OUTLOOK FOR GROUND-BASED AND SPACE OBSERVATIONS

G.S. Vaiana
Osservatorio Astronomico di Palermo (Italy) and
Harvard-Smithsonian Center for Astrophysics, Cambridge, MA (USA)

1. INTRODUCTION

The presentations given at this Symposium have made clear that we are witnessing an exciting period in the study of solar activity, and in the ancillary study of magnetic field dynamics in solar-type stars: the new space observations obtained within the past four years, together with the renaissance in ground-based observations, have occasioned a burgeoning in our phenomenological understanding of stellar activity in its manifold facets, and have encouraged a substantially greater interest in the problem of magnetically-coupled activity in stars by the theoreticians. This excitement must not distract us from the realization that in this branch of our science, much of the impetus towards new research has come, and very likely will continue to come, from observations; with this fact in mind, I would like to address the possible evolution of our experimental science in response to the new observational and theoretical results.

As we look to the future, two key facts ought to be kept in mind. First is that scientific progress in our science has basically resulted from instrumental developments which had their genesis some 10 to 20 years ago. Thus, the recent results in stellar optical and UV astronomy have been based on detector technology which predates current photon-counting techniques; the design of the EINSTEIN Observatory dates back to the 1960's. This historical lesson must be considered as we think about the future because it is very likely today's technology that will delimit the observational possibilities over timescales of the next decade or so. Second, we must ask ourselves whether the pace of technological development has remained steady; can we see on the technological horizon any new, promising techniques that may promise a real breakthrough in observational capabilities (for example, CCD technology has brought about in extragalactic optical astronomy), or have we reached a period of quiescence in which we are just likely to do more of the same (with some incremental improvements).

2. POSSIBILITIES FOR NEW OBSERVATIONAL CHALLENGES

In order to provide an organizational scheme for the discussion, I shall phrase the future observational goals in terms of specific scientific goals which I have heard articulated at this Symposium.

(i) *Structure of stellar interiors and dynamo activity.* A continuing problem in understanding stellar magnetic activity revolves about the nature of stellar interiors: how deep is the outer convection zone as a function of stellar parameters on the main sequence;

J. O. Stenflo (ed.), Solar and Magnetic Fields: Origins and Coronal Effects, 505–507.

what is the rotational state of stellar interiors; how does compressibility (departure from Boussinesq behavior and removal of the assumptions of mixing length theory) affect the flow structure of the convection zone; what is the nature of magnetic field generation in the interior of stars; how does the rate of magnetic flux generation depend upon the physical properties of the underlying star? Some of these problems may well be addressed by observational tools now under development. For example, the field of stellar seismology is just getting under way, having gotten off to a strong start with the recent successes of global mode detection for the Sun; in particular, the ACRIM results for bolometric solar fluctuations (which show evidence for 5-minute oscillations!) could well find application in the stellar domain, with possible trade-offs between sensitivity and long-term stability. Such measurements of the interior structure of stars will be complemented by direct measurements of surface magnetic fields, using recently developed techniques such as those used by Robinson, Worden, and Harvey (1980), and will be particularly powerful if one is able to go to the IR (as Zeeman splitting varies with wavelength), and will be complemented by other magnetically-sensitive diagnostics, such as Ca II and X-ray emission for solar-type stars. These may allow us to determine the variation of the rate of magnetic flux production as a function of stellar parameters: mass, radius, composition, age, rotation rate.

(ii) *The nature of photospheric surface activity.* The Sun has taught us that the essence of surface activity resides in its inhomogeneous nature: by and large what we refer to as "activity" occurs in association with observationally well defined photospheric features: the active regions, composed of the sun spots and ancillary plage regions. Perhaps the most crucial question related to the activity problem (which only stellar observations can address) is whether stellar surface magnetic fields are invariably inhomogeneous (intermittent): is the near equipartition between gas and magnetic fields at the solar surface an accidental attribute of the Sun, or is such behavior generic to solar-type (surface-convecting) stars? If generic (as we suspect it is), does the surface structuring depend upon the stellar state (in particular, its rotational state)? Such questions may also be susceptible of observational test in the foreseeable future: the newly-developed technique of "Doppler imaging" (Vogt 1983), which requires the combination of very high spectral resolution and sensitivity, will allow determination of the spatial structure of spectrally-anomalous surface features (such as "star spots"); while the further, continuing refinement of speckle imaging (which also places a premium on sensitivity) may allow direct spatial resolution of stellar surfaces of dwarf stars (at least in a gross sense).

(iii) *Nature of chromospheric and coronal activity.* It is by now one of the common-places of solar and stellar astronomy that chromospheric and coronal activity are largely governed by the dynamics of magnetic fields which emerge from the interior of stars. From this perspective, stellar surface activity can be used as a probe for stellar magnetic activity, and hence, indirectly, as a probe for dynamo action in stellar interiors. One of the prerequisites for such studies is however a better understanding of chromospheric and coronal activity in and of themselves. Specific problems to be considered include the gross characteristics of emission from stellar outer atmospheres, such as the mean luminosity and temperature, as functions of stellar parameters (as will be addressed by the coming generation of major space-borne observatories such as the Space Telescope, AXAF, EUVE, EXOSAT, and ROSAT); the physical (= geometrical) structure of stellar outer atmospheres (a problem which will only be accessible to study by the planned VLBA, which will allow us to go to the milliarcsecond resolution level at roughly VLA sensitivities); detailed atmospheric diagnostics, including more refined temperature, density, and velocity measurements (as will be accessible to study by means of the high spectral resolution spectrometers on board ST and AXAF).

SUMMARY

As we look to the future, three points ought to be kept in mind. First, it should not be surprising that the available instrumental capabilities are not particularly well-matched to the observational requirements: after all, much of the recent space-based observational work was based on the serendipitous discovery of the remarkable breadth of stellar activity; and certainly very few of the space instruments which fly today (or have recently flown) were designed with stellar activity observations in mind. Thus, in defining the capabilities of new instrumentation, we now have the not inconsiderable advantage that we roughly know what sensitivities and spectral resolution to aim for, and what appropriate design of observational programs is, in order to answer current observational questions.

Second, I must emphasize that many of the imposed instrumental requirements for the new observations do not demand major development of new technology: by comparison with the more esoteric branches of astrophysics, the instrumental constraints set by stellar activity observations are, in most cases, relatively modest, and can be easily met by available technology. It is also in this context that the issue of simultaneous multi-bandpass observations must be placed: it is most certainly the case that observations of stellar activity (especially those which aim at diagnostic information) find observations made in near temporal coincidence in the radio, optical, UV, and X-ray of great usefulness. Such observations can be accomplished by either a multiplicity of space platforms, or a single larger "bus" platform, either one in conjunction with ground-based observatories. Which is optimal is not obvious, but certainly an important ingredient in any calculation must be the relative probability of actually bringing either possibility to fruition: this will depend upon a number of trade-offs, including the relative ease of individual instrument optimization, the relative difficulty of insuring coordinated observations, and the relative total system reliability; my own assessment is that these trade-offs on the whole favor the notion of a succession of separate platforms, each optimized in instrumental and programmatic capability for the peculiarities of the particular instrumental passband.

Finally, the third point: the most critical issue in defining the capabilities of new observational tools is accessibility: stellar observations are peculiar in demanding large blocks of time, on a fairly regular and extended basis, demands which are generally difficult to deal with in the context of instruments which are to be shared with galactic and extragalactic astronomers. This crucial requirement implies one or more instruments dedicated to stellar observations (e.g., Explorer–class stellar satellites), and hence imposes a very special obligation on the solar-stellar activity community, especially in light of present fiscal contingencies: I suspect that we, as a community of scientists with common research interests, must be prepared to define a commonality of scientific goals to the point of achieving a consensus on what instrumentation ought to be built in the near future; this has been one of the crucial elements in the successes of extragalactic research within the past decade, and we must be prepared to do likewise.