when after some years of study, I feel that I have accumulated sufficient body of

> knowledge and achieved a view of my own, I have the urge to present my point of view, ab initio, in a coherent account with order, form and structure. (Chandrasekhar 1983a)

With some humility, he described his style more than once in his lectures and writings by quoting from Virginia Woolf:

> There is a square. There is an oblong. The players take the square and place it upon the oblong. They place it very accurately. They make a perfect dwelling place. The structure is now visible. What was inchoate is here stated. We are not so various or so mean. We have made oblongs and stood them upon squares. This is our triumph. This is our consolation. (Woolf 1931)

By the time he sent his stability book to the publisher (I have already described the frantic pace that he kept up), it was clear that he was done with fluid dynamics, a subject in which one builds one's intuition on the basis of specific problems requiring detailed calculations, many of which need numerical work and recourse to (or awareness of) experimental data. He was to say later (a comment that applies particularly to fluid dynamics): "I had been, for 30 years at that time, involved in specific calculations, specific problems, enlarging domains by solving a whole variety of problems, a range of problems, and putting them all together. Now I wanted to change the style of my work" (Weart 1977). And he had already started thinking about his next area, as he would later describe: "It all began while writing Chapter XIII of *Hydrodynamic and Hydromagnetic Stability* in March of 1960 . . . The basic ideas underlying these developments came to me one morning when walking to the observatory along the golf course" (Wali 2011, p. 56). (Who can say that golf courses do not inspire people!) Thus, Chandra began to work (with N. Lebovitz) for some five years on ellipsoidal figures of equilibrium, a subject that had earlier interested the likes of Jacobi, Dirichlet, Dedekind, Riemann, Poincaré, and Lyapunov. This work formed the basis of Chandra's Silliman Lectures at Yale and his later monograph (Chandrasekhar 1969).

5.1. Refuge in Relativity

And then he turned to relativity. The revolution in relativistic astrophysics was just on its way when Chandra entered general relativity. This work is not our concern here, except to note his motivations: He was to state that it satisfied his desire to work on "more contemplative matters" (Weart 1977). Later, he specifically stated,

> I consider myself very fortunate in having made up my mind to do relativity. Among other things, for the first time . . . certainly after the early forties, I felt I was working in an area in which others were working in many ways were far more equipped than I was. I felt that I had a chance of being in close scientific proximity with people of the highest caliber. Certainly, to have known well and consider among my friends people like Roger Penrose, Stephen Hawking and Brandon Carter—it's a marvelous experience. It's a kind of intellectual stimulation which I had not had before. Of course, I worked with Fermi. Fermi was a great physicist, but here I am now in a community of young brilliant men. (Weart 1977)

5.2. Chandra's Sense of Aesthetics in Science

Chandra was preoccupied with elegance, as much in personal appearance and behavior as in written and spoken word. He often discussed (see Chandrasekhar 1987) how various great poets and scientists thought differently about beauty in science, from John Keats at one end, who thought that all charms fly at the mere touch of cold philosophy, to Richard Feynman at the other end, who said that a knowledge of science only adds to the excitement and mystery. Chandra himself seemed most comfortable with the following two descriptions of beauty: "There is no excellent beauty that hath not some strangeness in the proportion" (Bacon 1853, p. 96) and "Beauty is the proper conformity of the parts to one another and to the whole" (Heisenberg 1971). Roughly restated, the latter statement is about the elegant interconnectedness of a piece of scientific work that does not stand alone on the sidelines, while the former statement is about a certain unexpectedness of such connections. Chandra himself thought that "beauty is that to which the human mind responds at its deepest and most profound" (Chandrasekhar 1987, p. 54).

5.3. Chandra's Sense of Happiness

Toward the end of Wali's (1991) biography of Chandra, one finds statements from Chandra such as these: "The hope for contentment and a peaceful outlook on life as a result of pursuing a goal has remained unfulfilled," "I don't really have a sense of fulfillment," "To pursue certain goals all your life only to become doubtful of those goals at the end [is something I find difficult to reconcile with]," and "[A fulfilled life] is not necessarily one in which you pursue certain goals, there must be other things" (pp. 305–7). Part of his discontentment may be his single-minded working style and the accumulated imprint of negative experiences; but part of it is that happiness is an experience synchronous with everyday living, not a distant goal.

5.4. A Star Dies but Does Not Fade

Chandra died of a heart attack at the University of Chicago Hospital in 1995; he had suffered a previous episode in 1975. He was survived by his wife of nearly 60 years, Lalitha Chandrasekhar, and left behind many ardent admirers in many parts of the world. After his death, Lalitha donated Chandra's Nobel Prize money to the University of Chicago to establish the Subrahmanyan Chandrasekhar Memorial Fellowship. She died at the age of 102 on September 2, 2013. They had no children. Chandra's work, however, lives on.

5.5. Chandra's Sense of One's Place in History

We now come full circle to Chandra's opening quote about one's place in history. One has to appreciate that he was writing for posterity. This is not to say that he did not care about the opinion of his contemporary colleagues, but he knew the fickleness of opinions formed right in the middle of action. He did not think that posterity would always be fair, but he did depend on fairness to emerge after the din settles; he was aware, too, that this settling of the din could take a long time, for which one must trust the effectiveness of rigorous scholarship in separating false fads from true accomplishments. One might even speculate that this outlook led Chandra to work in areas "mostly, in the lonely byways of Science" (Chandrasekhar 1983b), rather than at the forefront discovery areas of his time, such as quantum mechanics; his chosen path was akin to those of Rayleigh and Poincaré, not of Heisenberg and Dirac.

In the preface to the book assessing different facets of Chandra's work, Srinivasan (1996) stated, "In his achievement and in his intense scholarship, he has been compared with Lord Rayleigh and Poincaré" (p. vii). Chandra's shortcomings notwithstanding, approbation of this statement is an apt end to this article.

DISCLOSURE STATEMENT

The author is not aware of any biases that might be perceived as affecting the objectivity of this review.

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