Peculiar Versus Normal Phenomena in A-Type and Related Stars
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SUMMARY AND GENERAL DISCUSSION (Chairperson: Dr Don Kurtz)

KURTZ: It's my job to provoke this discussion, and I would like to start by predicting what will happen at the next meeting of this sort, in another place, perhaps five years from now. When we walk in to the room, we'll know the distances to the Ap stars from Hipparcos data, so we'll know their luminosities; those who work on temperatures will provide that data; we'll know the rotation periods, and we will know the magnetic field structures extremely well. We will have spectra that range from the x-ray right down to the radio region (essentially noise-free, of course). We will know all the line strengths, and Bob Kurucz will be there with perfect stellar model atmospheres which allow us to calculate these line strengths in terms of abundances and atmospheric structure with no problems, and Georges Michaud and his colleagues in the diffusion game will be there to give us parameter-free *a priori* predictions that explain our observations.

Maybe it won't be quite like that in five years, but I was really delighted at this meeting to see how much progress has been made. A lot of this new sophistication has occurred only in the past few years.

Something should be said about the way in which the range of our work relates to other fields of astronomy. We have already mentioned this in the context of magnetically-controlled winds this morning in the papers by Linsky, Shore and Lanz. Mention was made of comets and planetary astronomy. As you know, Voyager is still out there, looking at the magnetosphere, so we are relevant to the planetary astronomers. We were also told that we are relevant to the pulsar astronomers. White dwarfs were not mentioned, but I will remind you that there is a class of white dwarf and cataclysmic variables called "polars" in which there are megagauss fields which control the gas streaming down onto their surfaces; a huge number of astronomers is involved in studying them. Steve Shore drew an analogy between astronomy and psychology this morning, and the mention of white dwarfs reminds me of an incident that happened to two of my colleagues in the cafeteria at the University of Cape Town, where they were having an animated discussion about degenerate white dwarfs. A student who knew nothing about astronomy but who had been eavesdropping could stand it no longer, and came up to them and said, 'You guys are really weird. Are you in Psychology?' You have to see this in the context of South African politics to understand what he must have thought was meant by "degenerate white dwarfs." [laughter]

Another incident also comes to mind. A few years ago I was giving a talk about Ap stars in San Diego, and Geoff Burbidge was there. You all know how iconoclastic and irascible Geoff can be. I was introducing diffusion theory, and basically I said that, if diffusion theory is wrong, we are back to having no explanatory theory. At this point he exploded, and said, 'But you haven't considered any other model!' Now of course we have considered lots of models: magnetic accretion, surface spallation mechanisms, mass transfer in binaries, nuclear reactions in stellar cores with dredging to the surface, and abnormal atmospheres. None of these worked, and finally they were rejected in favour of diffusion because it does work, though not all by itself yet. We all know that we cannot yet predict *a priori* the abundances in a particular star, based on its known parameters. Something else is happening. Some of you have suggested at this meeting that one of the unknowns might be the stellar wind, or mass loss. This can be important, but we don't yet know if it is enough. Chuck Cowley always reminds us at these meetings to keep an open mind, because some of the other mechanisms we have rejected may still have an effect, and we should remember them as we try to solve these problems.

Before turning the discussion over to Georges Michaud, I would like to remind you of something that was once said by J.-C. Pecker. It refers to the relationship between theoreticians and observers:

> May I recall an old proverb, which can of course be turned all ways. Whenever an observer says something, everyone believes him but himself. Whenever a theoretician says something, no one believes him but himself.

And with that I am going to let a theoretician have his say. I'm sorry, Georges! [laughter and applause]

MICHAUD: Well, with that introduction you will understand why I hate concluding sessions! I generally miss them, except when I organize meetings and I have to speak at them.

I agree that one exciting fact at this meeting is the qualitative improvement in the data available, compared to the 1975 or 1985 meetings. The abundance distribution at the surface of magnetic stars was hardly believable in 1975. We knew there was something going on, but we had no reasonable evidence of the presence of rings, for instance, for which now there's thought to be significant evidence for many elements. Similarly, many important tracers of what goes on, such as lithium, had hardly been observed at the time of those meetings. It is the improvement in the quality of abundance determinations with electronic detectors which has allowed this considerable improvement and which is allowing significant tests of the models which have been proposed.

As to the models, I think in particular we have heard significant new data on λ Boo stars. The evidence is now accumulating, in fact from both the theoretical and observational points of view, that the diffusion model I suggested in 1986 for these stars (G Michaud and Y Charland, ApJ, 311, 326, 1986) probably is not the main process in these objects. Evidence has accumulated that they are present in young clusters, and have sometimes underabundances by factors of perhaps 100, and neither of those facts could be explained by the diffusion model which I proposed. Sometimes, diffusion has been criticised for not being vulnerable enough, so this is one case in which it is vulnerable, and it appears that this specific model based on diffusion is in serious difficulty. However, one should perhaps not exclude too rapidly that the same model would play a role in explaining the metal-poor A stars.

For Am and Fm stars there are considerable improvements in abundance data in clusters in particular, and I hope this will continue, since there is an age dependence of the anomalies, and this is an additional constraint which will be welcome. It can be compared to the more detailed evolutionary models taking diffusion into account that are becoming available. The expected abundances of more elements can be calculated and add constraints to the hydrodynamics of those objects, and I hope that the abundances of many more elements in the Am-Fm stars can be observed.

As for the helium-rich stars, it has been emphasized that important new constraints were coming from the radio emission and from the x-ray data. Contrary to what has been claimed, the suggestion that mass loss is involved is consistent with the models that are currently being proposed to explain the abundance anomalies. Indeed, it has been found that mass loss was essential to explain the observed abundance anomalies on helium-rich stars and on the cooler magnetic stars. It is only the magnitude of the mass loss that is perhaps causing a problem. Given the uncertainties of the models, this is not too surprising. It is very encouraging that the new evidence coming from the x-rays and the radio should be pointing to the presence of mass loss, just as the interpretation of the abundance anomalies is.

COWLEY: I have two different points that I'd like to raise. First of all, in connection with the separation of the gas and the grains, I can remember all of 15 years ago speaking with some great mogul who knew about the interstellar medium. I asked if it were ever possible for gas and grains to be separated, and I was told that this was quite impossible, because the grains were charged and the charged grains were then coupled to the gas, and that would couple with the magnetic field, and there was no possibility of this kind of separation occurring. Now possibly, at that time, the notion of small differences which we've been finding among the λ Boo or metal-weak stars, differences of a factor of two or three, were not even accepted as real. If you told someone that one star was a factor of two more abundant than another they simply didn't believe it. Now it *is* credible, and perhaps even the theoreticians would allow separations of grains and gas that would account for differences in the abundances of stars one way or another by a factor of two or three.

The scenarios that seem to be considered here have been primarily ones where grains and gas are separated from one another rather close to the object itself, as in the formation of something like a planetary system. We know in that case, solid material surely is separated from gaseous material, and that solid material formed the terrestrial planets. And so people have had that picture in mind, or, interestingly, the picture where the grains were formed in the wind and forced away from the star. It seems to me that it's surely worth considering a rather different scenario in which we have a clump of gas, perhaps a thousand solar masses, and some grain segregation would take place, perhaps precipitation to the center of this large cloud, or perhaps separation in another way. Let me suggest that, while some theoreticians think it would be hard to separate grains from gas, if you read carefully enough you can find some that don't. I could cite a paper by Joe Silk, for example, that discusses precisely this: the precipitation of grains to the center of a cluster. Now if you have turbulence, you might get blobs of the order of a solar mass, or two solar masses, or five solar masses. One blob would be enriched in the grains and one blob would be gas that had been depleted in grains, and therefore the heavy elements. So to me, it's not inconceivable that within the big mass from which a cluster formed, you could have some blobs which would go on and form metal-weak or λ Boo stars. Others might form stars that were richer in heavy elements, possibly things that we might give other names to, such as Am stars.

Secondly, as far as the chemical separation hypothesis goes, it seems to me that the thing one wants to start with is the initial pattern of the abundances. It's critically important that we establish, as a function of Z (it would be great if we could do A, but let's just try to do it for Z first) of as long a string of abundances as we can get. A nuclear pattern which is perhaps the default pattern, the standard abundance distribution pattern, has in addition to the peaks we all know about, primarily two signatures. One of them is the odd-even effect, and the second one is that the abundances are correlated with their

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neighbors. That is, if you build up the elements, one from the other, by adding neutrons, then you'ld expect element Z to be correlated with element $Z+1$ and element $Z-1$. If you had chemical separation, the pattern that you would like to look for is the one where you have correlations of elements that are very distant from one another in Z, that is, perhaps things that fall along columns in the periodic table. In order to make this kind of a discrimination, we need to work for as long a string of abundance patterns as we can, and I think we may be finally working in this direction.

HACK: If accretion from interstellar depleted gas can explain the λ Boo stars, we ought to expect a correlation between the deficiency of elements and the reddening of the stars. Or maybe not? We should expect that reddening increases with decreasing abundances.

SHORE: Vega has no interstellar reddening, but it has a circumstellar disk.

PARTHASARATHY: The reddening that is seen in the post-AGB stars, objects with large dust shells like HD 161796 or HR 4049, is not very significant. We have looked at the *IUE* data for the 2200 Å feature, and at the $(B-V)$ color to determine this. Grains of the order of a few microns in diameter can give rise to neutral extinction, which is another reason not to expect significant reddening, and this may be one of the reasons why we don't see reddening in λ Boo stars also. The amount of neutral extinction is difficult to predict if the grains are in a disk, because it is then very dependent on the unknown disk orientation and geometry.

SHORE: There is a problem I have noticed at this meeting, because I happen to work in another astronomical community that does star formation. Unfortunately, people here seem to be using results for the diffuse interstellar medium indiscriminately to describe what goes on in a molecular cloud. Both human birth and star formation take place in the deep, dark places where it's nice and comfortable. It takes a long time for the grains to emerge out into the world. When they do, they don't necessarily look like the grains out of which the stars were formed. But molecular observations show that whenever the densities are above 10^4 cm^{-3}, you always see the very heavy metals, at least heavy by molecular standards and the interstellar medium. So it doesn't look like you get any large metallic segregation or oxygen segregation in the denser places of the medium where the star formation is occurring. What happens when you get the stuff out into the diffuse interstellar medium is, these poor grains get exposed to ultraviolet and to cosmic rays differently from when they are in the molecular clouds, and the abundance patterns of the diffuse gas no longer directly reflect what the stars were actually made out of, but they reflect what the molecular clouds actually evaporate into. This is a problem that people in that community have not yet solved! You can't necessarily take what you see at 1.0 cm^{-3} and say that it's characteristic of something which is Jeans-unstable at a density of 10^5 cm^{-3}.

A second point is that ambipolar diffusion is the norm, not the exception in the interstellar medium. In fact, it's argued (at least west of the Rockies in the US) as being the primary cause for star formation, and the thing that maintains the stability of molecular clouds until the final formation.

But the question I'd like to throw out in general is something that a few of us were talking about the other night. If you look at the distribution of the periods of the Am stars, there is something that is very striking. Their periods are almost exactly the same as the RS CVn stars, that is, the Am stars have periods that are very frequently in the range of about one to two weeks, and mass ratios that are very near unity. When these stars grow up and leave home, are they going to appear in the Galaxy as RS CVn stars, and can we learn something about the later evolution of that class of stars by studying the Am stars? In other words, this is part of the general question: What do the descendants of the chemically peculiar stars look like? We don't know where they come from; could we at least say something about where they are going?

KURTZ: That same question can also be asked of the chemically peculiar stars of the upper main sequence and the barium stars. That problem is still outstanding. I don't intend to respond to that.

DEMIRCAN: Well, I'm not in this field; I work on the problems of very close binary systems. These are contact binaries. They show fast rotation (100 – 300 km s^{-1}) and a magnetic field, with common convective envelopes. Rotation-independent magnetic activity is explained in terms of the saturation of magnetic features on the surfaces of the component stars. More important, the mass transfer from the less massive to the more massive component is irreversible, and this requires increasing orbital period. However, this never happens; it is believed that a large amount of angular momentum is lost from the system due to the magnetically-driven wind, and this keeps the components in contact. We predict very large Alfvén radii, of the order 100 $R_\odot$ for these systems. Theory predicts that the components of contact binaries come closer and closer, and eventually merge to become a single star. Such stars should exist, mainly at spectral class A. Unfortunately, apart from the fast rotation, we do not know how to distinguish them. In time, they may decelerate by magnetic braking, and could form a group of peculiar stars. I am not aware of any work on evolution or the possible connection between such merged binaries and peculiar stars, and I would like to hear comments on this idea.

STEINITZ: I have a very short comment. As a theoretician, I remember the time that Babcock first discovered magnetic fields on sharp-lined Ap stars. The general belief then was that these were magnetic stars. Today we know that this is not the case; there is a much wider class of such stars. Yet listening here, and reading the literature, I am a little bit frustrated. I see that we get a lot of information, a lot of details. What I am lacking is a general pattern which will help me to understand what is happening. One example is what Helmut Abt talked about: there is a different velocity distribution of some peculiar – I would call them "sick" – stars in the Northern sky. I do not know whether this is related to their peculiarities, but it gives a hint. May I encourage observers to work with theoreticians, and ask questions which are related to discovering patterns.

KURTZ: Your encouragement is well received. I think there are many people in this audience who are trying to do just that.

STĘPIEŃ: I would like to raise a point which has been barely touched on, and this is the problem of the slow rotation of Ap stars. Actually, we have no idea why they rotate slower than the normal stars. Today we have seen some interesting new results concerning the existence of magnetospheres and perhaps stellar winds. However, one can hardly imagine that stellar winds and the loss of angular momentum via winds can explain slow rotation of all Ap stars. There are so many problems! For example, the observed periods of rotation are of the order of one week, roughly, independent of spectral type. Now, if the Ap stars lose angular momentum when on the main sequence, then that can happen

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only for a timescale of loss of angular momentum very accurately tuned to the spectral type. Otherwise, we would have a dependence of the period of rotation on spectral type and we don't observe this. In other words, I think that, while it is quite possible that magnetic winds can operate, particularly in hotter stars, at present it seems unlikely that magnetic winds are the sole cause of slow rotation. One can think that perhaps they lose angular momentum before reaching the main sequence. This raises problems which have already been discussed in the literature, and I won't repeat them. I would like simply to indicate that this is one of the still unsolved problems.

SHORE: I would like to urge people who are interested in such questions to look at the coupling between the history of the star in the pre-main-sequence phase, and what it does on the main sequence. There really hasn't been much work in this area. I remember a paper in the mid-80's by Obuko, and some papers by Spruit and Mestel, and that's about it. If you think about the fine tuning, the length of time a star spends on the radiative track while contracting, and the amount of mass loss while there, are both mass-dependent. It may be that we are seeing a convergence phenomenon as the stars go to the main sequence. This pushes the problem back to the birthline, and there are a lot of fields that are finding this to be a problem, such as binary stars. We are realizing that the initial conditions are set a lot earlier than we used to believe was the case. Looking back to the 1975 Vienna meeting, it is striking that the pre-main-sequence phase and phenomena close to the main-sequence were simply not discussed there. It was as if everything happens while the star is in hydrogen core-burning, and the world before and after just didn't exist. If there has been any dramatic change, it is certainly that we are starting to discuss this seriously. I echo what Dr Stępień said; it's very important.

GRIFFIN: May I add a note of caution? I think it was you, Don, who attributed the considerable advances in this field to the fact that observations nowadays are improving considerably in quality and in wavelength coverage. Now, if we assume that the numbers of papers produced by observational astronomers goes monotonically with the amount of observing time they get, Dr Jugaku has shown that the number of observational papers in this particular area of stellar research is relatively falling. Watch out! The handwriting is on the wall – jazz-up your observing proposals, because you're not getting as much observing time as the other people in other stellar researches.

BROWNE: I have a general question. As I understood it, the irregular variables were the largest proportion of magnetic stars, yet there has been very little said about them. People tend to talk about periods of magnetic variation, but what about those stars that don't show any respectable period? If they are a large fraction of the total, why has there been no discussion of them?

HACK: I have two questions for theoreticians to answer. First, we know of several binaries, with components of similar mass, spectral type, and rotational velocity, but with very different abundance anomalies. Why should this be so? Second, the stars belonging to the same open cluster, with the same age, born from the same cloud: why do some have strong magnetic fields, and others do not?

KURTZ: You are emphasizing the point that we still have parameters governing the line strength anomalies that we have not yet discovered.

ADELMAN: I have responses to several people. First, to Dr Demircan, who asked about contact binaries. In clusters, a number of the blue stragglers have peculiar spectra. In the Hyades, 68 Tau is an example. Analysing such stars is useful from your point of view.

Dr Browne asked about irregular variables. I think the feeling has been that if these stars are looked at closely, within the framework of the oblique rotator theory, then everything is consistent with these stars all being periodic. It has been more of a change of paradigm than anything else. We also have many problems dealing with events which occur only once; periodic phenomena are easier.

This whole business of the approach to the main sequence is very, very important, and I think it's going to be necessary to look at Ap binaries formed in common envelopes *vs.* those which were further apart. These are important for observers and theoreticians because the ages of the stars are more or less known.

The work of Drs Holweger and Lemke is very important, and I think it important to extend this and also to look at other types of stars, such as HgMn stars, where there are many abundance studies, and compare them. The work on x-ray and radio variability is also very interesting, although it is outside my field. The most exciting thing I have seen here from my own research area is the work of Dave Leckrone and colleagues on high resolution HST data.

COWLEY: I'd like to ask Herman Hensberge if there is any evidence at all of *any* irregular magnetic upper-main-sequence stars? As far as I know, all observations are compatible with a rotation period, even if that turns out to be very long. I know of no evidence for irregular variation. Can you elaborate?

HENSBERGE: I think that we can say that if you take all the studies that looked long enough, and with enough accuracy at the same star in the same photometric system, then people were able to find the period when it was above the limits of the accuracy. And, if the period that was found was long, then the star appeared *always* to have very sharp lines. In the data I presented in my invited paper, there were some broad-lined stars where we looked for 10 years, and not one broad-lined star showed any long-period variation, or any irregular variation. So I think that we can say, if the variation is not periodic over 10 years, we could set upper limits of typically 0.003 mag or so.

KURTZ: You [the audience] are beginning to look very tired to me. It has been a long week. The title of the meeting of course was 'Peculiar versus Normal Phenomena in A-Type and Related Stars', and certainly what I've learned this week is that the peculiar stars are in the majority on the upper main sequence. You all know from your statistics that that is what defines the norms, so the peculiar stars are normal! And I think with that, we bring the discussion to an end. Let's go have some coffee!