PREFACE

At the suggestion of Dr. Ai Guoxiang, IAU Colloquium 141 was held in Beijing Sept 6-13 1992. We felt that the development of new magnetographs and observing techniques in recent years permitted examination of the many problems associated with these fields. In many ways, the meeting grew out of our extensive coordinated observing program with the Big Bear and Huairou magnetographs, which gave us, for the first time, a continuous view of the evolution of magnetic fields. This meeting celebrated the maturity of work in this field in China, showing how Huairou could now compete well with the other observatories in this field. Since Beijing is far from the centers in this field, and there are other meetings scheduled, we frankly did not expect the large attendance at this colloquium. However, Dr. Ai and the other members of the LOC valiantly dealt with the problems of a large meeting and a great time was had by all.

The meeting took place at the Friendship Hotel in Beijing, at the northwest corner of the city, near the university and observatory. Meetings were set up for two sessions a day, either morning and afternoon or morning and evening. There were invited and contributed papers; in this volume the invited papers are limited to 10 pages and the contributed, to four. Because this still produces a very large volume, no attempt was made to record the questions or discussion, which were very lively. The individual contributions were edited by session chairmen or volunteers; we wish particularly to thank Drs. Bhatnagar, Gaizauskas, Hagyard, Harvey, Howard, Stenflo, Wilson and Zwaan, who promptly and effectively edited the contributions, and Drs. Gary and Ewell at Caltech, who edited some stray manuscripts. The reader will note that a few manuscripts of invited and contributed papers never arrived; these are included in the program. Dr. Ai and I particularly thank Dr. Haimin Wang who, with the assistance of Ms. Nora Knicker, did the real work of producing this volume. We also thank Dr. Wang Jingxiu for his extensive assistance with preparing material for the conference, and the Astronomical Society of the Pacific for producing reasonably priced proceedings. The National Science Foundation of China and the Chinese Academy of Sciences, as well as the IAU, provided financial support for the conference.

It is interesting to compare this meeting with IAU Symposium No. 22 (Lüst, 1965, Stellar and Solar Magnetic Fields, Amsterdam::North-Holland), the Tegernsee meeting of 1963. That meeting, held when many of you were very young, brought together most of the workers in solar *and stellar* magnetic fields in a single room. Looking at proceedings and

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papers, we see that the backbone of theory has not changed much, although simulation now replaces reckoning, and there has been an enormous explosion in the observational data, particularly in quality. In many ways we are at a crossroads -- we are always at crossroads, but it seems particularly true of studies of solar magnetic fields today. We have a few magnetographs that operate regularly, and several others, occasionally. We often obtain high-resolution sequences, we are now getting data in the infrared, we are getting vector field data, and now we see the wonderful Yohkoh data, which traces the three-dimensional magnetic field structure in the corona. We heard in our meeting of the new measurements of fields by radio techniques at various stations, and finally have data on transverse field changes with flares but with a sign opposite that expected. Some of the papers were rediscovery of material published years ago, but there is no harm in that; older material gets overlooked, and new data supports it.

We now have a number of high-class magnetographs, we have transverse magnetograms, we have almost continuous records. We have seen a sea of change in the field compared to 1963. Then most of the observational work was being done at American observatories; now, with developments in Japan, China, and the Canary Islands, the Americans must run a little bit to stay competitive. We have seen more and more complicated magnetic structure, but still mostly round sunspots. I wish people would study more complicated sunspots, which unfortunately don't appear on demand. I remember at one of these meetings Bumba showed the Evershed effect was associated with "flags." Now we have Doppler movies where we can see what his flags really are, these elements of outgoing material that are also associated with the moving magnetic features. By the way, we did not hear a word at this meeting on why the Evershed effect occurs. Where are our hand wavers? It occurs in all sunspots, even small ones, and is very important.

While there always is a need for better data, we are getting to where we are more in need of interpretation, or *setting the right questions*. Some of these questions are well known, such as the controversy over the true strength of the weak fields, which we have debated, or the Leighton field-diffusion problem. Others have been raised at the colloquium and will be answered in the future. We have been reminded that the great active regions of this cycle were quite unbalanced, with one polarity two or three times more extensive than the other. We have also seen two great regions at the same high latitude and possibly the same longitude. We hope that Yohkoh will show us how such field segregation takes place; is it confined to the biggest and most active regions? We learned more at this colloquium about the systematics of active regions; we heard about axes and tilts and the peculiar nature of δ spots. It is time to begin the discussion of the sources of sunspots, considering the peculiarities of δ spots and the other associations of spot morphology and activity. How indeed can large monopolar spots be formed? Why do big spots appear more complex than small ones? Recent work is beginning to look at these questions.

We must always remember that what we see is the *intersection* of a magnetic flux line with the surface. For example, we feel with some justification that the flows of the network keep these fields confined along the edges of the network. Obviously that can't work very long, and is open to an exchange instability in which the field lines pop back into the middle. Yet we believe that we can model these with floating corks. While cork models are terribly useful, we must remember that it is a three-dimensional structure, and cannot float freely. We've seen bizarre field structures, such as the cat's cradle (an English colloquialism), which Alan Title showed. We have also published such images; they are normal for the penumbra and of course unknown to modelers. The immense transverse fields I showed associated with delta spots are a more important feature, completely unexplained.

The problem of how complex fields form is still more significant; the biggest are the most complicated ones that would tend to support a model where many of them are formed complicated, perhaps because the smaller ones simplify on the journey to the surface.

To my knowledge, there are no proposed solutions to these problems. Most theoreticians do not know that they exist; because they come in from physics, they are ignorant of the observed Sun, and waste years modeling things that do not exist. Meetings like this are *important*, because they disseminate knowledge rapidly, and give people plenty to think about. An example: the famous Kopp-Pneuman mechanism shows how a simple bipolar spot has a current sheet above it which becomes unstable and produces flares. But regions of this type rarely flare; flares occur almost exclusively in complex delta configurations. The mechanism is a good one, but we would have many more flares if it actually happened that way.

There has been great preoccupation with field changes during flares. We now have evidence that the transverse field does change, but in a way different than we expected, namely the shear increases. The effects are large, and independent confirmation by other observatories is important. People should remember that we are looking for time-dependent changes, and subtle effects or corrections are unimportant in searching for big changes in strong fields. Even crude data would be useful, so long as it is consistent and frequent. It is important for all observatories with working transverse magnetographs to attempt to observe such changes, especially in the one or two highly active regions we expect to see in the remaining years of the cycle. It is even more important to build magnetographs with fast cadence and run them frequently to detect these changes, which do not occur on order.

Unfortunately, there is much nitpicking over whether filter magnetographs give correct results. At one meeting a well-known astronomer asked if the magnetic elements being tracked by Huairou and BBSO were real and not artifacts. He did not realize that Huairou is far from BBSO and has a different telescope; it would take a truly remarkable artifact to appear at both telescopes, and to locate on the Sun where each monochromatic feature appeared. Far too much emphasis has been given to details of the vector field, and over whether every possible error source has been eliminated and the magnetogram obtained in every possible point in the line profile. While such programs are useful in studying stable spots, observational cadence is so slow that data on flare-related changes cannot be obtained. I was particularly struck by a paper on calibration of a magnetograph. The author worked through the problem in exquisite detail, correcting for radiative transfer and every other small effect, finding all the 5% errors. Then, we were told, the results must be multiplied by a factor eight to match the Mt. Wilson spectroscopic data! A triumph of the academic approach, but not science.

Comparison of transverse fields in the same sunspot measured by several different observatories has in fact shown them to agree satisfactorily with one another and with low-resolution sunspot models. If there is an instrumental artifact (which has never been demonstrated), it will not suddenly change at the time of the flare, nor will it affect the other observatories that observe the same effect. If the instrument really changes at the moment of the flare, after working stably for weeks, we have invented a truly remarkable device.

The flare-connected fields are so large and the changes so abrupt that subtlety is unnecessary; continuous observation is much more important. Despite this, we hear endless unsupported complaints about other peoples' magnetographs from those that operate different devices, especially those that operate infrequently. I propose a truce: let each instrument do its thing, with whatever advantage it has or has not, and criticisms be limited to documented errors or inconsistencies rather than innuendo. Then a more concerted effort might be made to observe flare-associated changes. If there are people who suspect the data presented at this conference, let them demonstrate the error or arrange co-observing.

There are other fascinating results. The two biggest regions of the past cycle occurred at 33° N, and possibly the same longitude. But, as Gaizauskas pointed out, the longitude difference cannot be determined over two years without a much better knowledge of the differential rotation. Both these regions showed 80% of the field strengths in the polarity of the main umbra. There is no known explanation or even attempt to explain.

Mechanisms intrigue us and we heard many papers on mechanisms for flares, for magnetic structures that may or may not exist, for filaments and so forth. The Sun is the only place in the universe, except possibly the earth, that we can study these things. The stability of filaments, which can live for several rotations, and of the quiet, round sunspots, is remarkable. A quiet sunspot lives much longer than an active one that dissipates its energy in flares. We find tiny "invisible" micropores and umbral dots that far outlive reasonable models. So many unexplained features.

Another important factor in the flare process is the role of motions; high-resolution observations have made this clear, and we are further benefitting from coordinated observations, which make the observations more comprehensive. Models are becoming dynamic, and we look forward to better understanding of this phenomenon.

We are finally getting into the magnetic fields of the corona. A high point of the meeting was the presentation of the first Yohkoh results, where the X-ray images trace out the three-dimensional fields better than any magnetograph. From radio astronomy, the data now permits quantitative measures of the coronal field strengths over active regions. The Yohkoh data clearly show the importance of footpoint heating, both for hard and soft X-rays, and the subsequent development of loop-top emission. We surely will understand more about flares when this data is studied, and the availability of the atmospheric flux loop data will simplify our understanding of surges and ejecta.

The study of emerging flux regions (EFR) has become a recognized aspect of our work, and we heard several papers on this subject. This will become more and more a part of our literature. Since so much of the formation of spots takes place below the surface, and this is then played out before our eyes, it is most important to observe the parameters of flux emergence, the appearance of structures that just yesterday were below the surface. Thus we extend our three-dimensional view upward through Yohkoh and downward through the study of EFR's.

It has been a pleasure to work on this meeting, to meet with you, to continue our long collaboration with Huairou, and to familiarize ourselves with solar physics in China. We should once more congratulate Prof. Ai Guoxiang for organizing this successful meeting.

We hope that all will enjoy this book.

Harold Zirin
Pasadena, California
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