CORONAL HEATING BY D.C. CURRENTS

J. HEYVAERTS
Observatoire de Paris and Université Paris VII
DAEC
92195 Meudon Principal Cédex
FRANCE

ABSTRACT. Present views on DC current coronal heating are presented. The relation to AC mechanisms, the importance of MHD turbulence in both processes, and the convergence of presently proposed ideas is outlined.

1. Introdution

In the past few years, the problem of coronal heating and evolution has been expressed as follows : a low β plasma fills the half space $z>0$. Line tying is assumed to hold at the "boundary" (ill defined). which has given, random, motions. Calculate the dissipative electric response of the corona. It is assumed that there is no slip of concentrated photospheric flux tubes with respect of the motion of the less magnetized photosphere. This motion, also, is assumed to be free of any back-reaction, via Lorentz forces, from the coronal part. Most, if not all, of these assumptions can be challenged. Nevertheless, we shall discuss this simplified model because it is (hopefully) devoid of non-essential complications.

Wave and D.C. current heating mechanisms have often been regarded as terms of an alternative. But in fact they are different regimes of a common mechanisms : the dissipative response of the corona to the excitation introduced by the driver. This response depends on the space and time properties of the driver and on their matching to the space-time characteristics of coronal loops (and open field lines). Incidentally, the former actually determine, the latter via the statistical effect of reconnection and the statistical effect of evaporation and coronal rain.

Loops have a resonance Alfvèn frequency spectrum. According to whether these frequencies and their harmonics cover a large part of the frequency spectrum of the driver or not, the wave mechanism will take more, or less, importance compared to the D.C. one. Then, long loops, which have lower fundamental frequencies, are more likely to tap the driver spectrum in the A.C. (wave) mode than short loops which mostly react to it as D.C. loads. The matching of the space properties of the driver and of the loops (driver's correlation length as compared to the size of the footprints of loops, for example) is also a significant issue.

For DC currents, and this is largely shared by A.C.'s, the main problem is to understand the process by which the scales of the electric currents are so drastically reduced in the corona as compared to the characteristic lengthes of the driver. The latter cannot be much less than 100m, since this is the photospheric dissipation scale. Some colleagues like Parker (1983) or Van Ballegooijen (1986) developed the ("extrinsic") view that this is all the result of increasing coronal complexity, driven by boundary motions, and the idea that this evolution, by itself, leads to singular states is sometimes developed (Parker 1983, Syrovatski, 1978). Others think in terms of internally developping instabilities, as a response to boundary stressing ("intrinsic" view) (Chiuderi, 1980, Heyvaerts and Priest, 1984, Chiueh and Zweibel, 1987, Vekstein, 1987). This distinction should however not be overemphasized since the effects, in terms of scale reduction, of both are not so different.

E. R. Priest and V. Krishan (eds.), Basic Plasma Processes on the Sun, 207–214.

2. About the conept of direct singularity formation

The fact that boundary motions may generate current singularities is known from studies of the evolution of X points under boundary changes in 2-D MHD (Priest and Raadu, 1975). 3-D analogs obviously exist. Such singular layers, judging from the 2D problem, should be exceptional features. However, Parker (1983), develops the more radical view that boundary motions of small scales and amplitudes drive an initially potential coronal field into a state of non equilibrium involving a great many singular layers, reflecting the complexity of the driver. He solved (Parker 1972) the equilibrium equation around an initially uniform field in infinite space, this being justified to him by the smallness of the driver scale-length as compared to loop length. He found that the only possible solution should have translational invariance. Since boundary conditions, being random, cannot spontaneously lead to such a state while keeping the flux frozen, he concluded that the symmetric state was only accessible through reconnection driven by unbalanced Lorentz forces.

Parker's views have been challenged by a number of authors, for example by Rosner and Knobloch (1982) or Van Ballegooijen (1985). Sakurai and Levine (1981) and more recently Zweibel and Li (1987) found the exact solution for arbitrary, small, perturbations with boundaries at finite distance which turned out to be perfectly regular. Then, we cannot expect direct buildup of singularities in the linear approximation. But certainly finite thickness current sheets do form, and may well become unstable or be driven to a dissipative state. So, ultimately, the end result may be quite the same as if non equilibrium and singularities indeed developed.

3. Routes to turbulence

Actually, a number of works have identified plausible primary instabilities, generally of the reconnection type (tearing modes) (Arion, 1984, Chiuderi and Van Hoven, 1979, Bodo et al., 1987). These develop non linearly, driving coalescence and secondary reconnection (Finn and Kaw, 1977, Biskamp, 1985). The effect of such large-scale forces on current sheets present in the system has sometimes been modeled by an externally imposed force (Chiueh and Zweibel, 1987, Heyvaerts and Kuperus (1978)). In this latter paper, originally concerned with flares, the 2-D time dependant response of a thin sheet to a boundary pressure pulse has been calculated analytically, and shown to lead to a growth of current density as exp. $(t/\tau)^2$. Secondary instabilities have been identified in reconnection flows, of the Kelvin-Kelmholtz type (Chiueh and Zweibel, 1987) or of the tearing type (Biskamp, 1985).

The non linear development of complex reconnection is one route to turbulence, but there are others as well, which rely on the evolution of waves in the corona (Heyvaerts and Priest 1983, Hollweg 1983). Phase mixing of shear Alfvèn waves leads to secondary instabilities of the Kelvin. Helmholtz or tearing type (Heyvaerts and Priest 1983, Browning Priest 1984) and more generally their coupling (even in absence of phase mixing) leads in the weakly non-linear regime, to cahotic behaviour and cascades towards smaller scales (Nocera et al. 1986, Pettini et al. 1985). There may be, then, less fundamental difference between DC and AC phenomena than generally appreciated, since they may merely differ by the mode of injection of the turbulent energy, cascade process and eventual dissipation being otherwise quite similar. Incidentally, this means that diagnostics of the motions associated with heating, by line profile measurements for example, may fail to unambiguously determine the wave or DC nature of the basic mechanism.

In brief, there are many potential routes from an initial equilibrium to instability and then to turbulence.

4. About the role of field line stochasticity in coronal structures

In this context, Tsynganos et al. (1984) discussed the possibility that intrinsic stochasticity of field lines appear in coronal loops. Let us remind the reader that only a few symetric MHD states have for sure "magnetic surfaces" (Edenstrasser, 1980). In such cases an analogy with an integrable hamiltonian system may be derived, an "invariance coordinate" playing the role of time, and the projection of a point following a field line onto a plane perpendicular to that direction

https://doi.org/10.1017/S0074180900087957 Published online by Cambridge University Press

behaving as a 1-D hamiltonian system in its phase space. Tearing modes, however, are likely to introduce "symmetry-breaking" current perturbations which could be compared to the addtion of a time dependent hamiltonian perturbation in the equivalent hamiltonian system. The result is that some trajectories (field lines)become stochastic (i.e. Finn, 1985). This can be understood by analogy in terms of the KAM theorem of mechanics. See e.g. Whiteman (1977). Tsynganos et al. (1984) argue that, in such an intrinsically stochastic state, no equilibrium is generally possible since if the pressure is to be constant, but different, on the various field lines, it should become a random function in the stochastic zone ! Does this conflict with Sakurai and Levine's and Zweibel and Li's work ? I believe not. On the one hand, for vanishingly weak perturbations, of order ε, the width of the stochastic zone is known to vary as $\exp(-1/\varepsilon)$ (negligible and non-analytic in ε). On the other hand, the stochastic character of the field lines becomes only explicit for loop lengths much larger than the length it needs for an undisturbed field line to make one turn around the loop (the pitch-length, say). Though this automatically is the case in toroïdal systems, which close onto themselves, boundaries at the photosphere constitute, by contrast, a strong-limitation in the solar case, where the length of the loop is expected to be much less than the pitch-length. We should certainly not expect stochasticity in finite length loops to result in any field line ergodically filling (even in projection onto a cross-section plane) all the stochastic zone ! I would even expect, but this certainly is conjoncture, that most of the unbalanced forces which would develop in the infinite system will actually find their balance near the boundary. Something could still remain of the exponential behaviour of the separation between two neighbouring field lines which is a characteristic feature of stochasticity. Not much is known about this, as by now, however.

It is interesting to mention, in relation to that, that Similon and Sudan (1989) have recently studied the propagation of shear Alfvèn waves in a stochastic field, which proves very effective.

5. A tentative theory of "extrinsic" type.

In an effort to deal with the field-braiding idea, Van Ballegooijen (1986) took an ambitious attack to the problem. Assuming flux freezing and knowledge of the statistical properties of the driver in two parallel horizontal boundary plates separated by l_B, he went on to derive the spatial power spectrum of the magnetic field between the plates as a function of time. Weak perturbations, perpendicular to an initial uniform field $B_o \mathbf{e}_z$, at all time in force free equilibrium, are assumed. These equilibria are non-linear, though. Their explicit evaluation being impossible, only statistical properties can be derived, under some assumptions. Attention can be transfered to the vertical velocity gradient, which is related to the field perturbation **b** by $D\mathbf{b}/Dt = B_o \partial \mathbf{v}/\partial z$, where D/Dt is the lagrangean derivative. It has been assumed that $\partial v/\partial z$ has the same correlation time τ_v and horizontal correlation length, l_v, as boundary velocities. Its vertical correlation length, $l(t)$, is assumed to equal $l_B / n_{(braids)}$ where n(braids) is the average number of braids introduced in the system (a time-dependant quantity). It is considered that one new braid is introduced in the system when the time has elapsed, which is necessary for the fluid elements on the boundary to suffer an average displacement equal to the correlation length l_v. A random walk theory gives the probability that points separated by D_1 at t_1 become separated by D_2 at t_2. This is enough to evaluate the field and current spectrum as a function of time. It is found that these spectra suffer a boundary-driven cascade towards smaller scales at an exponentiating rate. A situation where dissipation becomes important is soon reached after a time :

$$t_1 = t_{braid} \ln R_m \qquad \text{where } R_m = (\mu_o l_v^2 / t_{braid})$$

The Poynting flux at that time should not differ very much from the stationnary heating rate, then it is found that :

https://doi.org/10.1017/S0074180900087957 Published online by Cambridge University Press

$$F_H = \frac{Bo^2}{\mu_o} \frac{v^2 \tau_v}{l_B} \ln (R_m) \qquad (1)$$

6. Tentative theories of the "intrinsic" type

Let us now consider the case when the coronal system gets rid of the energy injected in it by some internally developed turbulence. A formulation which would allow the calculation of the heating without resorting to the details of the non-linear phase which leads to dissipation is what is seeked for here. The existence, in turbulent systems, of quasiinvariants, which, for undriven turbulence, decay at a rate much slower than other global quantities is of some interest, because we may expect that rapidly decaying quantities reach a minimum value subject to the constraint that quasiinvariants keep their initial value. Quasiinvariants are associated with quantities suffering inverse cascades towards large scales, virtually subject to no decay. In 3D-MHD magnetic helicity is a quasiinvariant, while energy decays. The idea of selective decay towards a minimum energy state seems to be supported for undriven systems, by laboratory experiments in "reverse field pinches" (see i.e. the review by Taylor, 1986).

It is interesting to hear that a theory of driven turbulent systems is in progress (Montgomery, 1989). Previously lacking such a tool, some effort has been put into devising a description of coronal turbulent heating based on what was known of undriven systems. The true evolution is idealized as a succession of elementary steps during which a complete cycle of perfect MHD evolution followed by a turbulent decay episode is assumed to take place. At the end of each step, the system is idealized as having returned to the minimum energy compatible with its acquired magnetic helicity. The heating in one step is the difference between the magnetic energy hypothetically acquired by perfect MHD evolution and that of the relaxed (linear force free) configuration. In reality stressing and relaxation act simultaneously, and the system would never be found exactly in the lowest energy state. Nevertheless, this schematic approach should be adequate when departure from this state is always small, that is, when the rate of strain is small ($v\tau_D / l_B << 1$ in eq. (2) below). This idealized evolution has been calculated for a few simple field and velocity geometries (Heyvaerts and Priest 1984, Browning et al.1986, Dixon, 1987). Assuming a certain value of the time.needed to complete the stress/relaxation cycle, τ_D, the average heating flux, and is dependance on geometry, can be calculated, sometimes analytically. The result is of the form :

$$F_H = f \frac{B^2 v}{\mu_o} \left(\frac{v\tau_D}{l_B} \right) G \qquad (2)$$

where f is a numerical factor of order 1, pertinent to the geometry considered, v the characteristic boundary velocity, l_B the loop length, and G a factor less than unity, which may exceptionnally become much less if the boundary motions happen to spontaneously generate by perfect MHD evolution an almost exactly linear force free field, leaving very little energy available for dissipation in the relaxation.

The duration of the stress-relaxation episodes cannot be determined from considerations which involve only initial and final states, but no dynamics of the developing turbulence. This indeterminacy is a major shortcoming of such theories.

In a more recent paper, Vekstein (1987) tried to take simultaneous account of stressing and relaxation, by writing for the field evolution a phenomonological equation which incorporate the fact that in the absence of relaxation the field should follow a perfect MHD evolution, $B_M(t)$ say, while in the opposite limit, it should remain very close to the lowest energy state corresponding to its present helicity, $B_R(t)$. He proposes the equation

$$\frac{\partial}{\partial t}(B - B_M) = -(B-B_R) \;/\, \tau_D \qquad (3)$$

which correctly takes these two limits. The heating is the Poynting flux, minus that part of the energy that will remain in magnetic form after relaxation. Vekstein (1987) evaluates B_M, B_R and B by a linearization procedure for each frequency component of the driver spectrum. He then obtains :

$$F_H = \frac{B^2}{2\mu_o}\frac{\tau_D}{l_B}\int d\omega \frac{1}{1+\omega^2\tau_D^2}\left(\langle v^2(\omega)\rangle - \Gamma^2(\omega)\right) \qquad (4)$$

where $\langle v^2(\omega)\rangle$ is a space average of the power spectrum. The negative contribution Γ^2 represents the non-decaying part of the energy flux and plays the role of the factor G in (2). Neglecting it for simplicity, eq. (4) can be written as

$$F_H = \frac{B^2}{2\mu_o}\frac{\tau_D}{l_B}\langle\langle v^2\rangle\rangle \qquad \text{where } \langle\langle v^2\rangle\rangle = \int d\omega \frac{\langle v^2(\omega)\rangle}{1+\omega^2\tau_D^2} \qquad (5)$$

$\langle\langle w^2\rangle\rangle$ is a weighted mean of the driver's velocity power spectrum. Mainly those motions which are coherent on a time scale τ_D contribute to the heating. Otherwise their effect is reduced by the "incoherence penalty factor" $(1+\omega^2 \tau^2 D)^{-1}$

7. Convergences and differences in D.C. theories

Only simple configurations have been calculated for heating by the constrained minimum energy hypothesis described above. However, the same idea (with the same provisos) should be applicable to randomly braided systems, since they too seek for a minimum energy state compatible with their acquired quasiinvariants. This actually is Vekstein's idea. Note that Parker and Van Ballegooijen implicitly assume no net helicity injection, but this makes no difference of principle.

It is then no surprise that Parker's heating formula (Parker, 1983), $F_H = (B^2/\mu_0)\,(v^2/u)\,(h/l_B)$, where u is the "reconnection velocity" and h the "typical flux tube thickness" is just the same as eq. (2) if we adopt Parker's favourite value of τ_D, namely $\tau_D = (h/u)$, and ignore that f and G are not exactly unity. It is also interesting to note that, as expectable, van Ballegooijen's result (eq. 1) reduces to a form very similar, though slightly different, to Vekstein's result, if his time t_1 is adopted as τ_D and the velocity correlation time t_v is taken as $(1/\omega)$ in Vekstein's terms. We actually get eq. (1) in the form :

$$F_H = \frac{B^2}{\mu_o}\frac{\tau_D}{l_B}\left[v^2\left(\frac{v}{\omega l}\right)^2\right] \quad (6)$$

which contains an incoherence penalty factor $(v/\omega l)^2$ similar to Vekstein's $(1+\omega^2 t^2)^{-1}$. In the limit of motions which remain coherent on the time scale t_D or (l/v), both (5) and (6) reduce to the form (2). However this relative convergence of the results should not hide the fact that the effective dissipation time τ_D remains a parameter, or, if not, is very ill-defined in these theories. It is in incorporating the turbulence dynamics in the calculation of dissipative response that progress should be made. Van Ballegooijen's work is a first step in that direction.

Reference

Arion, A., 1984, PHD Thesis, Maryland University

Biskamp, D., 1985, in Magnetic Reconnection and Turbulence, MA Dubois et al. ed., Editions de Physique (Orsay), p. 19.

Bodo, G., Ferrari, A. Massalia, S., Rosner, R., 1987, APJ. 313, 432

Browning, P. and Priest, E.R. : 1984, Astron. Astrophys. 131, 283

Chiuderi, C., 1980 in Solar Phenomena in stars and stellar systems NATO adv. stud. inst. series, RM Bonnet and A.K. Dupree ed., Reidel Pub. Co., p 269

Chiuderi, C. and van Hoven, G., 1979, APJ Letters, 232, L69

Chiueh, T. and Zweibel, E.G., 1987, APJ, 317, 900

Dixon, A., 1987, PHD Thesis, University of St. Andrews

Edenstrasser, J.W., 1980, J. Plasma Physics, 24 (3), 515

Finn, J.M., 1975, Nuclear Fusion, 15, 845

Finn, J.M., and Kaw, P.K., 1977, Physics of Fluids, 20, 72

Heyvaerts, J. and Kuperus, M., 1978, Astron. Astrophys. 64, 219

Heyvaerts, J. and Priest, E.R., 1983, Astron. Astrophys. 117, 220

Heyvaerts, J. and Priest, E.R., 1984, Astron. Astrophys. 137, 63

Hollweg, J. 1983 in solar Wind V, M. Neugebauer ed., NASA conf. pub, P.5

Montgomery, D., 1989, these proceedings

Nocera, L., Priest E.R. and Hollweg, J., 1986, Geophys. Astrophys. Fluid Dynamics 35, 111

Parker, E.N. 1972, APJ 174, 499

Parker, E.N., 1983, APJ 264, 642

Pettini, M., Nocera, L., Vulpiani, A., 1985, in Chaos in Astrophysics, J.R. Butler et al. ed., Reidel Pub. Co., p. 305

Priest, E.R. and Raadu, M., 1975, Solar Physics 43, 117

Rosner, R., and Knobloch, E., 1982, APJ, 262, 349.

Sakurai, T., and Levine, R.H., 1981, APJ 248, 817

Similon, P.L., and Sudan, R.N., 1989 : Preprint

Syrovatski, S., 1978 : Solar Physics 58, 89

Taylor, J.B., 1986 : Rev. Mod. Phys. 58 n°3, 741

Tsynganos, K.C., Distler, J., and Rosner, R., 1984, APJ 278, 409

Van Ballegooijen A.A., 1985, APJ 298, 421

Van Ballegooijen A.A., 1986, APJ 311, 1001

Vekstein, 1987, Astron. Astrophys. 182, 324

Whiteman, K., 1977 : Report Progr. Phys., 40, 1033

Zweibel, E.G., and Li H.S., 1987, APJ 312, 423

DISCUSSION

RUDERMAN: As far as I understand, the proof of magnetic surface existence is correct not only in the case of translational symmetry, but also in the cases of rotational and helical symmetries as well. Is that right?

HEYVAERTS: Yes. However, the development of single island structure in toroidal symmetry does not lead to a state of any such symmetry and gives rise to stochasticity indeed.

ROBERTS: In the scenario of shuffling footpoints in the photosphere the effects of gravitational stratification are ignored. This is understandable for the corona, where the scale-height is large. But it is less satisfactory for the photosphere-chromosphere. So while we have a boundary layer in the lower atmosphere as far as the coronal loop is concerned, it is far from a boundary layer as far as the footpoint motion are concerned. In the photospheric-chromospheric region, changes at a level where tubes are isolated from one another are transmitted over several photospheric scale-heights by the time they reach the relatively unstratified coronal atmosphere. Do you think this could be important for the derivation of the heating rates from footpoint motions?

HEYVAERTS: It would be unimportant if the motion of the fluid in the part of the field lines which pierce these layers is approximately independent of height. However, since these are regions where forces other than magnetic could play a role, it is *a priori* conceivable that motions be quite different over a scale height. But if this is so, then a significant $\partial B_{Horizontal}/\partial z$ will be built up, and then a large horizontal current in the photosphere/chromosphere would be expected. It is possible to show that this effect would lead to very significant Lorentz forces in the photosphere/chromosphere, and the availability of other forces strong enough to fight this tension is by no means obvious (Heyvaerts,1974). This means that even a partial bend of the field in these layers could have a feed-back effect on the flow at these altitudes, which could then not be regarded as an "independent driver". However, if we nonetheless accept that (independent or not) the relevant statistical properties of the flow at some altitude are known, the "boundary surface" theory can still be used. If one expresses what happens in the corona in terms of a really independent "driver", one maybe should then follow the field down to the convective zone. There, the problem of flux freezing could be disputed, especially in the weakly ionized part of the photosphere. The situation becomes there similar to electric currents driven by quasi-neutral atmospheric winds when they flow across field lines in the ionosphere, and the physics becomes somewhat different in this "driver".

CHOUDHURI: Can we really take the calculations of Zweibel & Li as a conclusive proof that Parker's theory is wrong? They considered small footpoint motions, whereas one would expect finite footpoint motions to entangle field lines enough to produce complicated topologies and give rise to current sheets. So perhaps it is not surprising that Zweibel & Li did not find current sheets.

HEYVAERTS: I think the work of Zweibel and Li is a valid criticism of Parker's views as they have been presented in the literature. I do not expect small finite displacements to lead to more singularity in the weakly nonlinear regime (Van Ballegooijen). This does not mean that current concentrations of finite thickness are not going to be formed, in equilibrium to begin with, then perhaps (for significant displacements) in non-equilibrium. Anyway, I believe the "singularity war" is perhaps not worth fighting, since the important idea in

Parker's as well as others' views is that the system finds some way to release by (complicated) dissipative processes part of the energy the boundary wants to pump in. Whichever way it finds to go to small scales is of secondary interest, I believe.

VAN BALLEGOOIJEN: It should be noted that Parker (1972) considered a linearization in terms of the aspect ratio of the perturbations, whereas Zweibel and Li (1987) used a linearization in terms of the displacement amplitude (ratio of displacement amplitude to velocity scale at the boundary).