DISCUSSION

STENFLO (CHAIRMAN): After you have now heard the four "provocative" summaries, let me ask the audience if you find that there is something fundamentally missing in the presentations of Noyes and Stix concerning the scientific needs. In particular I would like the questions to focus on the interface observation – theory, because the close interaction between observation and theory seems to be fundamentally important for progress in the "solar-stellar connection".

FRISCH: One (obvious) tool that was not mentioned in the flash-reviews of our needs is the *computer*. From the view-point of theoreticians it appears useful to distinguish (as done by M. Stix) numerical experimentation from numerical modelling. In the former case we are mostly interested in understanding some mechanisms involving only a few physical processes (e.g. flux expulsion); in the latter case the description must be sufficiently comprehensive to allow actual prediction of numbers. I wish to stress that even the fastest existing "supercomputers" are hardly able to cope with realistic modelling of phenomena, which are actually three-dimensional (as is generally the case with magnetic fields present). Until we have access to substantially more powerful machines ($\geq 10^8$ words of core, $\geq 10^{10}$ FLOPS), it might be wise to put the emphasis on numerical experimentation more than on modelling; in particular we want to be very careful with semi-realistic modelling which (i) will not add much to our knowledge, and (ii) can be extremely expensive.

TITLE: One should be aware that the observations have the same problems as the computers. On SOT our experiment will be collecting data from 8, perhaps 10, 800 $\times$ 800 arrays at 12 bits per pixel, hopefully once every second. This will contain part of the information that the theoreticians have been asking for, but some clever people have to figure out how to get this information out of the numbers. We want to understand things on a scale of 1/10 arcsec, which means that we have to take several samples over 1/10 arcsec over a range of heights and over an area that is typically tens of arcsec. People are going to have to think very hard how to describe the data that they get.

IONSON: I think that we should guard ourselves against mathematical overkill. This is a common trap that theorists fall into when doing purely computational studies. It is not efficient to construct theoretical models that are orders of magnitude more detailed than observational input.

MOUSCHOVIAS: I have two comments. (1) This symposium has convinced me that the stellar-solar connection has been made. I think, however, that it is equally important to make the interstellar-stellar connection. The results of cloud collapse calculations can yield the distribution of angular velocity and magnetic field strength within a protostar. These are quantities which are essential as input to calculations such as those of P. Gilman. More effort should be put in this direction. (2) On the interaction between theory and observation: I find it ironic that, for a week now, the complaint has been expressed that theorists ignore observations, and, at the same time, when a theorist gets up and restricts the range of his input parameters as suggested by observations, another theorist complains of such an approach. What is needed is less (rushed) criticism and an effort to understand what each research endeavour can and cannot accomplish.

J. O. Stenflo (ed.), Solar and Magnetic Fields: Origins and Coronal Effects, 511–513.

GIOVANELLI: I agree with the two previous speakers. But it is also important to remember the development of theory over the past 10 years or so. The theorists have been concerned with the interactions of fields and motions — building up from roughly uniform weak fields. The mathematical solutions to these problems are elegant and informative. But the observers have been showing for 15 years or more that the fields are concentrated into tiny strong fluxtubes at the surface, and these should be concentrated to much greater strengths when carried down by convection. The problem then is very different from the original one — for example it is hard to find processes that will bring the fluxtube fields down towards equipartition values. The theoretical problem of the interaction of fields and motions becomes different. Of course it is also of the greatest importance to produce theories and mechanisms which conform to the modern discoveries such as the observed meridional flows, torsional oscillations, etc., rather than merely feeding in boundary conditions, which are purely fictional.

STENFLO: Let us now shift the discussion to the instrumental problems, to consider the summaries given by Zwaan and Vaiana. They presented the separate needs for ground-based and space observations, but I would like you to also consider the important question of coordinated observations in different wavelength ranges and with different telescopes.

MARCY: I would like to suggest that high-resolution infrared spectroscopy should be useful for measuring mass-loss rates and Zeeman broadening in cool stars.

LIVINGSTON: Much of the observational work today is at, or near, the state-of-the-art in terms of sensitivity or resolution. For this reason it is important that observers check up on one another in a critical way. It is dangerous to depend on single observers, or groups, to supply critical data. Duplication in this sense is a healthy thing.

DUPREE: I would like to add to Dr. Vaiana's list a mission that was mentioned in the US "Field Report", a far ultraviolet explorer satellite, which would primarily aim for the 900 - 1200 Å region, but, as talks are developing now, may well go from 100 to 1500 Å. This would provide us simultaneous coverage of plasma diagnostics over a very large temperature range, without having to try to schedule different satellites like Einstein and IUE to look at the same star at the same time.

WALTER: I think it should be pointed out that the Space Telescope does have an instrument which is basically stellar — the HRS (although it may be monopolized by the hot star observers). It has the high spectral resolution (up to about 2×10^5) and temporal resolution ($\sim$200 ms) which IUE lacks. With such resolution, chromospheric and transition-region flows and flares will certainly be observable. The stars, of course, will not be spatially resolved, but Doppler imaging techniques in the UV lines can be used to map out large-scale structures in stellar chromospheres. In addition, high time resolution observations of eclipsing binaries, as in the crude observations reported here by Gibson, Walter, and Basri, can also be used to determine chromospheric and coronal structures and their absolute sizes. Often a picture is worth thousands of words.

ROXBURGH: One of the top priorities must be to measure solar oscillations with high precision — this is perhaps the most promising development in recent years and will permit us to determine the internal structure of the sun. We will need a satellite such as DISCO to achieve the precision needed.

GIBSON: One needs to be aware that radio observations may provide the best available data on structures and physical parameters in solar and stellar coronae. Even better data may be available with the simultaneous 2 cm/6 cm system, which will become available at the VLA next year, as well as the extention of its wavelength capability to 327 MHz. Also VLBI will become available for observations of stars, and will be quite capable of resolving their coronal structures.

STENFLO: In conclusion, may I ask for your brief opinions on future international collaboration in the area of the "solar-stellar connection", in particular for bringing theoreticians and observers together? In considering future meetings as a follow-up of this one, we have several options: from regular small-scale meetings of workshop type to larger but infrequently occurring symposia. What are your feelings about the optimum size of a meeting, and how frequently should it be organized?

KUPERUS: There are already so many meetings going on, so one should be careful when organizing new ones. It is not good for communication to have too many meetings.

GIOVANELLI: May I just mention that there is this week another solar meeting in Boulder

SHEELEY: ... and a third one in Ireland.

DUPREE: I should mention that we have in Cambridge an ongoing series of Workshops, called "Cool Stars, Stellar Systems, and the Sun", the next one to be held in October 1983. Usually we have 100 - 150 solar and stellar astronomers, including a fair European representation. This seems to be a successfull way of integrating the communities.

SODERBLOM: Speaking for myself, I have to say that for the meetings I have ever been to, on the whole the quality of the meeting and the results obtained have been inversely proportional to the number of people attending, although some critical mass may be needed. One should not try to agglomerate the meetings. The value of a meeting like this is simply that it brings people with common interests together.

CAMPOS: About the question of size of a meeting there is an intermediate optimum: large enough to have a broad range of views, and small enough so that everyone can discuss with everyone else. I think that this meeting is of the right size.

About cooperation between theoreticians and observers, I think that this is what we all have been doing this past week.