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Cosmical Gas Dynamics, Why Was It So Difficult?

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1 Introduction

When the invitation for this meeting reached me, I did not hesitate. Of course, I wished to be present when it was Franz Kahn's turn for the formal move to say goodbye. Like many of us, he will probably sneak in again through the back door and continue working.

The choice of a subject was more of a problem. I am not an expert in gas flows on a galactic scale, and I missed (largely or wholly) two relevant symposia: "*The interstellar disk-halo connection in galaxies*" (Leiden, 1990), (Bloemen, 1991) and "*Back to the Galaxy*" (Maryland, 1992) (Holt and Verter, 1993). Yet I chose this subject, because for a very limited number of years in my career, I had ambitious plans for research in this direction.

My interest started early 1949, when Jan Oort gently put me to work for the Paris symposium on Cosmical Gas Dynamics, going to be held that summer. It ended 5–10 years later in steps. First, the discovery of the 21 cm line drew me into practical radio astronomy. Secondly, when Lo Woltjer became a professor at Leiden, I had learned enough to know how difficult these problems of gas dynamics are, and was happy to leave that part to him. Thirdly, COSPAR drew me from astrophysics into politics. Excuse me for mentioning these personal details.

And this, incidentally, explains the "was" in my title. The problems may be difficult even now, but I cannot tell, because I did not try.

During the eight years mentioned, I participated intensively, as editor or as organizer, in three symposia organized jointly by the International Astronomical Union IAU (read: Jan H.Oort) and the International Union for Theoretical and Applied Mechanics IUTAM (read Jan M.Burgers). The venues, titles, and references are:

- I Paris (1949), "*Problems of Cosmical Aerodynamics*".
J.M.Burgers and H.C. van de Hulst, editors;
Central Air Documents office, Dayton (Ohio), 1951.
- II Cambridge UK (1953), "*Gas Dynamics of Cosmic Clouds*",
IAU Symposium No.2, J.M.Burgers and H.C. van de Hulst, editors,
North-Holland Publishing, Amsterdam, 1955.
- III Cambridge Mass (1957), "*Third Symposium on Cosmical Gas Dynamics*"

J.M.Burgers and R.N.Thomas, editors;
Rev. Modern Phys, **30**, 905–1108, 1958.

The numbers I, II, III, will be used in the quotations below.

When Oort asked me to help him and Burgers with the preparation of Symposium I, I had barely finished my post-doc years. Franz Kahn, who gave three papers in Symposium II and **two-and-a-half** in Symposium III, must also have been a fresh Ph.D.

It was a great privilege for me to mix with the giants of the time, and, as an editor, to try to make sense of deep but very confused discussions. And—by the way—the private tutoring from both Burgers and Oort was invaluable.

Generally, I am not given to history writing. But in this particular instance, where most of the persons have changed, but many of the problems are still the same, I thought it useful and amusing to comment on the way the problems were seen at that time. History can be helpful, for it bares the roots of our present concepts, and thereby gives us a firmer ground from where to assess the ambitions, irritations, and misunderstandings that plague us today.

I shall work mostly by literal quotations from these three volumes. You probably understand that these are embellished versions of what actually has been said. In one of the discussions a participant accused another one of throwing mud in his eyes. The most amusing line from the original transcript of the taped record was:

Mayall: *Do you refer to secrets?* Oort: *Yes.*

Magnetic tapes were quite new then, and an American Air Force Office had offered to make and transcribe these records. “Secrets” is what they heard. Unfortunately, in the edited text (I, p.185) it had to be corrected to *Seyferts*.

2 The General Plan

Let me read to you the words by which Burgers prefaced the first symposium.

Burgers (I,i): *The plan to organize a Symposium on borderline problems between astrophysics and gas dynamics originated in 1948, as a result of several informal discussions on the subject treated by Professor J. H. Oort in the George Darwin Lecture for 1947 before the Royal Astronomical Society in London, entitled “Some Phenomena connected with Interstellar Matter.” The peculiar conditions presented by gaseous matter in cosmic space appeared to open promising new fields for study. While these conditions necessitate a far-reaching application of molecular and atomic physics, they reveal at the same time many features which are of great interest in connection with present-day developments in hydro- and aerodynamics. In particular, the problems of turbulence and those of expansion phenomena and of shock waves immediately come to the foreground.* Please note a slight error; the actual year of Oort’s lecture was 1946 (Oort, 1946).

Sir G. I. Taylor, in introducing the second symposium, adopted a slightly more sceptical tone.

Taylor (II,1): *The fact that hydrodynamicists have solved a limited number of problems of continuous media gave rise to the idea in the mind of Burgers that they may have something to contribute to cosmological theory and so, being a man who never lets grass grow under his feet when he sees a chance of promoting useful scientific cooperation, he arranged the first symposium in Paris. Since that meeting, and no doubt largely because of it, there have been developments in theory.*

The mutual understanding did not always come smoothly. The easy part of it related to factual information and led to many straight questions and answers. It is a pleasure to reread these discussions and to see how competently such questions were handled. I just quote some examples from (II,112) and (II,242).

Savedoff: *How does one obtain the spectra of luminous edges?*

Minkowski: *With high-aperture spectrographs and preferably not too big telescopes because the objects are fairly large.*

Van de Hulst: *In one spectrum of the Cygnus loop taken at McDonald I found a very strong change of the ratio of $H\gamma$ to 4363 along the length of the slit. How should this be explained?*

Minkowski: *This may be due to a local variation in electron temperature. Perhaps there is a hot spot.*

Liepmann: *Is it certain that the physical condition of the Cygnus wisps is stationary?*

Minkowski: *Yes, the relaxation times are of the order of a few years, while the loop has been observed for more than 60 years.*

Hoyle: *Another problem that should be settled by observations is the Chandrasekhar-Fermi theory of the magnetic field along a spiral arm. If the suggestion that the motion of the clouds is only strong enough to cause a simple corrugation of a homogeneous field is correct, this should show up in the statistics of cloud motions. Or are they sufficiently random to tell that this is not the case? Is there a hope to find out ?*

Oort: *There is a definite possibility by looking perpendicular to the arm and along it. So far, no evidence of a difference is available.*

Von Karman (II,180; after having received the answer that a suggestion he made about the flatness of galaxies was ruled out by the observations): *My imagination is not handicapped by any knowledge of the facts.*

This all referred to the 'easy' part. When it came to the 'deep' questions, there was repeatedly a tendency to jump to conclusions. This, in turn incited other aerodynamicists to serious warnings that the astronomers should not do that. In symposium III the irritation after two days of papers and discussions had grown so large that the organizing committee decided to cancel all papers that had been scheduled for the third day and, instead, inserted an interim summary discussion that took the full morning. Batchelor introduced this discussion as follows (III,994).

Batchelor (III,994): *Last night, Burgers, van de Hulst, Thomas and myself, feeling a little dissatisfied with the way the sessions had gone up to that time,*

decided that it would be useful to have some stock-taking, in order to see what has been accomplished so far.

It was then my turn to present a list of questions which had emerged from the brainstorming of the night before (questions 1–6), to which Batchelor added a further one (question 7). They were carefully distinguished into astronomical and physical questions, and to each category a list of ‘permissible’ answers was spelled out. For the present review it suffices to quote only the questions (III,994–995).

1. *How precisely can we estimate the conversion from energy produced internally by hot stars into kinetic energy of turbulent motion (or cloud motion) in the interstellar gas?*

2. *Does a magnetic field effectively inhibit dissipation of energy from turbulent motion (or cloud motion) into thermal motion?*

3. *Differential galactic rotation: Given a distribution of mass in the galactic system that defines a gravitational potential, and given in this gravitational potential field a disk-like distribution of gas, in which the gas moves in laminar flow with circular orbits, is this motion unstable and will it develop turbulence?*

4. *Is a galactic halo, which is half ionized, half neutral, as proposed by Pickelner, compatible with the observations of the 21-cm line?*

5. *Do the observations give convincing proof that the magnetic lines of force run along spiral arms?*

6. *Equipartition: does it establish itself, and how rapidly?*

7. *How literally may we regard the gas clouds in the spiral arms as discrete?*

Permit me to add the wish that organizing committees would envisage taking such liberties more often. A “workshop” usually is an euphemism for just another collection of pre-arranged papers. Drastic moves should remain possible and in a true workshop the printed program and schedule should not be considered to be sacrosanct.

The following sections deal with the contents of what was discussed in these symposia. I have arranged the topics under a few separate headings, which may be considered to be the main themes.

3 MHD Waves, a Problem that could be Solved

Well before the first symposium, Oort had suggested that I should study in particular the Alfvén waves or magneto-hydrodynamic waves. This concept was about 10 years old and unchallenged, but it had hardly penetrated into general astrophysical theory. Oort was worried that Alfvén, who would be one of the participants, might not get much of a response in the discussion.

I duly followed Oort’s advice and studied the MHD waves, not stopping at the usual place where the velocity is determined as an eigenvalue from the dispersion relation, but actually continuing—as an exercise—to find the amplitudes and phases of the field and velocity components, and the precise exchanges between the different forms of energy.

The actual history took a quite different turn from what Oort had feared. Early summer 1949 interstellar polarization was discovered by Hiltner and Hall. From the first day of the first Symposium, all speakers included magnetic fields in their theories and speculations. Evidently, many of the statements made were what now is called handwaving. This was fully recognized during the symposium. It became even clearer when, after the symposium, I was given the job, together with Burgers, to edit the hours of confused discussion from the taped record.

It then occurred to me that the least I could do was to extend my preparatory 'exercise', which I had to write up for publication anyhow, by including the effect of compressibility. That should lead to a restricted problem, the exact solution of which should be feasible. So I worked for a few months that same fall, mostly in switching the orders of the unknowns and of the equations around, until I obtained a pleasantly symmetric form. I still do not know why that is possible. Finally (I,50), the determinant equations, from which the eigenvalues can be solved, took the form reproduced in Fig.1.

Clearly, if you put the imposed, constant magnetic field zero, the coupling is gone and the matrices separate, as shown by the dashed rectangles. The solution then consists of unrelated wave forms. But the magnetic field terms couple them all. It gave me a big kick for the first time to have a set of equations at hand that in one distinct limit described light and in another distinct limit described sound.

4 Shocks and other fronts

The small-amplitude waves in a homogeneous medium with an imposed homogeneous magnetic field, which I just discussed, were only an exercise. That much was clear. The real world would be more violent, leading to non-linear phenomena. Shock waves were the classical example, and the astronomers had the benefit of hearing excellent tutorial papers both on the theory and on the laboratory experiments with shock waves.

Let me read part of the history lesson, which Von Neumann gave us (I,75).

Taylor: Riemann also inferred, essentially by physical insight, what happens when the continuous solution ceases to exist. He made it very plausible that a discontinuity of a certain type, a "shock wave" develops.

This was subsequently independently rediscovered and further developed, by Hugoniot. It is also true that in the entire literature up to 1910, i.e., up to the time of the work of Rayleigh and G.I.Taylor, there was a considerable confusion and disagreement between the authors on exactly what the shock looks like.

Several years later, a new generation of astrophysicists had caught on to this subject. Symposium III contains papers by Kahn (III,1058 and 1069) and Goldsworthy (III,1062) on shock waves and ionization fronts. What I remember most clearly, however, is how I sweated over the elaborate papers of Goldsworthy and Axford in the Philosophical Transactions (Goldsworthy, 1960; Axford, 1960). Why

were they so difficult?

I now feel that part of the reason was that I still was too attached to the basic simplicity of the linear wave theory. I just did not have enough patience, or enough guidance, to get to grips with the more general problems. An added reason may have been that I was used to making checks and double-checks by looking at the same problem in different coordinate frames. This had the consequence, that words like “before” or “after” the shock often left me in a state of confusion. I am told that this is not an uncommon experience for a beginning student.

5 Numerical solutions

Nowadays any student knows that a problem which is too difficult to be solved analytically has a good chance of being amenable to a numerical solution, given enough computer time. On looking back at the 8 years which my review covers, it is remarkable that there were so few numerical solutions or simulations. In these three symposia they played no role at all. This has to do, among other things, with the enormous memory space that a true fluid dynamics problem in more than one dimension takes, if solved on a computer. It just could not be done yet! Other speakers at this symposium will inform you about what can be done now.

Personally, I never became actively engaged in numerical gas dynamics, although I made certain preparations and remember several visits and conversations which made me aware of its large potential. Let me, in this connection, read you one further quotation. The development in this field was rather accurately foreseen by A.N.Lowan. The date happens to be also 1949, but the source (Lowan, 1949) does not have any relation to these symposia.

A.N.Lowan: When the Computation Laboratory has been thoroughly mechanized (perhaps “electronized” would be a truer description), it may be expected that the chief emphasis will be not on the preparation of new basic tables but rather on the solution of physical problems whose treatment by present-day methods is an unsurmountable task. Even then, however, it may be confidently predicted that there will still be a need for mathematically trained human operators equipped with desk calculators for the purpose of carrying on work of an exploratory nature and tasks which are either too small for high-speed calculators or which require the type of human intelligence, discrimination, and initiative which may not yet have been incorporated in the design of electronic computers.

6 Turbulence

The word “turbulence” had been loosely used in the astrophysical literature—and is still being used that way—as a word for any irregular gas flow that causes non-thermal Doppler effects. We had to learn from the gas dynamicists that in their vocabulary the word “turbulence” had a far more precise meaning. Yet—so we learned—even the simplest case of homogeneous, isotropic turbulence in

an incompressible fluid did not have a complete theory. Only the Kolmogoroff spectrum was generally accepted.

Starting from this incomplete theory, the discussion in Symposium I branched out primarily into two directions: turbulence with magnetic fields and turbulence with compressibility.

When, at the second symposium, it was time to take stock, the status of these two topics was summarized as follows.

Batchelor (II,117): *Firstly I should like to remind people that the determination of the asymptotic level of magnetic energy in a medium of high conductivity which is in statistically steady homogeneous turbulent motion, is still an unsolved problem—or, at any rate, is still a disputed problem—and it would be useful to hear of any recent developments.*

Lighthill (II,121): *If any applications of turbulence theory to astrophysics are to be made, it will be necessary to consider what effect compressibility may have on the postulated motions. This paper describes what is known, or can reasonably be conjectured, about the influence of compressibility on the turbulent motion of fluids, but makes no attempts to apply the results to any astrophysical problem.*

A detailed quantitative theory of the process, with good experimental backing, exists only when the root mean square Mach number of the turbulence is small compared with 1. For larger values of the root mean square Mach number one can make only tentative conjectures, . . .

According to these conjectures, the influence of compressibility becomes dominant for root mean square Mach numbers comparable with 1, or greater.

Misgivings about ‘supersonic turbulence’.

My further comments will be confined to the second topic. The notion of ‘compressible turbulence’ in the discussions soon got the name of ‘supersonic turbulence’. Some of the bolder thinkers interpreted it as becoming the same thing as ‘independently moving discrete clouds’. These speculations in turn led to cautionary remarks and serious warnings by the more conservative participants. I cannot give you a well-organized review but simply wish to read a few quotations to illustrate this confusion.

Von Karman (I,213): *I don’t understand why you say that if the flow is supersonic, the disturbance goes faster than sound. I think that this has no physical meaning. First, it has no meaning that the flow is supersonic. What does that mean? One can as easily say that the motion of the Earth is supersonic, because its velocity is 30 km/sec. The real statement should be this: We have supersonic motion of a solid body through a medium when the velocity of the body relative to the medium is greater than the velocity of sound, the latter being defined as the velocity of propagation of infinitely small disturbances. (The propagation of a finite disturbance will have a velocity which is larger than that of infinitely small disturbances.)*

But if we are talking of turbulence, I don’t think there is any difference between “supersonic” and “subsonic” turbulence.

Gold (II,238-239): *The occurrence of high Mach numbers can be more easily understood when we consider that such forces as gravitation, and not only pressure gradients, may be responsible for the motion. Lighthill's idea that turbulence at high Mach numbers may be interpreted in terms of an assembly of shock waves seems to me an extremely fortunate one for explaining a variety of astronomical features. It has been pointed out by Kantrowitz that shock waves appear to make order from chaos; they can lead to the appearance of smooth shapes and contours which would fit the observations much better than the chaos of ordinary turbulence.*

Liepmann (II,241): *I wish to protect—for the time being—the shock waves from misuse. We should not suddenly expect everything from shock waves. For example, shock waves cannot be considered as a simple alternative to shear waves or turbulence. It does not seem possible to conceive an ensemble of shock waves of finite strength without a coupled shear field. The interaction between two strong shocks always creates a vortex layer and hence a random ensemble of shock waves will be coupled with a turbulent field of random vorticity.*

Nevertheless, toward the end of the second symposium, Batchelor saw some light.

Batchelor (II,242): *The “statistical assembly of shock waves” that has been mentioned in earlier sessions is not a very clear concept for me. We are trying to imagine the properties of a turbulent motion in which the Mach number is very high. A way of doing this which may throw some light on the situation is to think of the high Mach number as being produced by a very small velocity of sound. In the limit of zero velocity of sound, or infinite Mach number, hydrodynamic pressure loses its meaning and the various particles of the gas move independently of each other. Thus it becomes necessary to think in terms of particle dynamics rather than in terms of hydrodynamics. From this point of view the picture of discrete clouds moving independently in a rarified background medium and occasionally making collisions may not seem to be so strange.*

But his assessment in the “inserted summarizing discussion” during the third symposium, four years later, was less positive.

Batchelor (III,997): *I, for one, have never been clear about the explanation of the discreteness of the clouds. I do not see how any theory of turbulent motions of high intensity can lead to the view that there would be separate clouds of high density, proceeding more or less independently of each other.*

7 Conclusions

After this somewhat haphazard review I must try to come to conclusions. In tribute to Burgers, I like to start off with a quotation from his closing words of the first symposium.

Burgers (I,237): *It is a curious thing that we can think and reason about the depths of the universe, in particular when one puts against it the material from which its structure and its laws are deduced: minute specks on photographic plates*

and human fantasy playing around with mathematical formulas.

It has been said sometimes that the order we see in the universe is something we put in it ourselves. Certainly our thinking is a continuous striving for bringing order in what we experience.

That our minds may reach to such images and to conclusions as those about which we have heard in these lectures, is a gift for which we cannot be too grateful.

This quotation shows Burgers as a philosopher. For those interested I may point out that Burgers went more deeply into such philosophical questions in his book “Ervaring en Conceptie” (Burgers, 1956).

My conclusions will focus on the differences between the situation then and the situation now. I have tried to formulate these differences in the form of six key words or brief statements, to each of which I shall add my private comments.

1. Proliferation of data. Since 1950 the observational data have proliferated enormously, in quantity, in sky coverage, in wavelength coverage, and in precision. The theoretical understanding has also grown, but by no means in the same proportion.

One can hardly begin to list all the observational improvements: molecular clouds, hot X-ray emitting gas, amazing details in 21-cm maps, the entire infrared sky, gamma rays, etcetera.

In illustration of this point I showed 7 slides, which cannot be reproduced in this printed version. Three slides were true-colour pictures of wide areas in the sky: the molecular cloud near ρ Ophiuchi, the dark rim in Ara, and the Vela supernova remnant. Further, one result from the Hubble Space Telescope was shown, the ‘spike’ in the Cygnus veil (Raymond, 1994). Finally, three slides represented other wavelength regions, a local radio polarization map showing unexpected streaks (Wieringa, 1991), an IRAS 100 μm map processed to make the structure of the ‘froth’ visible (Waller and Boulanger, 1993), and the gamma-ray sky copied by a baker in coloured candy from the COS-B results in 1980, to which the remark was made that the very recent EGRET data on CGRO give a similar picture with far greater precision.

2. Reduced confidence in Grand Theories. At the time of the first symposium there seemed to be no reason to drop the notion of a general, pervasive, and cool, interstellar medium. The HII-regions, observed as faint $\text{H}\alpha$ emission regions and predicted by theory as localized pockets of hot gas around groups of O- and B-stars, formed the only exception. Since observations outside the optical domain were still minimal, there was no serious suspicion, by theory or from observations, of an even hotter halo.

All of this changed during the fifties, and even more so when space astronomy started. It is understandable, however, that before that time the astronomers (or at least some of them) had pretty high-strung hopes that certain exact solutions worked out by the aerodynamicists, notably turbulence and shock waves, might be directly applicable to the interstellar gas.

The aerodynamicists responded to these hopes by being present as very active

participants of these meetings. Although the healthy voice of scepticism can be heard in virtually every discussion in these books, the basic feeling was that we were jointly working towards an overall understanding.

3. Do-it-yourself theorists. The present astronomers may be compared with present home owners. They know that many new techniques have been developed and they usually (but not always) do take care to orient themselves on what is available. But then, instead of calling the mason and the carpenter in, they tend to act by the do-it-yourself principle. I feel that this change of attitude is understandable in view of the much more varied picture that the observations show today. Yet I have reason to wonder if the collection of independent handymen (which we are as astronomers) could not profit again by a much closer contact with the professional aerodynamicists, notably in the field of Computational Fluid Dynamics (CFD).

4. Implantation of numerical computations. I use deliberately the medical metaphor of implanting an organ into a living body. The living astrophysical body consists of observations, interpretations, hunches, scenarios and theories. Nobody doubts that this body can greatly benefit from the solution of well-defined numerical problems on a large computer. However, the danger is—like in surgery—that this solution may remain a corpus alienum, that eventually will be rejected before it has grown to be a part of the body and has truly started to perform the function for which it had been intended.

I signal this difficulty, but at the same time I cannot give you an authoritative account of the actual state of affairs regarding this potential hazard.

5. Attachment to claims. My impression regarding this point is hard to support by facts. I have the feeling that prestige-oriented attitudes, including personal claim-staking and not giving up on 'one's own' model, are stronger in present-day astronomy than they used to be. If this is the true situation, it is not a healthy one. The opposite situation of everyone adhering uncritically to a theory once it has come into fashion (the bandwagon effect) is equally undesirable.

6. A true "work"shop is hard to organize. We fight a continual battle against sclerosis in scientific meetings. The name 'symposium' was invented to make it sound less formal than a 'conference'. Subsequently, the name 'workshop' was invented to make it sound less formal than a symposium. The spontaneous action to paste certain new results on the wall was institutionalized into posters. And the programme with its order and allotted times, conceived as a guideline, degenerated into something never to deviate from. I shall be happy if my review of how things were has revived in you some appetite for an occasionally more spontaneous and less predictable style of scientific exchange.

References

I, II, III. See section 1 of this paper.

- Bloemen, H. (ed.): 1991, *The Interstellar Disk-Halo Connection in Galaxies*, Kluwer Acad. Publ., Dordrecht.
- Holt, S and Verter, F. (eds.): 1993, *Back to the Galaxy*, Woodbury N.Y. (American Institute of Physics).
- Oort, J. H.: 1946, *Some Phenomena Connected with Interstellar matter*, *Mon. Not. Roy. Astr. Soc.* **106**, 159-179.
- Goldsworthy, F. A.: 1960, *Ionization Fronts in Interstellar Gas and the Expansion of HII Regions*, *Phil. Trans. Roy. Soc. A* **253**, 277-300.
- Axford, W. I.: 1960, *Ionization Fronts in Interstellar Gas: the Structure of Ionization Fronts*, *Phil. Trans. Roy. Soc. A*, **253**, 301-333.
- Lowan, A. N.: 1949, *The Computation Laboratory of the National Bureau of Standards*, *Scripta Mathematica* **15**, 33-63.
- Burgers, J. M.: 1956, *Ervaring en Conceptie*, Van Loghum Slaterus, Arnhem.
- Raymond, J.: 1994, paper in this volume.
- Wieringa, M.: 1991, *327 MHz Studies of the High-Redshift Universe and the Galactic Foreground*, Thesis Leiden.
- Waller, W. H. and Boulanger, F.: 1993, *Worms or Froth? Fine-scale Structure in the Far-Infrared Milky Way*, in *Back to the Galaxy*, eds. S. Holt and F. Verter, Woodbury N. Y. (American Institute of Physics).

Set I

	(hy)	(Ex)	(Ix)	(Vy)	(Vz)	(p)	(Uz)
(1)	$\frac{i\omega}{c}$	$-ik$					
(2)	ik	$-\frac{\epsilon i\omega}{c}$	$-\frac{4\pi}{c}$				
(3)		-1	$\frac{1}{\sigma}$	$-\frac{Hz}{c}$	$\frac{Hy}{c}$		
(4)			$\frac{Hz}{c}$	$i\omega\rho + k^2\mu$			
(5)			$-\frac{Hy}{c}$		$i\omega\rho + \frac{4}{3}k^2\mu$	$-ik$	
(6)					1	$-i\omega$	
(7)						1	$-ikC$

=0

Set II

	(hx)	(Ey)	(Iy)	(Vx)	(Iz)	(Ez)
(1)	$\frac{i\omega}{c}$	ik				
(2)	$-ik$	$-\frac{\epsilon i\omega}{c}$	$-\frac{4\pi}{c}$			
(3)		-1	$\frac{1}{\sigma}$	$\frac{Hz}{c}$		
(4)			$-\frac{Hz}{c}$	$i\omega\rho + k^2\mu$	$\frac{Hy}{c}$	
(5)				$-\frac{Hy}{c}$	$\frac{1}{\sigma}$	-1
(6)					$-\frac{4\pi}{c}$	$-\frac{\epsilon i\omega}{c}$

=0

Fig. 1. The two determinant equations from which the various modes of magneto-hydrodynamic waves in a compressible fluid may be calculated (I,50).