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Reminiscences: Exotic Stars and Exotic Elements – An entirely Self-Serving Document

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Abstract

This article is an expanded version of a talk given at a special session of the ASOS-8 colloquium, celebrating my 70th birthday.

The University of Virginia, where I was an undergraduate from 1951–1955, was known primarily for its astrometric programs. However, I began research work with Alexander Vyssotsky, classifying low resolution spectra. It was always fun to find a star with an unusual spectrum. Eventually, I learned that stars with unusual spectra typically had interesting chemical compositions. My earliest research work was concerned with finding chemically peculiar stars, barium stars, and peculiar and metallic line stars. Much of this work was done in collaboration with Anne P. Cowley.

The trouble started at the beginning of the 1970's when Margo Aller told me she was working on a star named HR 465 that had about every rare earth element there was. At the time, I had a brand new nuclide chart, and I could see that one of the lanthanides, promethium, had no stable or long-lived isotope. So I suggested she see if she could find lines of Pm II. The longest-lived isotope, ^{145}Pm has a half-life of only about 18 years. Wouldn't it be a gas if it were present. Well, it looked like lines were indeed present at the expected positions, so we sent a letter to the ApJ suggesting the possible presence of promethium in HR 465. It was published in 1970.

We could never convince our colleagues that Pm II was actually present in HR 465. To tell the truth, the evidence was not very convincing. I spent the next ten years looking for a star that had more rare earths, and might show demonstrable evidence of Pm II. In the 1970's I was able to use the superb facilities of the Dominion Astrophysical Observatory (DAO), and will always be grateful to Director K. O. Wright and colleagues at this institution for help of various kinds. The one star that might have as many rare earths as HR 465 was discovered by Antoni Przybylski, and has the designation HD 101065. It was too far south, and too faint to be observed from the DAO. But there were lots of other stars with interesting and unusual chemical elements.

While we didn't find Pm II, we had a grand time searching. The DAO spectra were state of the art. I would measure all of the wavelengths that could be seen on the plates. The measurements were analyzed by an automated program that tested them against a battery of laboratory wavelengths assembled by my colleagues and me. The idea was to see if there were a "significant" number of coincidences of the laboratory wavelengths with the stellar values. We made colored periodic tables that indicated whether the evidence for an element was overwhelming, suggestive, or absent. After a night's observing and a day's (or more) measuring, it was always exciting to see the results of the analysis come chugging out of an old line printer. Then we'd add another colored periodic table to the collection.

The most common pattern that we found among the rare earths was that cerium was usually identified at the highest confidence levels, followed by the other even-Z lanthanides with decreasing levels of confidence. Practically, what this meant was that a larger percentage of the Ce II lines in our search list would be found among the stellar lines than those of Nd II, and more Nd II than Sm II, and so on. Only stars that were very rich in rare earths would come up with high levels of confidence for Dy II or Er II. HR 465 was an exception, but the spectrum was variable, and the extreme rare-earth phase had a 20-year period. Between the maxima, the HR 465 spectrum was quite similar to those of other Ap stars, like HR 4816 or HR 4854.

Europium, with its half-filled 4f shell has a simple spectrum relative to its lanthanide congeners. The statistical technique we used to identify species didn't work well with such spectra. Nevertheless, the Eu II lines were extremely strong in some of these stars. Indeed, abundance studies seemed to show that europium was the *most* abundant of the lanthanides. This was not in line with our results. But the strong Eu II lines at 4205, 3819, and 4129 A were *very* strong.

One star, β CrB, had such strong Eu II lines that it seemed possible the common picture of lanthanide abundances might be correct, at least for it. Was there really more europium in β CrB than the even-Z neighbors samarium and gadolinium? If this were really true, the implication for the origin of the chemical peculiarities of these stars was profound. The nuclear processes thought to give rise to the chemical elements invariably produce more even-Z than odd-Z elements. A violation of this pattern would give strong support to the dominant non-nuclear theory for the origin of the chemical peculiarities in Ap stars—slow, gravitational and radiatively driven diffusion currents. Since I still had hopes that we were correct about Pm II in HR 465, which would imply recent nuclear activity, I wasn't happy with an "odd-Z anomaly" at europium.

There is an entry in my notes from 2 June 1973: Could Eu abundances in Ap stars be due to hyperfine structure and/or damping that is different for Eu and other rare earths. Mark Hartoog, Marc Allen, and I [1] eventually showed that neglect of the hyperfine structure in Eu II had caused much of the europium abundance excesses in Ap stars. The question may not be entirely settled to this day in β CrB, because of the additional complications due to the magnetic field and atmospheric structure of this star.

Shortly after the work with Eu II, it occurred to me that another abundance anomaly in peculiar stars, this time in Ba II stars could be resolved in a similar way. The Ba II stars, or barium stars, showed excesses of heavy elements that could be attributed to slow (s-process) neutron addition. Comparisons of the calculated

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nuclear pattern with stellar abundances were very suggestive, but praseodymium showed up as a badly discordant point. Marc Allen and I were able to show that inclusion of the hfs in Pr II resolved the anomaly [1].

We did not discover the significance of hyperfine structure for stellar abundances. I knew of Helmut Abt's work in 1955 on hfs in the sun from Unsöld's textbook *Physik der Sternatmosphären.*

Marc Allen did something most astronomers hadn't bothered to do. He read the papers by nuclear astrophysicists, and learned how to do his own s-process calculations. This was a significant step. The nuclear astrophysicists had presented their abundance calculations as a function of mass number, *A*. There were good reasons for this, but in the case of stellar abundances, we observed the elements, and could only present our results in the form of abundances vs. atomic number, *Z*. As a result, astronomers resorted to trying to force their results to fit the plots with *A* as the independent variable.

Following Marc Allen's work, an undergraduate student, Peter Downs, and I made s-process calculations relevant to the barium stars. Since we did the nuclear calculations ourselves, it was straightforward to add the abundances from the various isotopes and present the results vs. *Z*, which could be directly compared to the observations. Not only did we get a better fit than earlier attempts using *A* as the independent variable, but we showed that in the case of one star, which had sometimes been used as a canonical s-process case, the fit was actually rather poor. Nowadays, it is standard procedure to use *Z* as the independent variable in such work.

While "quick look" results from the DAO spectra were fully automated many elements could not be identified using wavelength coincidence statistics (WCS). Standard identification techniques had been applied to Ap stars by numerous workers, but the most skillful perhaps, was William P. (Billy) Bidelman. He produced identification lists for a variety of these stars, listing measured wavelength, strength of the stellar feature, and the suggested atomic or ionic identification. Bidelman's skill in dealing with these complicated spectra was uncanny.

Most of the features in the blue-violet, which our DAO spectra covered, were blended. Therefore, there would be two or more possible identifications for a given feature. Usually with strong features, the identification would be unambiguous, but for ever weaker lines, the problems became manifold. The laboratory intensities were rarely more than eye estimates of the blackening on a photographic plate, and thus of little help with subtle differences. In addition, nonlinearities in the stellar line strengths themselves made direct comparisons with laboratory intensities difficult. Suppose you decided a given stellar feature could be due to a line of Cr II. So you would look to see if other lines in the same multiplet were present. It could be that there was only one other usable line in that multiplet, but it also was blended. Then you had the problem of deciding whether Cr II or some other feature dominated the second blend, and one could quickly get into a frustrating regression. By the time I'd find an unambiguous identification, I could forget the species I was originally working on!

Somehow, Bidelman could follow these regressions, and not forget what he was doing, or perhaps he had some other secret. In any event, what I discovered was that in the case of complex spectra, where WCS worked reasonable well, our results agreed with him.

One of the most perplexing problems in the study of these stars was the presence of *strong* lines that couldn't be identified. I used to compare tracings of the spectra of Ap and Am stars using a light table, and even though they were all A-stars, there would be strong lines in some that were weak or absent in others. Eventually, many of these features became known. Most of them were either lines from iron-group elements, or third spectra of the lanthanides. Many lines of Fe I, Fe II, Cr I, Cr II, and occasionally Mn II or Co II that are strong in some Ap stars cannot be found in the Multiplet Tables, the astronomical spectroscopist's bible.

Sveneric Johansson and I have called these lines from common elements that are not in the Multiplet Tables "second-generation lines." A key to understanding them was provided by R. L. Kurucz and E. Peytremann in their monumental work, *A Table of Semiempirical gf Values.* This work provided far more than a large number of oscillator strengths of various merit. It introduced an entirely new concept, that one could use known energy levels to predict the wavelengths of the second-generation lines. Clifford Arnold and I [2] set about classifying a number of lines that had frustrated attempts to identify them.

Another source of identifications was provided by Henry and Hannah Crosswhite, who, along with their students, had studied the spectra of many lanthanides. I met them in the 1970's through my membership on the NRC's Committee on the Line Spectra of the Elements. Contacts that I made with the small community of atomic physicists have benefited me throughout my career.

The Crosswhites gave me unpublished material containing wavelengths of third spectra of the lanthanides, and it was immediately obvious that some of the very strong, unidentified lines in HR 465 were due to Nd III. With the publication in 1978 of Martin, Zalubas, and Hagan's energy-level tables for the rare earths, as well as subsequent spectroscopic material, much of the Crosswhites' material has been superceded. Nevertheless, their printer output of wavelengths and intensities are still the best source of information for lines that do not arise from known energy levels.

Mrs. Crosswhite, Chris Aikman and I identified lines of Dy III in the spectrum of the silicon star HD 192913 [3], even though the lines hadn't been so classified. Conway and Worden (UCRL-19944, 1970) gave an extensive list of Dy I and II lines, while R. Hussain, a Crosswhite student at Johns Hopkins, gave a list of Dy III and IV lines. Lines strong in HD 192913 that were in the Hussein list but not in the list of Conway and Worden, we assigned to Dy III. Work by Sugar and Spector as well as by J.-C. Wyart showed that we were mostly right.

What abundance information could be gleaned from the newly identified lines from the third spectra of the rare earths? Chemically peculiar A and B stars with strong rare earth lines ranged in temperature from perhaps 14,000 K down to (we now know) below 7000 K. A simple calculation shows that for the hotter of these stars, the dominant rare-earth ion was doubly ionized. Thus, mild rare-earth abundance excesses, characteristic of the cooler Am stars, were vastly exceeded in the hotter silicon Ap stars.

At the beginning of my career in the 1960's, there was a school of thought that attributed most if not all of the spectroscopic anomalies in chemically peculiar A stars to non-LTE. Today, we think non-LTE is important in many cases, but surely is not capable of reconciling the observed spectroscopic anomalies with solar abundances. But I wondered if some of the extreme anomalies found among the hotter Ap stars might be a question of non-LTE. Was the Saha equation predicting fewer singly-ionized atoms than there actually were in the star? This might account for large abundance excesses.

In the 1970's there was almost no information on oscillator strengths for the third spectra of the lanthanides. Nor did we know the relevant partition functions. A partition function can be even more important than an oscillator strength. The effect of one bad oscillator strength might be palliated if results were averaged with those for lines having good *gf*-values. But one bad partition function and the results for the entire spectrum, atom or ion, would be off. In the early days, the best partition functions were often guesses made by Bob Kurucz. These proved to be remarkably good, but for a few species there were sizable errors.

Through my membership in the Committee on Line Spectra of the Elements, I had the good fortune to meet Robert D. Cowan. His textbook on atomic structure has become our bible. He has put his computer codes in the public domain, and has been most generous in helping my colleagues and me with their applications.

It first occurred to me to implement Cowan's technique for using skewed Gaussians to estimate partition functions in complicated spectra. An early attempt along these lines [4] showed that the partition functions in Bob Kurucz's Atlas were too small for Nd III, Sm III, Gd III, Tb III, and Dy III. In a number of cases, Kurucz had improved the partition function estimate over that available from the known energy levels. The problem was that the analyses of the ions were severely incomplete.

As early as 1977, Bob Cowan had calculated energy levels in the third spectra of uranium and thorium which we used to calculate partition functions. These were then used to calculate line strengths of U II and Th II lines (cf. [5]), which could be used to estimate abundances. At that time, we did not know how to use the Cowan Code. Some years later, with Bob's help, I began to use his code myself. A summer student, Larry Barisciano, and I published partition functions for all third spectra of the lanthanides [6]. As of this writing, I do not know if these calculations have been superceded.

With the partition function problem in hand, if not ultimately solved, we faced the looming absence of oscillator strengths for third spectra of the lanthanides. Here, I had the good fortune to persuade my colleague Donald J. Bord to help me with the Cowan-code calculations. Our earliest results were presented as a paper at the American Astronomical Society meeting in Madison, WI [7]. We studied Ce II and Ce III abundances in the hot Ap star HD 200311, using spectra kindly provided by Saul J. Adelman. We found an excess for cerium of 5 orders of magnitude above the solar value. It seemed that non-LTE was not responsible for the large excesses. But there were more surprises from the third spectra of the lanthanides.

Bord and I reported on abundances in two stars based on oscillator strength calculations in Nd III [8]. For one star, Gamma Equ, we found the Nd III lines were giving an abundance significantly higher than values obtained from Nd II – a result that went in the opposite direction of what I had expected. I filed this discordant result in the drawer with other unresolvable anomalies, and actually forgot about it until reminded by my Russian colleague Tanya Ryabchikova, who had made detailed studies of the rapidly oscillating, or roAp stars. It is now generally accepted that these stars show ionization anomalies, especially of the lanthanide rare earths. These anomalies can be explained in terms of non-standard model atmospheres which are chemically stratified.

Extensive calculations have now been made of oscillator strengths and put in the public domain by Émil Biémont and his coworkers at Mons University in Belgium. This work dovetails nicely with the studies of Tanya and her associates of the roAp stars.

The first roAp star, discovered by Donald Kurtz, was HD 101065, Przybylski's star itself. Colleagues at ESO, Drs. Gautier Mathys, and Swetlana Hubrig have made spectra of this star available for line identification and abundance studies. We were able to set to rest a long-standing controversy over the presence of Fe I in the star's spectrum. Przybylski had claimed that evidence for Fe I was unconvincing, and he presented quite powerful arguments to this effect, which were based on the standard technique of looking for the presence of the strongest lines of the species. Others, particularly Gary Wegner, and David Petford had claimed that Fe I could be seen in the less-cluttered visual spectrum of the star.

It is now generally accepted that Fe I is present in HD 101065, but it is weak, and its abundance is sub solar, though by factors of perhaps 3 to 10 rather than at least 100, as it appeared to Przybylski, and at one time, to me (cf. [9]). High-quality spectra make it clear why this was so. It is simply a matter of spectroscopic clutter. The overwhelming strength of the lanthanide spectra obscured the weaker Fe I features in lower-resolution material. We note, however, that a study by WCS of a modest-resolution (9 Å/mm) Cerro Tololo plate taken by Anne Cowley [10] revealed the clear presence of iron-peak elements, at least to those with faith in the technique. We did, however, believe in a lower iron abundance than has recently been found.

It has been my good fortune to collaborate extensively with Don Bord. He quickly became far more proficient with the Cowan Code than I. With his help, I was able to return to an early area, solar abundances. We published several papers on abundances of discordant or difficult lanthanides (cf. [11]). It is a pleasure to thank Nicolas Grevesse and Jacques Sauval for advice and support in our solar work. A most welcome source of high-quality laboratory data has come from James Lawler and his collaborators. It has been put to good use (cf. [12]).

I have enjoyed the occasional divagation into atomic physics and its application to astronomical problems. I found selective excitations, such as those explained by Swings, Struve, and Bowen, quite fascinating. I only found one example, in a solar flare [13]. More recently Manuel Bautista and I were able to show that a broad absorption feature in the ultraviolet of early stars was due to the quasi-continuum caused by an inner electron jump in Fe II. Work like this won't upset the mass balance of the universe, but it is rewarding.

Edwin Hubble, the great pioneer of astronomical cosmology once wrote that "I end as I began." This may be one of the few things we have in common. I began intensive work on Ap-star spectra to see if I could find support for the promethium identification Margo Aller and I suggested in 1970. While a small number of results were found at the 0.05 significance level (1 in 20), I had studied nearly 200 stars, and used more than one Pm II line list. A few marginal results were expected by chance. Until 2003, I had never found any Pm II coincidences at the significance level of 0.01 or higher.

HD 965 was suggested as closely related to Przybylski's star by Gautier Mathys. We had worked together on HD 101065, and it seemed useful to have a detailed look at the HD 965 spectrum. Abundances were difficult to obtain because of the magnetic field, and the anomalous Balmer lines, which indicated the atmospheric structure could not be matched by a standard model. I summarized work Bord and I had done in an email to Gautier just before the IAU in Manchester in 2000. Of the trace species, there was a result for one of the Pm II lists at significance level 0.015, along with the comment "if real, nobody would believe it."

Trouble started again, some time in the summer of 2003, when Bidelman suggested that Tc II might be present in HD 101065. He had been adding identifications to our measured line list for that star. When the abundance study was done, we had only the region (3959–6932 Å). We then got additional ESO spectra from Swetlana that extended to 3291 Å, and it was while Billy was making identifications in this region that he thought there might be evidence for Tc II. In testing for the possible presence of Tc II, I ran a number of WCS trials using tolerances of 0.01, 0.02, 0.04, and 0.06 Å. These runs gave only marginal support for Tc II, but gave much stronger support to my old bugaboo Pm II.

Then I made the additional mistake of doing additional WCS tests on HD 965, and here, one set of measurements gave such strong results that the Pm II question had to be reopened. There seemed nothing to do but write the results up, in spite of predictable skepticism from colleagues. A detailed account is given in [14]. Thirty years experience in doing this sort of work has left us a little smarter than we were in 1970. I am pretty confident the coincidences we found in both HD 965 and HD 101065 were not due to chance.

There are two plausible explanations of our results. First, some kind of flare activity on the stars could produce and replenish the short-lived nuclides. The second plausible result is that our laboratory wavelength lists were contaminated. It turns out that astronomers don't like the first possibility, and laboratory spectroscopists don't like the second.

We will continue to measure and analyze the spectra of stars, making use of the superb material now available from large telescopes and powerful spectrographs. I am most grateful to colleagues who have shared their spectra with me. And there is much to learn. Billy Bidelman has admonished me not to take too seriously the opinions of those who think they understand the chemically peculiar stars.

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