DISCUSSIONS

ESTER ANTONUCCI / Solar Flare Spectral Diagnosis: Present and Future: 31–60.

SPEAKER: ANTONUCCI

CHENG: Have you compared Ca XIX and Fe XXV Spectra from limb flare with those from disk flare?

ANTONUCCI: During the impulsive phase, limb flares show the same degree of non-thermal broadening in soft X-ray lines as disk flares, and do not show blue-shifted emission. That is, they are characterized by symmetric line profiles.

NITTA: Do you have to assume the "low-energy cutoff" of hard X-ray emitting electrons when you compare the evaporation velocity with the energy input? The value of the low-energy cutoff has hardly been known. Is it possible to obtain the temperature of hot components reliably with Fe XXV lines alone, e.g., by using the differential emission measure technique?

ANTONUCCI: Temperature measurements improve, in principle, by using spectral emission from ions with contribution fuctions in different ranges of temperature. Differential emission measure techniques, however, depend significantly on the sensitivity of the different channels and the abundances of the different elements. For what concerns the "hot" thermal component in flares, its temperature is best measured by combining Fe XXV and Fe XXVI spectra, as shown by the work of Tanaka and co-workers.

SMITH: The evaporated material should drive shock waves. For Ca XIX from the calculations of Fisher (1987, Ap.J.) we know that the shock should have a velocity $\approx$ 3.1 times that of the moving material. Now we have much higher velocities $\approx$ 1000 km s^{-1} in Fe XXV. Have you had time to figure out what velocity of shocks these velocities would imply?

ANTONUCCI: The evaporation velocities derived from the Fe XXV spectra can be expressed in units of a limiting velocity (which has been assumed equal 2.35 times the sound velocity in the evaporating plasma); maximum values in the velocity distributions reach up to 0.6 times the limiting velocity and are in the range from 0.3 to 0.6.

PALLAVICINI: If I understand correctly, the velocity of the upflowing plasma depends both on the energy input and on the density of the preflare loop. So, when you conclude that the velocities derived from Fe XXV are more consistent with the thick-target model than with the thermal model, have you compared models for the same preflare conditions in both cases?

ANTONUCCI: The comparison of observational velocities with those resulting from numerical simulations, which has been used to conclude that evaporation velocities are more consistent with thick-target models, has been made without discriminating for initial conditions. However, most of the simulations performed up to now consider initial densities of the order of 10^9 cm^{-3}.

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P. B. BYRNE / Multi-Wavelength Observations of Stellar Flares: 61–74.

SPEAKER: BYRNE

BUTLER: I am no longer convinced that there was no X-ray counterpart to the flare observed in the U-band on YZ CMi. In fact an enhancement in the LE soft X-ray band was recorded by EXOSAT which peaked at 20h 06m UT; that is eleven minutes after the U-band peak. This, I think, was the corresponding X-ray event. The very short, impulsive, nature of the U-band flare made it appear that it was not connected to the later X-ray enhancement, but, if it had a more prolonged decay, I think there is no doubt that we would have associated the two enhancements as a single event.

BYRNE: Yes, this is so. A subsequent analysis of the ME data on this flare by R. Mewe (private communication) indicates that there is a weak flare close to the time of the U-band flare. A delayed soft (LE) X-ray flare 600-700 s after the U-band rise is not unreasonable as 1000 s delay between ME and LE was seen in the EQ Peg flare of Haisch et al. (1987, Astr. Ap. 181, 96). My main point in drawing attention to the lack of a strong LE flare was to establish the differences with the very similar optical flare on YZ CMi observed by Karpen et al. (1977, Ap.J. 216, 479) which was accompanied by a strong X-ray flare.

ACTON: It is interesting that both solar and stellar X-ray flares have electron densities in the $10^{11}$to few times 10^{12} range. I suggest that this represents the density at which flare energy release is quenched.

BYRNE: Yes, I agree. In spite of the enormous range in energies from small solar, through dMe, to RS CVn star flares, the densities do not vary very much. I feel that this must be significant.

ACTON: Can you think of a solar analogue to the event discussed by Doyle of a strong optical "flare" without detectable X-ray emission?

BYRNE: It is difficult to make a direct comparison between optical stellar flares and solar flares. The latter do not have a strong broad band optical signature, so it is difficult to know what to compare. A second reservation is that we should bear in mind the limited sensitivity of the present generation of X-ray detectors. So it is possible that a soft X-ray flare occurred which was undetectable. I would point out, however, that the Kahler et al. (1977) flare on YZ CMi was very similar in optical energy and had a strong soft X-ray signature which we would have detected easily.

FALCIANI: The red asymmetric emission in Balmer lines can be explained with a Stark broadened profile, symmetric, roughly centered at the previous ($V_{/\!/} = 0$) line center, originating in the deepest, denser chromospheric layers, superimposed on a blueshifted absorption, due to the cold material going up into the loop (evaporation, surge material). In some solar flares the velocity corresponding to the blue Doppler-shifted absorption fits very well the ascending velocity directly measured in the surge material that becomes visible later on. Do you think that this explanation might be applied to stellar flares, too?

BYRNE: Yes, it is possible to apply such a model but many others are also possible. It should be born in mind that the quality of the data are not sufficient to distinguish between the various possible models. Blueward absorption was recorded in the Mg II profiles of the II Peg 1983 February 2 flare (Doyle et al., 1989). I would caution that Stark broadened profiles do not fit the profiles of the Balmer lines of the YZ CMi 1985 March 5 flare (Doyle et al., 1988) at any phase of the flare.

PALLAVICINI: Just a short comment. Your statement that the EINSTEIN Observatory observed less energetic flare than EXOSAT is largely due to the fact that you have picked up the strongest ones among the flares observed by EXOSAT. Actually, in the complete EXOSAT data sample, there are X-ray flares which are weaker than those observed by EINSTEIN. And this simply reflects the fact that the energies of soft X-ray flares span more than 4 orders of magnitude and there are much less strong flares than small flares.

BYRNE: Yes, this is true. EINSTEIN, however, did not observe any flares as energetic as those observed by EXOSAT. There are systematic effects in the energy of flares as a function of their spectral type. So it may be that EINSTEIN spent more time on those stars which have less energetic flares.

MULLAN: dMe radio emission is often coherent whereas optical and X-rays are not. Therefore one does not expect to see a correlation in all cases between radio flares and flares in other spectral range.

BYRNE: Yes, this is true. But if we invoke a model which heats the lower atmosphere with a beam of highly relativistic electrons constrained by a magnetic field, it is difficult the see how these will not radiate in microwaves.

MULLAN: Are flares in RS CVn flares really an exact analogue of dMe flares? In the latter stars, radio emission is always coherent, whereas in RS CVn stars this is not (or hardly ever) true. Some of the energy release is RS CVn flares may be due to reconnection between the fields of one star and the other.

BYRNE: Again I agree with you that there are important differences between RS CVn and dMe flares. Not least of these differences are those of energy and time scale. Nevertheless, I have tried to show that there is evidence that the density, emission measure and, therefore, the flaring volumes are similar to dMe's. It is the duration of RS CVn flares which results in their having much greater energies. This would imply a very longlived heating episode, lasting at least 7-12 hours and maybe longer.

RODONO': In comparing the relative timing and behaviour of radio and X-ray fluxes for stellar flares we must bear in mind that flare time evolutions are different in the different wavebands. For example, we may safely say that the impulsive 2-cm radio emission follows quite closely the hard X-ray enhancement, while in the 6-cm and soft X-ray bands the flare has a slower time evolution. Recent microwave observations of stellar flares with VLA have also shown that the flare peaks at 2-cm before that at 6-cm.

BRIAN R. DENNIS and RICHARD A. SCHWARTZ / Solar Flares: the Impulsive Phase: 75–94.

SPEAKER: DENNIS

CHENG: *Is an impulsive HXR burst necessary for solar flares?* My question is based on the previous OSO observations which show that 1/3 of all the flares OSO observed have no impulsive HXR bursts. Is this a sensitivity problem? A reverse question is: Are there impulsive HXR bursts observed from SMM with no associated soft X-ray bursts?

DENNIS: We have never seen a hard X-ray burst with no corresponding soft X-ray flare. There is a linear relation with about one order of magnitude scatter between the total number of counts seen in the hard X-ray burst spectrometer (HXRBS) on SMM integrated over the duration of the burst and the rate seen in the Ca XIX channel of the Bent Crystal Spectrometer also on SMM. Thus, for the large soft X-ray events >100 count/s in BCS, we always see a hard X-ray event significantly above the HXRBS background rate. Thus, I suspect that the absence of a hard X-ray event for some of the smaller soft X-ray events, particularly the events with the largest rise times, is a threshold effect connected to the HXRB sensitivity.

MULLAN: Is there a lower limit on the time-scale on which Hα varies in a solar flare impulsive phase (by analogy with stellar flares)?

DENNIS: The Hα observations with the best time resolution that have so far been reported are <1 s and significant fluctuations are seen during impulsive phases on these time scales. Thus, there is no evidence for a lower limit down to these time scales. These rapid fluctuations occur in individual kernels, however, and contribute very little to the integrated flux from the whole flare. Thus it might be said that the time profile of the Hα emission integrated over the full area of the flare has a rise time that is generally longer than 1 minute, similar to that for the soft X-ray emission.

RODONO': When simultaneous flare data in hard X-rays and UV transition region lines or continuum are available, do they always vary simultaneously (± 0.1 s), or are there exceptions?

DENNIS: In the few examples that we have, the UV transition zone lines and the UV continuum always vary nearly simultaneously with the hard X-rays (see Orwing and Wordgate 1986, and Cheng et al., 1988).

BAI: You showed a number of flares for which soft X-ray time profiles are similar to the time integral of hard X-ray time profiles. But this does not mean that all the energy of the soft X-ray emitting plasma comes from high-energy electrons. Direct heating is likely to have the same profile as the acceleration of high-energy electrons.

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DENNIS: I agree. The uncertainty on the time integrated energy in the electrons that precipitate into the thick target is likely to be an order of magnitude because of our lack of information about the lower energy cutoff in the assumed power-law electron spectrum.

VIAL: Do you have many examples of the time lag of microwave radiation?

DENNIS: Yes, we have many examples of the time lag of the microwave radiation. A 2 second delay is about the longest that we see for an event that we would consider to be impulsive. Much longer delays up to ≈ 1 minute are seen for the more gradually varying, type C, flares.

GOPALSWAMY: The figure with footpoint HXR emission (Hinotori) shows that both footpoints have avoided the Hα ribbons. Why is this discrepancy in a disk event like this? Is this due to a projection effect?

DENNIS: I believe that the hard X-ray bright particles could be spatially coincident with the Hα ribbons to within the uncertainty in the relative alignment between the two images.

BHATNAGAR: In case of Hα kernels in a flare, all of them do not brighten simultaneously. Individual kernels are observed to brighten with delay times of several seconds to minutes. Hence, time correspondence with microwave and X-ray emission has to be taken with caution because one may consider good correspondence depending on which individual kernel is taken.

DENNIS: This is a good point and indicates the need for microwave and hard X-ray imaging with good time resolution. Nevertheless, Hα images taken with subsecond time resolution, as one now becoming available, can be usefully matched to hard X-ray and microwave time profiles with no spectral information. This is particularly true for the smaller, simpler flares, where the problem of multiple Hα kernels brightening at different times can be minimized.

MARTENS: I would like to argue that the case for the < 1 MeV proton beam model is stronger than you suggested: the proton beam model that Simnett, Henoux and myself propose (e.g. Ap. J. 330, L131) solves all these problems quite clearly. What is your comment on this?

DENNIS: The strengths and weaknesses of the < 1 MeV proton beam model will become evident with time as more work is carried out to explore the consequences of the model. While this model does suggest solutions to some problems with the electron beam model, there are many significant questions that must be answered such as, how are the hard X-rays and microwaves produced? Only when it has been subjected to the same scrutiny, as has the electron-beam model, we will be able to assess the strength of the case for the < 1 MeV beam model.

WOLFGANG DRÖGE, PETER MEYER, PAUL EVENSON, and DAN MOSES / Electron Acceleration in Solar Flares: 95–103.

SPEAKER: DROEGE

STURROCK: Do you have information on the ion abundances in energetic-ion events? How do the abundances compare for Class I and Class II events?

DROEGE: Class I events have a higher electron to proton ratio than Class II events. They also seem to be ^{3}He-rich.

SHAPIRO: Are proton data available for many of the flares you studied?

DROEGE: Proton spectra for 3 events included in our survey are published by McGuise and von Rosenvinge (1984, Adv. Space Res. 4, 117). In principle, more proton spectra from the IMP7/8 measurements should be available.

RIEGER: It is interesting to note, that for your short duration events the electron spectra get harder with increasing energy, in contrast to the proton spectra measured in space and inferred from the X-ray measurements, which have spectra that soften at high energies. For your long duration events the situation is the opposite. The question arises: Does stochastic acceleration create a harder electron spectrum than shock acceleration in contrast to the case for protons?

DROEGE: Diffusive shock acceleration without modifications produces power laws in momentum with the same spectral index for electrons and protons. In stochastic acceleration models, the spectral shape is determined by the rate at which particles gain energy from interacting with the plasma turbulence, and the rate at which particles escape from the acceleration region. Because electrons and protons might interact with different types of plasma curves, their spectra can be different, i.e. electron spectra can be harder at high energies than proton spectra.

A. GORDON EMSLIE / Models of Flaring Loops: 105–115

SPEAKER: EMSLIE

DOYLE: High time resolution (6 s) data on Ca XIX in the impulsive phase has been published for a few flares (Doyle & Bestlay). These show a complex picture, (although the signal-to-noise is poor) of several mass up-flows. We will have to wait for Solar A to obtain good data, as the sensitivity of the BCS is not sufficient for most C or M class flares to detect with high time resolution the mass up-flows.

EMSLIE: I believe that good signal-to-noise data, showing the changes in the Ca XIX spectra during the impulsive flare at, say, 3-5 s time resolution, should be extremely valuable in testing the type of models of which I spoke.

MOORE: Since your simple model works so well, what can you deduce about the electron acceleration mechanism?

EMSLIE: I can only say at this point that the data are consistent with the standard "black-box" picture in which the acceleration occurs near the loop of the flare loop. Questions regarding the acceleration mechanism are outside the scope of my talk, although it would be nice if someone could come up with a way of accelerating power-law electron spectra as required by the observations.

MACNEICE: I do not agree that excitation of EUV emission by a conduction front during the impulsive phase can be ruled out yet. Non-local heat flux may produce early pre-heating of the transition region ahead of the conduction front, preventing any initial dip in EUV emission, and reducing the lag time between hard X-ray and EUV emission. Also the temperature gradient in the transition region can be expected to be less severe. Finally even the numerical simulations using Spitzer-Hrm conductivity produce conflicting results regarding the initial EUV dip.

EMSLIE: Your comments are well taken. The conduction model used by Nagai and myself involved only local chemical conduction (with a replacement by a saturated heat flux where necessary), and it is possible that a more realistic treatement of thermal conduction, including global effects as you suggest, could produce a more satisfactory agreement with the observed behaviour of transition region lines during the impulsive phase. Notwithstanding this, however, the purpose of the comparison between conduction and electron heated models was to show that (i) the behaviour of EUV emission lines is not trivial to understand and (ii) agreement with observations can only be achieved for a spatially distributed form of energy inputs, such as collisional degradation of a nonthermal electron beam, or, perhaps, the global conduction physics to which you refer.

KOCHAROV: It seems to me that the observational data on solar neutrons and γ-rays are very informative. These data allow us to answer many questions concerning flare loops; the magnetic field and its gradient, the dimension of the arch, the number density and its gradient, so there is excellent possibility for solar plasma diagnostics in the acceleration and propagation region. New integrated experiments are needed. As far as theory is concerned, you have to consider together all the available data and try to explain them.

SERIO: How do your Ca XIX line profiles depend on the beam's low energy cutoff?

EMSLIE: For higher low-energy cutoffs, a larger portion of the beam energy resides in electrons which deposit their energy in deeper layers of the atmosphere. These layers can efficiently radiate away the deposited energy in UV and optical lines and continua, so that less energy is available to power the evaporation process that gives rise to the strong, blueshifted, Ca XIX profiles. Thus, for higher low-energy cutoff, the Ca XIX profiles are weaker.

NITTA: Isn't your argument that Ca IXX profiles depend on low-energy cutoff of electrons dependent upon the preflare atmospheric model you take?

EMSLIE: As long as the cutoff is such that the bulk of the electrons reach the preflare chromosphere (i.e., $E > 10$ KeV or so), then the results are insensitive to the preflare atmospheric model.

MARTENS: I have two questions regarding the relation between hard X-rays and micro-waves: 1) My impression from combined SMM-radio observations is that there is often a time delay of the micro-waves with regard to the hard X-rays: a time delay that is larger than the time resolution of the instruments; and, 2) Kundu showed yesterday an example that demonstrated that the number of electrons needed to generate the microwaves is much smaller than the number required for the hard X-rays. How do you reconcile these observations with a common electron population responsible for both types of radiation?

EMSLIE: Microwaves are radiated by much higher-energy electrons than these responsible for hard X-ray bremstrahlung. Because of the steep spectra involved, the number of microwave-producing electrons is much less than those producing hard X-rays. The observed time delay may have its origins in the trapping and precipitation of the electrons, noting that a given trap may be collisionally thin for 100 KeV + electrons, yet collisionally thick for ≈ 20 keV electrons.

BAI: I would like to answer Dr. Martens' question regarding the electron number problem and the delay of microwaves with respect to hard X-rays. Energetic electrons which precipitate to the chromosphere produce hard X-rays while loosing energy quickly. Energetic electrons which have large pitch angles are trapped in the flare loop and produce microwaves. The number of trapped

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electrons is smaller than that of precipitating electrons. In addition, trapped electrons have longer lifetimes; therefore, microwaves are delayed a few seconds with respect to hard X-rays.

SMITH: There is a recent paper in Ap.J. by Spicer and Emslie which attempts to help the electron number problem by using a magnetic trap with an electrostatic field. With this greater confinement, can you get enough dumping from the trap to explain the other phenomena observed, like the impulsive EUV? In other word, is there really any energetic advantage?

EMSLIE: I thank you for pointing out this paper to me! The aims of that paper were to produce observed hard X-ray bursts using fewer electrons than in a standard thick target model; no attempts at investigating the physics of the precipitating component were made. I suspect, however, that the fewer number of electrons could make a reconciliation with, e.g., Ca XIX spectra, difficult.

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BERNARD H. FOING / Stellar Flare Spectral Diagnotics: Present and Future: 117–133.

SPEAKER: FOING

LINSKY: Binarity in RS CVn systems can be responsible for the highly energetic phenomena observed because tidal forces induce rapid rotation and may change the differential rotation (with latitude) such that the dynamo generation of magnetic fields is enhanced greatly compared with slowly rotating single stars. Also, there is the possibility that magnetic fields of the two stars may interact and occasionally interconnect.

MACKINNON: I have to be pessimistic about the prospects for using radio observations, in the way hard X-rays have been used in the solar context, to develop a quantitative theory of energy transport in stellar flares. At least in dMe stars, radiation is coherent, and it is very difficult to say anything about total numbers, energy content, etc., of emitting particles. RS CVn may be better placed.

BYRNE: I would like to make a point concerning the scale of a stellar flare. In so far as we have information on the electron densities in stellar flares these would appear to be similar to solar values ($\approx 10^{11}$–10^{12} cm^{-3}). The emission measures in both mid-transition region lines and in soft X-ray are also not very different from those in large solar flares. The critical difference appears to be in the duration of the flares and, therefore, in their total energies.

RODONO': At first glance, it might appear quite discouraging to study stellar flares with time- spatial- and spectral-resolutions much worse than in the solar case. However, we must always bear in mind the general idea underlying solar and stellar studies: i.e., the understanding of the basic physical mechanisms that produce solar flares may help in interpreting the global characteristics of seemingly similar phenomena occurring in the rather different stellar environments. Of course, we may expect very different behaviour and, eventually, substantially different mechanisms that are triggered during the course of the most energetic events on stars. We have still to learn a lot, beginning with trying to understand, for instance, why the surface differential rotation rate on very active RS CVn stars seems to be one or two orders of magnitude smaller than solar. Certainly, the binary nature of RS CVn systems, specifically the tidal interaction between the two stars, has some effect in modifying the "natural" differential rotation regime of the individual stars. This, on the other hand, provides one of the necessary conditions (e.g., high rotation-rate) for the development of activity phenomena. Certainly, RS CVn stars appear to have found a way of preserving their ability of producing high-energetic activity phenomena, possibly by developping rapidly spinning cores or less dramatic regimes of radial differential rotation, which at present escape detection.

STERN: Referring to Brendan Byrne's comment, I believe that the largest flare volumes derived using soft X-ray observations are in fact orders of magnitude larger than solar flares. On the other hand, the volumes measured using UV line fluxes may be comparable to the solar case.

V. GAIZAUSKAS / Preflare Activity: 135–152.

SPEAKER: GAIZAUSKAS

KOCHAROV: What is the dependence of flare-precursor events versus time to flare?

GAIZAUSKAS: The microwave precursors to which I alluded occur within 10 to 20 minutes before flare onset.

KOCHAROV: Systematic studies of X-ray precursors were carried out from 1975 to 1979 in experiments on Prognoz 4, 5, 6, and 7 led by my laboratory. Owing to the high sensitivity of the equipment we succeeded in revealing for a large number of flares (> 1000), slight changes in the soft X-ray intensity (in ≈80%) of the cases, which were considered X-ray precursors. On the average, a precursor appears 10-20 min. before the flare, which coincides in time with the beginning of activation of the dark filaments frequently observed before the flare onset. The magnitude and brightness distribution of flares with precursor was found to be the same as for all flares detected over the same time interval.

GAIZAUSKAS: I look forward to seeing your detailed case studies of X-ray precursors associated with filament eruptions.

BORNMANN: What are the ages of the active region complexes when they start producing flares?

GAIZAUSKAS: Major flares are usually produced during the first one or two rotations after a complex is formed. Smaller flares are produced thereafter, unless a major eruption of new flux rejuvenates the complex.

MARTENS: What is your opinion on the question of triggering of filament eruptions? How often do triggers occur, and of what kind are they?

GAIZAUSKAS: I do not believe that we can isolate one phenomenon as an inevitable flare trigger. So many things are going on at once in an evolving activity complex that you can imagine anyone of them to push a filament, already stressed by evolutionary processes, into an unstable regime: emerging flux, cancelling flux, footpoint motions, remote flares, etc.

KUNDU: I have a few comments to make on your discussion of microwave precursors. With regard to microwave flares, the precursor region is the same as the flaring region, as seen for example at 6 cm with the VLA using ≈ 1 arcsec resolution. Several tens of minutes before the flare, the precursor appears as a heating of the region (up to ≈ 15 x 10^6 °K). This preheating by itself is not a necessary condition for triggering the flare onset. A few minutes (< 10 min) before the impulsive onset of flares, one of the following manifestations must take place: 1) change of polarization of the flaring active region; 2) change in orientation of the plane of zero polarization; 3) physical appearance of new regions/structures; and (4) in some cases we see oppositely polarized bipolar regions or quadrupole structures. All these are manifestations of some emerging of newly "visible" microwave structure interacting with pre-existing structure, which produce a current sheet strongly suggestive or magnetic field reconnection, which ultimately is responsible for triggering the impulsive onset of flares.

GAIZAUSKAS: I agree with your statement except for the insistence that specific microwave phenomena must take place. But there is no doubt that the magnetic field is somehow restructuring rapidly just before flare and that microwave signatures contain important information on that process.

ACTON: Please give us your comments, from the point of view of an observer, on Gene Parker's conceptual model of magnetic stress building up in the corona from the random wandering of magnetic field footpoints in the photosphere.

GAIZAUSKAS: I have no trouble with Parker's concept. You can easily imagine that the chaotic fine structure in the quiet Sun originates in random local reconnections. There is obviously more order in space and in time for the magnetic field within active regions or we would not see homologous flares or flare kernels appearing adjacent to, but on opposite sides of, polarity inversion lines. But within this quasi-ordered state, the assumption that random reconnection triggers flares does not conflict with the lack for consistent pattern of pre-flare phenomena.

M. R. KUNDU, S. M. WHITE, and E. J. SCHMAHL / Simultaneous Multi-Frequency Imaging Observations of Solar Microwave Bursts: 153–161.

SPEAKER: KUNDU

MULLAN: You observe not only at flare but also at quiescent times. Can you detect non-thermal electrons during quiescent times on the Sun or stars?

KUNDU: For the Sun, the active region producing a flare remains heated (sometimes up to 10×10^6 °K) until the next flare, if it occurs within a few hours. I'd like to interpret this heated region as due to gyroresonance radiation. If non-thermal electrons are involved in this region, they must show in the form of rapid fluctuations of mini-flares. So far, no one has shown conclusive evidence for this behaviour. As for the stellar case, the question is really that of quiescent emission - whether or not it is due an assemblage of many mini or microflares. We feel that *the quiescent emission consists of really quiet star* emission plus active region emission. Quiescent emission from UV Ceti, for example, varies between 1 mJy and 3 mJy. We feel that this emission is due to gyrosynchrotron radiations (producing many miniflares) and therefore there is evidence for nonthermal electrons.

JAN KUIJPERS / Radio Emission from Stellar Flares: 163–185.

SPEAKER: KUIJPERS

KRISHAN: I would like to inform you that I considered a "Free Electron Laser" mechanism for type III solar radio bursts eight years ago and more recently for the generation of nonthermal continuum of quasars. The question that is asked all the time is: " What are the observational signatures of a particular coherent radiation process? " Polarization is one characteristic but there are many ways of modifying it. Do you have any suggestions for the observational signature of free electron laser type mechanism?

KUIJPERS: The radiation process from double layers that I have in mind is not identical to the free electron laser but can probably be considered as a nonrelativistic extreme version. In view of the multitude of theoretically existing coherent radiation mechanisms, it is necessary to work out also the nonlinear development in time before it can be applied to the observations.

JEFFREY L. LINSKY / Solar and Stellar Magnetic Fields and Structures: Observations: 187–196.

SPEAKER: LINSKY

KLIMCHUK: I have both a comment and a question. First, concerning the hydrodynamic models of Mariska, in order to get velocities of reasonable magnitude, it was necessary to assume loop pressures that are quite low ($nT \approx 10^{14}$ °K cm^{-3}) compared to those observed in solar active regions. So, I feel that those results should be applied with some caution. My question is: What sorts of error bars do you get for the magnetic field measurements, both for the field strength and the filling factor?

LINSKY: I have called attention to some important systematic effects such as line blanketing, line saturation, and different thermal structures in the magnetic and nonmagnetic regions that can lead to systematic errors in the derived magnetic parameters. It is likely that systematic rather than random errors will dominate and the magnitude of the errors in the magnetic parameters probably depends more on the analysis technique than on the quality of the original data. I would estimate that the most careful analytic techniques now provide field strengths accurate to ± 30% and filling factors accurate to a factor of 2, although relative errors for data analyzed by the same techinque should be more reliable.

LIVI: Is there an explanation for the non uniform heating of magnetic loops in the Mariska model?

LINSKY: As I recall no explanation is provided in MARISKA's papers and asymmetric heating is strictly an ad hoc assumption that gives good answers. He places the heating in a 100 km wide region beginning 100 km above the base of the transition region in one footpoint of a loop. These numbers are arbitrary. One could speculate that the heating is localized to the footpoint in which there is a resonance between the local dissipation (controlled say by the particular geometry of the magnetic field) and the input of mechanical energy (say by convective motions or MHD turbulence).

PALLAVICINI: You have inferred magnetic fields of $\approx 10^2$ G in the extended coronae of RS CVn stars (at typical distances of ≈ 1 R_*). Are these magnetic fields consistent with what you expect or measure at the surface of the stars?

LINSKY: This is an important question. For an isolated magnetic dipole, $B(R) \approx R^{-3}$ and the deduced photospheric fields will probably be unreasonably large. However, the filling factor for fields in the photospheres of RS CVn stars is large, so $B(R) \approx R^{-2}$ is more sensible. Also the coronal fields are probably twisted so that the fields may be much larger than even a $B(R) \approx R^{-2}$ extrapolation.

SILVIA H. B. LIVI, SARA MARTIN, HAIMIN WANG, and GUOXIANG AI / The Association of Flares to Cancelling Magnetic Features on the Sun: 197–214.

SPEAKER: LIVI

POLETTO: Have you been able to establish any relation between the amount of flux which is cancelled and the importance of the flare?

LIVI: Not yet, but our goal is to do such quantitative analysis.

HENOUX: You use the word cancellation. Could you also interpret the magnetic decrease you observed as due to the submergence of a magnetic loop?

LIVI: Someone criticized our choice "cancellation", because the word had no physical meaning. That is exactly what we wanted: no preconceived model. The observations are not showing simple submergence of a magnetic loop, because ephemeral regions usually do not disappear by reversing their pattern of appearance and cancelling within themselves. Cancellation often occurs between one pole of the ephemeral region and a neighbouring magnetic fragment of opposite polarity. Besides, we also see brightenings and flares. I prefer to suggest reconnection as a related mechanism.

MOORE: I would like to point out that Dr. van Ballegoovijen has already a model (see his poster paper) for how such magnetic cancellation can lead to flares, and it fits your interpretation perfectly.

FALCIANI: Are you really sure that the phenomenon you are describing is well over the noise level of your videomagnetograph?

LIVI: While playing the movie again, you can see a transient effect that is happening at flare time. I could include in the reference Severny's idea that magnetic fields suddenly decrease during a flare, but that was based on very few magnetograms taken hours apart and was not confirmed. Cancellation is a gradual process in our line-of-sight magnetograms, and the number of consistent images is more than enough. The magnetograms are increasing in sensitivity on 21 November 1987 as the cancelling feature is disappearing. On a previons study (Aust. J. Phys. 38, 855-73, 1985) we showed that isolated features changed less than 10% during 5.5 hours, while 16 closely-spaced opposite magnetic features were conspicuously cancelling.

SARA F. MARTIN / Mass Motions Associated with Solar Flares: 215–238.

SPEAKER: MARTIN

ZWAAN: Concerning your definitions of flaring arches and surges, is my understanding correct that the difference is mainly in the inclination: in surges matter is thrown up nearly vertically, and in flaring arches nearly horizontally? If that is so, what is the point in defining two separate categories?

MARTIN: The difference between most surges and most flaring arches is not their inclination but rather their brightness and apparent energy. Surges are usually defined or described as consisting of a narrow spike of mass that appears to be injected into the corona from the chromosphere, slows with increasing height, stops, and then falls back to the chromosphere along the same path that the mass had when moving upward. Most surges are seen only in absorption. Flaring arches begin as emitting mass which is thrown into the corona from a flare, follows the trajectory of a complete arch, and falls to the chromosphere at a peripheral site, which has brightened during the flare. Flaring arches are also seen in hard X-rays while most surges are absorption features, which are known not to be associated with hard X-rays. However, I have here illustrated examples of bright surges and surges that are partially in absorption and partially in emission to make the point that there is a spectrum of energies among surges; these bright energetic surges have many properties in common with flaring arches. However, we do not know if these bright surges are associated with hard X-rays.

BALLEGOOIJEN: How long does it take for a filament to reform after the flare?

MARTIN: The time of reformation seems to depend on the magnitude of the photospheric magnetic field around the filament channel. In many cases, especially in active regions with high magnetic flux density of the photospheric field, the filament will begin to reform before the end of the flare. However, if the magnetic flux density around a filament is low, such as around quiescent filaments, the reformation is very slow and can take several days or it might not reform at all.

SVESTKA: I can inform you that we have found, very recently, that the bright surge of 8 July 1980 you showed was seen in X-rays by HXIS. The X-ray emission preceded the Hα emission, along very much the same trajectory, by one or more minutes.

MARTIN: This is an additional evidence that flaring arches are just very energetic surges.

VERMA: In most cases solar surges do not produce hard X-ray bursts. In my study of about 50 solar surges, none was found to be associated with hard X-ray emission (Verma, 1985, Solar Phys. 97, 301).

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KUIJPERS: Can you estimate if all of the gas in a surge falls back or if some of it escapes from the Sun?

MARTIN: There is no evidence that any of the mass of a surge escapes form the Sun. As far as we know, the surge mass flows into pre-existing closed coronal structures. Apparently, most of these coronal structures are large because the surges usually have a small amount of curvature. Only in a few cases, are surges observed to flow to the top of a closed arch and are then seen to flow down the other leg instead of returning to the source site.

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D. J. MULLAN / Solar and Stellar Flares: Questions and Problems: 239–259.

SPEAKER: MULLAN

KUIJPERS: Concerning the extreme Alfven speed you derive on the assumption of the electron cyclotron maser, I should like to note that in the solar corona, the cyclotron maser is not the only coherent mechanism. Would you agree that a similar state in stellar coronae removes your problem of extreme Alfven speeds?

MULLAN: Some coherent mechanism is needed to explain strong dMe radio flares. I suggest ECM may not work, but there must be some mechanism at work. In that case, there may be no need for $v_A > v_{AO}$.

KUIJPERS: In the same context your large source length is derived on the assumption of loop pulsations. In microplasma physics there are, however, other ways of obtaining short pulsations (see, e.g., Aschwsanden, M.J.: 1987, Solar Phys.111, 113).

MULLAN: If other modes of maser are at work, then conversion from a time scale to a length scale will require the appropriate group velocity.

KRISHAN: You mentioned collisional losses of electron beam and consequently a very high column density. Electron beams are known to suffer collective losses, which being extremely efficient, require much smaller column densities. Has anyone looked at this?

MULLAN: Yes: Hamilton and Petrosian (Ap.J. 1987 or 1988) have looked at how electrons can be stopped also by plasma waves. See also Winglee, Pritchett, and Dulk (Ap.J. 1987 or 1988) who have analyzed how a beam of electrons propagates (including electrostatic and collective effects).

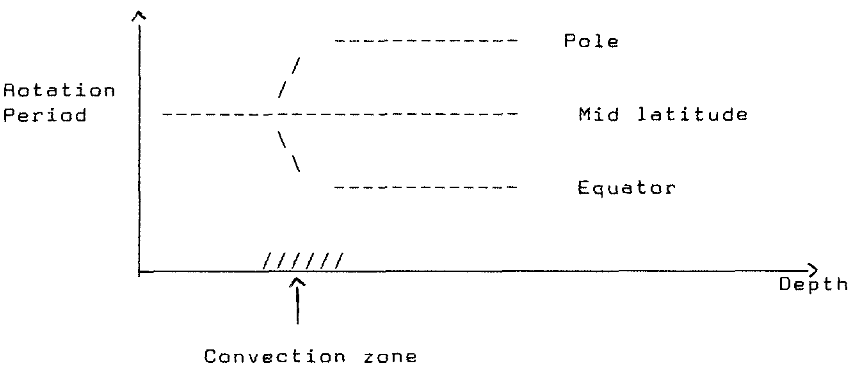
BYRNE: Recent studies of correlations between X-ray flares and VLA flares suggest that these are poorly correlated. So we may be observing different loops and even different events in the two frequency regimes.

MULLAN: A lack of correlation between radio and X-ray flares is not surprising because radio emission in dMe stars is coherent, whereas X-rays are not. Hence, a small change in physical conditions can drastically alter the ratio of radio to X-ray power. Therefore a large X-ray flare may be accompanied by either a huge radio flare or nothing detectable, even if both occur in exactly the same loop. Consequently, an underdetectable X-ray flare may give rise to an easily detectable radio signal if the physical conditions in the radio emitting gas are correct.

BYRNE: Using densities determined from the soft X-ray flares to determine preflare loop densities will overestimate this quantity.

MULLAN: Radio emission does not occur in a pre-flare loop, but in a flaring loop, although pre-flare loop density may be very high, much higher than "quiescent" loops (see Canfield, this conference). Electrons must stream down to the base of the loop and create a loss-cone distribution in order to create a velocity distribution favourable to coherent emission. Then radio emission can occur in the dense gas where the loss cone is created. Haisch's analysis gives mean densities for the whole loop: values in the dense lower gas will be higher than we have used, thereby exacerbating the problem. Admittedly, before the flare loop fills, the density will be lower overall, but this is offset by the higher densities in the radio emitting region. Moreover, in the pre-flare state the temperature will be small, so that the increase in density towards the feet of the loop will be very large. Hence, to calculate the column density, we have underestimated it by using the mean density. Even allowing for the effect you point out, electron beam penetration will be very difficult, especially since mean densities outside of flares are $\approx 10^{10}$ cm^{-3}, anyway (Katsova, 1988).

BORNMANN: You mentioned the problem in explaining the Butterfly diagram which requires $(d\Omega/dr) < 0$. The recent thesis work of C. Morrow (BAAS 1988, Kansas City meeting) using Fourier Tachometer data relates to this problem. She concludes that the rotation rate both increases and decreases with depth in the convection zone, depending on the latitude:



MULLAN: The data obtained by Harvey Duvall and Pomerantz at the South Pole Site in 1981 have been analyzed extensively and published in Nature (1986). The results show that the differential rotation, which is observed at the surface, persists inwards. That is, the pole continues to have the longest period, and the equator the shortest period, even at the base of the convection zone. This is consistent with the figure you have drawn, since the pole at the surface has a longer rotation than the equator. Hence, the existence of preferred longitudes suggests that the flare fields originate deeper than the convection zone. Also Duvall et al. (Nature, 1983) find that

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$d\Omega/dr$ is not negative, as required by dynamo theory, when averaged in latitude. It is not clear that this conclusion is necessarily inconsistent with the results you quote, depending on how much one weights the "equatorial" and "polar" regions in your plot.

VERMA: In addition to solar active longitudes, we have recently found for the first time that there are several active zones on the Sun, about six in each hemisphere (Verma et al.: 1987, Solar Phys. 112 , 341; Verma and Pande: 1988, Indian J. Radio & Sp. Phys. 17, 8).

DONALD F. NEIDIG / The Importance of Solar White-Light Flares: 261–269.

SPEAKER: NEIDIG

RODONO': Stellar flare observations show: a) pre-flare dips in a rather large fraction of flares; b) above 1 μm, in coincidence with the optical light increase, a "negative" flare (which is the mirror image of the optical one, but much fainter in relative intensity) is observed. Do you have evidence of similar behaviour in the solar case? Are IR observations of solar flares at wavelengths larger that 1 μm being done or planned?

NEIDIG: I know of no case where the solar white light flare shows pre-flare reductions in intensity. As far "negative" flares in the near infrared, observational coverage is extremely sparse and I know of no such examples. Presently we have no plans for flare patrols in the infrared, but perhaps we ought to.

KOJOIAN: In your talk, you have shown that the equation which characterizes the luminosity curves in both stellar and solar flares has the same functional form and is dependent on the same physical variables. This would appear to indicate that the same physical mechanism is acting in both cases but is just scaled. Equally, it would appear from other papers presented at this conference that the mechanism may be different. If you are also of this opinion, could you indicate how a different mechanism could account for the scaling which you have obtained and, further, what this other mechanism might be.

NEIDIG: The approximate equation given in the talk is of the form $L_{opt} \approx B_{5000\,Å}(T)\ \Delta\lambda\ A\ [1 - \exp(-\Delta\tau_{5000\,Å})]$, where A is the flare area and $\Delta\lambda \approx 10^4$ Å. This equation describes only the radiative losses, which, of themselves, imply nothing about the flare heating mechanism. If the temperatures in solar and stellar optical flares are sufficiently similar, then the emission mechanism should be the same, in which case the optical thickness, $\Delta\tau$, would be scaled by the same atmospheric density parameter.

E. N. PARKER / Solar and Stellar Magnetic Fields and Atmospheric Structures: Theory: 271–288.

SPEAKER: PARKER

LINSKY: Despite your general skepticism concerning the physics responsible for phenomena on the Sun, you have expressed some confidence that the physics of solar flares can be extrapolated to stellar fleres. The energy involved in flares on dMe stars can be 10^3-10^5 times larger. Does not this qualitative difference indicate a very different heating mechanism?

PARKER: Perhaps I have overstated my optimism. You are correct that the *extreme flares on certain other solitary stars are* so powerful as to suggest a situation that might be qualitatively different in some essential way from the solar flare. The enormous starspots occurring on some dM dwarfs are grossly different from sunspots too. And we have no right to assume that the solar repertoire includes every stellar phenomenon in the universe. Therefore, I should restrict my most intense optimism to the stars whose X-ray coronae are not qualitatively different from the Sun. On the other hand, we must not loose sight of the fact that present ignorance provides no evident objection to a considerable upward scaling of the magnetic active regions seen on the Sun. For instance, there is no objection of which I am aware to supposing that some stars have more vigorous photospheric convection, mixing the footpoints of bipolar magnetic regions five times larger in size, with mean fields five times greater than the usual 50-100 gauss observed on the Sun. The total magnetic energy of such a field would be 5^5 = 3125 times greater then its solar couterpart. For a given rms misalignment of the local lines of force, and the same rate of shuffling of the footpoints, the rate of energy input per unit area is 25 times larger than in the Sun. The larger energy input and the larger dimensions allow higher coronal temperatures without an overwhelming increase in coronal density. So we can go a long way toward the extremes of hot bright X-ray coronae and enormous stellar flares solely on the basis of a simple and completely unimaginative upward scaling of the active corona of the Sun. The qualitative differences between the solar X-ray corona and flare and the known extreme stellar cases may, or may not, involve phenomena unknown on the Sun (and hence unknown to us). Intensive observational studies and further theoretical work will be essential to clarify the picture.

JORDAN: You envisage the active region heating arising from the reconnection of magnetic fields stressed by jostling at the footpoints. If magnetic flux emerges in an unstressed condition, how long would it take to produce a loop heated to 4-5×10^6 °K.

PARKER: With the estimate that the footpoint wandering proceeds at a velocity $u \approx 0.5$ km/s, the interweaving of the lines of force (in a magnetic bipole of length L) reaches a level where heating should begin in a time that is inversely proportional to L and which has a value of 1-2 hours for L = 10^4 km. Hence a freshly

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emerging magnetic loop of a few thousand km should begin to show heating in a fraction of an hour, assuming that it was unstressed when it emerged.

STURROCK: I am dubious about your assertion that your proposed mechanism for coronal heating will give a volume heating rate that is independent of the overall linear dimensions. Uchida and I considered a similar model in 1979, and we found that the volume heating rate varies inversely as the square of the overall linear dimensions.

PARKER: The proposed mechanism predicts that the heating rate per unit area of the footpoints is independent of the length of the magnetic field between the footpoints, all other things being equal. Hence, the heating rate per unit volume is inversely proportional to the length of the field. If the line of sight, through a region filled with flux tubes, is proportional to the length of the flux tubes, the energy input per unit area is then independent of the length of the field.

MARTENS: There is one aspect in both the wave theory of coronal heating and in your theory of spontaneous formation of current sheets that does not satisfy me: Why does one see X-ray emetting LOOP structures, and not the whole active region lighting up in X-rays? Since the magnetic field probably fills the whole active region, why is the heating confined to loops?

PARKER: I do not know the answer to your question. The heat input needs not vary much between faint and bright flux bundles because of the heat lost by downward thermal conduction. I can suggest only that the shuffling and intermixing of the photospheric footpoint of the field is patchy, so that in some places a flux bundle is rapidly interwoven and in other places slowly interwoven. It may be too that a flux bundle subject to a uniform rate of intermixing tends to discharge its internal tangential discontinuities intermittently, with dormant periods between each phase of active dissipation. During the dormant time the tangential discontinuities accumulate until they reach some threshold and the active phase of heating begins again. But one can only conjecture on these matters. Perhaps one day we will observe the shuffling and mixing of the footpoints in the photosphere to see how patchy is the effect. And perhaps observation can determine if the dissipation of current sheets involves cooperative effects and tends to procced in intermittent phases.

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GIOVANNI PERES / Hydrodynamic Models of Solar and Stellar Flares: 289–298.

SPEAKER: PERÉS

HAISCH: How does the magnetic field that your hydrodynamic simulation implies agree with the one I derived from the Alfven wave propagation time versus X-ray light curve argument?

PERES: Our lower limit of ≈100 Gauss is a factor of four smaller than the lower limit you derived.

MACNEICE: You justified the use of an implicitly collisional model of the coronal loop on the basis of the electron collisional mean free path being less than the loop dimension. In fact the use of Spitzer-Hrm heat flux demands that a much more rigourous condition (mean free path less than a few percent of the scale height) be satisfied. The degree to which Spitzer-Hrm heat flux fails in the "SMM Benchmark model" to which you referred, is illustrated in the poster paper by Ljepojevic and myself.

PERES: As for the bulk of thermal electrons, their mean free path is very short with respect to the loop dimensions. This ensures a hydrodynamic treatment. As for the high energy tail of the electron distribution, namely that responsible for considerable part of the heat conduction, the standard Spitzer-Hrm description is expected to fail. Better descriptions of thermal conduction, like those of Karpen et al. (1988), yourself and Peres et al. (1987), are available.

JORDAN: Were there any measurements of the electron density in the transition region of Proxima Cen from observations with IUE?

PERES: The IUE observations of this flare lacks the time resolution needed to study the rapid evolution of the flaring atmosphere.

PALLAVICINI: The flare you have modelled on Prox Cen is a rather modest flare by stellar standards. Suppose you want to model a much stronger stellar flare, you have to dump much more energy which will probably affect heavily the optically thick part at the loop footpoints. So, the first question is: Do you expect that the treatment of the optically thick part at the loop footpoints will affect critically the hydrodynamics of the coronal part? My second question is: Can your model predict the optical flare that presumably originates at the loop footpoints?

PERES: Stronger flares show larger amounts of evaporated material, therefore when we try to model those flares we tend to choose initial atmospheres with larger base pressures and, therefore, larger amounts of material in the chromosphere. This should reduce the effect of the treatment of the optically thick part on the corona evolution. Our model, however, does not try to fit, so far, the data of the chromosphere.

And coming to your second question we surely predict the presence of a shock propagating downward in chromosphere which has been connected with white light flares; and study other general hydrodynamic features of the chromosphere. Therefore we can, so far, make general predictions on this aspect.

CARGILL: Your simulations create very hot dense plasmas in the corona and transition zone. What magnetic field strengths do you need to confine the plasma to avoid vioalating the I-D assumption on the Sun?

PERES: The magnetic fields which would ensure confinement are not unreasonable for the solar cases studied so far.

CARGILL: What fields are required in the stellar flares you discussed?

PERES: The more demanding stellar case, I just presented, implies a coronal magnetic field of the order of 100 Gauss, which could easily be accepted even for a solar compact flare.

B. R. PETTERSEN / A Review of Stellar Flares and Their Characteristics: 299–312.

SPEAKER: PETTERSEN

PALLAVICINI: I would like to add a word of caution about the X-ray flare detected from the A-type star α Gem (Castor). The EXOSAT flare is certainly real; however, we have to take into account that both components of α Gem (A1V + A5 Vm) are spectroscopic binaries. The mass function leads to a broad range of spectral types for the secondary (from M to A-type). Moreover, the properties of the X-ray flare (time-scale, total energy, X-ray temperature) are consistent with those of flares from M dwarf flare stars. Therefore, we should not disregard the possibility that the flare on Castor may have originated from an unseen late-type component.

PETTERSEN: That may be the case also for other early type stars. Some of the flares have timescales and lightcurve shapes that are reminiscent of dMe flares. Only more detailed observations will show if any of these cases can be explained by the presence of convective companion stars. Until that has been thoroughly investigated, I would like to regard the situation as open for debate.

RODONO': If we are dealing with dynamo-activated flares, we would expect a dependence on the Rossby number rather than simply on the mean density or convection zone volume. Did you explore this possibility? In fact, in addition to convection, rotation rate is another important parameter to be taken into account for activity correlation studies.

PETTERSEN: No, I have not plotted flare activity versus Rossby number. For very active (i.e. saturated) flare stars the actual value of the rotation rate seems not to affect the situation. All of them rotate faster than about 5 km/s at equator and those few that are really rapid (>25 km/s) still seem to produce about the same level of flare activity. Rotation, as a determining parameter becomes apparent only when slower rotation rates are *considered, as in K and M dwarf. For instance, for dK and dM* stars with no emission lines the flare activity is dramatically smaller than for saturated stars.

POLETTO: Can you give a figure about the relative frequency of huge energetic flares with respect to small ones for stars of any given class?

PETTERSEN: The distribution of flare frequency with energy is well studied for dKe-dMe stars, the Sun, and some cluster flare stars. Shakhovskaya showed a diagram related to this problem in her talk. In a zeroth approximation, you can say that the cumulative distributions of flare energy vs.frequency have slopes near unity. Therefore if you move down by one order of magnitude in flare energy, flare frequency increases by a factor of 10. This is not true in every detail because some stars have slopes different from unity. In fact, several observers have claimed that the slope changes systematically with the spectral type for solar neighbourhood stars.

SCHMITT: I wonder about the case of the activity being proportional to the volume of the convection zone. In a sphere most of the volume is concentrated near the surface, so in going from a solar-like G star (with $r_{conv} = 0.7\ R_*$) to a fully convective M star, most of the volume change is due to the radius change. Could one claim - using the same data - that activity is proportional to the star's surface area as to its volume?

PETTERSEN: Your point is correct, of course , from a geometrical point of view. However, if the stellar radius were the key parameter, the relationship would predict very high levels of atmospheric activity (flares, X-rays, etc.) in early type main sequence stars. Since that does not seem to be supported by observations, I believe that the radius of the convection zone gives a better parameter to describe the situation. Of course, that immediately has implications for the physics. It allows solar-like activity in stars with outer convection zones, such as FGKM main sequence dwarfs, rapidly rotating subgiants, and fully convective, rapidly rotating young contracting stars. Some forms of activity seen, (e.g., in early type main-sequence stars) would then have a different origin, or their outer structure would have to allow convection.

KUIJPERS: Do you think it important to follow up the first detection of variability in a Wolf Rayet star with a monitoring programm for flares?

PETTERSEN: The only flare reported in the literature implies a large energy release since it was seen against the background of a bright Wolf Rayet binary. I don't think such flares are abundant and a follow-up program could be very time-consuming. However, flare-like phenomena in early type stars are not well documented and I think it would be important to find out if these outbursts have anything to do with the magnetic-related event on the Sun and other convective cool stars.

GIANNINA POLETTO / Long-Duration Solar and Stellar Flares: 313–322.

SPEAKER: POLETTO

KUIJPERS: Why do you start with a magnetically open configuration? Should that not be explained precisely by a flare?

POLETTO: The reconnection model deals only with phenomena which occur after the field configuration has been torn open. To the model this is the initial field configuration and no attempt is made to account for events which occurred prior to this phase. While a comprehensive flare theory may consider mechanisms which lead to the field disruption, investigation of this problem was beyond our present purpose.

KUIJPERS: What is the physical basis that makes the field relax to precisely such a value that it can contain the X-ray emitting plasma?

POLETTO: If the field strength were smaller than required for magnetic confinement of the hot loop plasma, reconnection would not occur. On the other hand, if the field strength were larger than required for magnetic confinement of the plasma, reconnection would occur at a faster rate. Empirical evidence favouring this interpretation comes from the analysis of the 29 July 1973 flare on the Sun. From high resolution observation we may derive the electron density of the X-ray loops, as well as their temperature and their altitude, and determine the gas pressure at the tops of the loops. This value turns out to be in excellent agreement with the magnetic pressure predicted by the model at that height.

STURROCK: The results of your model depend on your assumption about how the " Y-type " point, separating closed from open field lines, increases with time. What assumption do you make, and on what basis?

POLETTO: According to the reconnection model the enhanced ohmic heating associated with fieldline merging shows up as localized thermal X-ray emission from the neutral point region. Therefore the X-ray source drift towards higher altitudes is supposed to trace the neutral point rise. In order to derive an analytical law representative of this upward motion, I made use of spatially resolved observations of X-ray loops in two-ribbon flares and each time I assumed the top of the newly formed X-ray loop to signal the position of the reconnecting region.

SCHMITT: Your model determines the rate of change of the magnetic energy whereas the observations relate to count rates in various passbands. Could you comment on your assumptions relating the change in magnetic energy to the observed radiative losses?

POLETTO: At this time one can only resort to the solar analogy and assume stellar flares to behave as solar flares. In the Sun radiative energy in the bands I used for the modelling amounts to about 1/10 of the total flare radiative losses. To be realistic, any model should provide at least as much energy. All the models I considered comply with this requirement.

ERICH RIEGER / Solar Flares: High-Energy Radiation and Particles: 323–345.

SPEAKER: RIEGER

SHAPIRO: Concerning particle beams: at least for very intense solar flares, energetic particles have been observed on earth. So at times, there are particle beams.

RIEGER: The beams you are referring to may have been produced later in the flare in the corona, or further out in interplanetary space. The question I am mainly concerned about is: Does the primary acceleration mechanism, acting for instance at the top of the loop, create beams or not? It appears to me that the detection of limb brightening can be explained by both: particle beam or omnidirectional distribution created, for instance, at the loop top.

MULLAN: Can you comment on the chemical composition of the target material from which the nuclear lines are emitted?

RIEGER: The shortness of time prevented me from talking about this subject. It is, however, extensively discussed by Ramaty and Murphy in 1987 (Space Sci. Rev. 45, 213).

BAI: You showed a very good correlation between X-ray fluences above 300keV and nuclear X-ray fluences. However, if you compare hard X-ray fluences > 30 keV and nuclear X-ray fluences, we do not find any good correlations. Therefore, your conclusion that one acceleration mechanism can accelerate all the particles cannot be applied to low-energy electrons.

RIEGER: What you say is certainly true. I think that at ≥ 30 keV in some events thermal emission, which does not reach to > 300 keV, may contribute to the radiation.

GRANDPIERRE: It seems to me that there is an energetic problem with the shock acceleration in the second phase. We know, that a large part of flare energy is involved in the particle flares. To be able to provide enough energy to these particles by shock waves, we would need much more energy in the shocks than in the flares, because of two reasons. The shocks propagates spherically and symmetrically, therefore their energy decreases with the square of the distance from their places of birth. Secondly, they can reach the particles only within a limited solid angle.

RIEGER: The total energy, which resides in the very high energy particles, about which I was talking, is only a minute part of the whole flare energy, so that I don't think, your energy argument holds.

TAKASHI SAKURAI / Magnetic Equilibria and Instabilities: 347–360.

SPEAKER: SAKURAI

KLIMCHUK: The method you have described for calculating sequences of magnetic equilibria, based on the so-called "generation function", places artificial constraints on the field. For example, in the case of an arcade that is uniform in the x-direction, the generation function dictates the B_x component of the field on each and every field line. This is an unphysical constraint that is not relevant to the Sun. What is relevant to the Sun is the connectivity of the field; that is, the locations of the footpoints in the photosphere. To my knowledge, none of the force-free equilibrium sequences calculated using connectivity boundary conditions, rather than the generating function, terminate with a loss of equilibrium. Regarding equilibrium sequences characterized by increasing pressure (with fixed shear), I share your concern that the pressures necessary for loss of equilibrium are unrealistically high. In the latest work of Zwingmann, for example, I believe that loss of equilibrium does not occur until the plasma beta is near 0.1, which is much too high for the corona. Finally, I believe that there is now a consensus on the issue of thermal stability. Most workers in the field, including Antiochos, agree that coronal loops are thermally stable, as long as they are not too low-lying.

MARTENS: I agree with your remark that the correct physical problem is done by describing the footpoint positions of the field lines. But I disagree with your statement that non-equilibrium has never been found. There is a poster here by Aad van Ballegooijen and myself that demonstrates the onset of non-equilibrium for just this situation.

HASSAM: From your presentation, it is apparent that you have studied the theory of ideal/non-ideal MHD instabilities in some detail. You have also studied vector magnetographs. To my knowledge, ideal MHD instabilities (or loss of equilibria) have growth quite disparate from non ideal instabilities. From your studies, would you associate solar flares with ideal instabilities, loss of equilibria or non-ideal instabilities?

SAKURAI: I would think that the primary driver for solar flares is either ideal MHD instabilities or a loss of equilibrium. These might lead to forced (or driven) reconnection, thereby releasing the magnetic energy in the form of heat and high energy particles.

CHEN: You stated that a force-free equilibrium (2-D arcade) can give information concerning pressure. It seems that the pressure may not be a physical quantity one can always prescribe in a coronal structure. The ability to prescribe pressure may depend on specific mechanisms. If so, a bifurcation with respect to specification of pressure need not be a physical one. This may be analogous to the point made by Jockers (in a 2-D arcade) versus specification of the toroidal field component (in a 2-D arcade) versus specification of footpoints. Do you have any comments regarding a 3-D structure such as a "toroidal" loop?

SAKURAI: I still feel it to be reasonable to suppose that we have the liberty of specifying the pressure as large as we like. For 3-D loops, some examples are presented by Low (1986) but much more remains to be done.

VELLI: In the 2-D calculations one finds instability when magnetic islands form above the photosphere. However it is not at all certain that instability would remain (linear or nonlinear), if the third, line-field dimension was included in the analysis. Would you comment on this point?

SAKURAI: I agree that the line tying in the third dimension (along the filament axis) is an important factor.

CARGILL: The growth rates for tearing modes are relevant for infinite or periodic media. The boundary condition of photospheric line tying has been shown (Hood and Priest, 1979; Einaudi and van Hoven, 1983, Mighredo and Cargill, 1983) to remove the possibility of tearing modes in such configurations because the singular surface present in infinite media is no longer present. This must be kept in mind when applying tearing mode theory to solar plasmas.

ANTIOCHOS: In your 3-D force-free fields calculations do you find any evidence for non-equilibrium? Do you find that your numerical method, i.e. code, works well for the force-free problem where the transverse field is specified at the photosphere?

SAKURAI: First, I encountered several cases where the iterative procedure did not converge. However this might well be a breakdown in the numerical scheme and may not be due to a non-equilibrium situation. Second, the force-free field may be determined by prescribing the normal component of the magnetic field (B_n) and the value of α, in either positive or negative polarity regions. The specification of all the three components of the magnetic field over-determines the solution.

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J. H. M. M. SCHMITT, J. R. LEMEN, and D. ZARRO / A Solar Flare Observed with the SMM and *Einstein* Satellites: 361–373.

SPEAKER: SCHMITT

DOYLE: I have two comments concerning your estimate of Ne and V:

a) From the EINSTEIN data you only use the radiative cooling time: the derived N_e will therefore be a lower limit since you neglected thermal conduction and mass downflows, both of which are important for the coronal region.

b) The estimate of N_e based on the EM and the flare size from the FCS data may also be a lower limit since the FCS cannot resolve the flare and therefore the filling factor may be less than 1.

Hence your comparison between Ne and V derived from SMM and EINSTEIN should be treated with caution.

SCHMITT: To answer your first comment what has actually been determined - according to Moore et al. (1980) - is that radiative and conductive cooling time scales are about equal. With this the conduction length scale can be estimated, which turns out to be less than the derived length scales (cf. Schmitt, Harnden and Fink 1987). In other words, the derived picture is physically consistent.

As far as your second comment is concerned, I agree with you that the FCS filling may be less than one and that the desired densities are still lower limits. The filling factor problem is obviously aggravated in the stellar case; the point to keep in mind is that even the possibly conservative "stellar" estimates indicate very small filling factors for the flaring plasma.

SIMNETT: I wish to caution you about the accuracy of the parameters you might derive from full-star observations of soft X-rays. From the SMM-HXIS solar observations it is very clear that for many flares more than one structure is involved in the flare. At some point in the flare the X-ray emission from these structures becomes comparable, i.e. often the emission from one structure decays while it rises, temporarily, in another. Treating the situation as a single, unresolved event will merely result in some "average" value of the derived parameters, which gives a very misleading conclusion in the case of solar flares. Unfortunately, I cannot offer any practical suggestions as to how to treat stellar flares in a more realistic way.

SCHMITT: Your warnings are well taken, in fact, some of the observed stellar X-ray light curves have a very complex structure which may indicate what a number of spatial components participate in the flare. On the other hand, the very purpose of this work has been to investigate to what extent the "stellar" modelling of a solar flare can be verified (as falsified) by detailed observations. In the (only) case studied in detail the physical parameters derived from the "stellar" modelling agree with those derived from dedicated observations to within astronomical accuracy. This success I consider very encouraging; of course, this result does not imply that the physical parameters of all stellar flares have been correctly determined.

RODONO': I completely agree with Dermott Mullan's comment on the importance of quantifying the uncertainty in deriving physical parameters from spatially unresolved stellar flare observations. I should like also to stress that this beautiful piece of work by Jurgen Schmitt does not exhaust what can be done by using well planned and accurate stellar observations: I believe that we can learn a lot on stellar flares from multi-wavelength observations for the purpose of studying the effects of the global stellar parameters if any, and of the plasma physical state on the flare triggering and evolution, bearing always in mind what we have learned and might learn from the study of spatially-resolved solar flares.

JORDAN: You found that the peak emission measure from the Einstein measurements was significantly larger than that found from the Ca XIX lines with the SMM BCS instrument. Have you taken the emission measure distribution with temperature derived from SMM and folded it through the Einstein band responses to make a comparison that way?

SCHMITT: No. The IPC emission measure was derived taking the cooling function at log T ≈ 7.2. Since, as you said, lower temperature material does contribute to the IPC measurements, this procedure underestimates the effective cooling and hence overestimates the emission measure. Therefore including a DEM distribution would actually reduce the apparent discrepancy between IPC and SMM emission measures.

N. I. SHAKHOVSKAYA / Stellar Flare Statistics – Physical Consequences: 375–386.

SPEAKER: SHAKHOVSKAYA

FOING: How do you correct the bias in the diagram log E - log v for (1) the detection threshold and (2) the time superposition of frequent small flares? If this statical correction is not made, any extrapolation or inferences about the role of small-scale flares for coronal heating may be incorrect.

SHAKHOVSKAYA: Only the upper parts of the flare energy spectra above the photometric detection threshold of flares are presented in Fig. 2. and were used to derive our statistical estimates.

RODONO': Have you tried to consider M_{bol} instead of M_V in your frequency energy correlations? The former might be more significant.

SHAKHOVSKAYA: No, I did not try.

RODONO': How did you compute the solar flare wide-band energy which is included in your plot of flare energy spectra?

SHAKHOVSKAYA: The solar flares were observed in Hα line and the corresponding energy in B-band were calculated as in Gershberg, Mogilevsky, and Obridko (1987).

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PETER A. STURROCK / The Role of Eruption in Solar Flares: 387–397.

SPEAKER STURROCK:

SERIO: What about small loops, say $\approx 10^3$ km?

STURROCK: Flares occurring in loops that are only 10^3 km high could not be due to the current-interruption process I have suggested.

MOORE: What is the diameter of a typical current filament that would interrupt giving rise to a " single loop " flare? Is the total current carried in many narrow threads that fill a small fraction of the flare loops?

STURROCK: If each current filament arises from a magnetic knot, with a flux of about $10^{18.4}$ Mx, and if the field strength in the corona is about 100 gauss, the radius of a typical current filament would be 10^8 cm.

GAIZAUSKAS: Two observational comments on your description of an erupting filament. It is very common for active region filaments to exhibit downflow with a magnitude of tens of km/s for many hours preceding the eruption. The downflow is usually quite distinct at one footpoint of the filament, so much so that the location of that footpoint is easily identified with respect to photospheric features. The other footpoint of the same filament is often vague and difficult to identify as you have stated in your description of initial conditions. Can you safely ignore downflow as one of your initial conditions?

STURROCK: The point you make indicates that we really have an inadequate understanding of the structure and origin of filaments. The downflow suggests that a filament may be continually evolving, perhaps the result of the slow but steady emergence of new flux from below the photoshere. Even so, the motion can probably be ignored in considering the MHD stability of filaments.

CHEN: With respect to your statement that introduction of pressure can allow physical bifurcation in a 2-D arcade, it seems that pressure of an arcade is not a physical quantity that can be prescribed.

STURROCK: I agree that specifying the pressure at each point, as a given function of space and time, does not sound like a reasonable "thought experiment".

CHEN: As an alternative mechanism for magnetic energy release in the corona, you mention the possibility that a "mechanical" energy release mechanism may be operative. For example, Chen (1987, and poster paper at this meeting) describes a mechanism whereby a magnetic/current loop can become unstable and expand. The expansion velocity can range from very slow to ≈ 1200 km in the corona, with a correspondingly wide range of energy release rates (up to 10^{32} erg in 30 min) via drag heating. This may possibly be relevant to some motion-related phenomena.

STURROCK: In many flares, the kinetic energy of mass motion accounts for the biggest share of the energy released during the flare. Hence your proposal may well be relevant to an important component of the flare process.

CARGILL: What instability are you exactly proposing for the anomalous heating model of ion acoustic; this implies $T_e \gg T_i$ in the loop.

STURROCK: The instability I have considered so far is the ion-acoustic instability, and you are quite right that this requires $T_e \gg T_i$.

CARGILL: What exactly is responsible for the prompt acceleration of the protons?

STURROCK: The prompt acceleration mechanism would be simply direct acceleration by the field-aligned electric field that develops as a result of the attempted current interruption.

MULLAN: If there is no steady coronal heating, would you expect that, once instability is quenched, all loops should cool on time scales of $\approx 10^2$ s and then reheat and re-cool repeatedly?

STURROCK: Yes, the picture is that coronal material would cool on a time-scale of order 10^4 s, then heat impulsively on a time-scale of perhaps 10^3 s, and so on.

HASSAM: I refer to your slide in which you listed four possible ways by which the reconnection rate could be enhanced over the *Furth-Killeen-Rosenbluth rate. I would like to point out that one* or both of two factors apply to each of the four mechanisms proposed: (a) the fact that resistivity, η, still acts as a nozzle in that the growth rate goes as some power of η; (b) the fact what some of these mechanisms have not yet been backed up by "hard" calculations. I would therefore submit that none of the four proposed mechanisms have convincingly shown us that the reconnection rate is sufficiently enhanced so that reconnection can be clearly considered as the process underlying flare phenomena.

STURROCK: I would submit that the work of Carreras and his colleagues, and of Sakai and his colleagues, are pretty "hard" calculations. However, I do agree that much work remains to be done, and that the precise role of reconnection in flares remains to be pinned down.

DING: Have you found any evidence of current sheet in active regions before solar flares?

STURROCK: Unfortunately we still have not discovered a way to detect current sheets in the corona.

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SMITH: One of the best studied examples of coronal heating is the large loop of the 1980, November 5 flare (Martens et al. 1985, Solar Phys.). Martens et al. found that only 10^{-3} of the loop was heated and proposed ion tearing as a mechanism. Have you calculated in your sporadic heating model what fraction of a loop is being heated at any one time?

STURROCK: My very preliminary estimates are that the "filling factor" would be in the range 0.01 to 0.1 .

ACTON: Please comment on the observability of the current interruption event? The density is so low that there must be very little emission measure.

STURROCK: As long as the plasma density is low, the principal result of the interruption event is particle acceleration. But as soon as bombardment of the chromosphere leads to evaporation that fills the loop with hot, dense gas, the principal output would be X-ray emission.

SHAPIRO: Would you please elaborate on the nature of the shocks that might accelerate higher-energy ($\approx$ GeV) particles?

STURROCK: I have in mind the shocks that are responsible for Type II radio bursts. These may be either blast waves, caused by the sudden energy conversion related to the MHD instability that leads to filament eruption, or possibly bow shocks that run ahead of coronal mass ejections.

SVESTKA: You showed simultaneous flux peaks from 40 keV through 40 MeV, but that was at the very onset of the flare, during its impulsive phase. Later on, the energy range is much smaller. Do you accept reconnection as the source of energy release later in the flare development?

STURROCK: Yes. Certainly one must expect that reconnection plays a role in energy release. For instance, the morphology of two-ribbon flares is strongly suggestive of field line reconnection in an overlying current sheet.

CHENG: Observations in soft X-ray and EUV show that the flaring plasma is heated up 10-20 minutes before the onset of the impulsive phase. This means that before the flare instability occurs, the flaring loop already shows appreciable and observable emission. I wonder how your cool and low density loop could reconcile with the observed preflare heating.

STURROCK: One possibility, that I mentioned, is that the filament eruption is coupled with field line reconnection. If this is the case, that stage of reconnection could be responsible for the soft X-ray emission that you mentioned.

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ZDENĚK ŠVESTKA / Solar Flares: The Gradual Phase: 399–417.

SPEAKER: SVESTKA

SIMNETT: I have a question regarding the energy supply for long-duration soft X-ray events. It was pointed out by Sheeley et al. (Ap. J., 1985) that soft X-ray events lasting more than 6 hours were correlated with coronal mass ejections. The latter normally are associated with shocks, and it is believed that such shocks accelerate particles as they go out through the corona. A significant fraction of the particles go back to the Sun, where they deposit their energy, presumably near the base of the corona. Why do you not consider this mechanism as an energy source for the long duration events?

SVESTKA: Because I do not see how the deposit of energy of these particles could give rise to the well-defined loops we observe, with impressive brightness maximum at their tops. The particle acceleration is a stochastic chaotic process: the particles should deposit their energy all throughout the active region; why just in the loops we see? And how can the deposition last for so many hours, selecting sequentially higher loops? Besides, the fact that we record particles in space does not necessarily mean that a similar amount of particles flows downwards (cf. Bai's results, e.g., comparying intensity of γ-rays and number of protons in interplanetary space).

VERMA: What are the basic conditions in the active region for a flare to be impulsive or gradual?

SVESTKA: The confined flare (which you call impulsive, I suppose) is a local instability in a preexisting loop or a system of preexisting loops in an active region due, for example, to twisting of field lines. The dynamic (gradual) flare reflects a global instability, probably due to excessive shear and a trigger, which we do not know.

MARTENS: I have a question regarding confined flares. I have heard different opinions regarding simple loop flares, mainly from theorists, whether single loop flares exist, or whether all events involve multiple loops. What is your opinion on that, and can you give a specific example of a simple loop flare?

SVESTKA: I think that simple loops flares do exist, but are very rare. The soft X-ray flare, I showed on the first slide, looks like a single-loop flare. But, of course, better spatial resolution ($\approx$ 4 arcsec in Skylab) might have shown even in this case that the "single loop" was actually composed of a conglomerate of loops. I think that Steve Kahler and Mukul Kundu could show you other examples of such simple flares.

I. TUOMINEN, J. HUOVELIN, YU. S. EFIMOV, N. M. SHAKHOVSKOY, and A. G. SHCHERBAKOV / Polarimetry of Stellar Active Regions and Flares: 419–429.

SPEAKER: TUOMINEN

MULLAN: Synchrotron emission may explain quiescent polarization. In 1975, I thought this was very difficult to understand, but since then, the connection between flaring and coronal heating suggests that synchrotron may now be considered as a viable source of polarization in M darfs even outside flares. IRAS data on flare stars show that 20% of flare stars are visible at 100 μm and 60 μm. Only synchrotron emission can explain the IRAS data. Hence, coronal heating apparently supplies the atmosphere of M dwarfs with lots of MeV electrons.

HENOUX: First a comment: During solar flares we observed linear polarization in Hα line, which seems to be due to particle bombardment of the solar chromosphere. My question is: What are the relative contributions of lines and continuum to the observed linear polarization for stellar flares?

TUOMINEN: It is difficult to say. Polarization for flares is small and difficult to measure. The magnetic intensification in lines seems to be the most probable mechanism in active regions of solar type stars.

V. V. ZHELEZNYAKOV and E. YA. ZLOTNIK / Cyclotron Lines in the Spectra of Solar Flares and Solar Active Regions: 449–456.

SPEAKER: ZHELEZNYAKOV

ACTON: Is the distribution of temperature with height derived from the theory or assumed?

ZHELEZNYAKOV: The distribution of kinetic temperature with magnetic field strength in loop cross-section was derived from observed cyclotron line profiles. The temperature distribution with height was then derived, assuming a definite configuration of the magnetic field above a bipolar group.

STURROCK: What are the ideal instrumental requirements of a radio telescope that could make the required observations to detect cyclotron lines?

ZHELEZNYAKOV: The angular resolution of the antenna should be better than 5-10 arcsec because the linear sizes of loops are about 50-100 arcsec. The desired frequency resolution of a spectrograph is determined by the linewidth; it must not be worse than 30-60 MHz at the operating frequency of, for example, 1800 MHz.