Magnetohydrodynamics

FLUID MECHANICS AND ITS APPLICATIONS
Volume 80

Series Editor: R. MOREAU
MADYLAM
Ecole Nationale Supérieure d'Hydraulique de Grenoble
Boîte Postale 95
38402 Saint Martin d'Hères Cedex, France

Aims and Scope of the Series

The purpose of this series is to focus on subjects in which fluid mechanics plays a fundamental role.

As well as the more traditional applications of aeronautics, hydraulics, heat and mass transfer etc., books will be published dealing with topics which are currently in a state of rapid development, such as turbulence, suspensions and multiphase fluids, super and hypersonic flows and numerical modelling techniques.

It is a widely held view that it is the interdisciplinary subjects that will receive intense scientific attention, bringing them to the forefront of technological advancement. Fluids have the ability to transport matter and its properties as well as transmit force, therefore fluid mechanics is a subject that is particulary open to cross fertilisation with other sciences and disciplines of engineering. The subject of fluid mechanics will be highly relevant in domains such as chemical, metallurgical, biological and ecological engineering. This series is particularly open to such new multidisciplinary domains.

The median level of presentation is the first year graduate student. Some texts are monographs defining the current state of a field; others are accessible to final year undergraduates; but essentially the emphasis is on readability and clarity.

For a list of related mechanics titles, see final pages.

Magnetohydrodynamics

Historical Evolution and Trends

by

S. MOLOKOV
Coventry University
UK

R. MOREAU
EPM-MADYLAM
St Martin d'Hères
France

and

H.K. MOFFATT
University of Cambridge
UK

Springer

A C.I.P. Catalogue record for this book is available from the Library of Congress.

ISBN 978-1-4020-4832-6 (HB)
ISBN 978-1-4020-4833-3 (e-book)

Published by Springer,
P.O. Box 17, 3300 AA Dordrecht, The Netherlands.

www.springer.com

Printed on acid-free paper

COST is an intergovernmental European framework for international cooperation between nationally funded research activities. COST creates scientific networks and enables scientists to collaborate in a wide spectrum of activities in research and technology. COST activities are administered by the COST Office.

Contents

Part I Dynamo, Astrophysics, and Plasmas

Part II High Magnetic Fields

Part III Turbulence

Part IV Electromagnetic Processing of Materials

Preface

Magnetohydrodynamics (MHD) is concerned with the flow of electrically conducting fluids in the presence of magnetic fields, either externally applied or generated within the fluid by inductive action. Its origin dates back to pioneering discoveries of Northrup, Hartmann, Alfvén, and others in the first half of the twentieth century. After 1950, the subject developed rapidly, and soon became well established as a field of scientific endeavour of great importance in various contexts: geomagnetism and planetary magnetism, astrophysics, nuclear fusion (plasma) physics, and liquid metal technology.

This volume surveys both the historical evolution of the field and some of the current trends. It is based on a workshop on the History of MHD organised at Coventry University, UK, 26–28 May 2004, by the working group on "High Magnetic Fields" within the European network "Magnetofluiddynamics" (COST Action P6). It contains contributions by the workshop participants, supplemented by several additional invited papers in order to provide more comprehensive coverage of the recent trends. It also includes reminiscences of scientists who worked during the period of pioneering discoveries in the field (1950s and 1960s), together with photos of at least some of the pioneers of the subject.

Topics covered in this volume include dynamo theory and experiment, astrophysics, plasmas, high magnetic fields, turbulence, and electromagnetic processing of materials. Other topics such as magnetoconvection, magnetic reconnection, and tokamak plasmas are not included, simply because to do justice to these important topics would have required a book of unmanageable proportions.

Judging by the vitality of the field as evidenced by this volume, we believe that MHD still poses challenges of great fundamental, as well as practical, importance, and that the prospects for its continuing vitality are bright.

We gratefully acknowledge the willing cooperation of all participants of the workshop and contributors to this volume, the financial support of the European Cooperation in the field of Scientific and Technical Research (COST), the help of Svetlana Aleksandrova in organising the event, the advice of Leo Bühler on the sometimes painful process of conversion between various pieces of software, and last but by no means least, the patience and understanding of the publishers (Springer).

Coventry, Grenoble, Cambridge *The Editors*
January 2006

Part I

Dynamo, Astrophysics, and Plasmas

How MHD Transformed the Theory of Geomagnetism

Paul Roberts

Department of Mathematics and Institute of Geophysics and Planetary Physics,
University of California at Los Angeles, Los Angeles, CA 90095, USA
`(roberts@math.ucla.edu)`

Summary. The main magnetic field on the Earth is generated by, and has been maintained throughout Earth's history by, a fluid dynamo operating in the Earth's electrically conducting core. The author gives his personal view of how understanding of this 'geodynamo' grew during his lifetime, and he includes recollections of some of the scientists involved. The remarkable evolution of the subject from simple applications of electromagnetic theory to today's sophisticated magnetohydrodynamic theory is outlined. The importance of Coriolis forces in core MHD is not fully appreciated even today, but it transforms MHD into an essentially different subject that is briefly reviewed here. Proposals are made to give it its own special name.

1 Early days

As this is a meeting about the *history* of magnetohydrodynamics (MHD), it seemed to me, when I was preparing my talk, that it might be appropriate to include some reminiscences[1]; I expected a fair number of people in the audience to be too young to have interacted with the founders of the subject but who might be interested to hear snippets about them, and as an old fogey I am in a position to oblige. But it seems I am wrong: the room is full of old fogies. Nevertheless I'll continue as planned.

When was MHD born? The answer to this question is subjective. It seems to me that no study that explores only one side of the interrelationship between the magnetic field and the motion of the conductor can truly be said to be a part of MHD. Thus Faraday's famous experiment [1] of 1832 on Waterloo bridge, that was intended to detect tidal motion in the Thames by electromagnetic (=em) induction, is not an MHD experiment, since the dynamical effect of the field on the motion is negligible and is not included. The same can be said of many early attempts to understand the geomagnetic field, as we shall see.

---

[1] Sergei Molokov has encouraged me to be equally informal too in this written account of my talk, which I nearly subtitled "on falling off chairs"; read on!;

S. Molokov et al. (eds.), Magnetohydrodynamics – Historical Evolution and Trends, 3–26.

Fig. 1. Hannes Alfvén

We have already heard at this meeting of exciting experiments in liquid metals in a tradition that go back to Hartmann and Lazarus [2] in 1937. These involve both sides of the MHD relationship, but to my mind they are not really MHD experiments for they do not exhibit the main feature of MHD. The interaction of conducting fluids and magnetic fields gives rise to a completely unexpected phenomenon, the Alfvén wave. That discovery [3], which was a triumph of theory over experiment, was made during World War II. As I see it, this marks the birth of MHD.

Perhaps it is time to start reminiscing. I first met Alfvén at a symposium in Saltsjöbaden near Stockholm in 1956 and, during the time I still worked in Newcastle, he visited there a couple of times. I remember two things about him. First, his colleagues held him in great awe. Second, he ate an apple in a most unusual way. He removed all the skin, where I'm told all the goodness lies, and then he ate *all* the rest including the pips in the core, which I've been told contain small amounts of prussic acid.

The Alfvén wave could have been discovered even before Maxwell introduced displacement currents. How had everyone else missed it? It was said that, as a research student, Ferraro had suggested to his supervisor, Sydney Chapman, that it might be interesting to look for wave motions, but Chapman had told him it would be a waste of time. So Ferraro discovered not the wave but (in 1937) his law of isorotation [4], which is not, according to my definition, an MHD phenomenon. True or false, this is a cautionary tale for all research students. Listen to your supervisor by all means, but do not necessarily take his/her advice. Your own instincts may be better!

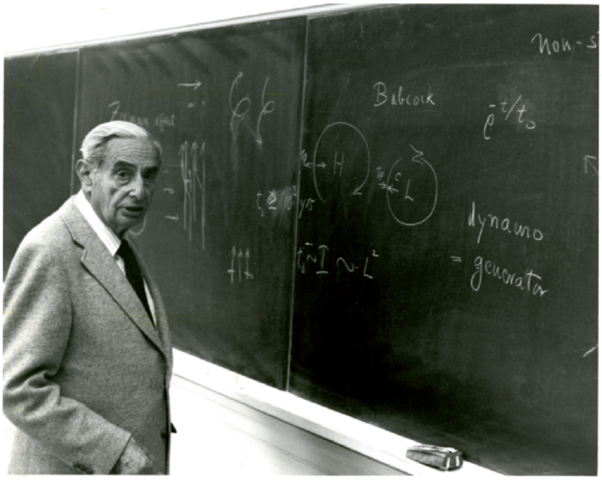

Fig. 2. Walter Elsasser; look at the blackboard!

Another great thing Alfvén did in explaining his wave was to give a physical argument [3] in support of his “frozen flux theorem”, which I shall call his first theorem:

Magnetic field is “frozen” to a perfect conductor as it moves.

Of course, we do not encounter perfect conductors much, but the theorem is very helpful in visualizing MHD processes whenever the *magnetic Reynolds number* is large. You will remember that this number is

$$R_m = \mathcal{L}\mathcal{V}/\eta, \quad \text{or} \quad R_m = \tau_\eta/\mathcal{T}, \tag{1}$$

where $\mathcal{L}$, $\mathcal{T}$ and $\mathcal{V}$ are typical length, time and velocity scales, $\eta = 1/\mu\sigma$ is magnetic diffusivity, μ is permeability, σ is electrical conductivity (SI units), and

$$\tau_\eta = \mathcal{L}^2/\eta \tag{2}$$

is the *electromagnetic diffusion time.* If we take $\mathcal{L} = 10^6$ m and $\eta = 2\ \mathrm{m}^2/\mathrm{s}$ as appropriate for the Earth’s core, τ_η is of order 10^4 years, and $R_m = 100$ for $\mathcal{V} = 10^{-4}$ m/s (as suggested by the “westward drift” of the field). But $\mathcal{L}$ is necessarily small in laboratory experiments with liquid metals and therefore R_m is small too. The frozen field picture is then not so useful; indeed, some say it is distinctly unhelpful. The magnetic field acts more like an anisotropic friction.

To detect an MHD wave it must be seen to “cross the apparatus”. The wave travels with the Alfvén speed

$$\mathbf{V}_A = \mathbf{B}/\sqrt{(\mu\rho)}, \tag{3}$$

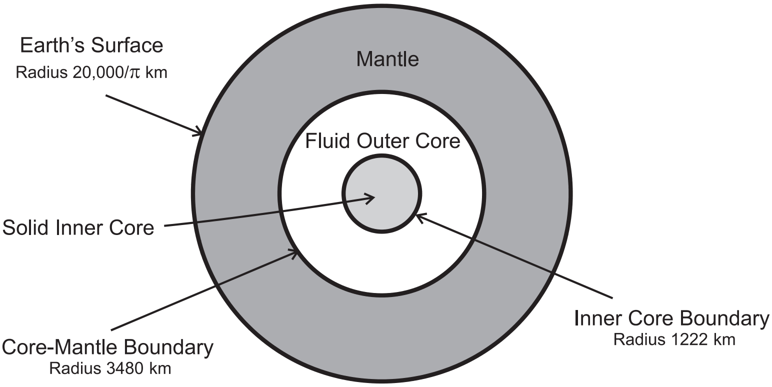

Fig. 3. Interior of the Earth

where **B** is the magnetic field and ρ is density. It crosses the apparatus in a characteristic time of $\tau_A = \mathcal{L}/\mathcal{V}_A$ but, as it does so, the induced current system that travels with it decays ohmically in a characteristic time of τ_η. The condition that the wave is detected is $\tau_A < \tau_\eta$, i.e., the *Lundquist number*,

$$Lu = \mathcal{L}\mathcal{V}_A/\eta, \tag{4}$$

must be "large enough", and generally it isn't in the laboratory.

In neutral Sweden, MHD got off to a fine start and it was several years after the war before the rest of the world caught up. Meanwhile, Lundquist wrote the first paper on magnetostatics in 1950, a famous review in 1952 [5], and attempted to demonstrate Alfvén waves in the laboratory using mercury as the working fluid [6]. Lehnert used liquid sodium instead. His method of disposing of used sodium led to an amusing incident that shows his skill as a conciliator rivals his prowess as a scientist. He relates this tale elsewhere in this book.

2 Early pioneers: Cowling, Elsasser, Bullard, and Chandrasekhar

The famous physicist, Walter Elsasser, who had emigrated to the United States in the 1930s for obvious reasons, began in 1939 to take an increasing interest in why the Earth is magnetic [7]. It may be appropriate here to remind you of the Earth's internal structure; see Fig. 3. Elsasser's studies led him ever deeper into MHD culminating in 1955/56 with a two part review [8], but initially he made use only of em theory. In 1950 he discovered the by now well-known Elsasser variables $\mathbf{V} \pm \mathbf{V}_A$, where $\mathbf{V}$ is the fluid velocity [9].

One of his most interesting papers [10] appeared in 1946, the second of a series on "Induction effects in terrestrial magnetism". This shows that he had

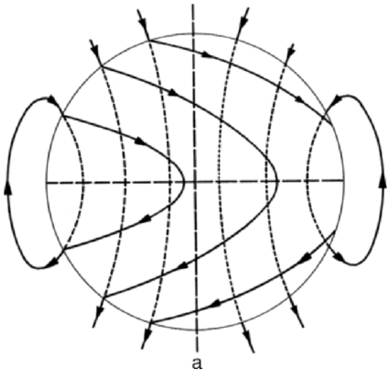

Fig. 4. The ω-effect

by now realized that rotation is very important in the MHD of the core. He got close to introducing the important dimensionless measure of the relative sizes of Lorentz and Coriolis forces in a rotating system:

$$\Lambda = \mathcal{B}^2/\mu\rho\eta\Omega = \mathcal{V}_A^2/\eta\Omega, \tag{5}$$

where Ω is the angular velocity. I christened this the *Elsasser number* in his honour and the name has stuck. Although he did not write down his number, he did introduce the important scale $\mathcal{B}_E$ based on $\Lambda = 1$, namely

$$\mathcal{B}_E = \sqrt{(}\mu\rho\eta\Omega), \tag{6}$$

which he estimated as about 12 gauss for Earth's core, i.e., much the same value as we would accept today, taking $\rho = 10^4$ kg/m^3. I will return to the Elssaser number later.

Elsasser was also the first to draw attention to the importance of the toroidal field in geomagnetism, the existence of which had previously been unsuspected since it is trapped in the core (assuming that the electrical conductivity in and above the mantle is negligible). One expresses the magnetic field, $\mathbf{B}$, as

$$\mathbf{B} = \mathbf{B}_T + \mathbf{B}_P, \tag{7}$$

$$\mathbf{B}_T = \boldsymbol{\nabla}\times(T\mathbf{x}), \qquad \mathbf{B}_P = \boldsymbol{\nabla}\times\boldsymbol{\nabla}\times(S\mathbf{x}), \tag{8}$$

where $\mathbf{B}_P$ is the 'poloidal field' that escapes to the Earth's surface where we view it, and $\mathbf{B}_T$ is the 'toroidal field'; here $\mathbf{x}$ is the radius vector from the geocenter. Elsasser pointed out, I think for the first time, that zonal motions easily create toroidal field from poloidal field, a process I later named 'the ω-effect.' With the help of the frozen flux idea, one can visualize the lines of force of an axisymmetric poloidal field, $\overline{\mathbf{B}}_P$, being stretched out along lines of latitude by a zonal shear, $\overline{\mathbf{V}}_T$ (Fig. 4). In this way a zonal $\overline{\mathcal{B}}_T$ is created of

Fig. 5. Thomas Cowling (*left*); Subrahmanyan Chandrasekhar (*right*)

order $R_m \times \overline{\mathcal{B}}_P$, where R_m is defined using the zonal velocity $\overline{\mathcal{V}}_T$. The belief quickly developed that

$$\overline{\mathcal{B}}_T/\overline{\mathcal{B}}_P \sim R_m, \qquad \overline{\mathcal{V}}_T/\overline{\mathcal{V}}_P \sim R_m \tag{9}$$

are large; unseen is to be believed! If $R_m = \mathrm{O}(100)$ (see above), $\overline{\mathcal{B}}_P = 3$ gauss, implies $\overline{\mathcal{B}} \approx \overline{\mathcal{B}}_T$ is about 300 gauss, the value used in making estimates below. For example, $\overline{\mathcal{B}} \approx 300$ gauss gives $\overline{\mathcal{V}}_A \sim 0.3$ m/s, so that $\Lambda \sim 100$ rather than $\Lambda \sim 1$. The concept (9) dominated core MHD until the mid-1990s, when numerical simulations did not support it [11]. One should not forget however that simulations were not, and still are not, in a geophysically realistic parameter range.

My own forays into geomagnetism started in 1951 when, as a 1st year research student I approached Herman Bondi, who had been the outstanding teacher of my undergraduate years to ask him for a topic in relativity and cosmology to work on for a Ph.D. The gist of his reply, as I recall it, was that, if he had a problem in that area to work on, he'd work on it himself. He suggested instead that I solve the dynamo problem either by generalizing Cowling's theorem [12] or by creating an example of a working dynamo.

I first met Cowling at a party at the British Embassy in Stockholm at the time of the Saltsjöbaden meeting. I found him a rather forbidding presence, especially so since he imbibed soft drinks while everyone else was getting plastered at British government expense. But I like this photograph of him (Fig. 5). A little smile plays around his lips. No doubt he was thinking at the time of all the trouble that his theorem was giving other theoreticians. Later, when I visited Leeds often to collaborate with Harold Ursell, I got to know and like "Tom" a lot better. But there is no denying that his scientific standards were high and his powers of criticism even higher. I am not the first to note that Cowling's theorem is a negative result:

Axisymmetric magnetic fields cannot be maintained by a dynamo.

When I later got to know Subrahmanyan Chandrasekhar, popularly known as "Chandra", it seemed to me that he felt that Cowling's exacting standards sometimes had a stultifying effect on the scientists. He propounded to me what he called Alfvén's second theorem:

Given Cowling, $\exists$ *no theoretical astrophysics.*

I never could discover whether Alfvén really had said that or whether this was a product of Chandra's impish sense of humour but, given its formal mathematical flavour, I suspect the latter [13].

While on the subject of Cowling's theorem, I cannot resist another story. Einstein and Elsasser maintained contact with each other after they had both moved to the new world. At one meeting, Einstein (who apparently had a long standing interest in the origin of the Earth's magnetism [14]) asked Walter for full details about his progress in solving the problem. Walter explained dynamo theory, as it stood in the early 1940s. He explained that it was still not known whether dynamos could function in simple bodies like spheres. He explained Cowling's theorem. "Enough" said the great man, "If dynamos do not work in such a simple case, they will not work at all". (I have been unable to remember or discover who told me this amusing tale, but I suspect it does grave injustice to an outstanding physicist.)

To go back to my own unfortunate experiences, I spent a year failing to generalize Cowling's theorem and also failing to find an example of a working dynamo. The nearest I came to it was something like Herzenberg's 1958 model [15], but I got lost in the maths. I was therefore very happy to accept Bondi's advice, which was to stop hitting my head against the wall, as the idea might not work anyway. A lost opportunity? Maybe. A lost year? Not entirely. I'd learned some MHD, and what did a year matter? At age 22 one is going to live forever. One pleasant part of the year was getting acquainted with that lovable character "Teddy" Bullard who was ultra supportive of research students perhaps partly because, as a research student himself, he had encountered less than tolerant treatment from Rutherford who had told him never to darken the doors of his (Cavendish) laboratory again. Bullard even invited me, a lowly research student, to be a guest in his home at the National Physical Laboratories in Teddington so that we could have more time to discuss the geodynamo. At that time he was Director of the NPL, in a position to drive its computing section crazy attempting to solve a kinematic dynamo model with the very primitive electronic computers then available [16].

My failure caused me to change direction and supervisor. In 1952, I started to work on other geomagnetic problems under the direction of Keith Runcorn, and I made my first positive contribution to geomagnetism, which I would

Fig. 6. "Teddy" Bullard (*left*); Keith Runcorn (*right*)

claim was actually the first success of MHD in the subject. At that time, DAMTP did not exist and contact between geophysicists and applied mathematicians, and between geophysicists and engineers, was non-existent. Apart from Raymond Hide (or "Spike", as he was known then), nobody I met seemed to know anything whatever about MHD. Most geophysicists thought that the sources of the geomagnetic field must lie in the upper 200 km of the core. The argument was based on the "skin effect": a field changing on a timescale of $\mathcal{T}$ penetrates a *solid* conductor only to a depth of order

$$d_\eta = (\eta\mathcal{T})^{1/2}. \qquad (10)$$

For $\mathcal{T} = 10$ years, a typical secular variation timescale, $d_\eta = 30$ km; for $\mathcal{T} = 1000$ years, $d = 200$ km. So all the sources had to be near the top of the fluid core.

Even Elsasser [10] swallowed this, as did Lowes and Runcorn [17]. In 1952, I pointed out to Keith that the argument was not convincing essentially because the Lundquist number based on a poloidal field strength of 3 gauss is large: $Lu > 1000$ for $\mathcal{L} = 10^6$ m. Thus, I claimed, Alfvén waves would carry the secular variations from deep within the core to its surface with essentially negligible dissipation [18]. Keith almost fell out of his chair and then told me that I "must have made a mistake". Of course, "imitation is the sincerest form of flattery", and Runcorn flattered me by including my insight into the published account [19] of an address he gave at a meeting of the American Geophysical Union in 1953, and he acknowledged the contribution I had made in a characteristic way: "It is a pleasure to record my gratitude to my colleagues in the Department of Geodesy and Geophysics of Cambridge University for much discussion and for permission to report on the results of their work prior to publication."

In reality, no permission was sought, from me at least, but who needs to seek permission from a lowly research student? My idea may seem to be rather obvious nowadays, but it was not so in 1952, and (after all) it *was* my idea and not his. So it rankled, and still rankles a half a century later (as you see!) [20].

In 1954, after my Ph.D., I became a postdoc at Yerkes Observatory of the University of Chicago where I got to know Chandra, whose early work on white dwarfs later led to a shared Nobel prize. When I visited, he was working frenetically on hydrodynamic and hydromagnetic stability, the subject of his later book, but he was vitally interested in MHD and dynamo theory. He was the first person to emphasise to me that a marginal kinematic dynamo might be an overstable solution of the em equations (i.e., one that oscillates sinusoidally in time) rather than the steady, "exchange of stabilities" solution that people such as Cowling and Bullard had so far been seeking.

After returning to England for military service, I moved to Newcastle but attempted little geomagnetism and even less MHD. What I did was mostly in partnership with Spike [21]. One valuable lesson he taught me was how to deal with adversity in research. Prior to that time, I would blame my stupidity if I failed to reach an objective. Though he was seldom in such a position, Spike would rebound quickly, merely opining that the objective itself was more difficult than he had originally anticipated.

I returned "permanently" to Yerkes in 1961 where Chandra was now working away frenetically (apparently the only way he ever worked) on the stability of rotating self-gravitating fluids. At the time, Keith Stewartson was on sabbatical at the Army Research Center in Madison and we started collaborating [22]. This gives me an opportunity to remind you of some of the outstanding work Keith did in MHD. With his strong background in aeronautical theory, a natural problem for Keith to analyse was flow over a "wing" with an aligned magnetic field (Fig. 7).

Others looked at this problem too, and had generated solutions that, as in the non-magnetic case, had a wake following the wing. Keith realized however that, when the Alfvén number

$$A = V/V_A \tag{11}$$

(sometimes called the 'magnetic Mach number') is less than 1, the solution is fundamentally different as the wing signals its presence upstream through Alfvén radiation. "Obvious", we might tend to say today but, when he spoke about it at the Williamsburg meeting [23] in 1960, it was still controversial and hotly disputed.

During this period, Stewartson challenged me to solve a problem on MHD duct flow concerning the steady flow of conducting fluid down an insulating pipe in the presence of a large transverse magnetic field. He wanted to

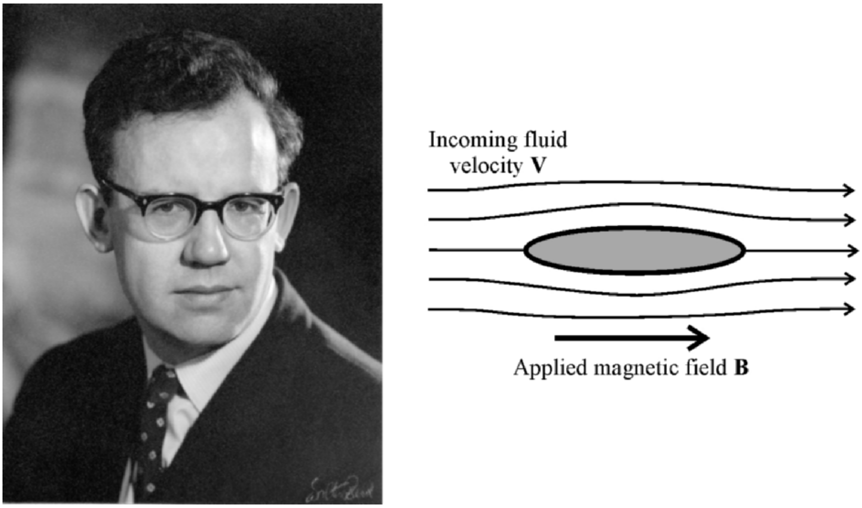

Fig. 7. Keith Stewartson (*left*); aligned flow (*right*)

determine the structure of the singularities in the Hartmann layer that occur at the points where the applied field is tangential to the walls. I eventually rose to his challenge [24], and it was Stewartson's turn almost to fall out of his chair. (He also challenged me to solve the corresponding singularity structure for the Ekman layer, but that was too tough for me.)

I returned to Newcastle in 1963 and began to get interested in geomagnetism and MHD again. I started a project with Stan Scott that achieved some notoriety and recently some controversy [25]. It was based on MHD and Alfvén's theorem and argued that the secular variation could mostly be explained as the advection of field "frozen" to fluid motions beneath a boundary layer at the core surface.

3 Stanislav Braginsky, father of geomagnetic theory

In 1965 I lunched with Bullard at some meeting or other and he asked me if I'd looked at "extraordinary claims" made by "some Russian or other" about kinematic dynamos. That Russian was Stanislav Braginsky. At first sight, his paper [26] seemed obviously wrong. How could the mathematics of his nearly symmetric kinematic dynamos possibly simplify in the remarkable way he claimed? I spent a long time trying to evaluate this work. I was not helped by the notorious terseness of Soviet papers, caused by the strict length limits imposed by Soviet autocracy (because of the well-known shortage of paper caused by the death of trees in Russia!). It was the kind of situation where it was hard to re-derive results even when they are known to be correct. One marvelled at the pioneer who had faith to continue along such a complicated

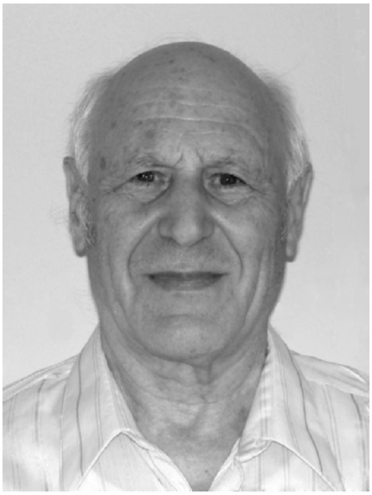

Fig. 8. Stanislav Braginsky

path without really knowing for sure where it would lead. When I realized the paper was correct, it was my turn almost to fall out of my chair! Later, Andrew Soward [27] found an alternative and more fundamental route to Stanislav's result, but it is hardly simpler!

It would take me too long a digression to describe this work here. Suffice it to say that it is based on a simple idea: the magnetic Reynolds number, R_m, of the zonal core motion is large (see §2). Therefore even a small deviation from axisymmetry should be enough to defeat Cowling's theorem. Stanislav developed an asymptotic solution of the induction equation in powers of $R_m^{-1/2}$, in which (9) holds, and in which the asymmetric components $\mathbf{V}'$ and $\mathbf{B}'$ of the velocity and magnetic field are of order $R_m^{-1/2}\overline{\mathbf{V}}_T$ and $R_m^{-1/2}\overline{\mathbf{B}}_T$, respectively. Since $\mathcal{B}'/\overline{\mathcal{B}}_P = \mathrm{O}(R_m^{1/2}) \gg 1$, it appears at first sight that this solution is geophysically irrelevant, but Stanislav was able to show that the largest terms in the expansion of $\mathbf{B}'$ could not leave the conducting core; he demonstrated that $\mathcal{B}'_{\text{external}} = \mathrm{O}(R_m^{-1}\mathcal{B}'_{\text{internal}}) = \mathrm{O}(R_m^{-1/2}\overline{\mathcal{B}}_P)$, consistent with the small inclination of the magnetic dipole to the geographic axis.

In this work [26], Stanislav provided the first really compelling mathematical support for Eugene Parker's concept [28] of a "$\mathit{\Gamma}-$effect", though he did not have in mind em induction by turbulent motions as 'Gene had. I had heard' Gene talk about his ideas at Yerkes in 1955, but I was slow to realize their significance. At the time it seemed to me that he had merely replaced one equation (in three space dimensions) that I could not solve by another equation (in two space dimensions) that I could not solve either. Parker's work was taken further by Steenbeck, Krauze and Rädler [29]. They clarified *the α-effect* (Parker's $\mathit{\Gamma}$-effect), invented *helicity* (or "Schraubensinn" as they called it – roughly "screwiness"), explored the relationship between the two,

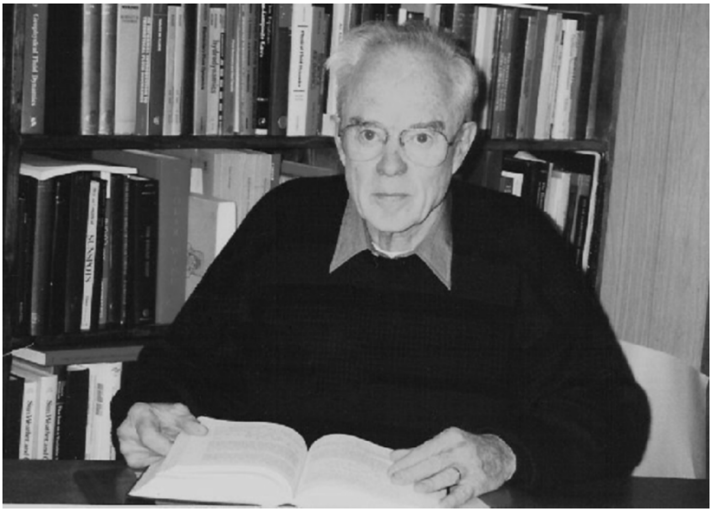

Fig. 9. 'Gene Parker

and created a new subject: *Mean Field Electrodynamics* (MFE). They provided a mathematical and computational model of solar activity in terms of a 'dynamo wave', much as Parker had envisaged [30].

Stanislav [31] combined his asymptotic analysis with numerical integrations, performed in his own machine code on the computers available to him in the USSR, which were even more primitive by today's standards than those used in the West at that time. This combined analytic–numeric approach to the kinematic geodynamo may fairly be said to be the first successful solutions.

Stanislav also generalized his first asymptotic theory. In [26] he had assumed that the α-effect was created by a single asymmetric planetary "wave", brought to rest by choice of reference frame. In an even more complicated generalization [32], he demonstrated that the $\alpha-$effects of an arbitrary number of such planetary waves, each having its own angular velocity about the symmetry axis, are additive. This paper illustrates how the idea of a dynamo operating by means of planetary waves had already taken root in Stanislav's mind.

The astrophysical impact of the work [29] of the Potsdam trio has been considerable. The geophysical impact of a companion paper, in which again turbulent motions in the core are held responsible for dynamo action, has been much less for what I believe are the following reasons.

Let us contrast convection in the Sun and in the Earth's core. In the former an abundant energy supply drives wildly turbulent motions. In the latter the question has always been, "Where on Earth (sorry!) does the core get enough energy to maintain the dynamo against ohmic losses?" The magnetic field in the Sun is highly intermittent. Violent motions bring flux tubes together and reconnect them with the release and dissipation of magnetic energy.

Fig. 10. The Potsdam trio; from left to right, Max Steenbeck, Karl-Heinz Rädler, and Fritz Krause

Correspondingly, the timescale of magnetic activity is greatly shortened. Instead of τ_η being of order 10^{11} years, as one might expect if one used a molecular or η, it is of order 10 years, suggesting that a turbulent diffusivity, η_T, about 10^{10} times greater than the molecular η is relevant. Models of the solar activity cycle, routinely assume that all diffusivities in the solar convection zone are of order 10^9 m^2/s. In contrast, one cannot expect such vigorous turbulence in the Earth's core, which is struggling to make do with the little energy it has available. There is absolutely no evidence that the timescales of the geomagnetic field are greatly shortened. An analogue of the regular activity cycle on the Sun is the irregular polarity reversal of the geomagnetic field. Palaeomagnetism assures us that it takes of order 10^4 years for the Earth to switch its polarity and this is about what is suggested for τ_η with the estimated molecular η. Theoretical discussions of turbulent induction link the turbulent α-effect to the turbulent diffusivity, both being proportional to the Reynolds number defined by the small scales, i.e., if the turbulent diffusivity η_T is small, so is the turbulent α-effect. This may be a simplistic way of stating the situation, since it omits the obvious influence of Coriolis forces and density stratification, but (to my mind) it distils the essence of the matter. Although it would be a brave man that said that turbulence plays no part in the generation of the large-scale geomagnetic field, it seems to me that such a statement would be nearer the truth than saying the opposite, that the main geomagnetic field is mainly produced by small-scale motions in the core. It is the large-scale motions that are responsible, and Andrew Soward was able to define a helicity for these and to relate it, in Braginsky's large R_m analysis, to his α.

I hope I have not given the impression, as Andrew Soward has told me I have on past occasions, that I believe turbulence is unimportant in the core. The Earth's core is a liquid metal, having a magnetic Prandtl number,

$$Pr_m = \nu/\eta, \tag{12}$$

of about 10^{-6}. Thus core turbulence has to supply the deficiencies of molecular processes by transporting large-scale momentum and heat. Turbulence therefore plays a crucial role in core MHD, though not in em induction processes. The significance (and anisotropy) of core turbulence was recognized in another paper [33] that Stanislav wrote in 1964, his "annus mirabilis". This contained several other important ideas too and, although it is hard to choose, I would rank this as his most important paper; I shall refer to it henceforth as "the 1964 paper". Later developments on core turbulence can be found in [34, 35].

At the time Stanislav wrote the 1964 paper [33], his estimate of η for the core was about 3 times what is accepted today and it seemed that thermal buoyancy could not provide the energy needed to offset ohmic dissipation. (Recall here that the greater the σ, the larger the $\mathcal{B}_T$ for the given, observed $\mathcal{B}_P$, and therefore the bigger the ohmic dissipation.) Stanislav came up with a new mechanism [33, 36]: compositional buoyancy. Jack Jacobs had cogently argued [37] that the Earth's solid inner core had been created by the solidification of the iron-rich fluid alloy originally filling the core, and further that the inner core surface is, even to this day, continually moving slowly upwards as freezing continues. Verhoogen [38] had recognized that the concomitant release of latent heat would assist core convection. Stanislav made two important points, first that the light components of the alloy, the "admixture", would be preferentially released during freezing and would help stir the core too; second, he pointed out that such *compositional convection* would be thermodynamically much more significant than *thermal convection*, which is limited by (essentially) the Carnot efficiency. Others provided mathematical backing for these ideas [39] (and still others have received credit for them). Ideas of convective efficiency now dominate discussions of planetary dynamos, e.g., see [40]. The idea that core turbulence would mix entropy as effectively as it mixes the light constituents released from the inner core led Stanislav to the anelastic approximation. It is interesting to contrast his concise derivation of that approximation with those of earlier authors.

Another important idea in the 1964 paper [33] concerned the effects of rotation and buoyancy on Alfvén waves. This topic was not completely new though its application to the Earth's core was novel. I will digress again. Alfvén discovered his waves during attempts to explain the solar activity cycle;

see [18]. His colleague Walén developed [41], and Alfvén endorsed, a theory of “whirl rings”, which resembled circular flux tubes. They visualized that these were produced near the Sun’s energy producing core and travelled upwards as Alfvén waves, riding on the general poloidal field of the Sun. When Lehnert was a visitor to Yerkes Observatory in 1953 he analysed Alfvén waves in highly rotating systems and he found that they are replaced by slow waves, now often called ‘Lehnert waves’ in his honour [42]; they are sometimes also called ‘MC waves’ (see below). They are highly dispersive. The whirl rings would therefore not preserve their identity as they rose from deep within the Sun to its surface. I heard that Lehnert was not as thrilled by his discovery as you or I might have been. He was greatly in awe of Alfvén and did not know how to break the news to him.

The characteristic timescale of the Lehnert waves is

$$\tau_{slow} = \mathit{\Omega}\mathcal{L}^2/\mathcal{V}_A^2, \tag{13}$$

which is about 200 years, for $\mathcal{L} = 10^6$ m and $\mathcal{B} = 100$ gauss ($\mathcal{V}_A = 10$ cm/s). Since

$$\tau_{slow} = \mathit{\Lambda}^{-1}\tau_\eta, \tag{14}$$

it is also, for $\mathit{\Lambda} = 100$, roughly the timescale over which ohmic dissipation would obliterate the waves.

Stanislav’s had built his kinematic theory [26,32] on the dynamo action of waves, and he needed an explanation of how these waves are generated and maintained. So in the 1964 paper [33] he undertook a simple (Cartesian) analysis similar to Lehnert’s but with the crucial difference that buoyancy forces were also included. This led him to the concept of the MAC wave, where M = Magnetic, A = Archimedean (buoyancy), and C = Coriolis. This acronym is mainly significant for what it omits: inertial and viscous forces (although a pressure gradient is, as always, significant). To improve geophysical realism, he later studied MAC waves in spherical geometry [43]. He argued that the geomagnetic secular variation is a manifestation of slow planetary MAC waves; see also Hide [44]. Buoyancy feeds energy to the waves and prevents their disappearance; the waves create the non-local α-effect that maintains the geomagnetic field. It’s a nice idea, though a difficult one to develop theoretically; doubtless it will be pursued more in the future.

Andrew Soward joked once that the Alfvén velocity is that velocity with which no wave travels in the Earth’s core! Certainly rotation changes everything. He would, however, make an exception of the torsional oscillations (TO) which are a kind of Alfvén wave travelling across the geostrophic cylinders which are coupled together by the s-component of the prevailing poloidal field (Fig. 11); here (s, ϕ, z) are cylindrical coordinates with Oz parallel to $\mathbf{\Omega}$. In a highly rotating body of fluid like the Earth’s core, it is convenient to divide the fluid motion into geostrophic and ageostrophic (non-geostrophic) parts: $\mathbf{V} = \mathbf{V}_G + \mathbf{V}_N$. The geostrophic part, $\mathbf{V}_G$, is in the zonal (ϕ) direction, and is defined, for each geostrophic cylinder $\mathcal{C}(s)$, by the average of V_ϕ over

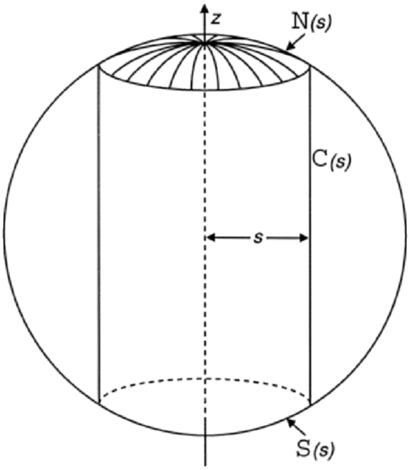

Fig. 11. Geostrophic cylinders; for simplicity, the inner core is ignored

$\mathcal{C}(s)$:

$$V_G(s,t) = \frac{1}{4\pi z_1} \int_{-z_1}^{z_1} dz \int_0^{2\pi} d\phi \; V_\phi(\mathbf{x},t), \tag{15}$$

where $z_1 = \surd(R^2 - s^2)$. The significance of the division $\mathbf{V} = \mathbf{V}_G + \mathbf{V}_N$ is that the Coriolis forces associated with $\mathbf{V}_G$ can be absorbed into the pressure gradient, i.e., it is ineffective. This means that the inertial force, which has (compared with the Coriolis force) a negligible effect on the (ageostrophic) MAC waves, determines the fate of the geostrophic motions in much the same way as for an Alfvén wave. Stanislav [45] founded the theory of TO too and, based on some observation and analysis, these oscillations, which have a decadal timescale, have been detected [46].

This brings me to Stanislav's current work, which is motivated by a perplexing property of the geomagnetic field: the dipole varies on a decadal timescale. This cannot be explained by the TO, because these are axisymmetric and leave the dipole unaffected. Large-scale MAC waves create a dipole variation, but only on a timescale that is much too long. Stanislav currently attributes the dipole variation to MAC waves in a stably stratified "ocean" at the top of the core, about 80 km deep and having a density that is about 0.9999 times that of the fluid below it [47]. For such small-scale MAC waves, (13) gives τ_{slow} in the decadal range. It is fair to say that the geophysical community at large is not yet convinced of the existence of such an ocean. Another of Stanislav's ideas, "Model-Z" [48], has also not won general acceptance either. Nevertheless, Stanislav has been right before……

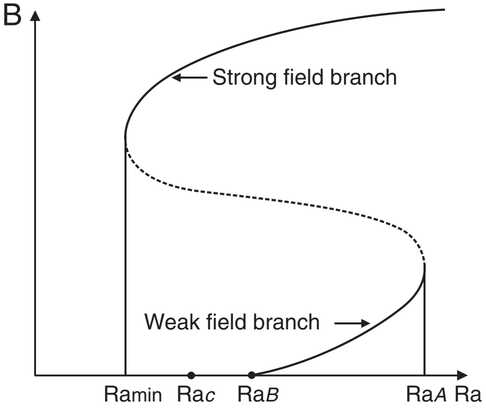

Fig. 12. Sketch of postulated bifurcation diagram

4 Epilogue: a question

I have sketched out a scenario, largely due to Stanislav Braginsky, of the MHD of the Earth's core and its consistency with what can be gleaned from past and present data on the geomagnetic field.

I will conclude by riding one of my favourite hobby-horses: the way that Coriolis forces transform traditional MHD into almost an unrecognizable form. I have touched on this topic several times already but another striking manifestation of this is the strong field dynamo which is illustrated here by a sketch (Fig. 12) that I was showing in my talks even in the 1960s. (Apologies to other old fogeys here present!) Consider the planar dynamo operating in a Bénard layer rotating about the vertical. This was originally studied by Steve Childress, Andrew Soward, and Yves Fautrelle [49], but recently it has been the focus of computations by Chris Jones in association with myself and Jon Rotvig [50]. This is the context in which my statements have so far the best theoretical backing. Figure 12 is a sketch of the $(Ra, \mathcal{B})-$plane of solutions; Ra is the Rayleigh number and Ra_c marks the onset of non-magnetic convection; if Ra is gradually increased from this state, the motions grow in magnitude and ultimately, at $Ra = Ra_B$ become strong enough to maintain a magnetic field. As Ra is further enhanced, $\mathbf{V}$ and $\mathbf{B}$ increase too and the *weak field branch* arises, on which the Hartmann number $Ha = \mathcal{BL}/\sqrt{(\mu\rho\eta\nu)}$ is O(1), i.e., on which $\Lambda = \mathrm{O}(E)$, where $E = \nu/\Omega\mathcal{L}^2$ is the Ekman number, which is of order 10^{-14} if we assume that $\nu = 10^{-6}\mathrm{m}^2/\mathrm{s}$ is a molecular viscosity and of order 10^{-8} if we assume that ν is a turbulent viscosity of order 1 m^2/s. Ultimately, at the asymptote $Ra = Ra_A$, there is runaway field growth to the *strong field branch* where the Elsasser number, Λ, is O(1). Also evident on the strong field branch are subcritical solutions, i.e., solutions that exist at even smaller Rayleigh numbers than those at which convection can first occur. The Earth appears to be operating a dynamo on the strong field branch.

This is but one of the striking ways in which the Coriolis force transforms MHD. I believe it is an essentially different subject and deserves a different name and acronym. So I conclude this presentation by asking my audience (and readers) what that name and acronym should be. In the past I suggested ‘RMHD’ standing for ‘rotating MHD’ but that has been appropriated both by ‘reduced MHD’ and by ‘relativistic MHD’. I recently proposed CMHD, standing for ‘Coriolis MHD’, but Stanislav has put forward an attractive alternative: ‘MACHD’, which is specially appropriate for the Earth’s core, as it emphasises the central importance of Coriolis forces. (One could even take this idea further by reserving MAHD for the MHD of liquid metals in general, while using ‘MHD’ for the entire field, including plasma MHD.) What do you think?

Acknowledgements. I am grateful to Stanislav Braginsky and ’Gene Parker for providing pictures of themselves to use in this report. I also thank the Royal Institute of Technology, Stockholm, for supplying a photograph of Alfvén. Johns Hopkins University (the Eisenhower Library) gave permission to reproduce the photograph of Elsasser. The pictures of Bullard, Cowling, Runcorn, and Stewartson, which appeared in the biographical memoirs of the Royal Society, are reproduced by arrangement with Geoffrey Argent. Photo of Chandrasekhar courtesy AIP Emilio Segrè Visual Archives. The help of the editor, Sergei Molokov, is gratefully acknowledged. Stanislav Braginsky offered advice on an earlier draft.

5 Supplementary notes

1. Faraday M (1832) Experimental researches in electricity, Second series, Philos Trans R Soc Lond 122:163–194
2. Hartmann J (1937) Hg-Dynamics I. Theory of the laminar flow of an electrically conductive liquid in a homogeneous magnetic field. Det Kgl Danske Vid Sels Mat-Fys Medd XV(6):1–27
   Hartmann J, Lazarus F (1937) Hg-Dynamics II. Experimental investigations on the flow of mercury in a homogeneous magnetic field. Det Kgl Danske Vid Sels Mat-Fys Medd XV(7):1–45
3. Alfvén H (1942) Existence of electromagnetic-hydrodynamic waves. Nature 150:405–406
   Alfvén H (1943) On the existence of electromagnetic-hydrodynamic waves. Arkiv f Mat Astron o Fys 29B(2):1–7
4. Ferraro VCA (1937) Non-uniform rotation of the sun and its magnetic field. Mon Not R Astr Soc 47:458–472
5. Lundquist S (1952) Studies in magneto-hydrodynamics. Arkiv f Fysik 5:297–347
6. Lundquist S (1949) Experimental investigations of magneto-hydrodynamic waves. Phys Rev 76:1805–1809
7. Elsasser W (1939) On the origin of the Earth’s magnetic field. Phys Rev 55:489–498

8. Elsasser W (1955) Hydromagnetism. I. A Review. Am J Phys 23:590–609
   Elsasser W (1955) Hydromagnetism. II. A Review. Am J Phys 24:85–110
9. Elsasser W (1950) The hydromagnetic equations. Phys Rev 79:183
10. Elsasser W (1946) Induction effects in terrestrial magnetism. Part II. The secular variation. Phys Rev 70:202–212
11. For example see
    Glatzmaier GA, Roberts PH (1997) Simulating the geodynamo. Contemp Phys 38:269–288.
    On p. 114 of
    Braginsky SI (1991) Towards a realistic theory of the geodynamo. Geophys Astrophys Fluid Dynam 60:89–134,
    Stanislav points out that an important question needs addressing: the stability of MAC waves (see §3). Stanislav speculates that, if the set of developed MAC waves in the core is unstable, MAC wave turbulence might result. He points out that this could well be the main mechanism that transports large-scale heat and light admixture in the core and might also create a nonlocal $\alpha-$effect that reduces $\mathcal{B}_T/\mathcal{B}_P$
12. Cowling TG (1933) The magnetic field of sunspots. Mon Not R Astr Soc 94:39–48
13. Chandra’s picture (Fig. 5) does not do him justice; he comes across as a somewhat haughty individual, quick to take offense at anything he could interpret as a criticism. I invariably found him supportive. He had a nice sense of humour, which I can illustrate by an amusing incident involving his colleague, Gerard Kuiper. One of Kuiper’s ideas had featured in the national press and his young son rushed round to Chandra’s office and demanded Chandra’s opinion about it. “Well”, admitted Chandra, “I’ m not really an astronomer. . . ” “Yes! yes!” interrupted Kuiper’ s son, “That’s what my father says. . . ”. Chandra used this to tease Kuiper unmercifully whenever the occasion arose. Chandra also had a fund of amusing anecdotes, most of which unfortunately I have forgotten. I liked one particularly, as it nicely illustrates the pecking order of mathematics. Apparently the famous algebraist, Hermite, was a staunch conservative who sided with the government in the infamous treatment of Dreyfus that rocked France in the late nineteenth century. Hermite’s favourite student, Hademard, was a liberal who sided with the victim. When Hermite heard of this he broke into a furious denunciation of Hademard finishing with the most damning criticism of all: “And I hear that he has now taken up analysis!”
    My time as a research associate with Chandra (1954–1955) was refreshing, rewarding and highly educational, a revelation in fact. Chandra was ultra hardworking; his students claimed the initial ‘S’ of his first name stood for ‘Superman’. He even had us working on the morning of Christmas Day, 1954. He also set himself high standards: he told me once that he disliked writing letters of reference because it was so hard to be objective especially when he liked the person concerned; at least 5 drafts were necessary, he said. (But that was nothing new: he claimed that all his scientific work went through 5 drafts too.) His fairness shone through in his biography of Arthur Eddington:
    Chandrasekhar S (1983) Eddington, the most distinguished astrophysicist of his time. Cambridge University Press, Cambridge.
    Who, reading this monograph, would suspect that this man had treated Chandra in a rough and humiliating way? At various times he had encountered colour

prejudice even amongst fellow scientists but he did not show pain or anger: “Goodness gracious”, he would say, “fancy that!” One could sometimes sense his displeasure, however, by pejorative remarks he made about their understanding of scientific issues.

Chandra could be stubborn. I believed (and still believe) that Eddington’s famously imaginative explanation of Cepheid variability did not come “out of the blue”, but was inspired by Rayleigh’s explanation of Rijke’s tube a few years earlier; see p. 231 of

Strutt JW (1896) The theory of sound, vol 2. 2nd Edition. Cambridge University Press, Cambridge [Dover edition, 1945]

Chandra rejected my speculation “out of hand”. He was also strongly opposed to asymptotic methods, which he seemed to feel were mathematically “dirty”. Perhaps he heeded too much Abel’s famous dictum: “Divergent series are the inventions of the devil, and it is shameful to base on them any demonstration whatever”. In such supping as he did with the devil, he used a very long spoon! Three examples come to mind from his book

Chandrasekhar S (1962) Hydrodynamic and hydromagnetic stability. Oxford University Press, Oxford

On page 104, he discusses his $Ra \propto T^{2/3}$ law; on page 177, he does the same for his $Ra \propto Q$ law; and in §32 he examines rotating convection for zero Prandtl number Pr. If he had been willing to entertain asymptotic methods, he would have quickly discovered why the first term in the $Ra \propto T^{2/3}$ law performs so badly for no slip boundaries: the second term in the asymptotic expansion for $T \to \infty$ is $\mathrm{O}(T^{7/12})$, i.e., it is almost as big as the first! See

Roberts PH (1965) On the thermal instability of a highly rotating fluid sphere. Astrophys J 141:240–250

The strange result for $Pr = 0$ has been elucidated by

Zhang K, Roberts PH (1997) Thermal inertial waves in a rotating fluid layer: exact and asymptotic solutions. Phys Fluids 9:1980–1987

The corresponding MHD problem is addressed in

Roberts PH, Zhang K (2000) Thermal generation of Alfvén waves in oscillatory magnetoconvection. J Fluid Mech 420:201–223

14. Merrill RT, McElhinny MW, McFadden PL (1983) The magnetic field of the Earth. Academic Press, San Diego

    On p. 17 it is reported that, in 1905 shortly after writing his special relativity paper, Einstein described the problem of the origin of the Earth’s magnetic field as being one of the most important unsolved problems in physics.
15. Herzenberg A (1958) Geomagnetic dynamos. Philos Trans R Soc Lond A250:543–585
16. Bullard EC, Gellman H (1954) Homogeneous dynamos and terrestrial magnetism. Philos Trans R Soc Lond A247:213–278
17. Lowes FJ, Runcorn SK (1951) The analysis of the geomagnetic secular variation. Philos Trans R Soc Lond A243:526–546
18. I had unwittingly stumbled onto the identical idea that had led Alfvén to the discovery [3] of his waves:

    Alfvén H (1943) On sunspots and the solar cycle. Arkiv f Mat Astron o Fys 29A(12):1–17.

    He was convinced that sunspots were produced by magnetic activity deep within the Sun, and discovered his waves when seeking an agency that would carry this