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REPLY TO COMMENTS BY E. N. PARKER

D. B. MELROSE

Research Centre for Theoretical Astrophysics, School of Physics, University of Sydney, Sydney, NSW 2006, Australia

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ABSTRACT

There are three important points on which a recent paper by E. N. Parker implies disagreement with a previous paper by this author where none actually exists: (1) The way dynamics should be treated in magnetohydrodynamics (MHD), (2) the speed of energy propagation in a plasma, and (3) the conditions under which line tying is valid. Points on which there is disagreement between Parker and this author are as follows. (1) The significance of the vector magnetogram data for flare models: Parker suggests that these data cannot be used to deduce currents because they do not resolve the presumably fibril state of the magnetic field, and I disagree strongly with this view. (2) Whether or not the current viewpoint is useful in understanding the physics of solar flares: I maintain that the currents need to be considered when deciding on appropriate boundary conditions at the edges and ends of portions of MHD models for flux tubes. (3) The location of the dynamo that supplies the magnetic energy stored in the corona and released in a flare: Parker argues for a dynamo region at a depth of 5×10^3 km, and I argue that such a dynamo is inconsistent with the vector magnetogram data.

Subject headings: MHD — plasmas — Sun: corona — Sun: magnetic fields — sunspots

1. INTRODUCTION

The comments by Parker (1996, hereafter P96) on a previous work by Melrose (1995, hereafter M95) include both general points related to a long-standing magnetic view/current view controversy and specific criticisms of M95. The opposing viewpoints in the long-standing controversy are referred to by P96 as "the ***B-v*** paradigm" and "the ***E-J*** paradigm" and by M95 as the "magnetic viewpoint" and the "current viewpoint." The opposing view to that expressed by P96 is associated primarily with Alfvén and coworkers (see P96 for some references). In M95, I attempted to not adopt either viewpoint uncritically and to express criticisms of both. In replying to the comments in P96, it is important for me to distinguish between (*a*) those of P96's criticisms of the current viewpoint that I share and (*b*) actual differences of opinion between M95 and P96.

I see no disagreement between P96 and M95 on the following two central points:

1. *Dynamics should be treated using MHD.* I agree with the view expressed in P96 that the ***B-v*** paradigm and not the ***E-J*** paradigm must be used to treat the local dynamics in solar flares (and other astrophysical plasmas). I endorse the statement in P96 that "the example shows the unique and complete determination of ***J*** and ***E*** from the unique determination of ***B*** and ***v*** by formal solution of the dynamical equations with the appropriate boundary conditions." The contrary opinion, which is criticized in both M95 and P96, arises from the uncritical use of circuit equations to describe the dynamics of solar flares.

2. *The speed of energy propagation is the Alfvén speed.*—P96 criticizes correctly the myth, which has arisen from the uncritical use of circuit models, that energy can propagate "instantaneously" around a circuit. This point was made explicitly in M95: a circuit model "does not take account of the fact that changes in the magnetic field propagate at the Alfvén speed, v_A, in a plasma." A detailed model for the energy propagation (Melrose 1992, hereafter M92) shows that the circuit equation describes the energy propagation correctly only on a timescale much longer than the Alfvén propagation time.

On the foregoing two points, P96 suggests differences of opinion with M95 that do not exist. The criticisms made by P96 concerning these two points appear to be motivated by the wider, long-standing controversy, and they are not relevant to the views actually expressed in M95.

The central point of disagreement between M95 and P96 concerns the following claim: *The current viewpoint usefully complements the magnetic viewpoint.* It was claimed in M95 that a "model for a solar flare must make sense from both the magnetic and the current viewpoints." Specifically, one needs to take account of the requirements on the current in determining the "appropriate boundary conditions" in point 1 above. This claim is rejected in P96.

In this reply, I concentrate on the differences of opinion related to whether or not the current viewpoint is useful. There is disagreement on the following specific points:

1. *The implications of vector magnetogram data.*—In M95 (see also Melrose 1991, hereafter M91), it was argued that vector magnetogram data have important implications for flare models. The significance of the vector magnetogram data is recognized only partially in P96, in which it is suggested that one might still justifiably ignore the data and their implications as far as currents are concerned. I disagree with P96 on this point.

2. *Boundary conditions on solar magnetic flux tubes.*—Do coronal magnetic flux tubes carry a net current or not? In M95, this question is answered in the positive, based on the vector magnetogram data. In P96, this question is answered in the negative, based on the assumption that large-scale magnetic structures are made up of "isolated" flux bundles or fibrils. (Here "isolated" and current "neutralized" are synonymous, meaning zero net axial current.) At least for the large-scale magnetic structures relevant to flares, either the long-standing interpretation of the vector magnetogram data is wrong, or standard models for the buildup of magnetic energy prior to a flare are untenable.

3. *Line tying.*—In M95 it was argued that line tying is not valid on the relatively long timescales required for photospheric motions to build up magnetic stresses in the corona prior to a flare. The criticism of this point in P96 appears to concern the location at which the boundary con-

dition is imposed, rather than any difference in principle. Nevertheless, an important difference of opinion remains concerning the location of the source (the "dynamo region") of the (magnetic) energy ultimately released in a flare.

4. *The current produced by a twisting motion.*—In M95 (and M91), it was claimed that a subphotospheric twisting motion (at either footpoint) leads to no net coronal current, and that this is inconsistent with the vector magnetogram data. In P96, a supposed counterexample to this argument is presented. As discussed in § 5 below, P96's example may be used to demonstrate the point made in M95, subject to the proviso that the twisting motion is confined to a finite region.

Below I discuss each of the above points of disagreement in detail.

2. THE IMPLICATIONS OF VECTOR MAGNETOGRAM DATA

There is a sharp difference of opinion between M95 and P96 concerning the implications of the vector magnetogram data on the currents related to flares. For over three decades, observers have inferred consistently that the vector magnetogram data imply very strong currents that correlate with flare kernels and that are unneutralized. That is, the current is unneutralized in the sense that it flows up at one footpoint and down at the other. As pointed out in M91 and M95, an unneutralized current is inconsistent with magnetic models that involve a buildup of current prior to a flare due to twisting or shearing motions imposed in the subphotospheric regions. Thus, if the interpretation of the vector magnetogram data is correct, then models for solar flares that assume storage of magnetic energy in the corona due to twisting or shearing motions are untenable.

Since M95 was written, three further studies of vector magnetogram data have supported the conclusion that the large currents associated with flares are unneutralized. Leka (1995) addressed the question whether emerging flux regions carry electric current. In particular, using a procedure that applies "even if the return current were filamented and buried in the noise," Leka found no evidence for return currents. Gary & Démoulin (1995) found that "large vertical current distributions that are observed are true patterns and imply that they have a subsurface origin since there are no large 'return' currents[s] surrounding them and the current seems to span the neutral line." Wang et al. (1996), reporting on a particular active region, remarked that "vertical currrents in this region generally emerge from areas of negative line-of-sight magnetic field and return to areas of positive field." These results are entirely consistent with the unneutralized currents found in earlier investigations, as cited in M91 and M95.

The attitude adopted in M95 is to accept the implication of the vector magnetogram data that currents are unneutralized and to explore flare models that are compatible with an unneutralized current.

The attitude adopted in P96 is to seek other interpretations of the vector magnetogram data that are compatible with theoretical models. The specific argument given in P96 is predicated on an assumption that the large-scale magnetic structures relevant to flares are made up of fibrils, each of which is current-neutralized. The argument is that integrating around a contour that intersects many unresolved fibrils "provides a fictitious mean current that has nothing to do with the actual, vanishing mean current." I do not accept the premise on which this argument is based; specifically, I do not accept that large-scale magnetic structures are made up of fibrils each of which carries zero net current. Nevertheless, for the sake of argument let me accept this premise and assume that each fibril has an axial current I flowing in its interior and a neutralizing longitudinal current $-I$ flowing on its surface. Then the "fictitious mean current" that one might expect on intersecting a large number, N, of fibrils is $\sim \pm I/N^{1/2}$. This is unacceptable as an interpretation of the vector magnetogram data for two reasons. First, the magnitude of this fictitious current is too small. For example, for fibrils of radius r with an axial field B_z, the maximum current is $\sim 2\pi r B_z/\mu_0$, and if these fibrils make up a large-scale magnetic structure of radius $R \sim Nr$, then the fictitious current is $\lesssim 2\pi R B_z/\mu_0 N^{3/2}$. However, the currents inferred from the vector magnetogram data are a sizable fraction of the maximum current $\sim 2\pi R B_z/\mu_0$. Hence, any model with $N \gg 1$ is unacceptable. (A model with $N = 1$, that is, a single neutralized current, is also unacceptable; see M91.) Second, the sign of this fictitious current is random, and this is inconsistent with the observed current having opposite signs at opposite footpoints.

In brief, the two approaches are to accept the stated implications of the vector magnetogram data (M95) or to hope that some alternative interpretation for these data might be found (P96). In my opinion, after over three decades in which the resolution of the data has improved greatly and yet the data imply almost uniformly the same pattern, the hope that these important implications can be avoided now seems forlorn.

3. BOUNDARY CONDITIONS ON SOLAR MAGNETIC FLUX TUBES

Another major difference of opinion between M95 and P96 concerns whether or not one needs to take the current into account in deciding on appropriate boundary conditions.

Consider a model for a solar flare in which there are several coupled regions: the dynamo region somewhere below the photosphere, a subphotospheric flux tube from the dynamo region to the photosphere, a coronal portion of the flux tube, and, during the flare, the energy-release region in the coronal flux tube. Each of these regions is modeled individually using MHD, and we are concerned with the boundary conditions on their surfaces. For simplicity, the subphotospheric and coronal portions of the flux tube are modeled as cylindrically symmetric (cylindrical coordinates ϖ, ϕ, z). The boundary conditions on these flux tubes are those at the edge, $\varpi = r$ say, and the ends (at $z =$ constant) at which they are joined together.

The conventional MHD view is that the compatibility of the magnetic and current viewpoints is automatic: $\operatorname{div} \boldsymbol{J} = 0$ ensures automatically that all current lines are closed and hence that it is unnecessary to consider the current explicitly. However, the argument applies only to current closure within the boundary of the region being modeled by MHD. Current flow through the region being modeled implies that some current lines enter and leave the region by crossing its surface. Boundary conditions are needed to specify the current flow on or across all surfaces of the region being modeled. My general point above is that appropriate boundary conditions must be consistent with the requirements on the current flowing through (or on the surface of) the region. Two boundary conditions that need to be examined critically in this context are (*a*) the boundary condition at the edge corresponding to the requirement that magnetic flux tubes be "isolated," which is discussed in this section, and (*b*) the boundary condition at the end corresponding to line tying, which is discussed in the next section.

The concept of isolated magnetic flux bundles with $B_\phi = 0$ and $B_z = 0$ at $\varpi > r$ is suggested by observations of the photospheric magnetic field, which tends to emerge in such apparently isolated bundles. Are flux bundles actually isolated? The following discussion of this question is aimed at making the point that consideration of the current is important in discussing the question, rather than in making any specific point about the outcome of the discussion.

The boundary condition required to give $B_\phi = 0$ and $B_z = 0$ at $\varpi > r$ implies a surface current at $\varpi = r$. This surface current is not force free (e.g., Gold & Hoyle 1960; Melrose, Nicholls, & Broderick 1994). Thus, this boundary condition is acceptable for the subphotospheric portion of the flux tube but not for the coronal portion of the flux tube, which must be (approximately) force free. Nevertheless, there is a long tradition of ignoring this inconsistency and considering isolated flux tubes in the corona. It is consistent with the force-free condition to impose the boundary conditions $B_\phi = 0$, $B_z \neq 0$ at $\varpi > r$ ("neutralized" case in M95) or $B_\phi \propto 1/\varpi$, $B_z = 0$ at $\varpi > r$ ("unneutralized" case in M95). The fact that it is not consistent with the force-free condition to have an isolated flux tube $B_\phi = 0$ and $B_z = 0$ at $\varpi > r$ is obvious when one considers the current explicitly.

Whether or not coronal flux tubes carry a net current has an important implication for models of solar flares. An unneutralized current implies magnetic coupling to neighboring magnetic structures. Such coupling was included in the flare model of Gold & Hoyle (1960), but it has since been ignored in most models for flares. Although the detailed model of Gold & Hoyle is not consistent with observational data, the suggestion remains valid that magnetic coupling to neighboring flux tubes needs to be considered. This is especially the case for any description of the temporal and spatial evolution of substructures in flares. As suggested in M95, a simple way of including such coupling is through a time-dependent inductance or a mutual inductance in a circuit model.

In summary, the neglect of the current when considering the boundary condition on coronal flux tubes has led to (*a*) wide acceptance of the concept of an isolated flux bundle, despite this being inconsistent with the force-free condition, and (*b*) a neglect of the role that magnetic coupling between neighboring (current-carrying) flux tubes is likely to play in the evolution of flares.

4. LINE TYING

The discussions of line tying in M95 and P96 contain several different points that need to be considered separately. These include (i) the line-tying boundary condition, (ii) the condition for validity of this condition, (iii) the photospheric boundary condition, and (iv) the location of the dynamo region. There appears to be no significant difference of opinion on points (i)–(iii), but there is a difference of opinion on point (iv).

The condition stated in P96 for the line-tying condition to be valid is that the flow speed v exceed v_A. There is no disagreement on this point. The objection in M95 to line tying at the photosphere is that v does not exceed v_A there, and hence the postulated kink in the magnetic field and associated cross-field current propagates away as an Alfvén wave. P96 objected to this use of "wave." What was meant in M95 is that the kink propagates away as described by solving the dynamical equation that has Alfvén waves as one solution. Detailed calculation demonstrating this point was presented by Wheatland & Melrose (1994). The important physical point is that the dynamical equations preclude the assumed steady buildup (implied by the line-tying boundary condition) of twist or shear of the coronal magnetic flux tube.

There is no actual disagreement between M95 and P96 concerning this point. Both agree that the line-tying condition is not satisfied at the photosphere. In P96, it is argued that the line-tying condition is satisfied at a depth of 5×10^3 km below the photosphere. The implication in P96 is that this is the location of what is referred to here as the dynamo region. That is, according to P96, the energy stored in the corona and released in a solar flare originates from this region at a depth of 5×10^3 km, where line tying couples the magnetic field to the convective motions.

The suggestion in M95 that the dynamo region must be at a much greater depth is based on an argument that is not considered by P96. This is that the postulated line tying at 5×10^3 km (or anywhere else between the photosphere and the base of the convection zone) would lead to a neutralized current, and not an unneutralized current as the vector magnetogram data imply. This point is discussed in detail in the next section.

5. THE CURRENT DUE TO A TWISTING MOTION

A point that is central to this discussion is that the current generated by a twisting motion in the dynamo region is neutralized. The literature (on MHD models for coronal flux tubes) on this point is somewhat unclear. Some detailed models, e.g., Zaidman & Tajima (1989), lead to unneutralized current, whereas others, e.g., Jones & Galloway (1993) and Sakai & Fushiki (1995), lead to neutralized currents. The important differences between these MHD models involve the boundary conditions. Zaidman & Tajima (1989) assumed periodic boundary conditions, and the other authors chose boundary conditions that correspond to zero current (equivalent to line tying) at a lower boundary.

A qualitative argument as to why the current generated by a twisting motion must be neutralized is as follows. Consider a cylindrical flux tube of radius R that initially carries no current. In a localized (dynamo) region, $z_0 < z < z_0 + \Delta z$, an azimuthal flow is assumed to be turned on at $t = 0$. This flow tends to wind up the magnetic field (increasing B_ϕ) in the region $\varpi < R$, with $v_\phi = 0$ at $\varpi \geq R$. This twisting implies an associated current pattern. The current lines are necessarily closed, and after time t the current lines can have propagated along the z-axis to a distance no greater than $v_A t$. Consider any surface $z =$ constant outside the dynamo region; that is, at $z > z_0 + \Delta z$ or $z < z_0$. Because the current lines are closed, those current lines that cross such a surface do so in oppositely directed pairs. The equal and opposite contributions from all such pairs implies that the net axial current is zero. That is, any current pattern set up in this way is necessarily neutralized.

Another version of the foregoing argument involves consideration of the force (torque) that drives the twisting motion and the reaction of the plasma to it. In opposition to an imposed twisting motion, a $\boldsymbol{J} \times \boldsymbol{B}$ force is set up in the dynamo region. Specifically, an azimuthal force arises due to a radial $\boldsymbol{J}$ and the axial component B_z. The integral of div $\boldsymbol{J} = 0$ across the height of the dynamo region implies a nonzero change ΔJ_z in J_z across the region, with $\Delta J_z(\varpi) \propto \varpi^{-1} d(\varpi v_\phi)/d\varpi$. The requirements $v_\phi = 0$ at $\varpi = 0$ and at $\varpi \geq R$ imply that ΔJ_z change sign, say at $\varpi = \varpi_0$, where $d(\varpi v_\phi)/d\varpi$ is zero. Suppose $\Delta J_z(\varpi)$ is positive (up) at $\varpi < \varpi_0$ and negative (down) at $\varpi > \varpi_0$. Let us refer to these two regions as the "core" and the "outer layer" of the flux bundle. If the boundary condition $J_z = 0$ (line tying) is imposed at the lower boundary $z = z_0$, then this dynamo drives an axial current at $z > z_0 + \Delta z$ that is up in the core, down in the outer layer, and such that the total current, I, is zero. That is, the currents in the core and in the outer layers are equal and opposite (as is demonstrated below using P96's example). If one imposes some other boundary condition at $z = z_0$, so that the current may flow both above and below the dynamo region, the same conclusion applies: both at $z > z_0 + \Delta z$ and at $z < z_0$ the currents in the core and in the outer layers are equal and opposite.

The foregoing conclusion is consistent with the results of Jones & Galloway (1993) and Sakai & Fushiki (1995), who imposed line tying at the lower boundary. However, Zaidman & Tajima (1989) imposed periodic boundary conditions, and they found that an unneutralized current results. The periodic boundary condition is unrealistic because it requires current lines to close at infinity ($z \to \pm\infty$), and the current lines can reach infinity only after an infinite time.

The conclusion that the current generated by a twisting motion must be neutralized is challenged by P96, who presents a supposed counterexample. P96 postulates an azimuthal velocity $v_\phi(\varpi)kz$, with k a constant. (Thus, it is postulated tht v_ϕ is nonzero for all z, with v_ϕ diverging for arbitrarily large $|z|$.) This velocity field is shown to build up a net current that increases linearly with time. The current density after time t found in P96 is

$$J_z\varpi = \frac{Bkt}{\mu_0} \frac{1}{\varpi} \frac{d}{d\varpi} [\varpi v_\phi(\varpi)] ,$$

which has the same dependence on ϖ as discussed above. In P96, it is found further that the current inside a radius ϖ is

$$I(\varpi) = \frac{2\pi Bkt}{\mu_0} \varpi v_\phi(\varpi) .$$

Provided that the flow velocity is confined to a finite region, that is, provided that one has $v_\phi(\varpi) = 0$ at $\varpi \geq R$, one has $I(\varpi) = 0$ at $\varpi \geq R$. Thus, the current in the model is neutralized, as expected from the foregoing argument. In P96, an unneutralized current is obtained by allowing $v_\phi(\varpi)$ to be nonzero for all ϖ, and this requires an implicit return current at $\varpi = \infty$.

The fact that a twisting or shearing motion (confined to a finite region) generates only neutralized currents (outside this region) implies that all models that rely on twisting or shearing motions to store energy in the corona are inconsistent with the vector magnetogram data.

6. CONCLUSION

Several of the criticisms by P96 of M95 are ill directed in that they apply to views that may be expressed in some of the literature on circuit models for flares, but not to views actually expressed in M95. In particular, there is no disagreement concerning the use of "the ***B-v*** paradigm" rather than "the ***E-J*** paradigm" in treating the dynamics, and there is no disagreement concerning the speed of propagation of disturbances being restricted to v_A. It might be remarked that the fact that circuit models erroneously allow propagation at a speed greater than v_A is due to the second time derivative in the dynamical equations being thrown away in making the circuit approximation; as shown in M92, the circuit approximation can describe the temporal evolution satisfactorily only on a timescale longer than the Alfvén propagation time.

In M95, it was claimed that the current viewpoint is needed to select appropriate boundary conditions. This claim is rejected in P96: "The additional insights proposed by Melrose arise from his declarations, which are generally contrary to the dynamical equations for fluid and field." I maintain that consideration of the current does provide important insights into the choice of appropriate boundary conditions, and that these boundary conditions are consistent with the MHD equations.

There are specific unresolved differences of opinion between M95 and P96 on the following points:

1. The significance of the vector magnetogram data: I disagree strongly with the suggestion in P96 that these data cannot be used to deduce currents because of the presumably fibril state of the magnetic field. In my opinion, the continued neglect of these data has impeded progress in understanding the physics of flares (§ 2 above). The primary motivation for the discussion in M95 is to develop a flare model that is consistent with these data.
2. Whether or not the current viewpoint (including the judicious use of circuit analogs) is useful in understanding the physics of solar flares. As discussed in detail above, I maintain that the current viewpoint provides specific insights into (*a*) the implications of the observed current being unneutralized, (*b*) the boundary condition at the edge of a flux tube, and (*c*) the line-tying boundary condition at the ends of a portion of a flux tube. Furthermore, the unjustifiable neglect of magnetic coupling between neighboring current-carrying flux tubes, at least since the model of Gold & Hoyle (1960), is a direct consequence of the refusal to think in terms of currents.
3. The location of the dynamo region that supplies the magnetic energy stored in the corona prior to a flare: I disagree with P96's suggested location of this dynamo region on the grounds that P96's dynamo would generate a neutralized current.

I thank E. N. Parker for raising these important points and providing the opportunity to present our respective views in an appropriate forum.

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