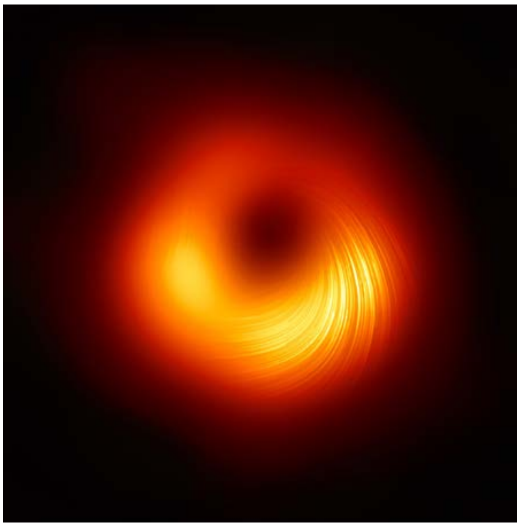
Highlights

by Hans Goedbloed

A road less travelled

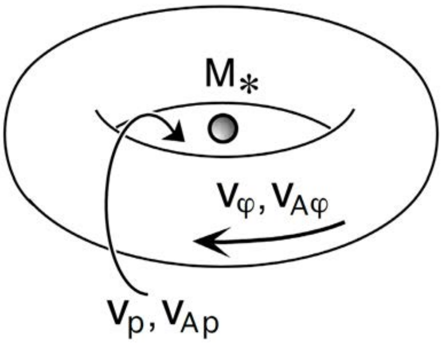
During the last decade of the twenty-first century, three miracles occurred:
(1) the detection of gravitational waves in 2016,
(2) the first `photograph' of a black hole in 2019,
(3) the detection of the magnetic field structure `at the edge' of that black hole in 2021.
All this is happening precisely hundred years after Einstein's discovery of the fundamental laws for the motion of massive objects molding the dynamics of the space time continuum of our Universe. For the first miracle, it took more than a thousand scientists closely working together during a long period of time to obtain the incredible accuracies that even Einstein himself did not believe would ever be obtainable. The second and third miracle concerned a similar effort of a large number of astronomers involved with the world wide Event Horizon Telescope. When Monika Moscibrodska of Radboud University presented that magnetic field structure at a DIFFER seminar in May last year, I was all excited. Here we are looking at the magnetic field of a toroidal plasma (the accretion disk about a black hole of millions of solar masses) with an obvious toroidal component at a distance of some 50 million light years: Very similar to a tokamak, and we at DIFFER have a lot of relevant knowledge on that! That may sound as an audacious claim. I will try to substantiate that claim in this article.



Magnetic field at edge of M87 black hole

Think big!

Teaching theoretical plasma physics to classes at universities, I usually stated somewhere during the course that "Magnetized plasmas are the main visible constituent of matter in the Universe, and that is why the majority of courses in this department is devoted to that". Students usually reacted to that with a kind of sheepish smile (is this guy trying to pull our leg?), but hopefully they got the first part of the message: We have hardly begun to explore the plasma features of nature, just wait and see! And that is precisely what is happening at this time and, in hindsight, it completely justified the titles of our books on "Magnetohydrodynamics" which we (myself, Rony Keppens and Stefaan Poedts of Leuven University) extended with the specification "of laboratory and astrophysical plasmas": Those plasmas are to be treated on an equal footing! If you do, some spectacular discoveries are to be made, as I know now from experience. However, I first have to describe the rather backward road getting there.



Accretion disk: a tokamak!

Second rate physics

When I finished my studies at Delft Technical University as an experimental physicist, I had realized that I did not really have the skills, nor the desire, to do experimental work. I wanted to be a theoretician, of course as everybody at that time (and still) in elementary particle physics! And I nearly got an appointment in one of those high brow theory groups, but got rejected in the end when they realized that I had absolutely no background in that direction. Big disappointment, but actually (as I later realized) a blessing in disguise: starting from scratch and then immediately finding myself in the position of having to compete with other young scientists, at that time doing their Nobel prize winning work, would have been an absolute guarantee for mental breakdown! So, instead, I accepted an offer to become “the house theoretician” of the screw pinch group of the FOM Institute for Plasma Physics at Rijnhuizen. I did not know anything about plasmas either, and I was definitely infected by the low esteem of the “real” theoreticians that plasmas, since classical, could not be very fundamental: second rate physics by second rate physicists. On top of that, at the very first international conference I was attending some big shot in the field asked me “What are you working on?”. When I answered “Stability of the screw pinch”, his reply was “Oh, that is an old fashioned subject, that is all understood, that has been solved by Newcomb. Did not you know that?”. The problem of my thesis has already been solved! What can I do?

A firm basis

OK, I have to study the 1960 paper by W. A. Newcomb on “Hydromagnetic stability of a diffuse linear pinch” and I do. Again, I did not realize it at the time: another blessing in disguise! Here is very solid analysis on the singularities of Alfvén waves propagating in all magnetized plasmas, irrespective of where they occur, in laboratory or astrophysical plasmas. Newcomb’s paper is frequently quoted by plasma physicists but, as I later find out, most of them have not actually read it. And I do not regret the enormous amount of time I have invested to understand everything in that paper. I can build upon that, first my thesis, and next, nearly everything I have published since then! One step is missing at that time though: the realization that the equations of magnetized plasmas are independent of scale. That came much later (in our 2014 MHD book), and eventually it will ty everything together: tokamaks and accretion disks about black holes described by the same equations! There is hardly any limit to the applications. Second rate physics? What a nonsense! But remember, this is all in hindsight, knowing the outcome. There are still some disappointments to come.

Fighting an authority

It turns out that the spectrum of waves and instabilities in plasmas, as described by the equations of magnetohydrodynamics, is the central topic of study at the Courant Institute in New York, where I am invited to work after completing my PhD study, surprisingly, precisely because of the content of my thesis: not so bad after all! But wait a second, the big boss (Harold Grad) completely disagrees with one of the theorems I have proved there: Excepting the singular modes (the ones Newcomb was concerned about), “the MHD spectrum is monotonic with respect to the number of nodes of the eigenfunctions” (like in classical mechanics of vibrating strings). His statement is that “your proof is of the number three category” (the different categories being: (1) wrong, but not so bad since it can be fixed, (2) wrong, and whether it can be fixed or not is not clear: not so good, (3) wrong, and cannot be fixed because it is wrong: very bad!). Also, he has a counter example for his claim. At this point in time, I have gotten rid of all my reserves: either he is right or I am, and in both cases I will win, if not by being right then by learning some non-trivial mathematics. I prove his counter example to be invalid, but that is not the end of the fight. An intense period of nearly two years follows where he ever raises new objections, which I then negate again one by one. In fact, I learn a lot, in particular that any well-formulated scientific problem has an answer, it just takes perseverance to find it. However, I am quite disturbed with his anger at me and I take the wrong decision not to publish my results, just put them in my desk for 24 years. At that time, the false claims on the MHD spectrum are revived by other people. I then simply copy the 1974 Memorandum, add a very short introduction and send it to a journal. And, one may have predicted that, I have to fight once more, a referee this time who has more confidence in the wrong spectral results than in an old text produced on an electric typewriter. I then extend the introduction and convert the Memorandum into TeX. This works miracles: the same referee is impressed now (“a real tour de force!”) and the thing gets published. Morale: if you want a large number of publications, don’t delve too deeply!

Large-scale computing

At the end of that period, reluctantly, I decide to follow the advice of a wise man to accept the invitation to come to Los Alamos. I then encounter another blessing in disguise: collaboration and friendship for life with Jeff Freidberg, and exploiting the modern computer facilities of Los Alamos Scientific Laboratory (developed for the design of nuclear weapons, we now exploit them for something useful). We develop a large-scale computer program on stability of toroidal plasmas that I keep operational for a long time, up to the point that I get contracts to do stability studies at

JET. At this point, I have learned the other basic craft of a theoretical physicist, besides analysis: hunting out errors in a Fortran code! I know how to do that so that I can later instruct students when they ask me to help them to find an error in their code: “Who wrote the code?”, “I did”, “So, you have to find the error”, “How long would that take?”, “I do not know, if you are lucky an hour, but it may also take a couple of weeks. But I can assure you, it gives a kick when you find it.”

A powerful couple

When analysis and computing go hand in hand they are extremely powerful in solving deep scientific problems. Analysis is most effective in the neighborhood of a singular point (where some physical quantity becomes infinite), computing is imperative away from such a point. Together, they may be used to approximate a solution everywhere. I will illustrate that by the example of the stability of a magnetized accretion disk about a black hole. However, `jumping’ from tokamaks to astrophysical plasmas only came after a long roundabout route, when `we’ (groups from Leuven, Garching, JET and Rijnhuizen) realized that the finite element codes we were exploiting for our studies at JET could be converted to any other magnetized plasma, e.g. the corona of the Sun. This turned out to be a success formula. Getting plasma physics projects approved all of a sudden became much easier than before: inter-disciplinary projects attract much more attention than mono-disciplinary ones. Moreover, since large-scale computing inevitably requires the collaboration of scientists with different expertises, it was essential to get competent computational physicists involved. At this critical point in time, I was extremely lucky to first get Stefaan Poedts involved and, a number of years later, Rony Keppens. Together with our PhD students we solved many problems in tokamaks, in solar and later in accretion disk physics. To my intense pleasure, that collaboration and friendship continues to the present time!

Thirty years of relief

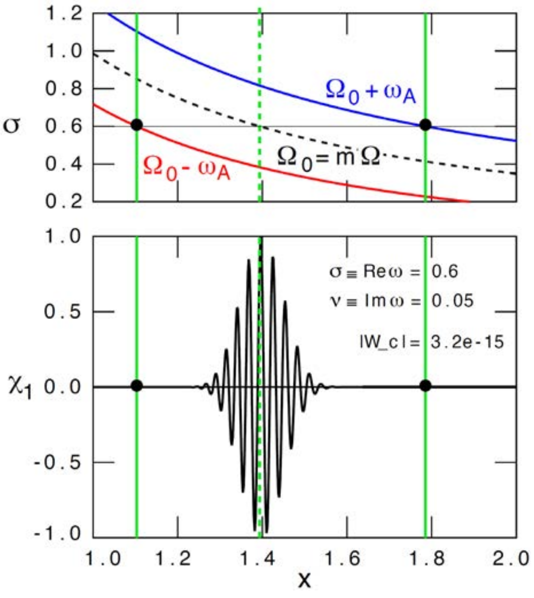
Back to the accretion disk problem now. Of course, an accretion disk is not the same as a tokamak. Whereas scale-independence of the plasma equations implies that the enormous difference in size is not important for a proper analysis, the difference in equilibrium certainly is. A tokamak is confined by the Lorentz force between the magnetic field and the plasma current, the accretion disk is kept in equilibrium by the balance of gravity and rotation, like the Keplerian rotation of the planets about the Sun. However, the rotation of an accretion disk does not last ‘forever’. It is observed that the plasma spirals inward toward the black hole on a short time scale: there is accretion, but why? The friction of a regular fluid completely fails to explain this time scale, but some anomalously large dissipation mechanism associated with the turbulence caused by a plasma instability should do the job. When, in 1991, Balbus and Hawley proposed the Magneto Rotational Instability (MRI) for this purpose, together with a simple cartoon illustrating the physics, the astrophysical community was relieved: problem solved, it is all understood! (sounds familiar). However, the analysis of the MRI is based on the simplifying assumption that the instability conserves the rotational symmetry of the equilibrium: there is no angular dependence of the modes ($m = 0$). Obviously, turbulence does not respect such a constraint. Also, dropping it immediately implies that the instabilities will no longer be purely exponentially growing but will attain a huge real frequency component proportional to the rotation frequency: the Doppler shift. Why was the MRI analysis not modified in the thirty years after its proposal to repair such an obvious defect? Surprisingly, the two-year covid 19 pandemic turned out to be instrumental both to understand the omission and to cure it.

The Super-Alfvénic Instability in accretion disks about a black hole

Rony Keppens and myself had already been studying different routes to turbulence in accretion disks for quite some time, and we were planning to write a paper on it exploiting the different methods we had developed, but then the pandemic interfered. For me, this did not change a lot, I was already used to the seclusion in my study, I actually liked it. This was an excellent opportunity to finally address the central problem of accretion disk stability: extend the analysis by breaking the axisymmetry and introducing the Doppler shift! The first attempt, exploiting the WKB method that worked so well for the MRI, turned out to produce complete nonsense. This was followed by numerous other attempts producing many pages of algebra that wound up in the trash can. I will not describe any of them, but quickly jump to the final approach that worked. Suffice it to say that all of these failed attempts were accompanied by a similar number of ups and downs of my spirit. In the end, of course, I had to return to what I had learned from Newcomb: take those Alfvén singularities more serious than the astrophysicists do! That produced much more than the correct solution. First, concerning the axisymmetric MRIs ($m = 0$), there are Alfvén singularities there, but they are the same as in tokamaks: stable continuous spectra, not affected by the rotation, and located far away from the finite set of instabilities in the complex omega–plane. If the rotation then enters with the Doppler frequency of the non-axisymmetric modes, the picture becomes much more interesting. For any value of m, there are infinitely many unstable modes now, that are not far away from the Doppler-shifted continuous spectra, but emitted by them with infinitely many oscillations: Very unlikely that nature would have chosen the small set of the old axisymmetric MRIs rather than these infinite sets of the new non-axisymmetric modes to excite turbulence in accretion disks! Since the crucial difference with the MRIs is that the Doppler frequency is large, exceeding the Alfvén frequency, we called them Super Alfvénic Rotational Instabilities (SARIs).

Continua of quasi-discrete SARIs!

The discovery of the SARIs would have been more than enough for a nice publication on the subject, but there is much more to be extracted from a careful singular analysis! So far, both the MRIs and most of the SARIs could be computed with the new method of the Spectral Web, that I published in Physics of Plasmas in 2018. This yields curves in the complex omega plane which intersect at the eigenvalues. However, for some parameter choices that method appeared to produce numerical rubbish: the Spectral Web fragments in myriads of loops in the complex plane, suggesting continuous regions of unstable eigenvalues (exciting!), but never really crossing (disappointing!). This happens when the continuous spectra overlap, as shown in the picture. Any real frequency in a large range intersects them in two points (black dots in the top frame) that correspond to two radial points of the disk (black dots in the bottom frame). At those radii, singular skin currents are induced that completely isolate the mode from its surroundings, as if `virtual walls' are placed there. That is good news: the modes are neither influenced by poorly known conditions at the inner edge ($x = 1$), corresponding to the black hole horizon, nor at the outer boundary ($x = 2$), corresponding to the inter-stellar medium. This remarkable localization is revealed by blowing up the eigenfunction by a factor of 10^{10}: approaching the two dots from the outside, the eigenfunction `explodes' from completely negligible to `very small' but large enough to produce the visible oscillation about the Doppler point further down. The interaction of the two singularities has produced a complex quasi-discrete SARI hovering over the real Alfvén continua. This happens over large continuous regions of the complex omega-plane: we have found continua of quasi-discrete SARIs! The prefix "quasi" indicates that the `eigenfunction' still has an extremely small discontinuity (of the order of the machine accuracy of the computations), corresponding to a tiny amount of energy needed to drive the mode. Such amounts of energy are abundantly available in the disk: perfect for turbulence!



Alfvén spectrum and SARI 'eigenfunction'

Déjà vu

We now have a complete, but very non-standard, picture of all the non-axisymmetric modes in an accretion disk. Time to submit and, you already guessed it: necessary to fight an authority again! The referee, by the way confiding "to know the field", belittles our results because he does not believe that modes that are so strongly determined by the outside boundary conditions could be responsible for turbulent accretion. Interesting, that would also rule out the MRIs! Ah, but this is an easy one to rebut: he has not read (or not understood) the second part of our paper (see the picture above) and we ask for another referee. That one is from quite another tribe: first, he has read the whole paper very carefully and comes up with numerous suggestions for extensions and relevant references that we have to include, which we do of course. His final verdict is: "A true tour de force, and I therefore share in their excitement of their method's power and insights it may hold for the future". The paper is accepted for publication in Astrophysical Journal Supplements. If you are interested in the details, the full text may be found at https://doi.org/10.3847/1538-4365/ac573c.

More miracles

In hindsight, (1) having been `condemned' to work in a corner of physics; (2) there and then learning about the miracle of plasma singularities present everywhere in the Universe; (3) finding collaborators who became friends to help unravel the consequences of this; and (4) to live at a time when it all becomes visible by numerous astronomical observations:
I am a blessed man ("gezegend mens", in Dutch).