SESSION IX

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SUMMARY OF CONFERENCE

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For a meeting of people from such widely different fields, this Symposium has exhibited a remarkable degree of unity. There has been one key concept running as a thread throughout the Symposium: the concept of magnetic field line reconnection, or magnetic field line merging as I prefer to call it. It was dealt with directly in many papers, and many others dealt indirectly with it and various related aspects. The concept was applied in the Symposium to an amazing variety of objects and was examined from many points of view and by many different techniques. Magnetic field line reconnection or merging is a paradoxical concept. It clearly depends upon magnetohydrodynamics (MHD); for example, constraints imposed by the MHD relation between the magnetic field and the plasma flow are essential to set it up - without these constraints (if, for example, the electric field parallel to the magnetic field could assume any desired value) the problems we discuss under the heading of magnetic reconnection would merely be moderately complicated problems of magnetostatics. At the same time, departures from ideal MHD are also an essential and unavoidable part of the concept.

It is thus appropriate, before dealing with magnetic field line merging itself, to discuss its prerequisite, the MHD coupling between the magnetic field and the plasma flow. An important example of this coupling is provided by what is sometimes called a magnetic flux rope. It is a system where a magnetic flux tube, typically with the ratio of plasma pressure to magnetic pressure $\beta >> 1$ at both extremes, is subject to a twisting or shearing flow at one extreme and a different twisting or shearing flow at the other extreme, so that the magnetic field between the two extremes of the flux tube becomes twisted or sheared; in many cases, $\beta << 1$ within the in-between region and the magnetic field assumes a nearly force-free configuration. Many examples were discussed at this Symposium, ranging from flux ropes at Venus, directly observed in situ (Russell, Elphic), to coronal loops and similar structures on the sun, remotely observed (discussed by Drake, Alay, Ray, Wu, and by others in connection with coronal heating), to speculative flux ropes associated with extragalactic jets (Eilek). More generally, we may regard a situation where different plasma motions are imposed at the two ends of a magnetic flux tube and the

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question is what happens in between as the prototypical problem of cosmic electrodynamics. As examples, in addition to the flux ropes already mentioned, on the sun one has motions imposed by the photosphere at the two ends; in the magnetospheres of planets the motion is imposed by the solar wind at one end and by the planet's ionosphere at the other; in binary star systems the motion is imposed by the rotation of one star and by the rotation or the orbital motion of the other; in accretion disks the motion may be imposed by the disk at one end and by the rotation of the accreting object at the other, or else, analogously to the sun, by turbulent motions within the disk acting on flux tubes anchored therein. Thus we have a very general problem, and the key point, in my opinion, is that in order to construct magnetic field models one must really understand the imposed motions, what it is that is twisting up the field. In the cases of the solar wind and (to some extent at least) the sun, we can actually observe the motions; in other cases we are for the most part limited to speculation. A piece of advice particularly for the galactic jet theorists: look for the plasma flows if the twisted magnetic field model is to be placed on a firmer footing.

The electrodynamical description of this problem is identical with what is commonly called, in discussions of planetary magnetospheres, the theory of magnetosphere-ionosphere coupling. That name was mentioned during the Symposium only in connection with limited and localized aspect related to the earth's polar aurora, but in fact it is a very general theory of the electrodynamic interaction between a high-β region (the ionosphere) and the region threaded by magnetic field lines from it (the magnetosphere). The theory constitutes one of the most highly developed and extensively tested facets of magnetospheric physics (see, e.g. reviews by Vasyliunas, 1980; Wolf, 1975, 1983; Boström, 1974), and astrophysicists should take note that it has advanced well beyond the stage of simple circuit analogs.

Now the general effect of such twisting or other imposed motions is to store energy into the magnetic field, which is then available to be released if a suitable mechanism for the release can be found; also, the associated changes of the magnetic field configuration may set the stage for the magnetic field line reconnection or merging process. These concepts were discussed extensively, particularly for the case of the sun and the solar corona. The gradual release of the energy in the twisted magnetic field is a candidate for the heating of the corona, the so-called D.C. heating discussed here by Heyvaerts and by Ionson. It is an alternative to the heating of the corona by the interaction and dissipation of hydromagnetic waves, also widely discussed here, both specifically for the sun (Heyvaerts, Van Hoven, Nocera) and in a more general context (Hasegawa). It is not always clear where exactly the dividing line lies between the D.C. heating, from twisting of the field by turbulent motions, and the wave heating, since the effects of imposed motions must propagate out as waves; in practice, though, there do seem to be two distinct types of theories. The old theory of coronal heating by acoustic waves is considered to be dead; there does not seem to be a sufficient energy flux in acoustic waves to be of any importance.

Having twisted the magnetic field lines and set up the nearly antiparallel fields or whatever the required configuration might be, we now come to the discussion of magnetic field line reconnection or merging. At the Symposium there were two general reviews of magnetic merging, one slanted toward the terrestrial magnetosphere (Sonnerup) and the other toward the sun (Priest). The reported developments represent a significant step beyond what are sometimes called the "classic" models of reconnection, developed in the period from the late '50's to the early '70's (see e.g., review by Vasyliunas, 1975). The name has absolutely nothing to do with the distinction between classical and anomalous transport properties; it is used for the irrelevant reason that these models are mostly contained in a few papers regarded as "classic" - and we should recall the definition given by Hines (1974): "a 'classic' paper is one that many researchers no longer read to see what it actually says, for they think they know what it must have said." These models were developed as simplified two-dimensional steady-state treatments, not because anyone thought that was a particularly good approximation, but because they were intended to address the simple and basic questions which were then current: can the merging process occur at all, at any reasonable rate? If it does occur, what does it look like? What signatures should we look for? That was the classic phase, a sort of existence proof: it showed that the process does exist and has certain well-defined attributes (antiparallel field components, plasma streaming out, and so on). We have now gone beyond this. The emphasis now is on three-dimensional and time-dependent effects and on global aspects.

Magnetic merging in the magnetosphere of the earth was rather little discussed at the Symposium - papers by Russell and by Sonnerup and a discussion of some possibly related magnetotail observations by Lui. (Workers in this area may be saving their papers and travel budgets for the Chapman Conference on Reconnection scheduled to take place two months after this Symposium.) Most of the discussion was concerned with reconnection on the dayside of the magnetosphere, where the geometry is complicated, the field line configuration is skewed and highly variable, and rather little is known as yet, compared with reconnection on the nightside of the magnetosphere. There were brief mentions of magnetic merging in the magnetospheres of Mercury and Jupiter (Russell, Aly). In the solar wind, magnetic merging within the interplanetary current sheets was discussed by Coppi as a possible mechanism for bending the current sheet up and down (such bending can explain the sector structure of the interplanetary magnetic field.)

Most of the discussion of magnetic merging at the Symposium was concerned with the case of the sun (Priest, Van Hoven, and others). A long-standing idea is that a sudden release of magnetic energy through the reconnection process is what produces the energy dissipation associated with a solar flare. Merging of magnetic fields on two different spatial scales was proposed by Sturrock as a way of accounting for two phases of a flare, a gradual phase (lasting some tens of minutes) and a superimposed short-lived impulsive phase. There is now some direct evidence, from observations of polarization, for solar magnetic field changes of the required character occurring during flares on short time scales (Kundu).

Both the rapid magnetic merging in the corona (flare-associated effects) and the gradual release of stored magnetic energy (DC coronal heating) were scaled up to apply to flare stars by Mullan, who reported fairly reasonable agreement between the scaled-up solar-based models and the observed properties of flare stars. More speculative applications of magnetic merging to other astrophysical objects included discussions of RS CVn binaries (Uchida), dwarf novae (Gilden), and accretion disks around black holes possibly related to quasars or galactic jets (Coroniti, Gilden).

Plasma experiments in the laboratory received considerable attention, with reviews by Stenzel and Bratenahl on experiments designed specifically to study the process of magnetic field line merging; in addition, Liu discussed various plasma phenomena in tokamaks and other fusion plasma devices that may be of interest in connection with, or analogous to what happens in, astrophysical plasmas (the devices themselves, of course, were not designed for studying the problems of astrophysical plasmas as such). Laboratory experiments do not have the extreme parameter range of space and astrophysical plasma systems (such as the huge magnetic mirror ratios or the enormous spatial scales) but they do allow controlled conditions and provide detailed in situ measurements with global coverage - in contrast to space observations on the one hand, where in situ measurements are possible but only locally, at isolated spots, and to solar and other astrophysical observations on the other, where only remote sensing is possible, with global but fairly coarse coverage and by indirect methods.

The results reported from laboratory investigations of magnetic merging emphasize, again, three-dimensional effects and time variations. Of particular interest is an observed time variation known as current interruption which may perhaps be similar or analogous to flares on the sun or substorms in the earth's magnetosphere. There was also much emphasis on global aspects and on the role of the external circuit. It is not entirely clear what would be the analog of the external circuit in space and astrophysical applications, where there is no external circuit as such. There does exist an analog to walls, as pointed out by Bratenahl: the photosphere of the sun or the ionosphere of a planet act in many respects, in relation to the overlying plasma, similarly to walls in a laboratory plasma experiment (thus walls may not necessarily be the unmitigated nuisance they are usually regarded as being).

Processes in tokamaks and other fusion devices that may have analogs in space and astrophysical plasmas include particle acceleration, excitation of plasma waves, and spatial diffusion of particles, possibly driven by similar mechanisms. With all the differences between the laboratory on one side and space and astrophysical plasmas on the other, there are enough similarities to suggest that a comparison of notes between people in the two areas might prove mutually illuminating.

Computer simulations concerned with magnetic merging and related problems were discussed by Birn, Tajima, Sato, and by others in connection with anomalous transport properties. Computer simulations allow, of course, very detailed control of inputs and very extensive diagnostics - you can pull out any number you want and see what is happening. On the other hand, they must deal (because of limited computer capabilities if for no other reason) with relatively simplified configurations, often though not always two-dimensional, and with limited time development - in most cases one cannot follow the system for very long periods, nor explore very many types of possible time variations. Nevertheless, the results are interesting and instructive. There are some discrepancies between the results of the various simulations, possibly (and in some cases almost certainly) attributable to the use of different boundary conditions. What is perhaps lacking to some extent is a careful discussion and understanding of the implications of different boundary conditions. For example, a "free boundary" condition may be assumed on some edge of the simulation region, but in cosmic plasmas there are no free boundaries; this boundary condition is equivalent to some assumption about the physics of the problem, and one would like to know what the assumption is. More generally, I am not sure if we really know yet what constitutes a well-posed problem in this context, what boundary conditions one is allowed to specify. The computer code will always return an answer, whether the problem is well-posed or not, but, to really know what the answer means, a proper discussion and understanding of boundary conditions is essential.

My general impression about the problem of magnetic field line merging, on the basis of both laboratory work and computer simulation, is one of continued solid progress, starting from and on the basis of the previous "classic" models (and not in opposition to them or in a completely different direction). Many ideas which were previously guessed at or derived only intuitively have not been confirmed, refined, or extended. The results, even when they support the intuition of the early pioneers, represent a significant advance beyond their achievements.

As I mentioned before, reconnection always involves essential aspects of departure from MHD as well, and these too were extensively discussed at the Symposium. One approach is to consider the non-MHD dissipative effects in a global and time-dependent framework; to keep the problem tractable, one is then forced to adopt a rather simple model for the non-MHD terms, usually a representation simply as an effective (so-called "anomalous") resistivity. The most important example of this approach was the discussion of the tearing mode instability and its development (Drake, Steinolfson). A similar approach in a somewhat different context, development and effects of large-scale instabilities in the ionosphere, was discussed by Keskinen. The alternative approach is to consider in great detail the anomalous plasma effects due to instabilities (reviewed by Huba, Dum); then one usually has to treat a simplified geometry on a local scale, an important special case being the so-called diffusion region around the magnetic neutral line associated with magnetic field line merging. The classical approach is to first identify the instability, then compute its linear growth, and then the non-linear (or, in some cases, quasi-linear) development to obtain an effective collision frequency that determines the transport coefficients. But Dum emphasized that the system may be so complicated and the final effect (on, e.g., the distribution functions) may represent such a profound modification of the initial conditions that it may be necessary to treat all the steps of the calculation essentially simultaneously rather than in sequence; there is little point in worrying much about the instability of the initial distribution if that distribution is going to be changed into something completely different.

I have the impression that in the solar and astrophysical community there exists a general view that enhanced resistivity is necessary for magnetic reconnection. Most theorists at the Symposium who tried to make solar flares looked for ways of producing an anomalous resistivity; the one exception was Van Hoven who pointed out that the classical Coulomb-collision resistivity could be greatly enhanced, by cooling the plasma through a radiative instability, to the point where it was no longer as negligibly small as usually assumed. In the terrestrial magnetospheric physics community, on the other hand, there seems to be more reliance on inertial and/or finite gyroradius effects as a way of producing departures from ideal MHD; they were mentioned, in the context of magnetic merging, by Sonnerup, and Hasegawa presented a theory of finite gyroradius effects on MHD waves.

Electrostatic double layers constitute another example of non-MHD effects. They appear in some laboratory plasma experiments; Stenzel described a particularly dramatic development of a double layer associated with current interruption. The general theory of double layers was reviewed by R.L. Smith. It is widely thought that double layers exist in the earth's magnetosphere and are responsible for the acceleration of auroral electrons, although the direct evidence for strong double layers (as distinct from general evidence for electric fields parallel to the magnetic field) is perhaps not as complete as commonly assumed. Smith pointed out that existing computer simulations and laboratory experiments on double layers refer to parameter ranges that are very different from what is found in space and astrophysical plasmas, so that any extrapolation should be viewed with great caution. Having sounded this warning, Smith proceeded to make a far-reaching extrapolation himself. In laboratory experiments one typically finds one strong double layer, but there is now one experiment where, when the length of the system was increased, two weaker

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double layers appeared instead; extrapolating to the very large astrophysical scales, Smith suggested that there one may have very many double layers, each very weak. Now there are two extreme approaches to the non-MHD phenomenon of electric fields parallel to the magnetic field: one is the double layer approach, where the parallel fields are concentrated over a very narrow region, forming essentially a discontinuity, and the other is the anomalous resistivity approach, where they are distributed over a wide spatial scale. Evidently, Smith's extrapolation might point to a way of bridging the gap between the two approaches.

Plasma turbulence, whether hydromagnetic in character or associated with non-MHD effects, leads to wave-particle interactions, which in turn may lead to particle acceleration, a topic that was discussed at the Symposium in very many contexts: acceleration of ions in tokamaks and other laboratory devices (Liu), acceleration of ions above the auroral ionosphere (Lotko, André), acceleration of ions in the geomagnetic tail (Sakai). Acceleration of electrons in solar flares was discussed in a number of papers (Holman, Krishan, Tanaka, D.F. Smith), some of which were aimed at explaining observations of very short X-ray bursts; an important related problem, the return current to the electron beam, was discussed by Spicer and by Vlahos from two different points of view that seem to be roughly analogous to the previously discussed anomalous-resistivity and double-layer approaches to non-MHD effects - broadly distributed vs. localized. Going to a larger scale, there were papers on cosmic rays in the galaxy and their interaction with Alfvén waves, to account for their isotropy or otherwise, their escape from the galaxy, and possible effects of galactic winds (Wentzel, Spangler, Nelson), and, on an even larger scale, acceleration of electrons in extragalactic jets, to account for their synchroton emission and in particular for bright knots and other localized emission regions (Henriksen). A somewhat related topic was the hydromagnetic treatment of a very energetic electron-positron plasma assumed present in the Crab nebula (Kennel); the model, where such a plasma flows out as a wind from the Crab pulsar and interacts with the swept-up interstellar medium, forming two shocks (one the outward-propagating blast wave, the other propagating inward into the plasma), is able to account for some of the luminosity and other features of the Crab nebula.

The general impression on both the topics of turbulence and acceleration and of non-MHD effects is one of enormous complexity: there are very many possibilities in the theory, very many instabilities of various kinds, but at the same time there are very many different phenomena in nature. The real problem is to match the appropriate one of the many theoretical possibilities to the appropriate one of the many phenomena observed in various systems in nature and, equally important, to recognize when one has made a proper matching, given the fact that the observations are limited and the theories are simplified. There appears to be some tendency for people working in these very complex areas to split off from the rest of the community. It is important that this should not happen, that we should continue talking to each other and not withdraw each into one's own specialist shell; researchers on instabilities and turbulence should make an effort to remain intelligible to the others, and conversely, the others should make an effort to understand what is being done on instabilities and turbulence.

Finally, there is the topic of astrophysical jets, mostly extragalactic (or perhaps one should say supergalactic). General reviews were presented by Norman and by Ferrari. The practically universal assumption is that jets are produced by outflow or ejection of matter from the central galaxy. I am an outsider in this field, hearing the evidence for this assumption essentially for the first time, and my first impression is that the evidence, although certainly as good as anything on the average in astrophysics, is not as compelling as, say, that for the binary nature of X-ray pulsars; sometimes I fancy that perhaps one has adopted a fairly implausible model because anything else one can think of is even more implausible.

Various models for jets were discussed: pure hydrodynamic flow models, where the problem is to account for the observed very narrow collimation and to avoid instabilities (Eichler, Ferrari); magnetically confined models, with questions of magnetic field structure, whether flux ropes or something else (Eilek, Hardee, Uchida); turbulent acceleration models (Henriksen); nozzle models, with solar-wind-like acceleration to supersonic flow (Tsinganos). The main problem is that, as was said by Ferrari, there are too many theories that all seem to fit the data. In this respect we are at the opposite extreme from the situation in magnetospheric and solar physics, where one sometimes has the feeling that none of the theories fit the data (and therefore, presumably, there are as yet too few theories - a theory that fits the data exists, one hopes, but has not yet been found). And so this Symposium may be viewed as an encounter between people from both ends of the spectrum of the parameter (ratio of theories to observations); the ratio is very high at one end (extragalactic jets) and very low at the other (the magnetosphere, the sun), and we may hope that the encounter will help both of us to approach our common ideal - each set of observations explained by one and only one theory.

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