The A-Star Puzzle
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Panel discussion section E

CHAIR: **S. Bagnulo**

SECTION ORGANIZER & KEY-NOTE SPEAKER: Gautier Mathys
INVITED SPEAKERS: G.A. Wade, D. Moss
CONTRIBUTION SPEAKERS: J. Brathwaite, Th. Lüftinger

Discussion

BAGNULO: I would like to ask Mathys and Moss to comment about the possible detection of the radial gradients of the magnetic fields in Ap stars. What would be the consequence of such a feature on the stellar atmosphere? I refer in particular to that the length scale for horizontal variability of the field is not less than 10^5 km, whereas the depth of the photosphere may be 100 times less.

MOSS: As I understand it, the suggestion is that field strengths vary quite dramatically in the vertical direction on much shorter length scales than any horizontal field variation. This would imply the presence of strong currents and dissipation, and would appear to be hard to reconcile with long-term stability of the magnetic fields.

BAGNULO: Is it possible that the apparent detection of a magnetic field gradient actually reflects an imperfect knowledge of the Landé factors?

MATHYS: I do not think so. Landé factors are fairly secure in general, at least at the kind of level of accuracy that we are talking about. But for longitudinal field determinations on both sides of the Balmer discontinuity, one should keep in mind that possible systematic effects may be introduced by the quarter-wave plate, which may not have exactly the same retardance throughout the whole wavelength range of interest. Romanyuk (2005, *These Proceedings*, EP4) pointed out to me that his measurements blueward of the Balmer jump are hampered by the low sensitivity of the spectrograph in that spectral region. On the other hand, for the field modulus measurements of Nesvacil *et al.* (2005, *These Proceedings*, EP9), the main limitation arises from the very small number of diagnostic lines available on the blue side of the Balmer jump.

WADE: There are two things to remember concerning vertical gradients. First, the length scale over which the gradient is presumed to occur is very short, hundreds or thousands of kilometres. Second, the magnetic fields are measured with spectral lines. We know very well that surfaces of Ap stars are nonuniform. Therefore, lines of different elements routinely produce different field strengths, whether these lines are formed in the same region of the spectrum, or in different regions. Similarly, lines of different strengths can produce different field strengths, whether or not they are formed in different regions of the spectrum. Thus one has to be extremely careful in interpreting measurements particularly from small samples of lines.

RYABCHIKOVA: Before drawing any conclusions about magnetic field gradients in Ap star atmospheres, one has to estimate the line formation depth, taking into account abundance stratification effects. One cannot take for granted that lines much before ($\sim \lambda 3300$) and much after ($\sim \lambda 5000$-6000) the Balmer jump are formed at different optical depths.

Noels: I have a question concerning the evolutionary status of Ap stars. As I understand it, the mechanism proposed by the theory for the generation of the magnetic fields tries to explain what happens at the ZAMS, and some observations suggest that magnetic Ap stars are somewhat evolved away from the ZAMS. Can you comment on this?

Braithwaite: The field strength increases with time for stable configurations. So it may well be that a certain time is required for the field on the surface to become strong enough to be visible. Or maybe not. The timescale on which the field strength increases is a little uncertain. The timescale found from my work is 2×10^9 yr, which is somewhat longer than the Main-Sequence lifetime of an Ap star. To explain Hubrig *et al.* 's result that Ap stars become observably magnetic only once they have completed 30% of their Main-Sequence lifetime, we would need the surface field strength to increase on a shorter timescale.

Moss: What I would like to emphasise again is that I do not understand why the behaviour of the more massive Bp stars appears to be so different. In these stars we see strong surface fields at ages of the order of 10^7 yr, much less than the claimed age at which surface fields manifest themselves in the lower mass Ap stars. If we think that the Bp stars form a continuum with the Ap stars, then something very strange would appear to be happening.

I would also like to point out the idea that the surface field changes on timescales of 10^7 to 10^8 yr is not new, as it is present in models of the faster rotators that included meridional circulation, published some 20 years ago. However, we now have a further theoretical mechanism to drive such changes.

Wade: I just want to underscore that the situation is not that there are no known magnetic Ap stars that have ages consistent with a small fraction of their Main Sequence evolution, but rather that only a very small number of the low-mass Ap stars are relatively young. There are certainly a larger number of well established Bp stars with masses between 3 and 5 $M_\odot$ that have completed less than a few percent of their Main Sequence evolution. If you believe that the higher mass magnetic stars and the lower mass magnetic stars are all part of the same sequence that is connected by the same physics, then there is still a need for the presence of fields at the ZAMS and, I am sure, before the ZAMS too.

Piskunov: Can the changes and the evolution of the near-surface magnetic fields affect the anticipated chemical stratification? If this is the case, we should see characteristic anomaly patterns for stars of similar evolutionary status.

Michaud: In the outer atmosphere, vertical diffusion timescales are very short, of the order of 10^4 yr, so that diffusion adapts rapidly to the changing magnetic configuration, as long as the stability of the atmosphere is not too affected. On the other hand, if the structural changes of the magnetic field required horizontal diffusion, this could take much longer.

Vauclair: Emerging magnetic fields can have some influence on the abundances if they sweep up regions where some elements are mostly neutral while others are mostly ionised. The magnetic lines bring the ionised atoms upward, but not the neutrals. Of course, this depends on the timescales and on the exact structure of the field, since the ions can also fall back along the magnetic lines if they are inclined.

Cunha: We are now starting to be able to use the global oscillations of Ap stars to probe the magnetic structure of the interior, both in terms of the average magnetic field intensity and of its geometry (although for the geometry, we are limited to those layers that are magnetically dominated, that is, right below the photosphere, because these are the layers that influence the oscillations). In relation with this, it was mentioned that the magnetic fied structure could be different in the stellar interior from what is observed at the star's surface. What do you mean by stellar interior? Is it very deep in the star?

Mathys: It could be just below the surface.

Wade: Observationally, we do not know anything about the field below the line-forming region. We have to leave that to people like Braithwaite and Moss. Something that may potentially be useful to look into is the "10% problem". That is, let us tentatively interpret the fact that we observe magnetic fields in only 7% of the Main Sequence A stars not as meaning that only 7% of these stars are magnetic, but that only 7% show their field on their surface. This may not make things simpler for the theoreticians, though.

Adelman: I have a comment and a question. My comment: if you build an H-R diagram and look at the distribution of rotation periods, there is a tendency for the rotation to become slower as one proceeds from the ZAMS to the giants and supergiants, upon which a stochastic distribution of periods is superimposed. A proper statistical study of this effect remains to be performed. As to my question, how well known are the Landé g factors?

Mathys: I have been limiting myself to iron peak elements, for which I was using Landé factors from Kurucz's CD-ROM. When there is a measured Landé factor, it is used in this list. Otherwise Kurucz derives the Landé factors from the same approximate model atoms that he uses to compute the other pieces of information included in his table. I have shown (1990, A&A 236, 527) that the Landé factors that he obtains in this way are considerably better than those computed by application of the LS-coupling formula. I do not know what VALD uses as a source for the Landé factors. Perhaps one of the people involved in this project who are present here could comment on this.

Landstreet: I would like to dispell the impression that there are only a few Landé factors in the literature. Most laboratory spectroscopists measured Landé factors to classify energy levels, and Charlotte Moore obtained these values and included them in the NBS *Atomic Energy Levels* volumes. There are thousands of values in these volumes.

Mathys: I concur, but there are still some cases of great practical interest, such as the high-excitation Fe II lines (which are useful diagnostic lines for observations of Ap stars in the red spectral region), for which no laboratory values are available.

Ryabchikova: Our studies of the Zeeman structure of different spectral lines show that the surface fields obtained from nearby lines (such as the Fe II $\lambda\,6149$ doublet and the pure triplet Fe I $\lambda\,6173$) may differ by 100–200 G. We find this for instance in γ Equ, which has a surface field of 4 kG. I also obtained surface magnetic field estimates using other lines. All other pure triplets yield field values closer to that obtained through the analysis of the $\lambda\,6173$ triplet. My feeling is that the famous Fe II $\lambda\,6149$ doublet tends

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to provide field values slightly smaller than the other lines. I checked the Landé factors in the NIST publications, and they seem to be correct. This situation should be taken into acccount in all studies of the fine behaviour of magnetic fields, such as magnetic gradients.

Mathys: I think that you are right. The Fe ii $\lambda\,6149$ line is a doublet only in first approximation, because it is formed in a regime of the partial Paschen-Back effect. Therefore I would not be surprised if the field values derived from measurements of this doublet showed some systematic effect at the level of a couple hundred Gauss; the magnitude of the effect may actually depend on the strength of the field in the considered star. The main interest in using this diagnostic line is that it is visible in virtually all Ap stars and it has a really large splitting. If you use it consistently in a given star, you can trace the variation of the magnetic field with phase in a meaningful way. But you should exert great caution, I agree, in using it for the study of effects like magnetic gradients.

Kochukhov: I have a comment concerning the relation between the magnetic field and the surface inhomogeneities. According to our most recent abundance Doppler imaging studies, there does not seem to be any clear systematic correlation between the surface chemical inhomogeneities and the magnetic field structure. Therefore there is no reason to believe that only the magnetic field can induce nonuniformities in the distribution of chemical elements over the stellar surface. Thus in my opinion, the use of nonuniformities (e.g., of Hg in α And) as a proxy for the existence of a magnetic field is questionable, because it seems that other processes besides magnetic effects can modify the chemical diffusion in Ap stars. This is easy to understand: if for a certain ion, the radiative and gravitational forces are balanced, then any external perturbation such as, e.g., rotation or the effect of a binary companion, can potentially induce changes in the diffusion over the stellar surface.

Kurtz: There is another sequence of peculiar stars, the Am stars, which have similar rotation properties, similar structures, and similar ages. But they do not show any horizontal abundance differentiation. The only known difference is that the Ap stars have magnetic fields, which implies that the latter are the major cause of the horizontal abundance inhomogeneities in Ap stars.

Vauclair: I just want to point out that the effect of the magnetic field on diffusion is not trivial. It may be much more complicated than simply having a spot at the magnetic poles or a ring at the magnetic equator. The observed difference between abundance and magnetic patterns may be due to this nontrivial coupling (which may be different for different elements).

Adelman: There are a few papers suggesting that young Am stars have abundance distributions different from older Am stars.

Wade: As the only example of well-established HgMn stars with clear, undisputed line profile variations, α And, is extremely important for the understanding of the role of the magnetic field and of other processes in producing abundance structures. On the other hand, in older abundance Doppler images (e.g., Hatzes), you could see very coherent geometrical structures (e.g., rings, caps) that were apparently associated with the underlying magnetic geometry in a clear and "natural" way (e.g., rings at the magnetic equator, spots at the poles). Are *any* of these kinds of relationships evident in the latest generation of Doppler images?

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KOCHUKHOV: Only few chemical elements show obvious correlations with the magnetic field geometry. For the majority of the species, we find fairly complex chemical patterns, without rings or spots symmetric relative to the magnetic field. This observation is based on the mapping of 17 chemical elements in the roAp star HR 3831 (Kochukhov *et al.* 2004, A&A 424, 935).

BAGNULO: North (1998, A&A 336, 1072) has shown convincing evidence that magnetic Ap stars do not loose angular momentum during their main sequence life. I should like to ask Saul Adelman if he finds a different result.

ADELMAN: My paper (2002, Baltic Astron. 11, 475) suggests that most, but not all, of the most rapidly rotating magnetic CP stars are close to the ZAMS, and some of the least rapidly rotating magnetic CP stars are the furthest from the ZAMS. The problem when you do this kind of study is that you really want to make sure that you know the periods quite well. For a fair fraction of the Ap stars for which periods have been reported, these periods are not correct. So this study is based on those stars with periods determined in my own studies and in those with my associates, so that I was absolutely certain that the periods were correct. I also made sure that I had reliable distance estimates, so that I have used Hipparcos parallaxes. If you mix this data with that from other sources, you would have to worry about different systematic errors.