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CMEs: How do the puzzle pieces fit together?

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Abstract

This review consists of questions to participants in the S-RAMP Symposium (S3) on CMEs and Coronal Holes, as well as to a few others, and their responses in a “town meeting” format (originally conducted on Hugh Hudson’s website). Here we deal only with CMEs. The questions we ask aim at probing the weaknesses of existing models and highlighting controversies, thereby providing guidance toward a more complete view of solar eruptions. Topics covered include: the “solar flare myth”, flux ropes, new phenomena (EIT waves, dimmings, global brightenings), helicity and sigmoids, and transequatorial loops (as sources of CMEs). Although this is a review, we’re more concerned here with what is not known than what is already agreed upon. We asked people to speculate freely in advance of the observational, analytical, and theoretical work that will provide definitive answers—this is not the standard Scientific Method at work!

Keywords: Coronal Mass Ejections; Solar activity

1. Introduction

With *Yohkoh*, SOHO (Solar and Heliospheric Observatory), and TRACE (Transition Region and Coronal Explorer), observations of new aspects of coronal mass ejections (CMEs), an example of which is shown in Fig. 1, seem to be accumulating faster than they can be incorporated into a coherent picture. The hot newer items include: EIT (EUV imaging telescope) waves, dimmings, sigmoids, flare ejecta seen in X-rays, transequatorial loops (as sources of CMEs), and global brightenings. How indeed do the pieces fit together? As a guide and impetus to future studies, we have asked participants of S-RAMP Symposium S3 on “CMEs and Coronal Holes”, and a few others, to share their working hypotheses on CMEs. We stressed to participants that this would be a no-holds-barred exercise, conducted under *sumo* rules of civility, and we encouraged them to speculate freely in response to our questions so that all might benefit from their intuitions. As is often seen in science, an inspired guess, even when wrong, can lead to progress by triggering a response in another’s mind.

In the following sections, we conduct a Q&A session on a variety of topics, beginning with the “solar flare myth” (Gosling, 1993). More than any paper in recent years, Gosling’s review both galvanized and polarized the community. We then consider the question of how the Sun makes flux ropes—do they spring fully formed from beneath the photosphere or are they formed via magnetic reconnection? Next, we consider waves (including type II bursts), dimmings, and global brightenings. The questions here are no less fundamental than those regarding flux ropes. Then on to sigmoids, helicity and transequatorial loops.

2. Questions and answers

In the following subsections, the questions will be preceded by the initials of the question-poser (either *C* for Cliver, *H* for Hudson, or *C&H*). The full name of the interviewee will be given preceding the response.

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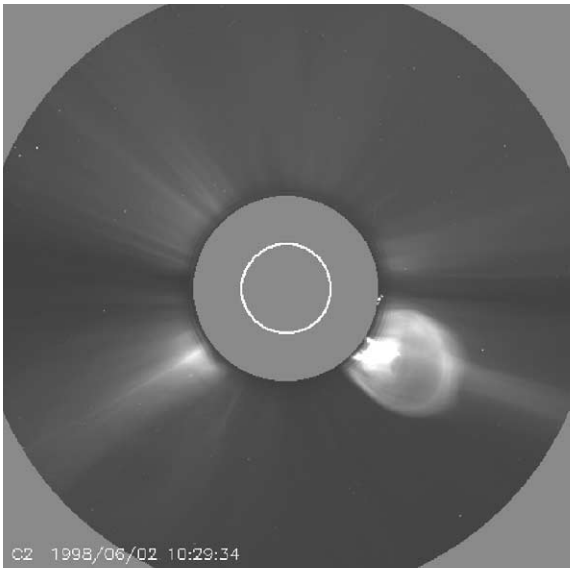

Fig. 1. A CME observed in the field of the LASCO C2 coronagraph at 1030 UT on 2 June 1998 (Plunkett et al., 2000). The classic three-part structure of the CME (front, cavity, and prominence) is apparent. The white circle marks the limb of the Sun behind the occulting disk.

For context, we encourage interested readers to refer to review papers that concentrate on what is known about CMEs, rather than what is not. Several such excellent reviews are available including Crooker et al. (1997), Hundhausen (1999), Forbes (2000b), and Webb (2000). We've also just written a review of non-coronagraphic observations of CMEs (Hudson and Cliver, 2001).

The solar flare myth: *Gosling's famous paper meant different things to different people*.

Background: Jack Gosling's 1993 paper challenged the utility of research on solar flares for space weather forecasting. Summarizing the findings of two decades of research (see Kahler, 1992), Jack argued that CMEs, not flares, were the critical element for large geomagnetic storms, interplanetary shocks, and major solar energetic particle (SEP) events.

C: Hugh (Hudson), I think that you are the last person in the community who thinks that flare brightening plays an important role in driving CMEs or at least you are the last person who is willing to be vocal on this point. When you make such a fuss about the brightening and dimming in flares being simultaneous, it calls to mind Lin and Hudson (1976) where the flare accelerated particles in big flares are thought to provide the energy for everything that follows: mass ejection, shock, and large solar energetic particle events. Do you still believe that the faint brightenings that accompany some CMEs (e.g., Webb et al., 1998) are anything but a response of the solar atmosphere to the CME?

Hugh Hudson: I find little good evidence that the radiative and mechanical aspects of flares and/or CMEs can be separated physically into cause and effect. In this sense we are far from needing a new paradigm for what happens in the lower corona during a flare or the launching of a CME. We need to focus more on the impulsive phase of flares and/or the acceleration phase of CMEs, which coincide rather nicely as a rule. There is even better evidence now for huge fractions of flare energy in non-thermal particles (Ramaty et al., 1995), so the general conclusion that CME theory needs to handle particles self-consistently was right enough. The idea that flares are somehow irrelevant to the whole process should not be encouraged.

But yes, I concede, geoeffective CMEs truly can happen without anything a reasonable person would call a flare, nor even an arcade event (streamer blowout, or flabby flare, as I like to think of them). I had reserved judgment on this until we'd seen the *Yohkoh* imaging, since this is a lot more sensitive than GOES. But it is true—even really heroic geoeffectiveness seems to occur (albeit rarely) without much of a fuss in the low corona. I offer my thanks to the community for allowing me some slack here, but it was purely in the interest of skepticism and objectivity! And there is no question that the fast CMEs get their acceleration in close time association with the flare effects.

C&H: Dave (Webb), in your review papers (e.g., Webb, 1995) on CMEs you often cite Gosling (1993) as evidence that CMEs are the "causal link" between solar activity and terrestrial disturbances. Isn't this the intellectual equivalent of saying "guns don't kill people, bullets do?" No one ever said that flares traveled to 1 A.U. Please expand on your interpretation of the solar flare myth and tell us why it has any real scientific content.

Dave Webb: Admittedly, I put the "causal link" phrase in the 1995 IUGG review because the editor wanted the papers to read like exciting newspaper articles. To me the main thrust of Gosling's argument is that CMEs, not flares per se, are the fundamental class of solar activity that leaves the Sun, propagates through the heliosphere, and occasionally envelops the Earth, directly causing or driving major nonrecurrent geomagnetic (GM) storms, and transient interplanetary shock waves and their attendant SEP events. [*H*: Guns don't kill people, bullets do.] The Solar Flare Myth is the old paradigm that flares by themselves somehow caused these *interplanetary* effects. Re the statement that no one ever thought flares traveled to 1 A.U., some very respected researchers such as Hale (1931), Chapman (1950), and Pudovkin et al. (1977) have considered streams of matter emanating directly from large flares to be the prime cause of geomagnetic storms! [*C*: That's not the same as saying the flare travels to 1 A.U.; the key question is what drives the ejection.] To most of the community it is now apparent that some storms can be traced to solar filament eruptions having NO significant flares (e.g., Joselyn and McIntosh, 1981) or (gasp!) to no obvious surface manifestation at all! The importance of the January 1997 event (e.g., Webb et al., 1998) is that it showed that a geoeffective (halo) CME could occur in the absence of "anything a reasonable person would call a flare", as Hugh eloquently puts it. To me the issue of cause and effect between flares and CMEs that has driven most of the Flare Myth controversy was a *secondary* issue to Gosling. Flares and CMEs may or may not be related to each other, although one is not a necessary and sufficient condition for the other, but CMEs, not flares, cause major geomagnetic storms and transient shocks.

C&H: Nariaki (Nitta). Your S-RAMP talk left us confused if interested. You started by saying that flare ejecta followed CMEs but then indicated that TRACE data showed the flare impulsive phase may play a role in CME acceleration. Correct us if we're wrong.

Nariaki Nitta: The confusion may have come from the variety of appearances of the flare-associated ejecta, which may not be explained in a uniform fashion, as Shibata et al. (1995) maintain (see also Shibata, 1998). As far as SXT (*Yohkoh* Soft X-ray Telescope) data are concerned, we sometimes see expanding loops over an active region starting several minutes before a flare, and I think these are an inner part of the opening or stretching process higher in the corona (not directly observed), which may be regarded as the CME onset. A good example may be the 13 November 1994 event (without coronagraph coverage) reported by Hudson et al. (1996). But in Nitta and Akiyama (1999), we dealt with ejecta seen in the impulsive phase, i.e., after the flare onset. I still think these SXT ejecta represent disturbances in the wake of or below the CME, after it takes off. It is possible, however, that the ejecta give an extra kick to the CMEs that are observed to be fast ones. In my talk, I may have emphasized the possible role of the impulsive phase, by showing a TRACE movie. But ejecta seen in EUV wavelengths (at temperature of 1–2 MK) are still under-explored, and their relation with CMEs is not known. We may just see the same thing as an Hα filament, which gets activated before the flare and erupts in the impulsive phase. Recently, Zhang et al. (2000) analyzed LASCO (large angle spectroscopic coronagraph) C1 data and concluded that the CME onset is during the impulsive phase, but I don't know how they managed this, given LASCO's poor temporal resolution. Lastly, I still buy the flare (blast wave) origin of CMEs (see Lin and Hudson, 1976) for X-class-flare-associated CMEs (e.g., 6 November 1997).

Ed Cliver: Nariaki, you seem to be saying that there are two classes of CMEs or at least two contributing processes. First, a CME may leave more or less of its own volition (e.g., as a result of helicity charging (Rust and Kumar, 1996; Pevtsov et al., 1996) or mass unloading (Low, 1999, 2001)) prior to a flare and then processes during the impulsive flare related to ejecta and/or a shock can result in further acceleration. If no flare or only a weak flare occurs, then we would have the slowly-accelerating eruptive filament events of MacQueen and Fisher (1983) and Sheeley et al. (1999) but if a flare occurs, then an additional acceleration process might act on the CME. The key observation is the acceleration profile of the CME (or filament) during the flare. Kahler et al. (1988) looked at this for four filament eruptions and found "no new acceleration attributable to the impulsive phase" but as my co-author correctly points out the evidence is not conclusive. The authors themselves note that two of the events appear to have a sharp added acceleration at the onset of the impulsive phase and suggested that a new mode of energy release, corresponding to the impulsive phase, might be initiated when the speed of the prominence exceeds ~ 100 km/s. On the other hand, during the Skylab workshops, Rust et al. (1980) concluded there was no fundamental distinction between the accelerating mechanisms of eruptive prominences and flare sprays. So it's an important open question. Incidentally, the blast wave picture of mass ejection propulsion can be traced back at least as far as Parker (1961).

C: Sara (Martin), I was always impressed by the prescience of your paper with Harry Ramsey in the early 1960s that pointed out activations of filaments in advance of flares and provided early evidence for the flare myth view of the relationship between flares and CMEs. I would be curious to know if you view this work (Smith and Ramsey, 1964) in the same way. What is your reaction to the controversy following Jack's paper, which continues at some level to this day?

Sara Martin: Since very early in the field of solar astronomy, there has been general recognition that eruptive solar events often have many components. The flare (in the most general terms, a brightening at the Sun) is one component, erupting filaments are often another component and some kind of disturbance, including particles, that can propagate to Earth was at least a third component. These have all been recognized for over half a century, long before the name "coronal mass ejection", "proton event" and other useful terms came on the scene as part of the accepted nomenclature in learning about the propagating parts of eruptive solar events.

To me, the idea that there is a "flare myth" is itself a myth that arose because the term "flare" was often informally used in place of "eruptive solar event". Colloquially, people (and news reporters) tend to say "such and such a big flare caused a geomagnetic major geomagnetic storm at such and such time". But workers in the field have long known that it was the "overall magnetic event resulting in the flare brightening, erupting filament, particle ejections, and other unknown effects" which was somehow related to the geomagnetic storm. So I think the flare myth controversy is a product of simplified and sloppy use of the language. Do not most myths arise either out of repeated misunderstandings or the telling of tall tales?

C: Jack (Gosling). While the question of what propels matter from the Sun to 1 A.U. remains open, there is no question about who propelled CMEs to the forefront in solar-terrestrial physics (or space weather as it is called today). Hats off! Recently, Sheeley et al. (1999) (see MacQueen and Fisher, 1983) have shown that CMEs can have two basically different types of velocity profiles, with some events starting slow and accelerating (eruptive prominence type) and others starting fast and decelerating (active region/flare type). You told me once (at Solar Wind 9) that this indicated that the flare-associated events were getting an extra "kick". Please elaborate. Do you think the flare plays a role in the events that start fast? Or is the physics the same in both types of events (eruptive prominence and spray) with just a different time scale (Rust et al., 1980; see Cliver, 1999)? Or is it too early to tell?

Jack Gosling: The acceleration profiles of the slow CMEs in the Sheeley (1999) Solar Wind 9 paper look very similar to the acceleration profiles of the ordinary slow solar wind provided by the "blob" measurements on SOHO shown by Sheeley et al. (1997). This suggests to me that the outward acceleration of the slow CMEs (those with speeds less than about 200 km s^{-1} at 2 R$_O$) may be caused by essentially the same forces (pressure gradients) that accelerate the slow solar wind away from the Sun. That is, the slow CMEs do not seem to require any special acceleration once they begin to rise outward. To me, the real question with the slow CMEs is what causes the initial release rather than what causes their outward acceleration. On the other hand, the CMEs with speeds greater than about 300 km s^{-1} at 2 R$_O$ seem to be born fast and receive little further outward acceleration beyond 2 R$_O$. Most of these fast CMEs do require an extra acceleration over and above that provided by the normal solar wind expansion. I do not know what provides the extra acceleration in such events, but strongly suspect it is not flare heating. Rather it seems likely that a rapid reconfiguration of the coronal magnetic field produces both the rapid CME acceleration and any related flaring. I am of the opinion that the physics of the acceleration is probably different in the fast and slow events, but that remains to be proven. It may be just a matter of time scales that determines how effective the field is in accelerating a given CME.

Origins of flux ropes: *We see flux ropes at 1 A.U. but the physics of their formation at the Sun remains controversial.*

Background: Magnetic flux ropes (helical structures in which the tension of the curved field lines balances the plasma and magnetic pressure) were identified in the solar wind two decades ago (Burlaga et al., 1981). Recent solar and interplanetary observations have emphasized the importance of flux ropes for CMEs. St. Cyr et al. (2000) reported that 30–50% of LASCO CMEs have "disconnection" features (concave-outward structure, Illing and Hundhausen, 1983; McComas et al., 1991; Webb and Cliver, 1995). From an analysis of interplanetary disturbances associated with halo CMEs, Webb et al. (2000a) suggested that flux ropes (magnetic cloud-like structures) are a general characteristic of CMEs. How does the Sun make flux ropes? In the conventional CSHKP picture of solar eruptions (named for Carmichael, Sturrock, Hirayama, Kopp and Pneuman; see Švestka and Cliver, 1992; Hudson and Cliver, 2001), flux ropes can be formed via magnetic reconnection when field lines pinch off below a rising CME, as shown in Fig. 2 taken from Anzer and Pneuman (1982). (See Forbes, 2000b for a discussion of recent CSHKP-type models and variants, e.g., Antiochos, 1998.) Alternatively, equilibrium flux ropes could form on the surface prior to eruption as a result of surface flows and shears and reconnection (e.g., van Ballegooijen and Martens, 1989; cf., Amari et al., 2000) or they might emerge intact from below the photosphere (Chen, 1989, 2001). For these latter scenarios, post-eruption reconnection of an overlying canopy of fields that initially constrains the flux rope might further modify it to create the entity eventually seen at 1 A.U. In this way, the ample evidence for reconnection (arcade formation) on which CSHKP-type modes are based could be reconciled with the view that a flux rope exists prior to eruption. The Rust–Martin debate, which turns on interpretation of detailed observations of solar filaments, lies at the core of the differing views of flux rope formation. Rust's picture requires a pre-existing flux rope while in Martin's model the filament is tied down along its length and the ejected flux rope is formed via reconnection following eruption, as in the CSHKP picture. Simplified schematics giving cross-section views of the pre-eruption state of the Martin and Rust pictures are given in Figs. 3(a) and (b), respectively.

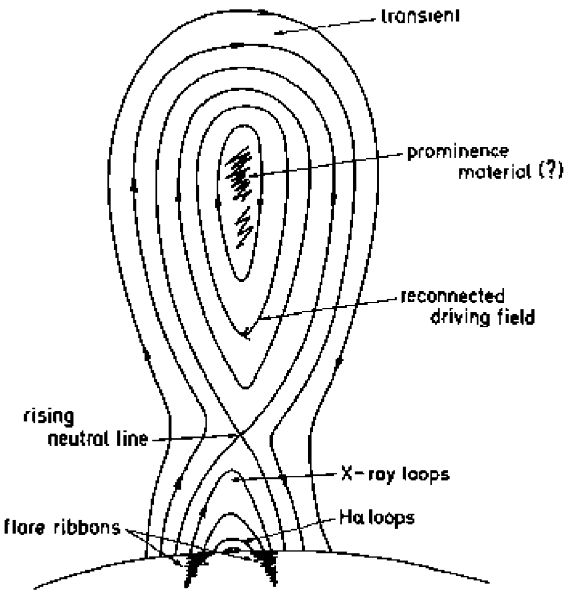

Fig. 2. Cross-section view of a flux rope CME and arcade formed via reconnection in the CSHKP topological model of solar eruptions (taken from Anzer and Pneuman, 1982).

C&H: Sara (Martin), what are the key points in the Rust–Martin controversy (see Crooker et al., 1997)?

Sara Martin: The friendly Rust–Martin debate stems from two opposing models for the magnetic fields of filaments. Rust's view, expressed in Rust and Kumar (1994), assumes a magnetic field configuration for a prominence in which the prominence mass rests in the concave-up part of a helical coronal magnetic field (Fig. 3b). This configuration is in common with a number of preceding original

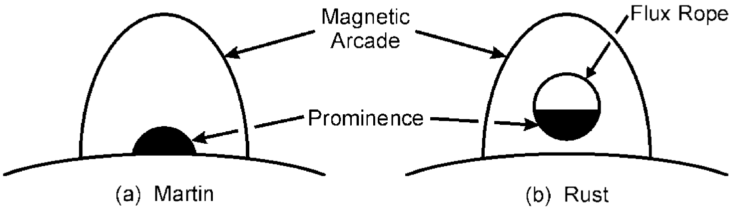

Fig. 3. Over-simplified schematic illustrating the difference between the Martin (a) and Rust (b) interpretations of solar filament observations. The cartoons give cross-sectional views of the filament and overlying arcade.

models: Pneuman (1983), van Ballegooijen and Martens (1989), Low and Hundhausen (1995).

The Martin view—described by Martin and Echols (1994) and summarized in Martin (1998b)—presents a new and non-traditional magnetic field configuration for filaments deduced from observations; in this empirical model, the filament consists of a nest of tightly packed, magnetic loops of varying lengths. In images of low spatial resolution, the filament is not easily recognizable as a loop system because of the tight packing of the loops and the flattening of the top of the system by the overlying coronal arcade. Recent observations of counterstreaming in filaments, shown during the S-RAMP symposium, are consistent with the concept of tightly packed but distorted loops; counterstreaming occurs in the fine-scale threads of filaments and reveals mass flowing in opposite directions along adjacent threads (Zirker et al., 1998). A second difference from the traditional models, also confirmed by counterstreaming, is that the mass of the filament occupies the vertical components of the filament structure as well as the horizontal parts; the counterstreaming flows are concurrently seen moving up and down the barbs and in both directions along the spine of the filaments. A third difference from the traditional models is that, above the photosphere, the filament magnetic field is envisioned as a completely separate magnetic structure from the surrounding coronal magnetic fields and it is in dynamic equilibrium with these surrounding coronal fields. The separate identity of the filament and surrounding coronal fields above the photosphere is thought to be related to the opposite chiralities observed in the filament and surrounding coronal arcades. (Right-handed filament magnetic fields lie beneath left-handed coronal arcades and vice versa.) Opposite chirality implies opposite helicity and therefore opposing currents in the filament and the overlying fields. [*C&H*: Eruption and reconnection of the filament fields in relation to reconnection in the flare is discussed in Martin and McAllister, 1997.]

A virtue of the very different magnetic field configurations represented in the Rust–Martin debate is that they represent two views which predict opposing signs of helicity for the filament depending on whether the filament is dextral or sinistral (see Martin and McAllister, 1997 for definitions of these terms). Hence both models cannot be right. Rust's and similar models predict left helical magnetic fields for dextral filaments while the empirical model derived by Martin predicts right helical magnetic fields for dextral filaments. Rust and Martin agree that there is a definitive observational test of the models and both of us are working to acquire the observations. The test is observing helical structure in erupting prominences and comparing the observed sign of helicity with the filament chirality (either dextral or sinistral) before eruption.

C&H: So can we expect a resolution of this debate soon? What observational programs are in the works?

Sara Martin: Glad you asked. This is our "hottest" project: Last summer at HelioResearch I initiated the observing phase on a new small research grant in which one of the specific goals is to obtain Hα Doppler data on erupting prominences which can resolve this debate. As our Hα Doppler data are usually five or more wavelengths in steps of 0.2 or 0.3 Å on and around the Hα line, we will be able to determine some of the three-dimensional character of prominences as they erupt even though some parts of the eruptions will have speeds that exceed the tuning range of our narrow band filter. Three-dimensional information is essential for unambiguously determining whether twisted structure in erupting prominences is left-helical or right-helical.

C&H: Sara, a last question. Can the interplanetary observations of flux ropes help to decide between the two pictures? For example, in your model, would you expect to see interplanetary flux ropes in which the helicity changes as you pass from the front (overlying arcade) to the back (filament). Or does the reconnection sort that all out such that the dominant hemisphere rules are followed?

Sara Martin: Excellent question! Both the detection and recognition of filament magnetic fields at 1 A.U. make it much more difficult to resolve the controversy using interplanetary data for a number of reasons. First, the filament occupies a smaller volume of space than the surrounding CME and is less likely to be detected than the surrounding flux rope. Secondly, the erupting mass appears to have many complex geometries leaving the Sun so we do not know if there is any characteristic recognizable flux rope signature for filaments at 1 A.U. However, if only a few interplanetary observations revealed good evidence for a flux rope of one sign of helicity imbedded within a larger flux rope of the opposite sign of helicity, that would confirm the Martin concept. On the other side, if such events are not confirmed soon, we could not yet say that the controversy is resolved in favor of Rust because we do not know whether the embedded flux rope suggested in the Martin picture would retain the separate identity that it has close to the Sun. In short, test observations near the Sun are easier, definitive for both pictures, and are not subject to the complications of changes during transit to 1 A.U. Hopefully, observations near the Sun will soon resolve this issue.

C&H: Dave (Rust), you have the floor.

Dave Rust: Aulanier's work (see below) shows in very nice detail what Ashok Kumar and I were thinking of when we said that the filament threads must be on the underside of the flux rope. They appear to float above the chromosphere and they appear not to be connected to magnetic features in the photosphere below. I still have yet to see a convincing example of barbs connected to fluxules (see Karpen et al., 1993 for an early use of this word to describe magnetic flux elements in the photosphere). Although the shape of some barbs is influenced by the positions of underlying fluxules, the large-scale flux rope determines the orientation of most of the threads in a filament (as in Fig. 4(a) in Aulanier's comment below). That figure shows that in the best images, filament chirality can be determined from the threads. Because the scale of the flux rope is probably much larger than the width of the filament or the density scale height, one expects that the threads and barbs will be more or less horizontal.

Guillaume Aulanier: In a flux rope type of geometry for a prominence (even with less than one turn along the flux rope), a filament foot (or barb) can be interpreted as a collection of magnetic dips. Their distribution then forms a continuous pattern which links one side of the flux rope down to a magnetic parasitic polarity located on the photosphere (Aulanier and Démoulin, 1998; Aulanier et al., 1998).

Numerical calculations which use observed magnetograms as boundary conditions have been able to confirm this interpretation through the successful modeling of several filaments observed in Hα (Aulanier et al., 1999; Aulanier et al., 2000). One example is shown in Fig. 4.

In summary, in a flux-rope geometry for prominences, it is very likely that the feet are indeed pointing downward. But according to our model, they are probably not formed by simple magnetic arcades originating from the prominence body (as suggested by Martin and McAllister, 1997), but rather by a continuous distribution of magnetic dips.

H: Dave (Rust), you probably can remember a time before Kopp and Pnueman (1976) when people considered the possibility that the rising post-flare loop system might be explainable just as a cooling effect, since the cooling time would probably be longer for longer and higher loops. Nowadays everyone just assumes that the loop system rises as reconnection occurs at progressively greater heights, in large part due to the analysis of the need for post-impulsive energy input in your 1979 paper with MacCombie (see Moore et al., 1980). While this paper provided the best early evidence for continued energy release, wasn't it still only a three-sigma result? Has this effect been confirmed properly since that time? I find it amazing that the whole edifice of the CSHKP model is based on such flimsy evidence! Please comment.

Dave Webb: Let me interject a comment here that may be superseded by Dave Rust's reply. First, the evidence for a rising energy source in these events and the likelihood it is due to a rising X-type neutral point was strong from the Skylab data and continues to be confirmed by more recent data, such as your own favorite SXT data! These latter include a hard X-ray source above the soft X-ray loops, cusps above the arcades, and the highest temperature loops connecting to the tops of the cusps. What's wrong with a three-sigma result, especially for LDEs (long duration soft X-ray events) which have e-folding decay times of many hours? MacCombie and Rust's Fig. 2 shows the classic result from EUV data of line emission images at multiple temperatures that, at a given time, show the different temperature loops declining systematically in height from the hottest (soft X-rays) to the coolest (here HeII). More recent results are highlighted by the series of papers on the June 26, 1992 event, especially van Driel-Gesztelyi et al. (1997) who confirm the same pattern and derive quantitative results similar to those of MacCombie and Rust (1979). Finally, a recent review by Forbes (2000a) summarizes strong support for this view, both observationally and theoretically.

Dave Rust: I wasn't aware that MacCombie and Rust (1979) had such influence. You should cite it more often! I do remember when rising loops were interpreted simply as a cooling effect (e.g., Goldsmith, 1971). Hirayama (1974) may have been the first to link loop prominences with reconnection in the aftermath of a filament eruption. Kopp and Pnueman (1976) picked up the theme later. Hard X-ray images, such as the HXIS images from SMM provided the best evidence for reconnection and added energy input above loops. MacCombie and Rust probably should be checked with *Yohkoh* data since it was hardly a three-sigma result. But it was a rare quantitative analysis of the pressure and temperature in X-ray flare loops.

Lidia van Driel-Gesztelyi: For two long-duration flares observed by *Yohkoh*, Harra-Murnion et al. (1998) combined soft and hard X-ray data to show that there is a hot source above the soft X-ray loops.

C&H: Dave (Rust), you have done key work on both sides of this issue. On the one hand, your filament work (Rust and Kumar, 1994) argues for the existence of flux ropes on the solar surface prior to eruption. On the other your Skylab work (MacCombie and Rust, 1979) is one of the pillars of the CSHKP picture. How do you reconcile the evidence for pre-existing flux ropes with the view that the flux ropes are formed via reconnection upon eruption?

Dave Rust: Since my thesis work was on filaments, I was accustomed to thinking in terms of erupting flux ropes

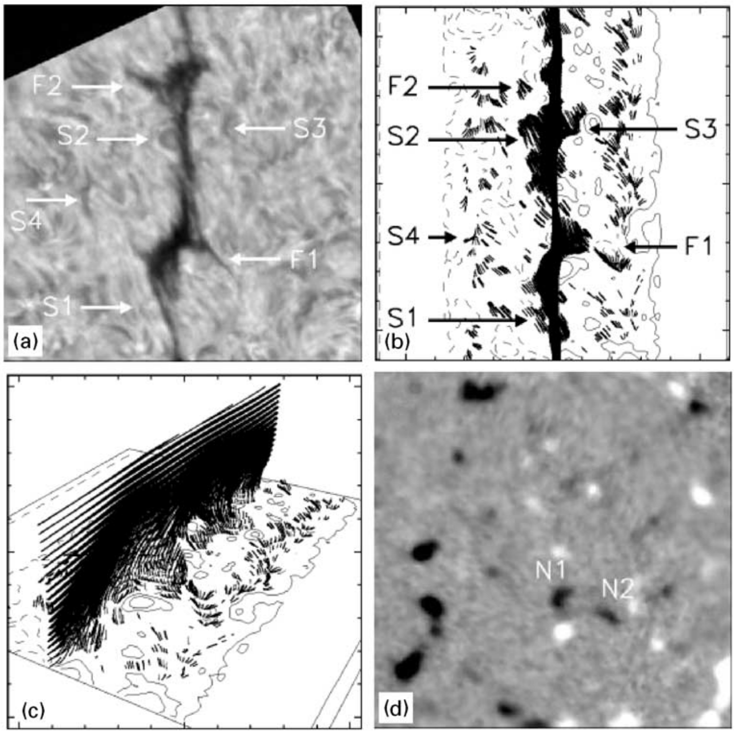

Fig. 4. Filament observed at disk center in Hα with the MSDP instrument at the VTT (Tenerife). (d) Photospheric magnetogram from SOHO/MDI (Michelson Doppler Imager). (b,c) Magnetohydrostatic model of the filament viewed from the top (b) and in projection (c). The model incorporates a weakly twisted flux rope and the observed magnetogram as boundary conditions. The thin lines correspond to isocontours of the vertical field. The thick lines correspond to the lower bottom of dipped field lines, which are supposed to be filled by dense prominence plasma. Note how the twist is barely noticeable, and how the feet (barbs) point downward, and are composed of a continuous distribution of magnetic dips (adapted from Aulanier et al., 1999).

(filaments) when discussing flares. Looking back at Fig. 9.4 of Sturrock's (1980) flare review article, I realize that I never paid any attention to the part of the CSHKP model that used reconnections to disconnect a bit of the corona and make 'ejected plasma' as it is labeled in that figure. So, MacCombie and Rust's analysis of flare loops never had much to do with supporting the 'disconnection' part of the CSHKP picture.

I believe that a filament and its coronal cavity are features of the flux rope. According to Rust and Kumar, this flux rope becomes unstable and pushes open the overlying fields. These fields reconnect after the flux rope, which carries the ejected plasma, leaves the scene. Evidence for plasma in the antisunward part of the pinched-off fields was always weak, I think. See Dere et al. (1999) who suggest how disconnection events can be reinterpreted in terms of ejected helical flux ropes.

C&H: Jim (Chen). Your work on flux ropes at the Sun (Chen, 1989; Chen and Garren, 1993; Chen, 1996) has been a key factor in the growing acceptance of this view of CMEs by the solar community. In your picture, the flux ropes eventually seen at 1 A.U. pop out fully formed from beneath the photosphere. Why do they stop to rest on the solar surface?

Jim Chen: The implicit assumption in the Chen and Garren (1993) erupting flux rope model of CMEs is that a flux rope is ultimately connected to the solar dynamo deep in the convection zone, with the subphotospheric part of the flux rope acting as a conduit of magnetic energy (i.e., electric current) from the dynamo (Chen, 1989). That is, a coronal flux rope is merely the tip of a much larger current/magnetic field structure connected to the dynamo. Such a model is most consistent with the scenario in which fully formed flux ropes rise through the convection zone into the corona as the dynamo "injects" more magnetic energy into them.

As a flux rope rises into the corona, it enters a region with discontinuously lower pressure and faster characteristic speeds (hundreds of kilometers per second in the corona versus a fraction of a kilometer per second below the photosphere). The flux rope tends toward an equilibrium at the fast coronal speed and timescale. The equilibrium consideration of Xue and Chen (1983) shows that there are equilibrium states available to such flux ropes. Recently, Krall et al. (1998) showed that buoyancy forces that may drive a flux rope to emerge through the photosphere and chromosphere decrease significantly as the flux rope rises into the corona, with the flux rope reaching an equilibrium at the base of the corona.

The flux rope then stays in near equilibrium until the next significant packet of poloidal flux is injected into the now coronal part of the flux rope. The fact that prominences and arcades can remain in quasi-equilibrium indicates that there is no significant poloidal flux injection from below the photosphere during the quiescent periods. If, however, a packet of significant poloidal energy emerges, the flux rope must seek a new equilibrium that is far away from the initial configuration. This occurs at the coronal speeds and is interpreted as an eruption.

C&H: Jim (Chen), as you point out, the approach of starting with a current-carrying flux rope emerging from below the photosphere (Chen, 1989, 2001) is in contrast to the prevailing view that in the solar corona any (organized) current-carrying magnetic structures occur as a result of twisting the photospheric footpoints of initially "untwisted" potential (current-free) magnetic field lines (e.g., Birn et al., 1978; Mikić and Linker, 1994; cf., DeVore, 2000). What's the evidence for the new point of view?

Jim Chen: Tanaka (1991), Lites et al. (1995), and Leka et al. (1996) have found evidence of the emergence of current-carrying flux ropes. Wheatland (2000) has examined the vector magnetic field measurements of 21 active regions and concluded that large-scale currents are not neutralized in most of these active regions.

Alex Pevtsov: I think, that most of the coronal current systems have their origin in subphotospheric processes, i.e., dynamo or field-plasma interaction in the convection zone. There is limited observational evidence (e.g., Leka et al., 1996; Zhang, 2001) that magnetic fields emerge already twisted (and so, carrying electric currents). Also, there are several large-scale patterns that are present in the coronal and photospheric fields and hint on their subphotospheric origin, e.g., large-scale coronal flux systems (Pevtsov and Canfield, 2000; cf., Pevtsov and Canfield, 1999) that are 50–60° in size and maintain the same sign of helicity for up to five solar rotations. I find it is hard to explain these large-scale patterns by surface processes such as, say, sunspot proper motions or differential rotation. On the other hand, the asymmetry in the dynamo action or large-scale convection pattern (giant cells?) seems a reasonable explanation. Still, surface processes may play some role, e.g., in shearing coronal arcades. Some time ago, Loren Acton showed me one example of a sigmoid that appeared to be formed by a gradual shearing, although I think this is a deviation from the rule. On average, a sigmoid persists for about 3–4 days, and it is just not enough time for differential rotation to create a noticeable shear (and electric currents).

Terry Forbes: In regard to homologous flares, it could really be that all of the free magnetic energy needed for the next event in the sequence is regenerated in the corona (as a result of differential rotation or footpoint motions) in the short time between successive events. However, a study by McClymont and Fisher (1989) argues that for large events there isn't enough time between the events to do this, at least from the observed surface motions, implying that the magnetic fields emerging from the convection zone may already be in a stressed state. [*C&H*: In his recent review (Forbes, 2000b), Terry notes that the McClymont and Fisher result resonates with a statement once made to him by Hal Zirin that "big flares are born bad".]

C: Jim (Chen). For the benefit of a wider audience, please tell me again about the "conveyer belt" (my term, but I think I picked it up from somewhere else) concept of repetitive (homologous) CMEs from an active region that we discussed at S-RAMP.

Jim Chen: Let me propose the following scenario as a physical, albeit provocative, possibility. It is speculative in that none of the subphotospheric aspects can be directly observed. However, every aspect is based on well-understood and reasonable physics, and the photospheric manifestations should be quantifiable by realistic calculations. This being a very new scenario, only a limited amount of specific calculations has been done to date.

Consider first a simple 1-D neutral sheet, which is defined by a current sheet and a magnetic field reversal across the current sheet. It is well known that such a current sheet is unstable to the tearing mode in the resistive MHD regime (Furth et al., 1963) as well as in the kinetic regime (Coppi et al., 1966). More generally, current-carrying plasmas are subject to various filamentation instabilities. Fig. 5(a) illustrates a current sheet that has undergone the tearing instability and has perhaps nonlinearly saturated. Fig. 5(b) shows how such a current sheet might occur in the solar atmosphere. This figure depicts a 2-D cut through a 3-D structure illustrated in Fig. 5(c), which is a schematic of the current system viewed orthogonally to that shown in Fig. 5(b). The field lines are ultimately anchored in the solar dynamo, probably at the base of the convection zone.

Let the entire system be in equilibrium. Suppose that the dynamo works harder for a period of time, injecting poloidal magnetic energy into the existing structure. The excess magnetic energy, which is now out of equilibrium, must be distributed throughout the system. That is, the current increases, and the entire system tends to rise. As the excess magnetic energy reaches the corona at the slow subphotospheric Alfvén speed, the coronal part rises at the fast coronal Alfvén speed, which is interpreted as an eruption. If sufficient poloidal energy is also added to the current loop below the photosphere, it can rise into the corona and take the place of the one that has expanded away.

Physically, the entire system, including the coronal flux rope and the larger subphotospheric system of currents, relaxes on the local MHD velocity and time scales to the increased magnetic energy generated by the dynamo (Chen, 2001). The fact that successive eruptive flares/CMEs

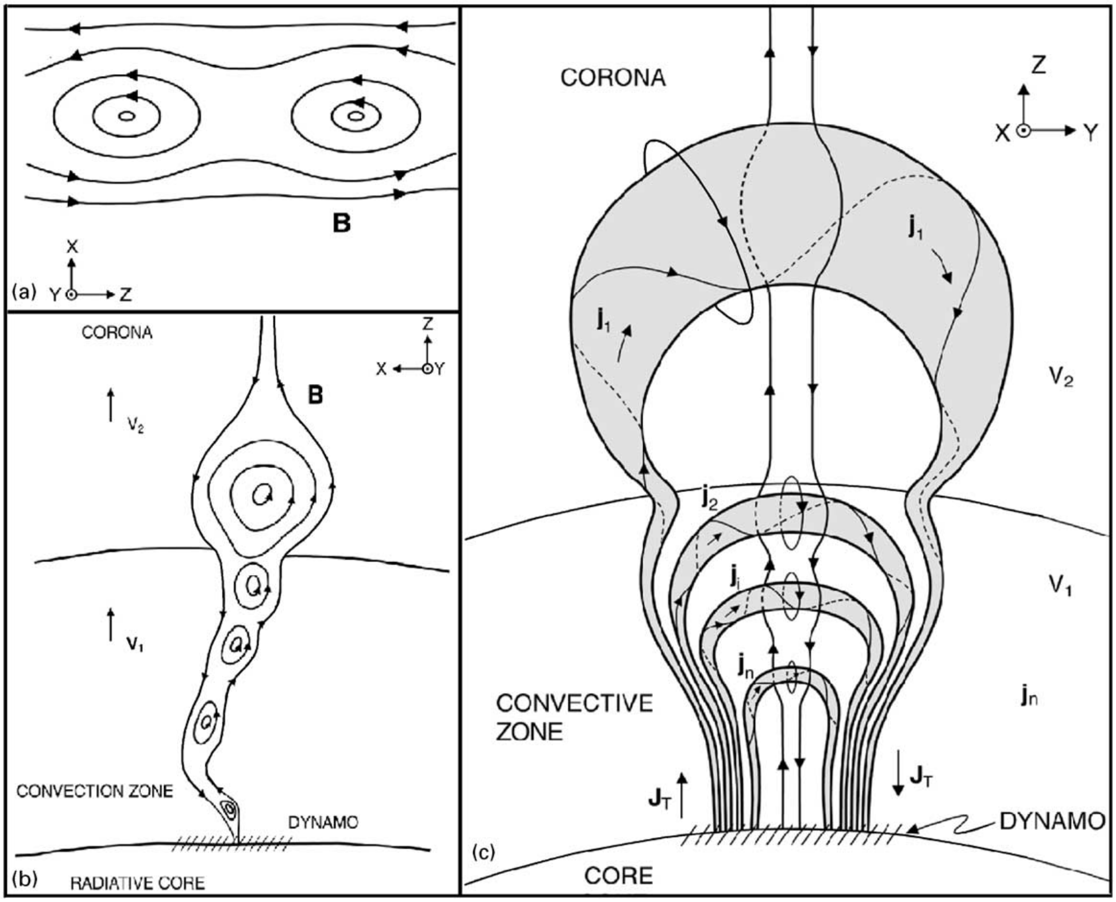

Fig. 5. "Conveyor belt" model of solar eruptions. (a) A generic current sheet that has undergone the tearing mode instability. (b) Schematic of how such a current sheet might occur in the solar atmosphere. (c) Schematic of the current system viewed orthogonally to that in (b). The distinguishing aspect of this picture is the emergence of intact flux ropes from below the photosphere. Solar eruptions are viewed as a direct manifestation of dynamo action. Panel c is an elaboration of Fig. 1 of Chen (1989).

occur in essentially the same location is consistent with the presence of an overarching magnetic organization and is a natural consequence of a system depicted in Fig. 5(c). Some key questions that must be answered include what observable signatures in the photosphere and the corona may arise. These have not been addressed, except that any photospheric motions would occur at the local magnetosonic speed (of the order of 1 km/s or less) and be highly nonuniform (Chen, 2001), implying that poloidal flux injection should be manifested as enhanced broadening of spectral line profiles rather than Doppler shift of coherent large scale motions in the photosphere.

Note that Figs. 5(b) and (c) are similar to Fig. 2 of Parker (2000) in that all coronal field lines ultimately are traced to the base of the convection zone in complicated ways. The important difference is that I have explicitly included in Fig. 5 an electric current distribution that is filamented. Unless the magnetic field and plasma medium (e.g., pressure) are constrained to be uniform and smooth, naturally occurring current distributions strongly tend to break up into filamentary current elements. This necessarily produces a poloidal component in the magnetic field of each current filament as given by Faraday's law. This is what we call a flux rope. Thus, flux ropes are prevalent building blocks of natural plasma systems. As the initial flux rope rises from the base of the convective zone, it may fray and evolve into complex strands (e.g., Piddington, 1979). It has been found by some researchers that complex strands of flux ropes provide the most natural explanation of observed magnetic activities associated with flares (e.g., Ishii et al., 1998, 2000).

C&H: Jim (Chen), from an Occam's Razor point of view, this picture is appealing—with a single flux rope remaining intact from the base of the convection zone to 1 A.U. But there are some loose ends. Where does the evidence for reconnection (that goes back at least as far as Bruzek's (1964) report of rising knot formation) fit into your picture? Since, in the conveyor belt model, reconnection plays no role in flux rope formation, is reconnection merely an interesting (from the point of view of particle acceleration) peripheral phenomenon or side-show? Can this picture handle large-scale CMEs such as those associated with transequatorial loops (Khan and Hudson, 2000) or polar crown filaments? Finally, it seems that ultimately you have to have reconnection somewhere to prevent the indefinite buildup of magnetic flux in the interplanetary medium, the "flux catastrophe" (Gold, 1962; Gosling, 1975). These questions will require more space than we have here to debate but, in the spirit of this exercise, please comment.

Jim Chen: You've given me four questions (and a space constraint). Since the "conveyor belt" model is pretty new, I have to calculate more quantitative consequences in terms of photospheric and coronal signatures of emergence of flux ropes as implied in Fig. 5. Let me simply state that the model does not require reconnection in the corona and that there is no intrinsic limitation based on size (as in transequatorial loops). However, flux catastrophe is a point that requires a bit more detailed comment. This was discussed at some length in Chen (1996; Section 5.1); there it is shown that in the limit of strictly zero resistivity, the magnetic energy in a heliocentric sphere of any finite radius diverges logarithmically with the number of eruptions. If, however, there is any nonzero resistivity, the cumulative magnetic energy associated with erupting flux ropes tends to a finite value determined by the average resistivity in the flux ropes. Therefore, as long as there is some dissipation, there is no flux catastrophe. In my picture, reconnection occurs on the microscopic ion inertial scale. That is, rather than viewing magnetic reconnection (disconnection) as identifiable "events" on macroscopic scales (e.g., the eclipse comet event of 1893, Cliver, 1989), one would treat reconnection as a part of wave–particle interactions working to dissipate magnetic energy throughout the volumes of flux ropes and solar plasmas.

I would make a general comment regarding magnetic reconnection in the solar corona. The theoretical concept of reconnection is reasonably well understood on the scale of current sheets. In the solar corona, the thickness scales as the local Larmor radius, which is many orders of magnitude smaller than the system sizes and the current limit of spatial resolution. Thus, it is not known how reconnection occurs to change the magnetic topology (i.e., connectivity, not geometry) of a macroscopic system, e.g., to convert an arcade into a flux rope. In numerical MHD simulations, reconnection occurs via numerical diffusion on the chosen grid size.

C&H: The dissipation you describe could explain the fate of the ends of the flux ropes (one of the 10 key unanswered questions of CME physics (!) listed in Hudson and Cliver, 2001) identified with the double dimmings seen in certain events.

Waves (*Including Type II Bursts*)/*Dimmings*/*Global Brightenings and Flares*/*CMEs*: *Six phenomena in search of a paradigm.*

Background: The surfeit of new observations of solar eruptions from SOHO and *Yohkoh* have many in the solar community working in a data survey mode. Recently discovered phenomena are not yet well enough characterized, let alone understood, to lend themselves to a coherent picture.

C&H: Simon (Plunkett), perhaps the two greatest discoveries in the new observations are the coronal (EIT) waves (Thompson et al., 1998), an example of which is shown in Fig 6, and the dimming regions that appear to mark out the footprints of CMEs (Sterling and Hudson, 1997; Thompson et al., 2000a). (Of course there were precedents for each of these discoveries in Moreton waves (Smith and Harvey, 1971) and transient coronal holes (Rust, 1983), respectively; nothing new under or on the Sun!) It is still not clear how these phenomena relate either to each other or to CMEs and flares. Please give us your working hypothesis on how these various phenomena fit (or do not fit) together.

Simon Plunkett: I don't see the waves as necessarily flare-induced phenomena. There is at least one very good example of a wave produced by a filament eruption well away from any active regions (23 October 1997). With this caveat in mind, it is true that most waves are flare-related. The cadence of the EIT observations is usually not good enough to determine the timings unambiguously, but the data are consistent with the view that the flare and the wave start at about the same time. There is also not a one-to-one correspondence between waves and CMEs. There are many CMEs without waves, and many waves with no visible CME in LASCO. The waves are not simply the ground track of the CME. A recent paper by Thompson et al. (2000b) shows that the EIT waves are the coronal counterpart to chromospheric Moreton waves.

Coronal dimming is a much more reliable signature of CMEs. Substantial dimming is always accompanied by a CME (don't ask me to quantify substantial). However, it is not clear how to relate many dimming events to the features observed in CMEs. The twin dimmings seem to be good signatures of erupting flux ropes (Webb et al., 2000b), but the majority of dimmings don't look like this. Many energetic events have extended dimming regions, often with a ragged appearance, that seem to span the entire width of the CME (e.g., Thompson et al., 2000a). For the wave-associated events, at least some of the dimming is due to material that is swept up by the wave as it propagates through the corona (the 12 May 1997 event, Thompson et al., 1998, is a good example of a density depletion behind the bright wave front). Again, the timing is unclear, but the observations are consistent with the view that the dimmings occur simultaneously with the onset of the CME (and wave, if there is one). But beware of the big flare syndrome—these features are easiest to see in the big events, where everything seems to happen together with an impulsive energy release.

C: A follow-up. When you say many waves lack associated CMEs, are you taking into account the location of the wave on the disk? Stated otherwise, do you see waves near the limb without CMEs?

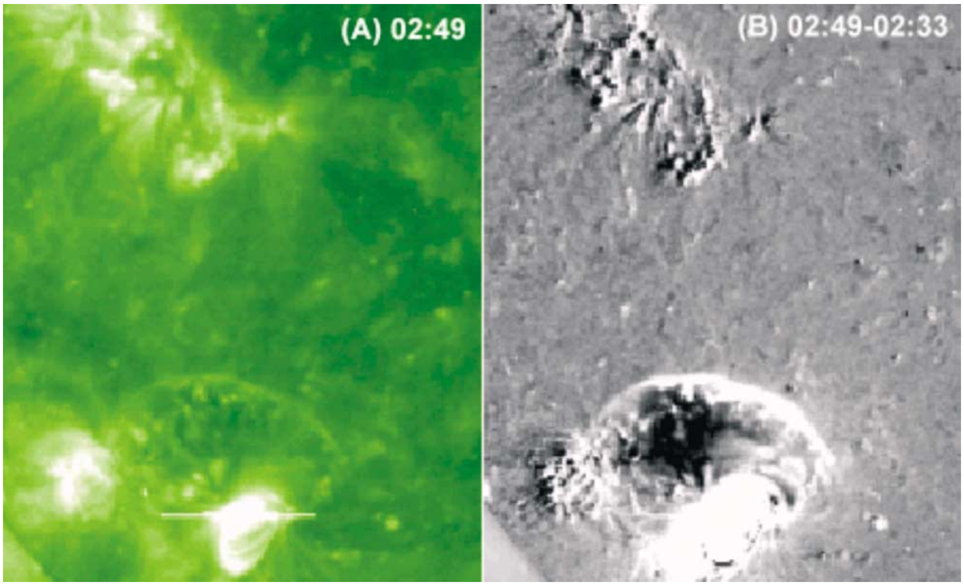

Fig. 6. A 195 Å image of an EIT wave at 0249 UT on 24 September 1997. (b) Difference image (0249 UT-0233 UT). (adapted from Thompson et al., 2000b).

Simon Plunkett: I don't have a good answer to your question. My recollection is that most "wave without CME" events were disk events. I can't think of a case where a wave on the limb did not have a CME, but that does not mean it hasn't happened.

Joan Burkepile: I was quite surprised to read Simon Plunkett's statement that there are "many waves with no visible CME in LASCO". I have heard statements to the contrary. Barbara Thompson and colleagues have compiled a list of coronal waves recorded in EIT along with an assessment of their confidence levels that the phenomenon recorded is indeed a wave. These levels are based on the visibility of the wave and the quality of the data. Barbara and colleagues have recorded 170 wave candidates between March 1997 and June 1998. A quick glance at the LASCO CME listings seems to indicate there are many waves with potential CME associations but there are days such as 25 April 1997 where LASCO reports no new CMEs and Barbara reports 3 possible waves; one with a confidence level of 25% to 50%, one with a level of 50% to 75% and one with a confidence level of 75% to 100%. The EIT wave list and the LASCO CME lists by St. Cyr, Plunkett and others will greatly expedite the task of quantifying the association between coronal waves, CMEs, and flares.

A comparison of the Thompson et al. coronal wave list with GOES X-ray flux plots indicates the coronal waves are associated with all levels of GOES X-ray emission, from A to X (10^{-8}–10^{-4} W m^{-2}, respectively). From the 170 wave candidates identified by Thompson et al., 51 have a confidence level greater than 50%. The GOES peak emission, listed in increasing emission values on a logarithmic scale, associated with these 51 potential waves are as follows: 1 A level, 18 B level, 15 C-level, 10 M-level and 6 X-level flares. Since X-level flares are relatively rare it is apparent that an appreciable percentage of X-level flares have waves (and CMEs) associated with them. However many hundreds of A- and B-level flares occur per year and only a tiny percentage of these are associated with waves. A quick look at the LASCO CME lists suggests the low intensity X-ray events associated with waves are the events that are also associated with CMEs. While it is possible that more than one phenomenon may be producing waves in the corona, I believe that CMEs are responsible for generating many of the waves seen in EIT. White-light coronal images have recorded deflections produced by the passage of CMEs through the ambient corona. It is widely believed that these deflections are generated by compressive MHD waves produced by the CME as it displaces coronal material (Gosling et al., 1974; Hundhausen, 1993).

More extensive studies of the available data, coupled with models of CME and flare driven waves need to be completed to answer your question to the satisfaction of the community. [*C&H*: A detailed comparative study of CMEs with EIT waves (and dimmings) by Doug Biesecker, Dawn Meyers, and Barbara Thompson is in progress.]

H: Cecile (Delannée) and Guillaume (Aulanier), you have interpreted the EIT waves, at least some of them, as stationary bright fronts (Delannée and Aulanier, 1999). How can a flare-associated coronal wave be stationary? Or, better phrased, what is your view of which coronal field lines become open during a CME eruption?

Cecile Delannée and Guillaume Aulanier: In fact many of the EIT waves are indeed stationary (three new cases are described in Delannée, 2000). If you accept this statement, then the EIT wave phenomenon is probably not a physical wave-like propagation. But you might not believe that most of the EIT waves are stationary for two major reasons: (1) The only EIT waves which are very often shown during scientific meetings are those of 7 April and 12 May 1997. Those are indeed clearly propagating. But we found very few similar cases. (2) The image processing which is often done (i.e., running difference images or RDI) is very misleading because: (i) a sequence of RDI images does not show continuous time variations, but only the variations from one image to another, and (ii) the images are rarely corrected for solar differential rotation (note the $\sim$ 10–15 min time resolution of EIT observations). The above effects create many artificial changes of brightness in time, which might be confused with propagations (waves, motions).

A much better way of emphasizing real variations is to do base difference images (BDI), which consist of subtracting one single early image from all of the following ones, and correcting each image for solar rotation. Using the BDI method, one very often finds stationary wave fronts, or slowly moving fronts which become stationary and remain still for a long time (Delannée and Aulanier, 1999; Delannée, 2000).

Recently, Yi-Ming Wang (2000) showed that under some conditions, a propagating MHD wave may appear stationary, due to a change of direction from parallel to the photosphere, turning upward. Viewed from above this may appear as an artificially stationary brightening. But one has to keep in mind the limitations of his approach which are very honestly written in his paper, e.g., unrealistically low propagation speeds and the lack of field opening by the CME.

To answer the second version of your question, probably most of the field lines which were initially overlying the (small scale) erupting region do open. One finds the dimmings at the footpoints of all these field lines, and brightenings at the boundary between those and the ones which remain closed (Delannée and Aulanier, 1999; Aulanier et al., 2000). Why do brightenings appear there? We think it is because strong currents must develop in these locations during a fast eruption (see the analysis of an MHD simulation of a CME in Delannée and Amari, 2000).

C&H: Nat (Gopalswamy), the global brightenings (Gopalswamy et al., 1999b, 2000) you have reported seem every bit as interesting as the dimmings they presumably precede but they are as yet unappreciated. First question, are they real? Since you see them in partial flare images from SXT, is there any chance that they could be due to scattered light? As a follow-on, how do you think the emission is excited? Finally, how are the global brightenings related to EIT waves (both moving and stationary, see above) and dimmings? Or to soft X-ray global effects, as reported for example by Hudson, Acton, and Freeland (1996)?

Nat Gopalswamy: Are they real? Looks so to me. The global enhancement was first detected in X-rays (Gopalswamy et al., 1999b). Co-author N. Nitta undertook to show that the enhancement is not due to scattering and a paragraph to this effect can be found in the paper. In EUV, these are frequently detected (see Gopalswamy et al. (2000) for several examples). In this paper, we showed an example (also on the cover of the GRL issue) which was examined by an EIT expert (F. Auchere) and “certified” to be not due to scattering. Another reason to think this enhancement is real is that a Nancay radioheliograph radio image clearly shows a radio source (moving type II burst?) coincident with the enhancement, especially at the sharp outer edge. My preliminary interpretation is that the global enhancement is the “baby CME”. What we are seeing is an emission measure effect due to compression. I have not analyzed them in more detail, but my feeling is that the EIT wave surrounds the global enhancement. I have not looked into the relationship between global enhancements in EUV and X-rays.

H: Ed (Cliver), I think you are the last person who really believes that metric type II bursts are all driven by CMEs. Doesn’t that give you pause?

Ed Cliver: No más. I capitulate. I think some of the recent observations, particularly those of Klein et al. (1999) showing images of type IIs in close association with flare ejecta with the CME leading edge well above, provide convincing evidence that flare ejecta drive at least some type IIs. This, of course, can account for the close timing between flares and type II bursts, neglect of which prompted you to call our paper (Cliver et al., 1999) a “swindle” (Hudson, private communication, 2000). That said, we (Gopal, Chris St. Cyr, and I) have yet to find compelling evidence for a type II burst that lacks a CME. [No case of a type II associated with a limb flare and no CME in LASCO through June 1998.] And as Nitta and Akiyama (1999) have shown, flare ejecta seen in soft X-rays (are these simply the hot counterparts of Hα flare sprays or eruptive filaments?) are only observed in association with CMEs, possibly because of field lines previously opened by the CME. There is no evidence for pure blast or simple type II shocks, i.e., shocks initially lacking a material driver. The best argument against such shocks is the poor correlation between type II occurrence and flare size, with some very small flares (with CMEs) producing type IIs and some very large flares (without CMEs) not. This could be an effect of ambient conditions near the flare site but I find this hard to believe given the broad extent of shocks in the corona. I still like the single shock picture from an Occam’s razor point of view. Two shocks (one driven by the CME and one by the ejecta; or the CME rising front vs. its lateral expansion?) would have interesting implications for particle acceleration by coronal/interplanetary shocks.

C: Nat (Gopalswamy), in Gopalswamy et al. (1998), you essentially argued that all metric IIs were blast waves because they tended to die out in the corona. You allowed that the blast waves need not be pure (simple waves, no driver) but could be driven by short-lived ejecta (problematic because some of the short-lived ejecta you reported (e.g., Gopalswamy et al., 1997) were not, in fact, short-lived but escaped the corona!). You've done a lot of work on this topic in the meantime. Please give us an update on your current world view.

Gopal: When Gopalswamy et al. (1998) was written, there were no type II bursts observed in the Decameter–Hectometer (DH) domain. Now there are many DH type II bursts. All of these DH type II bursts are associated with CMEs that are faster and wider than typical CMEs (Gopalswamy et al., 2000, 2001). This result is consistent with the older result that all of the kilometric type II bursts are associated with CMEs. In other words, we can say that from about 2 R_O to 1 A.U., almost all type II bursts are associated with CMEs. The question now is whether we can extrapolate this result to metric type II bursts. This is where I am pretty much stuck due to lack of evidence. For instance, the high frequency ($\sim$ 500 MHz) type II bursts suggest a shock very close to the solar surface, at the typical height of a flare. We do not have CME data at present to support the possibility of a CME-driven shock at such a low altitude. In a recent paper, we determined that the fast-mode speed may be as low as ~ 200 km s^{-1} in the inner corona ($\mathbf{r} < 1.5R_O$), so it is easy to produce a shock there (the mass motion needs to exceed only 200 km/s to drive a shock). The fast-mode speed profile has a bump in the 2–3 R_O range (where the DH domain starts) with a peak fast-mode speed of several 100 km/s. The shock generation will therefore be short-lived if the driver speed is less than the peak fast-mode speed. It does not matter whether the driver escapes the corona or not; what matters is whether it can drive a shock or not. As I maintained in Gopalswamy et al. (1998), if CMEs are fast enough at low enough heights, they can be a source of metric type II bursts. A clear example of a metric type II burst driven by a CME (19 May 1998) has been published (Gopalswamy, 2000). [*C*: Reiner and Kaiser (1999) interpreted this same event as a flare blast wave type II.] The fact that DH type II bursts are due to fast and wide CMEs suggest that we need to see if the metric type II bursts are due to narrower and slower CMEs. I agree with you that we have not found a limb type II event without a CME. I present this as a challenge to the community to come up with a list of limb type II bursts without CMEs [*C*: Or limb EIT waves without CMEs.]. Of course, this does not completely solve the problem: We still need to show that the type II burst is due to the CME and not the flare in a CME-flare-type II event. In summary, I continue to be skeptical that all the metric type II bursts are due to CME shocks.

Dave Webb: As a long-time co-conspirator in the CMEs-drive-all-type IIs controversy (e.g, Cliver et al., 1999), let me emphasize that to me the evidence is still strong that all observed type IIs are driven by mass ejecta, whether in the corona or interplanetary space. Statistically we have shown a strong association between CMEs and metric IIs in all the coronagraph data sets (Skylab, Solwind, SMM and LASCO). The observations of flare ejecta that Ed and Nariaki refer to involve mass ejecta near the surface which are well associated with white light CMEs seen further out. The new idea is that bits and pieces of an ejection near the surface seem to be able themselves to drive type II bursts. But I would still argue that it is the mass ejection, not the flare, that is a necessary condition, what Ed calls a "special condition", for a type II. The logical extension is that the type II is a piston-driven phenomenon, not evidence of pure blast waves. Proper comparative analyses of the EIT wave-type II-CME data should help us make further progress.

Table 1
Dominant hemispheric helicity conventions for different solar features

Features [Ref][a]	Northern hemisphere	Southern hemisphere
Sunspot whorls (1,2)	Counter-clockwise	Clockwise
Helicity sign in ARs (3,4)	Negative	Positive
Sigmoidal corona (4,5,6)	Backward-S	Forward-S
Structures in filaments (7)	Dextral	Sinistral
Coronal arcades (8)	Left-handed	Right-handed

[a](1) Hale (1927).
(2) Richardson (1941).
(3) Seehafer (1990).
(4) Pevtsov et al. (1995).
(5) Rust and Kumar (1996).
(6) Canfield et al. (1999).
(7) Martin et al. (1994).
(8) Martin and McAllister (1997).

Helicity and Sigmoids: "*S*" *marks the spot.*

Background: Rust (1994) linked the chirality or handedness of magnetic clouds at Earth to the predominant chirality of the solar hemisphere from which they originated and, in so doing, introduced the concept of the helicity of the solar magnetic field into the study of coronal mass ejections (see commentary by Glanz, 1995, and a recent paper discussing helicity in a broader context by Blackman and Field, 2000). The magnetic helicity is a conserved quantity, defined to be the volume integral of the dot product of the magnetic vector potential and the field. As an application of helicity, Canfield et al. (1999) (see also Canfield et al., 2000) showed that S-shaped soft X-ray structures (sigmoids) observed with SXT on *Yohkoh* could be useful for CME prediction.

C: Lidia (van Driel-Gesztelyi), you had a nice crib sheet view graph explaining simple hemispheric rules (see also Martin, 1998a) to those like me who are helically challenged and intimidated by terms like chirality. As a prologue to this discussion, please send us your view graph (Table 1).

H: Lidia (van Driel-Gesztelyi), you've shown that the isolated active region in 1996 continued to produce CMEs while its flare occurrence rate decreased. The natural interpretation of this, quite interesting, is that the CME process might result from the need of the Sun to regulate its coronal helicity content. For CMEs, this would replace the "flare buildup" scenario in which coronal free energy, rather than helicity, would be limited by flare energy release. Is this how you see it? Could you comment on how you imagine coronal helicity to increase, and not the coronal non-potential energy at the same time? While you're at it, could you reassure us that the flare rate you quote is not sensitive to threshold effects—the GOES data are biased towards higher temperatures.

Lidia van Driel-Gesztelyi: Following the evolution of a single active region (NOAA, 7978) from the cradle to the grave between July and December 1996, we indeed found that flares were observed only during the first three rotations, and the importance of flares steadily decreased with time both in soft X-rays and Hα (SGD, see van Driel-Gesztelyi et al., 1998). However, the above study did not include microflares and we believe that small-scale reconnection and dissipation was present even during the decay phase of the AR. Since large- and small-scale flows continuously disperse the field, after the third rotation the photospheric magnetic flux density became already low, under 20 Mx/cm^2 (van Driel-Gesztelyi et al., 1999) so consequently the free energy density was quite low in the corona as well. When reconnection occurs in such a low magnetic density configuration, it is normally not a fast one and we do not expect energetic events leading to strong evaporation, thus bright flare ribbons and loops. The CME productivity of the AR was much more even: after flaring ceased we still observed 3–6 CMEs per rotation, basically related to the eruption of the filament along the magnetic inversion line of the AR. Since the volume of the AR was steadily increasing with time, even with a low magnetic free energy density there was a considerable amount of helicity contained in the AR. As the Sun has to get rid of the continuously amounting helicity, created by the solar dynamo (e.g., Seehafer, 1990) and brought up by the emergence of twisted flux, furthermore, injected by differential rotation (DeVore, 2000), CMEs are prime candidates for performing that task (Low, 1996). So giving an explicit answer to your question: I indeed believe that there is a build-up of helicity before CMEs, just like the flare build-up scenario.

C: Dave (McKenzie): I don't think I have to tell you that many are skeptical about the value of sigmoids as a forecasting tool. I will give you my own reservations as a representative solar physicist. The original paper folded active region size into the analysis, lending a "big active region syndrome" flavor to the result. Also, while I realized that the first study ignored timing considerations, I was surprised to learn at the meeting that it also ignored causality (no distinction whether the sigmoids were observed before or after the CMEs). Please put my fears to rest on these reservations.

Hugh Hudson (speaking for Dave McKenzie): There's wide misunderstanding of the sigmoid business, which is really pretty simple. The observations show that hot S-shaped structures in the corona correlate with signatures of eruption from those active regions. This implies that the shear signature long known from chromospheric (or photospheric) observations relates directly to coronal heating, and that this coronal heating in turn has some relationship to eruptivity. Causality? We're not that far along yet, but we don't need to know about causality to recognize the rather obvious relationship—you don't need to be a weatherman to tell which way the wind is blowing.

C: Hugh, congratulations on an unusually succinct answer. Dave (McKenzie), do you concur or have anything to add?

Dave McKenzie: I think the "big active region syndrome" is inferred from remembering only *half of* the result of Canfield et al. (1999). In the sample of regions that we examined, when the spot size is ignored (as in Table 2 of that paper), we find that sigmoidal regions are two-thirds more likely to produce eruptions than non-sigmoidal regions. In all size categories, sigmoids were more likely to erupt than not.

The most important shortcoming in the usefulness of "sigmoidicity" as a CME-predictive tool is the lack of a widely accepted objective measure of sigmoidicity, particularly as may be applied to coronal imagery (with no comment about the more quantitative vector magnetograms). Moreover, the definition of what is a sigmoid may be a topic of discussion, as Glover et al. (2000) have suggested that a distinction between S-shaped loops and S-shaped regions may be important.

As for timing and causality, we must not neglect that any one region may, and often does, produce more than one eruption, since a single CME may not remove all helicity from the magnetic structure. The term "sigmoid to arcade" is a description of the observational signature that an eruption has occurred, and should not imply that a region necessarily ceases to have an obvious "S" shape after the eruption—though this has occurred in some well-known events.

Alex Pevtsov: In recent paper (Pevtsov and Canfield, 2001) we compared the strength of geomagnetic storms and the orientation of the associated solar magnetic fields, derived by using the shape of the sigmoids and the polarity of their footpoints. We find a clear dependency for sigmoids with one orientation to produce stronger geomagnetic storms. So I think that sigmoids may be a useful forecasting tool after all.

C: Alexi (Glover). You've sounded a cautionary note on the value of sigmoids as a CME forecast tool (Glover et al., 2000). Please briefly recount your findings and give us your view on the utility of sigmoids for predicting solar eruptions.

Alexi Glover: Our study concentrated on sigmoidal morphology rather than size, looking at active regions previously classified as both sigmoidal and eruptive by Canfield et al. (1999). We found that, using both full and partial frame *Yohkoh*/SXT data, these regions could be reclassified according to whether a single, transient S-shaped loop or a collection of loops projected onto the disk to form an overall S (or reverse-S) shape within the region gave them their 'sigmoidal' appearance.

We found that in only 41% of regions could a single, 'sigmoidal' structure be observed; 35% of these sigmoidal regions were found to have eruptive signatures in SOHO/LASCO and/or EIT. Our results do suggest that 'sigmoidal' loops have a slightly stronger tendency to erupt than sigmoidal regions observed as a result of projection effects. Unfortunately, owing to our decision to include SOHO results in the survey, our statistics are not great and so our results are fairly preliminary. It would be interesting though to see if this result holds true for a larger sample.

In terms of the sigmoid's value as part of a useful prediction technique, I agree that we need to know more. As Dave has mentioned above, the term 'sigmoid' is frequently used to describe any region with an overall S-like appearance irrespective of whether this structure is comprised of a single loop or a projection of many. It may also form prior to or following an eruption; either in an active region or the quiet corona over a wide range of sizes. For these reasons I think that before sigmoids can really be used as an effective prediction tool, a stronger definition is needed.

C&H: Dick (Canfield). You've seen what the others ("the nattering nabobs of negativism", to quote another Spiro A.) have written here and elsewhere regarding the usefulness of the sigmoid concept. As the prime proponent of sigmoids (Canfield et al., 1999), we give you the last word.

Dick Canfield: I just want to call attention to two red herrings: Red herring #1: Canfield et al. (1999) ignored causality (no distinction whether the sigmoids were observed before or after the CMEs). This simplification was made for good reason. We know from photospheric studies that (a) active regions maintain their large-scale chirality for their lifetime (Pevtsov, Canfield and Metcalf, 1995), except for possible brief excursions associated with major solar flares, and (b) the twist of coronal fields inferred from sigmoidal structure is the same as that inferred from photospheric vector magnetograms (Pevtsov et al., 1997). Hence, if observations clearly reveal a twisted structure in the corona of an active region at just one time, we have reasons for believing that twist is present in the corona throughout the lifetime of the region. That being said, it is conceivable that we will find that the appearance of a sigmoid of only certain attributes, e.g. a certain size or total twist, presages an eruption. That's consistent with the anecdotal "merging fish hooks" observation of Pevtsov et al. (1996). Red herring #2: We need to refine the definition of "sigmoidal" used by Canfield et al. (1999), i.e., both S-shaped loops and S-shaped patterns of loops show that the twist density is non-zero. We have good reason to believe that both the twist density and the total twist in a given coronal magnetic flux system are important. The definition of sigmoidal used by Canfield et al. (1999) measures the former. Reconnection can transfer twist from one flux system to another, so if a given region has lots of twist on small scales, reconnection can transfer it to larger scales and, perhaps, render the system more MHD unstable. If the goal is to identify regions that are likely to erupt sometime, the definition we used is fine. If the goal is to identify regions that are going to erupt on a certain time scale, one needs to identify several parameters, perhaps the twist density, the twist scale, and the presence of factors which enable reconnection and eruption (perhaps topological factors).

Transequatorial loops: *Joe Khan and Hugh Hudson put these sources of CMEs on the map, but have yet to get a buy-in to their shockwave trigger mechanism.*

Background: The notion that CMEs have much larger angular scales than either active regions or flares was well established by the work of Harrison, Hundhausen, and others from SMM and Solwind observations. Khan and Hudson (2000) captured the cover of GRL with a "now you see it-now you don't" picture of a transequatorial loop in May 1998 that they linked to a CME (Fig. 7). While such a CME source might have been inferred from earlier work (e.g., Kahler, 1991), the Khan and Hudson images removed any (well almost any) doubt that transequatorial loops (TLs) could erupt to form CMEs. Another aspect of their work — the suggestion that these eruptions were triggered by a flare-induced shock wave impinging on one footpoint of the TL — has been met with skepticism in some circles.

C&H: Lidia (van Driel-Gesztelyi), while your helicity rules (Table 1) are still fresh in our minds, please comment on the helicity of the active regions that marked the proximate endpoints of transequatorial loops for the April–May 1998 events as well as for the earlier spate of energetic activity in November 1997.

Lidia van Driel-Gesztelyi: Canfield et al. (1996) and Pevtsov (2000) have found that there is a tendency for active regions (ARs) which have the same handedness (helicity sign) to form transequatorial loops. This implies that one of the ARs should disobey the hemispheric helicity rule. Both NOAA 8100 and NOAA 8210, the active regions most prominently associated with CME activity in November 1997 (Delannée and Aulanier, 1999) and April–May 1998 (Khan and Hudson, 2000; Pohjolainen et al., 2001), respectively, were peculiar in some sense. For example, NOAA 8100 was a south hemispheric region, which had negative helicity (Green et al., 2001) opposite to the majority of ARs on that hemisphere. NOAA 8210, on the other hand, disobeyed the Hale–Nicholson polarity rule for cycle 23. NOAA 8210 was connected via a transequatorial loop to NOAA 8214. Pevtsov (2000) determined the helicity signs for these two ARs and found them opposite, though in his measurements one of the ARs (presumably NOAA 8214) was represented by a single magnetogram.

C: Joe (Khan), I think that your work with Hugh (Khan and Hudson, 2000) on the origins of some CMEs in transequatorial loops is fundamental, representing both a confirmation and a clearer picture of the view espoused by Harrison (1986). It continues the departure from the view that has held sway since Carrington (1860) that solar disturbances or eruptions involve only a single active region on the Sun.

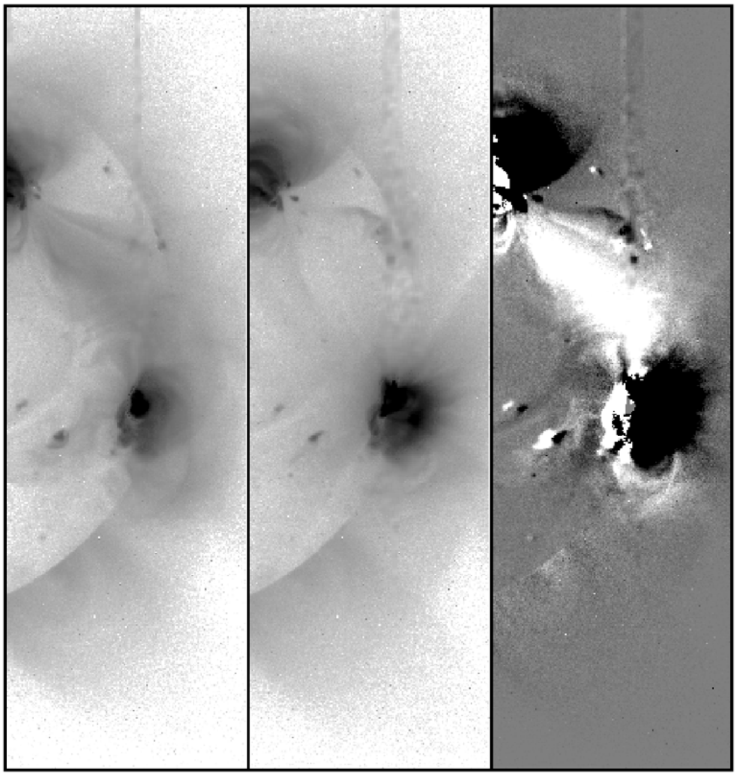

Fig. 7. *Yohkoh* SXT observations showing the disappearance of a transequatorial loop associated with a coronal mass ejection on 6 May 1998: (left) 0608 UT; (middle) 1114 UT; (right) difference image. The images are scaled logarithmically to exaggerate the brightness of fainter features and are displayed with a reverse color table (dark = high intensity) (adapted from Khan and Hudson, 2000).

(Of course, the double dimming events (Sterling and Hudson, 1997) provide compelling evidence that some CMEs do indeed arise from a single active region.) At the same time, I do not believe your picture that the CMEs from the transequatorial loops you investigated are triggered by flare shocks. For example, for the 6 May 1998 event, the leading edge of the CME was imaged at 2.75 R_O at 0805 UT, near the time of the earliest reported onset of metric type II emission by Potsdam at 0803.3 UT (Fig. 8) and the first observation of the SXT wave at 0803.6 UT (Khan and Hudson, 2000).

Joe Khan: I question whether the feature you mention at 0805 UT is related to the leading edge of the CME. The feature you obtain a height for at 0805 UT is much fainter than the leading edge seen at 0829 UT (and, in fact, it may be visible in images at 0731 UT or earlier). One would expect the CME leading edge to get fainter with time, not brighter. Thus I think it is doubtful whether the feature seen at 0805 UT is related to the CME features seen at 0829 UT. I think you should consider 0829 UT as the first appearance of the CME in the LASCO C2 data. If you do, then there is no problem with the timing of the shock wave triggering the eruption. Also if we take the direct observation of the dimming (i.e., the TL disappearance) to be a signature of the CME onset, as suggested by Gopalswamy et al. (1999a) (and as mentioned earlier by Simon Plunkett) then the appearance of the SXT wave precedes the CME onset. Note the wave need only travel the relatively short distance from the vicinity of the flare site to the footpoint of the TL.

Ed Cliver: I looked at the 0805 UT image again and I think you are right. Although the feature observed at that time is in the same approximate location as the front observed at 0829 UT, it is very faint and its height-time point lies well above the constant velocity fit in Fig. 8. But how do you distinguish between your X-ray waves and Nariaki's ejecta (Nitta and Akiyama, 1999)? Couldn't the wave simply be ejecta that drives the type II shock?

Joe Khan: I would have to say that currently we do not have a good handle on how to distinguish clearly between waves, moving loops or ejecta seen in the SXT data. So, yes, the SXT features we see could be ejecta driving the shock wave. They may also be the wave before it is shocked. I think too few events have been examined to date to be able to give a clear answer at the moment. We need to examine more events. Recent work with Hudson examining the features in the 1998 May 6 flare seem to indicate that it is wave-like.

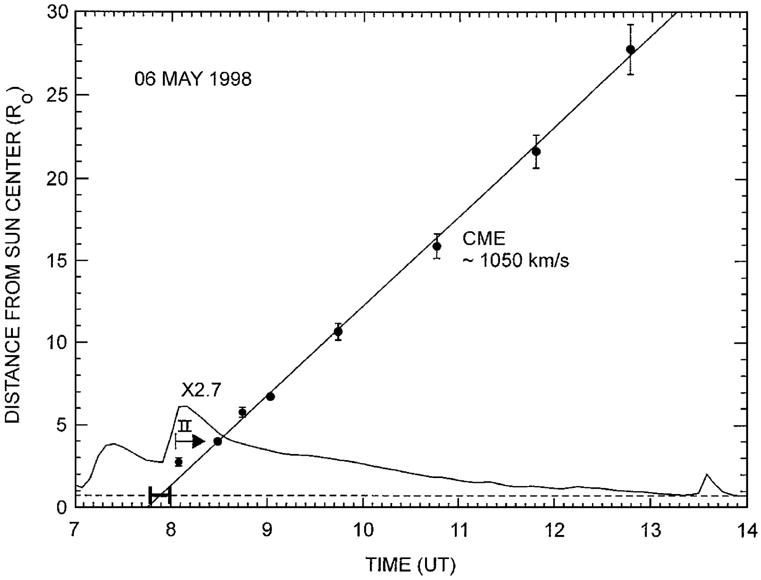

Fig. 8. Height–time plot for the leading edge of the CME (measured in the plane of the sky) associated with the disappearing transequatorial loop (shown in Fig. 7) analyzed by Khan and Hudson (2000). The approximate CME speed is given. The dashed line corresponds to the distance of the related flare from disk center. The heavy bar on this line gives the uncertainty in the extrapolated CME lift-off time. The figure shows the 1–8 Å data and the metric type II burst occurrence, the latter arbitrarily placed at 4 R_O for ease of viewing.

The propagating disturbance shows bending of the wavefront towards the chromosphere. This is suggestive of the refracting of shock waves towards regions of low Alfvén speed used by Uchida (1968) to explain Moreton waves.

Simon Plunkett: A question for Joe and Hugh: What is the link between transequatorial loops and CMEs seen by coronagraphs? Put another way, what feature or features in the associated CMEs do you identify with the transequatorial loops? You have argued that most of the CME mass may originate in these large-scale loops. This would imply that most of the plasma we see in CMEs is hot (3–4 MK), and originates in the low corona. I find it difficult to believe this picture. What happens to the mass in the streamer that (presumably) overlies these loops? I am also not convinced by the arguments that flare-driven shocks cause CMEs. The initial observations of the CMEs in your GRL paper were all at heights $> 2.5\ R_O$ (Fig. 8). It requires a leap of faith to make detailed timing comparisons between these observations and events occurring in the low corona.

Hugh Hudson: Thanks for the question. Joe will want to add to the answer, but I'll point out right away that the transequatorial events are a small minority of the CME precursors that we see in SXT images. Most of the others are more directly in active regions. As for the mass overlying the TL — give us an estimate please! I think we don't know much about the mass and/or field distribution at these heights, especially during transients.

We don't claim that flare-driven shocks cause all CMEs. But for these transequatorial cases, there is little room for maneuver in the data. Don't forget that *Yohkoh* can have much higher time resolution than EIT or LASCO.

Joe Khan: I am not really sure what is the precise relation between the soft X-ray TLs and the loop-like CMEs seen in LASCO. But I think that a substantial fraction (don't ask me to quantify substantial) of the CME mass may originate from these large-scale loops (in these types of events only, of course). So yes, that implies that a lot of the mass seen in these types of CMEs is hot. Whether it is in the low corona depends on what you mean by low corona. Judging from the LASCO C2 images, it seems that most of the mass may be the loop-like CME features. Concerning the timing comparisons, no leap of faith is necessary, see what I said above. It should be borne in mind that this discussion refers only to a specific class of CME, not all CMEs in general.

Simon Plunkett: I don't mean to imply that the observation of disappearing large-scale loops is not important. Harrison (1986) and Plunkett et al. (2001) have shown that the flaring region or erupting prominence in the low corona can be offset by tens of degrees from the central location of the CME. Thompson et al. (2000a) showed that the EUV dimming regions in energetic events were also offset from the associated active regions, and mapped out the angular extent of the CME. The transequatorial loops appear to me to be another link in this chain connecting small-scale activity (active regions, flares) to large-scale activity (CMEs). It would be interesting to study the spatial and temporal relationships between the loop disappearances and these extended dimming regions.

Joe Khan: I agree. Work by Pohjolainen et al. (2001) examined a flare-CME event on 1998 May 2 involving a disappearing TL similar to those examined by Khan and Hudson (2000). In that event we find that the region of SXT dimming (the disappearing TL) and the region of EIT dimming are significantly offset. My impression is that the shock wave may have cleared out the apparently lower lying EIT features as it propagated (giving rise to the EIT dimming), while the SXT TL may have erupted by becoming destabilized by the shock wave passing close to the TL footpoint nearest the flare region.

Nat Gopalswamy: The Khan and Hudson (2000) events are from the same region reported by Gopalswamy et al. (1999a). On 27 April 1998, a disappearance of transequatorial loops was observed in X-rays and in EUV as dimming and was found to be the signature of the CME onset. In this event, the flare followed the dimming (and CME) by about 45 min. We also contradicted Hudson et al. (1996) who suggested that dimming was causally related to the flare in another event. There were multiple episodes of type II bursts during the 27 April 1998 event which do not seem to support the Khan and Hudson (2000) view of CME initiation by metric type II bursts.

Joe Khan: I accept your idea that dimming may be taken to be the first signature of the onset of a CME seen on the solar disk, but I don't agree that you have demonstrated convincingly that the flare follows the dimming for the 1998 April 27 event. Defining the start of that flare is difficult. The GOES soft X-ray flux shows a roughly smooth rise in flux from about 07 UT. In summary, the 1998 April 27 event is not a 'clear' case because the flare start time is not easily pinned down.

We do not claim in our paper that all observed dimmings must always follow the flare for all kinds of events. Also, we do not suggest that all CMEs are triggered by shock waves, just those three events we observed (and any others found to be like them). Hugh and I do not mean to suggest this is a model that should be applied to all CMEs.

We are not suggesting a universal model!

3. Summary

What have we learned as a result of this exercise?

(1) Hugh Hudson embraces one aspect of the Solar Flare Myth—weak flares may accompany strong CMEs.
(2) Aulanier's model proposes to reconcile the differing viewpoints in the Martin–Rust filament controversy while Martin's observational campaign promises a definitive test.
(3) Jim Chen's "conveyor belt" model of repetitive eruptions envisions flux ropes traveling intact from the base of the convection zone to 1 A.U.
(4) "Six phenomenona in search of a paradigm" best describes the current state of understanding of the relationship of CMEs to their various surface and low coronal manifestations.
(5) Ed Cliver caves in (with caveats) on the relationhip between CMEs and metric type II bursts.
(6) "S" (sort of) marks the spot.
(7) Wide-ranging criticism of the Khan and Hudson paper on transequatorial loops as sources of certain CMEs suggests they are on to something.

4. Conclusion

We have seen the best minds of our generation driven mad by writing review papers. Even with less at stake, we did not want to take the chance and so hit on this expedient of asking our friends and colleagues to do the heavy lifting. Rather than constructing a collage by committee, however, we opted for this Q& A format that highlights controversies, illuminates inconsistencies, and (we hope) clarifies. The participants have responded admirably and we thank them for joining us in this exercise in solar poetry. *Doumo arigatou gozaimashita*, *muito obrigado*, many thanks, etc.

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References

Amari, T., Luciani, J.F., Mikić, Z., Linker, J., 2000. A twisted flux rope model for coronal mass ejections and two-ribbon flares. The Astrophysical Journal (Letters) 529, L49.

Antiochos, S.K., 1998. The magnetic topology of solar eruptions. The Astrophysical Journal (Letters) 502, (L181).

Anzer, U., Pneuman, G.W., 1982. Magnetic reconnection and coronal transients. Solar Physics 79, 129.

Aulanier, G., DeLuca, E.E., Antiochos, S.K., McMullen, R.A., Golub, L., 2000. The topology and evolution of the Bastille Day flare. The Astrophysical Journal 540, 1126.

Aulanier, G., Démoulin, P., 1998. 3-D magnetic configurations supporting prominences I. The natural presence of lateral feet. Astronomy and Astrophysics 329, 1125.

Aulanier, G., Démoulin, P., Mein, N., van Driel-Gesztelyi, L., Mein, P., Schmieder, B., 1999. 3-D magnetic configurations supporting prominences III. Evolution of fine structures observed in a filament channel. Astronomy and Astrophysics 342, 867.

Aulanier, G., Démoulin, P., van Driel-Gesztelyi, L., Mein, P., DeForest, C., 1998. 3-D magnetic configurations supporting prominences II. The lateral feet as a perturbation of a twisted flux-tube. Astronomy and Astrophysics 335, 309.

Aulanier, G., Srivastava, N., Martin, S.F., 2000. Model predictions for an observed filament. The Astrophysical Journal 543, 447.

Birn, J., Goldstein, H., Schindler, K., 1978. A theory of the onset of solar eruptive processes. Solar Physics 57, 81.

Blackman, E.G., Field, G.B., 2000. Coronal activity from dynamos in astrophysical rotators. Monthly Notices of the Royal Astronomical Society 318, 724.

Bruzek, A., 1964. On the association between loop prominences and flares. The Astrophysical Journal 140, 746.

Burlaga, L.F., Sittler, E., Mariani, F., Schwenn, R., 1981. Magnetic loop behind an interplanetary shock: Voyager, Helios, and IMP 8 observations. Journal of Geophysical Research 86, 6673.

Canfield, R.C., Hudson, H.S., McKenzie, D.E., 1999. Sigmoidal morphology and eruptive solar activity. Geophysical Research Letters 26, 627.

Canfield, R.C., Hudson, H.S., Pevtsov, A.A., 2000. Sigmoids as precursors of solar eruptions. Proceedings IEEE Transactions on Plasma Science 28, 1786.

Canfield, R.C., Pevtsov, A.A., McClymont, A.N., 1996. Magnetic chirality and coronal reconnection. Magnetic Reconnection in the Solar Atmosphere, Astronomical Society of the Pacific Conference Proceedings, Vol. 111, Bentley, R.D., Mariska, J.T. (Eds.), San Francisco, CA, pp. 341.

Carrington, R.C., 1860. Description of a singular appearance seen in the Sun on September 1, 1859. Monthly Notices of the Royal Astronomical Society 20, 13.

Chapman, S., 1950. Corpuscular influences upon the upper atmosphere. Journal of Geophysical Research 55, 361.

Chen, J., 1989. Effects of toroidal forces in current loops embedded in a background plasma. The Astrophysical Journal 338, 453.

Chen, J., 1996. Theory of prominence eruption and propagation: interplanetary consequences. Journal of Geophysical Research 101, 27 499.

Chen, J., 2001. Physics of coronal mass ejections: a new paradigm of solar eruptions. Space Science Reviews 95, 165.

Chen, J., Garren, D.A., 1993. Interplanetary magnetic clouds: Topology and driving mechanisms. Geophysical Research Letters 20, 2319.

Cliver, E.W., 1989. Was the eclipse comet of 1893 a disconnected coronal mass ejection? Solar Physics 122, 319.

Cliver, E.W., 1999. Comment on “Origin of coronal and interplanetary shocks: A new look with Wind spacecraft data” by N. Gopalswamy et al., Journal of Geophysical Research 104, 4629.

Cliver, E.W., Webb, D.F., Howard, R.A., 1999. On the solar origin of metric type II bursts. Solar Physics 187, 89.

Coppi, B., Laval, G., Pellat, R., 1966. Dynamics of the geomagnetic tail. Physical Review Letters 16, 1207.

Crooker, N., Joselyn, J.A., Feynman, J. (editors), 1997. Coronal Mass Ejections. Geophysical Monograph 99, American Geophysical Union, Washington, DC.

Delannée, C., 2000. Another view of the EIT wave phenomenon. The Astrophysical Journal 545, 512.

Delannée, C., Amari, T., 2000. Are the EIT waves really waves? Paper #06.03 presented at Solar Physics Division Meeting 32 of the American Astronomical Society.

Delannée, C., Aulanier, G., 1999. CME associated with trans-equatorial loops and a bald patch flare. Solar Physics 190, 107.

Dere, K.P., Brueckner, G.E., Howard, R.A., Michels, D.J., Delaboudiniére, J.P., 1999. LASCO and EIT observations of helical structure in coronal mass ejections. The Astrophysical Journal 516, 465.

DeVore, C.R., 2000. Magnetic helicity generation by solar differential rotation. The Astrophysical Journal 539, 944.

Forbes, T.G., 2000a. Solar and stellar flares. Philosophical Transactions of the Royal Society of London 358, 711.

Forbes, T.G., 2000b. A review on the genesis of coronal mass ejections. Journal of Geophysical Research 105, 23 153.

Furth, H.P., Killeen, J., Rosenbluth, M.N., 1963. Finite-resistivity instabilities of a sheet pinch. Physics of Fluids 6, 459.

Glanz, J., 1995. Does magnetic twist crank up the Sun’s outbursts? Science 269, 1517.

Glover, A., Ranns, N.D.R., Harra, L.K., Culhane, J.L., 2000. The onset and association of CMEs with sigmoidal active regions. Geophysical Research Letters 27, 2161.

Gold, T., 1962. Magnetic storms. Space Science Reviews 1, 100.

Goldsmith, D.W., 1971. Thermal effects in the formation of loop prominences. Solar Physics 19, 86.

Gopalswamy, N., 2000. Type II Solar Radio Bursts. Radio Astronomy at Long Wavelengths. In: Stone, R.G., Weiler, K.W., Goldstein, M.L., Bougeret, J.-L. (Eds.), Geophysical Monograph, Vol. 119. American Geophysical Union, Washington DC, pp. 123.

Gopalswamy, N., Kaiser, M.L., Lepping, R.P., Kahler, S.W., Ogilvie, K., Berdichevsky, D., Kondo, T., Isobe, T., Akioka, M., 1998. Origin of coronal and interplanetary shocks: A new look with spacecraft data. Journal of Geophysical Research 103, 307.

Gopalswamy, N., Kaiser, M.L., MacDowall, R.J., Reiner, M.J., Thompson, B.J., St. Cyr, O.C., 1999a. Dynamical phenomena associated with a coronal mass ejection. In: Habbal, S.R., et al. (Eds.), Solar Wind 9, AIP Conference Proceedings, Vol. 471, American Institute of Physics, Woodbury, NY, pp. 641.

Gopalswamy, N., Nitta, N., Manoharan, P.K., Raoult, A., Pick, M., 1999b. X-ray and radio manifestations of a solar eruptive event. Astronomy and Astrophysics 347, 684.

Gopalswamy, N., Kaiser, M.L., Thompson, B.J., Burlaga, L.F., Szabo, A., Lara, A., Vourlidas, A., Yashiro, S., Bougeret, J.-L., 2000. Radio-rich solar eruptive events. Geophysical Research Letters 27, 1427.

Gopalswamy, N., Kundu, M.R., Manoharan, P.K., Raoult, A., Nitta, N., Zarka, P., 1997. X-ray and radio studies of a coronal eruption: shock wave, plasmoid, and coronal mass ejection. The Astrophysical Journal 486, 1036.

Gopalswamy, N., Lara, A., Kaiser, M.L., Bougeret, J.-L., 2001. Near-sun and near-earth manifestations of solar eruptive events. Journal of Geophysical Research 106, 25 261.

Gosling, J.T., 1975. Large-scale inhomogeneities in the solar wind of solar origin. Reviews of Geophysics 13, 1053.

Gosling, J.T., 1993. The solar flare myth. Journal of Geophysical Research 98, 18 937.

Gosling, J.T., Hildner, E., MacQueen, R.M., Munro, R.H., Poland, R.I., Ross, C.L., 1974. Mass ejections from the Sun. Journal of Geophysical Research 79, 4581.

Green, L.M., López-Fuentes, M., Démoulin, P., van Driel-Gesztelyi, Mandrini, C.H., 2001. Helicity evolution and CME activity of NOAA 8100 (November 1997–February 1998), in preparation.

Hale, G.E., 1927. The fields of force in the atmosphere of the Sun. Nature 119, 708.

Hale, G.E., 1931. The spectrohelioscope and its work Part III. Solar eruptions and their apparent terrestrial effects. The Astrophysical Journal 73, 379.

Harra-Murnion, L.K., Schmieder, B., van Driel-Gesztelyi, L., Sato, J., Plunkett, S.P., Rudawy, P., Rompolt, B., Akioka, M., Sakao, T., Ichimoto, K, 1998. Multi-Wavelength observations of post flare loops in two long duration solar flares. Astronomy and Astrophysics 337, 911.

Harrison, R.A., 1986. Solar coronal mass ejections and flares. Astronomy and Astrophysics 162, 283.

Hirayama, T., 1974. A model for solar prominences. Solar Physics 34, 323.

Hudson, H.S., Acton, L.W., Freeland, S.L., 1996. A long-duration solar flare with mass ejection and global consequences. The Astrophysical Journal 470, 629.

Hudson, H.S., Cliver, E.W., 2001. Observing CMEs without coronagraphs. Journal of Geophysical Research 106, 25 199.

Hundhausen, A.J., 1993. Sizes and locations of coronal mass ejections: SMM observations from 1980 and 1984–1989. Journal of Geophysical Research 98, 13 177.

Hundhausen, A., 1999. Coronal mass ejections. In: Strong, K., et al. (Eds.), The Many Faces of the Sun. Springer, New York, NY, pp. 143.

Illing, R.M., Hundhausen, A.J., 1983. Possible observation of a disconnected magnetic structure in a coronal transient. Journal of Geophysical Research 88, 10 210.

Ishii, T.T., Kurokawa, H., Takeuchi, T.T., 1998. Emergence of a twisted magnetic flux bundle as a source of strong flare activity. The Astrophysical Journal 499, 898.

Ishii, T.T., Kurokawa, H., Takeuchi, T.T., 2000. Emergence of twisted magnetic-flux bundles and flare activity in a large active region, NOAA 4201. Publications Astronomical Society of Japan 52, 337.

Joselyn, J.A., McIntosh, P.S., 1981. Disappearing solar filaments: A useful predictor of geomagnetic activity. Journal of Geophysical Research 86, 4555.

Kahler, S.W., 1991. Coronal mass ejections and streamers associated with the new cycle active regions at solar minimum. The Astrophysical Journal 378, 398.

Kahler, S.W., 1992. Solar flares and coronal mass ejections. Annual Reviews of Astronomy and Astrophysics 30, 113.

Kahler, S.W., Moore, R.L., Kane, S.R., Zirin, H., 1988. Filament eruptions and the impulsive phase of solar flares. The Astrophysical Journal 328, 824.

Karpen, J.T., Antiochos, S.K., Dahlburg, R.B., Spicer, D.S., 1993. The Kelvin–Helmholtz instability in photospheric flows: effects on coronal heating and structure. The Astrophysical Journal 403, 769.

Khan, J.I., Hudson, H.S., 2000. Homologous sudden disappearances of transequatorial interconnecting loops in the solar corona. Geophysical Research Letters 27, 1083.

Klein, K.-L., Khan, J.I., Vilmer, N., Delouis, J.-M., Aurass, H., 1999. X-ray and radio evidence on the origin of a coronal shock wave. Astronomy and Astrophysics 346, L53.

Kopp, R.A., Pnueman, G.W., 1976. Magnetic reconnection in the corona and the loop prominence phenomena. Solar Physics 50, 85.

Krall, J., Chen, J., Spicer, D.S., Zalesak, S.T., Cargill, P.J., 1998. Simulation of buoyant flux ropes in a magnetized solar atmosphere. The Astrophysical Journal 500, 992.

Leka, K.D., Canfield, R.C., McClymont, A.N., van Driel-Gesztelyi, L., 1996. Evidence for current-carrying emerging flux. The Astrophysical Journal 462, 547.

Lin, R.P., Hudson, H.S., 1976. Non-thermal processes in large solar flares. Solar Physics 50, 153.

Lites, B.W., Low, B.C., Martinez Pillet, V., Seagraves, P., Skumanich, A., Frank, Z.A., Shine, R.A., Tsuneta, S., 1995. The possible ascent of a closed magnetic system through the photosphere. The Astrophysical Journal 446, 877.

Low, B.C., 1996. Solar activity and the corona. Solar Physics 167, 217.

Low, B.C., 1999. Coronal mass ejections, flares, and prominences. In: Habbal, S.R., et al. (Eds.), Solar Wind Nine, AIP Conference Proceedings, Vol. 471. AIP, Woodbury, NY, pp. 109.

Low, B.C., 2001. Coronal mass ejections, magnetic flux ropes, and solar magnetism. Journal of Geophysical Research 106, 25 141.

Low, B.C., Hundhausen, A.J., 1995. Magnetostatic structures of the solar corona, II. The magnetic topology of quiescent prominences. The Astrophysical Journal 443, 818.

MacCombie, W.J., Rust, D.M., 1979. Physical parameters in long-decay coronal enhancements. Solar Physics 61, 69.

MacQueen, R.M., Fisher, R.R., 1983. The kinematics of solar inner coronal transients. Solar Physics 89, 89.

Martin, S.F., 1998a. Filament chirality: a link between fine-scale and global patterns. In: Webb, D., Rust, D., Schmieder, B. (Eds.), New Perspectives on Solar Prominences. Astronomical Society of the Pacific, San Francisco, CA, pp. 419.

Martin, S.F., 1998b. Conditions for the formation and maintenance of filaments. Solar Physics 182, 107.

Martin, S.F., Bilimoria, R., Tracadas, P.W., 1994. Magnetic field configurations basic to filament channels and filaments. In: Rutten, R.J., Schrijver, C.J. (Eds.), Solar Surface Magnetism. Kluwer Academic Publishers, Dordrecht, pp. 303.

Martin, S.F., Echols, C., 1994. An observational and conceptual model of the magnetic field of a filament. In: Rutten, R., Schrijver, C.J. (Eds.), Solar Surface Magnetism. Kluwer Academic Publishers, Dordrecht, Holland, pp. 339.

Martin, S.F., McAllister, A.H., 1997. Predicting the sign of magnetic helicity in erupting filaments and coronal mass ejections. In: Crooker, N., et al. (Eds.), Coronal Mass Ejections. Geophysical Monograph 99. American Geophysical Union, Washington, DC, pp. 127.

McClymont, A.N., Fisher, G.H., 1989. On the mechanical energy available to drive solar flares. In: Waite Jr., J.H., Burch, J.L., Moore, R.L (Eds.), Solar System Plasma Physics, Geophysics Monograph 54. American Geophysical Union, Washington, DC, pp. 219.

McComas, D.J., Phillips, J.L., Hundhausen, A.J., Burkepile, J.T., 1991. Observations of disconnection of open coronal magnetic structures. Geophysical Research Letters 18, 73.

Mikić, Z., Linker, J.A., 1994. Disruption of coronal magnetic field arcades. The Astrophysical Journal 430, 898.

Moore, R.L. et al., 1980. The thermal X-ray flare plasma. In: P.A. Sturrock, Solar Flares. Colorado Associated University Press, Boulder, CO, pp. 341.

Nitta, N., Akiyama, S., 1999. Relation between flare-associated X-ray ejections and coronal ejections. The Astrophysical Journal 525, 57.

Parker, E.N., 1961. Sudden expansion of the solar corona following a large solar flare and the attendant magnetic field and cosmic ray effects. The Astrophysical Journal 133, 1014.

Parker, E.N., 2000. The physics of the sun and the gateway to the stars. Physics Today 53 (6), 26.

Pevtsov, A.A., 2000. Transequatorial loops in the solar corona. The Astrophysical Journal 531, 553.

Pevtsov, A.A., Canfield, R.C., 1999. Helicity of the photospheric magnetic field. In: Brown, M.R., Canfield, R.C., Pevtsov, A.A. (Eds.), Magnetic Helicity in Space and Laboratory Plasmas, Geophysics Monograph, Vol. 111. American Geophysical Union, Washington, DC, pp. 103.

Pevtsov, A.A., Canfield, R.C., 2000. Coronal structures as tracers of sub-surface processes. Journal of Astrophysics and Astronomy 21, 185.

Pevtsov, A.A., Canfield, R.C., 2001. Solar magnetic fields and geomagnetic events. Journal of Geophysical Research, Special Section on Solar Eruptive Events, in press.

Pevtsov, A.A., Canfield, R.C., Metcalf, T.R., 1995. Latitudinal variation of helicity of photospheric magnetic fields. The Astrophysical Journal (Letters) 440, L109.

Pevtsov, A.A., Canfield, R.C., McClymont, A.N., 1997. On the subphotospheric origin of coronal electric currents. The Astrophysical Journal 481, 973.

Pevtsov, A.A., Canfield, R.C., Zirin, H., 1996. Reconnection and helicity in a solar flare. The Astrophysical Journal 473, 533.

Piddington, J.H., 1979. Solar flares: Models and predictions of the flux-rope theory. The Astrophysical Journal 233, 727.

Plunkett, S.P., et al., 2000. Simultaneous SOHO and ground-based observations of a large eruptive prominence and coronal mass ejection. Solar Physics 194, 371.

Plunkett, S.P., Thompson, B.J., St. Cyr, O.C., Howard, R.A., 2001. Solar source regions of coronal mass ejections and their geomagnetic effects. Journal of Atmospheric and Solar-Terrestrial Physics 63, 389.

Pneuman, G.W., 1983. The formation of solar prominences by magnetic reconnection and condensation. Solar Physics 88, 219.

Pohjolainen, S., Maia, D., Pick, M., Vilmer, N., Khan, J., Otruba, W., Warmuth, A., Benz, A., Alissandrakis, C., Thompson, B.J., 2001. On-the-disk development of the halo coronal mass ejection on May 2, 1998. The Astrophysical Journal 5561, 421.

Pudovkin, M.I., Zaitseva, S.A., Oleferenko, I.P., Chertkov, A.D., 1977. The structure of the solar flare stream magnetic field. Solar Physics 54, 155.

Ramaty, R., Mandzhavidze, N., Kozlovsky, B., Murphy, R.J., 1995. Solar atmospheric abundances and energy content in flare accelerated ions from gamma-ray spectroscopy. The Astrophysical Journal (Letters) 455, L193.

Reiner, M.J., Kaiser, M.L., 1999. High frequency radio emission associated with shocks driven by coronal mass ejections. Journal of Geophysical Research 104, 16 979.

Richardson, R.S., 1941. The nature of solar hydrogen vortices. The Astrophysical Journal 93, 24.

Rust, D.M., et al., 1980. Mass ejections. In: Sturrock, P.A. (Ed.), Solar Flares. Colorado Associated University Press, Boulder, CO, pp. 273.

Rust, D.M., 1983. Coronal disturbances and their terrestrial consequences. Space Science Reviews 34, 21.

Rust, D.M., 1994. Spawning and shedding helical magnetic fields in the solar atmosphere. Geophysical Research Letters 21, 241.

Rust, D.M., Kumar, A., 1994. Helical magnetic fields in filaments. Solar Physics 155, 69.

Rust, D.M., Kumar, A., 1996. Evidence for helically kinked magnetic flux ropes in solar eruptions. The Astrophysical Journal (Letters) 464, L199.

Seehafer, N., 1990. Electric current helicity in the solar atmosphere. Solar Physics 125, 219.

Sheeley Jr., N.R., 1999. Using LASCO observations to infer solar wind speed near the Sun. In: Habbal, S.R., et al. (Eds.), Solar Wind 9, AIP Conference Proceedings, Vol. 471. American Institute of Physics, Woodbury, NY, pp. 41.

Sheeley Jr., N.R., et al., 1997. Measurements of flow speeds in the corona between 2 and 30 R_O. The Astrophysical Journal 484, 472.

Sheeley Jr., N.R., Walters, J.H., Wang, Y.-M., Howard, R.A., 1999. Continuous tracking of solar outflows: Two kinds of coronal mass ejections. Journal of Geophysical Research 104, 24 739.

Shibata, K., 1998. Evidence of magnetic reconnection in solar flares and a unified model of flares. Astrophysics and Space Science 264, 129.

Shibata, K., Masuda, S., Shimojo, M., Hara, H., Yokoyama, T., Tsuneta, S., Kosugi, T., Ogawara, Y., 1995. Hot-plasma ejections associated with compact-loop flares. The Astrophysical Journal (Letters) 451, L83.

Smith, S.F., Harvey, K.L., 1971. Observational effects of flare associated waves. In: Macris, C., Reidel, D. (Eds.), Physics of the Solar Corona. Dordrecht, Holland, pp. 156.

Smith, S.F., Ramsey, H., 1964. The flare-associated filament disappearances. Zeitschrift für Astrophysik 60, 1.

St. Cyr, O.C., et al., 2000. Properties of coronal mass ejections: SOHO LASCO observations from January 1996 to June 1998. Journal of Geophysical Research 105, 18 169.

Sterling, A.C., Hudson, H.S., 1997. Yohkoh SXT observations of X-ray “dimming” associated with a halo coronal mass ejection. The Astrophysical Journal (Letters) 491, L55.

Sturrock, P.A., 1980. Flare models. In: Sturrock, P.A. (Eds.), Solar Flares. Colorado Associated University Press, Boulder, CO, pp. 411.

Švestka, Z., Cliver, E.W., 1992. History and basic characteristics of eruptive flares. In: Švestka, Z., et al. (Eds.), Eruptive Solar Flares. Springer, New York, pp. 1.

Tanaka, K., 1991. Studies of a very flare-active delta group-peculiar delta spot evolution and inferred subsurface magnetic rope structure. Solar Physics 136, 133.

Thompson, B.J., Plunkett, S.P., Gurman, J.B., Newmark, J.S., St. Cyr, O.C., Michels, D.J., 1998. SOHO/EIT observations of an Earth-directed coronal mass ejection on May 12, 1997. Geophysical Research Letters 25, 2465.

Thompson, B.J., Cliver, E.W., Nitta, N., Delannée, C., Delaboudiniére, J.-P., 2000a. Coronal dimmings and energetic CMEs in April and May 1998. Geophysical Research Letters 27, 1431.

Thompson, B.J., et al., 2000b. Observations of the 24 September 1997 coronal flare waves. Solar Physics 193, 161.

Uchida, Y., 1968. Propagation of hydromagnetic disturbances in the solar corona and Moreton’s wave phenomenon. Solar Physics 4, 30.

van Ballegooijen, A.A., Martens, P.C.H., 1989. Formation and eruption of solar prominences. The Astrophysical Journal 343, 971.

van Driel-Gesztelyi, Wiik, J.E., Schmieder, B., Tarbell, T., Kitai, R., Funakoshi, Y., Anwar, B., 1997. Post-flare loops of 26 June 1992 IV. Formation and expansion of hot and cool loops. Solar Physics 174, 151.

van Driel-Gesztelyi, L., Mandrini, C.H., Thompson, B., Plunkett, S., Aulanier, G., Démoulin, P., Schmieder, B., DeForest, C., 1998. In: Schmieder, B., Hofmann, A., Staude, J. (Eds.), Proceedings of the 3rd ASPE: Magnetic Fields and Oscillations, ASP Conference Series, 184, pp. 302.

van Driel-Gesztelyi, L., Thompson B., Démoulin, P., Orlando, S., Bocchialini, K., Olàh K., K Hovàri, Z., DeForest, C., Khan, J., Fludra, A., Mandrini, C., 1999. Proceedings of the SOHO-8 Workshop Plasma Dynamics and Diagnostics in the Solar Transition Region and Corona, Paris, France, 22–25 June 1999, ESA SP-446, October 1999, pp. 663.

Wang, Y.-M., 2000. EIT waves and fast-mode propagation in the solar corona. The Astrophysical Journal (Letters) 543, L89.

Webb, D.F., 1995. Coronal mass ejections: The key to major interplanetary and geomagnetic disturbances. Reviews of Geophysics (Suppl.) 33, 577.

Webb, D.F., 2000. Understanding CMEs and their source regions. Journal of Atmospheric and Solar-Terrestrial Physics 62, 1415.

Webb, D.F., Cliver, E.W., 1995. Evidence for magnetic disconnection of mass ejections in the corona. Journal of Geophysical Research 100, 5853.

Webb, D.F., Cliver, E.W., Gopalswamy, N., Hudson, H.S., St. Cyr, O.C., 1998. The solar origin of the January 1997 coronal mass ejection, magnetic cloud and geomagnetic storm. Geophysical Research Letters 25, 2469.

Webb, D.F., Cliver, E.W., Crooker, N.U., St. Cyr, O.C., Thompson, B.J., 2000a. Relationship of halo coronal mass ejections, magnetic clouds, and magnetic storms. Journal of Geophysical Research 105, 7491.

Webb, D.F., et al., 2000b. The origin and development of the May 1997 magnetic cloud. Journal of Geophysical Research 105, 27 251.

Wheatland, M.S., 2000. Are electric currents in solar active regions neutralized? The Astrophysical Journal 532, 616.

Xue, M.L., Chen, J., 1983. MHD equilibrium and stability properties of a bipolar current loops. Solar Physics 84, 119.

Zhang, J., Dere, K.P., Howard, R.A., Kundu, M.R., White, S.M., 2000. On the temporal relationship between coronal mass ejections and flares. The Astrophysical Journal 559, 452.

Zhang, H., 2001, Formation of current helicity and emerging magnetic flux in solar active regions. Monthly Notices of the Royal Astronomical Society 326, 57.

Zirker, J, Engvold, O., Martin, S.F., 1998. Counterstreaming gas flows in solar prominences as evidence for vertical magnetic fields. Nature 396, 440.