The rise and fall of beam-foil spectroscopy

- A history of its brief time -

This is a collection of anecdotes and gossip. Of course, the collection represents only a minor fraction of the stories floating around when (ion-) beam-foil / beam-gas / beam-laser colleagues meet socially. Since the first call for contributions to this collection went out via electronic mail, colleagues whose addresses were not known or who did not have e-mail at the time were missed. Unfortunately a number of the people involved in this field, particularly in its infancy, have explicitly denied to contribute anecdotes on some prominent people whom they consider as too influential as to dish out stories about them (although they clearly hinted that they would have stories to tell!). Maybe time will make those oral history documents available after more than 30 years ... and bring to the light the stories about those special characters as well.

Most of the stories are based on hearsay and have been filtered through the editors' imperfect memories. (American readers are advised that the language may be rough (non-PC) in parts, and that parental guidance may be warranted for the meeker souls.) There are vast gaps, many of them obvious from the notes below. Since my fellow editor, Indrek Martinson, the kind spirit and international communicator, has died in December of 2009, please send your comments, corrections, complaints, and complementary material to me at traebert@astro.rub.de.

E. T.

The late years

Late in the 1980es, the users' community (astrophysics/plasma physics/atomic structure theory) begins to realize that there are good and even unique data available from fast-beam spectroscopy. The turn-around of perceptions was perhaps formally indicated by Lady Anne's (A.P. Thorne of Imperial College (London), a precision spectroscopist with classical light sources) talk at the Third International Colloquium on Atomic Spectra and Oscillator Strengths for Astrophysics and Fusion Research (ASOSAF), which was held at Amsterdam in 1989. In a British way of demonstrative and decorative understatement she referred to some recent good data from ion spectroscopy quarters, and all the beam-foilists in the audience got the point and felt slightly elevated for a moment. Hardly anybody else noted - a drawback of the figures of indirect speech.

The same data were referred to by Indrek in a note to Elmar, reaching him on a visit to Stanley's:

> 4-Apr-1991
> Hej Elmar. We all envy you, just thinking of Tucson, the good friends there, the fine weather, etc. I have been thinking of BoLund, too [The Bochum-Lund collaboration meeting, first at Lund in 1987 (?), a year later at Bochum). .... Well, here is another idea. We have been thinking of a small conference on laboratory astrophysics some time at the end of this year. Then the FTS (evidently this means something different in US [Fourier transform spectrometer vs. Federal Telephone System]) instrument will be here, and we could have a celebration. So perhaps we could invite you and Herr Heckmann (+ Frau Träbert + Frau Heckmann to Lund), to celebrate the outstanding Lund-Bochum collaboration. I still think of the solar Fe XIII and
> Fe XIV lines that were discovered in Bochum, at a gravitational potential of -10 m, well below zero. Indrek
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> [Realizing that these intercombination lines observed at the Bochum accelerator coincided with unidentified lines in the solar corona spectrum, was Indrek's merit. Here his early involvement in the field and the mental cross-links with the astrophysical community paid off. The much more precise wavelength measurement was a boon of the solar observations, but the accelerator-based work yielded the identification of the goodies. By the way, the accelerator lab is only one floor below street level (and above ground at the Botanical Garden side, so the discovery was made at -3.5 m, perhaps.]

Things happen

Roger Hutton incidentally opened electronically controlled valves at the Lund Pelletron accelerator, but he overlooked a manual shut-off valve, and diffusion pump oil rises into the beam pipe. Elmar Träbert remembers a comparable incident from Bochum where a three-way valve admitted atmospheric pressure to the forevacuum side of a hot diffusion pump. Funnily, the expanding oil vapour imaged apertures onto surfaces in Hans-Heinrich Bukow's nearby beam-foil chamber. Bukow was not amused, for sure. By a quirk of physics, the „culprit" chamber itself suffered no oil damage – it remained visibly clean. Indrek recalled that Berkeley had achieved a similar oil excursion during an experiment years before, and he called it beam-oil spectroscopy. The technique is good, because no foil breaks. However, you cannot measure f-values this way, because you took the „f" out (of the foil ...)!

Funding declines steadily towards zero

Atomic physics had its surges and declines throughout the twentieth century. After the Bohr model, Schrödinger and Heisenberg descriptions and the development of quantum dynamics led to a basic understanding of the atom (i.e. the electronic shell structure), nuclear physics became the rage. Atomic physics was briefly rediscovered in the late 1940es, when radiative corrections to the (relativistiv) Dirac description proved necessary and initiated quantum electrodynamics. Again the field fell largely dormant until the development of lasers (for studying mostly neutral atoms with extreme precision) and the need to understand the lack of progress with controlled nuclear fusion experiments using hot plasmas. Space-borne observations of the sun and the diagnostics of hot terrestrial plasmas both needed data on highly charged ions, and beam-foil spectroscopy could help quite a lot. However, highly charged ions developed a life of their own, leading to devices (like laser-produced plasmas and Electron beam ion traps (EBIT)) which avoided the expense and complication of large accelerators in the reach for high ionization stages. On the laser side, lasers turned out to be useful for so many things outside of atomic physics that the latter profited from lasers, but did not receive a lasting boost for the field as such. Falling from earlier favors, beam-foil spectroscopy became an ageing, flailing section of fast beam spectroscopy. The latter dwindled in the number of laboratories and workers, in mutual interaction with the reduced funding. Work continued at some specialized small facilities and at ever larger accelerators left behind by the nuclear physics community on their way to extreme energies and particle physics. Such leftover small facilities have maintenance problems (and therefore they still cost money, beyond the people who need salaries to continue working even at cheap machines), whereas big machines are intrinsically expensive and, being few, are fiercely fought about for accelerator time. Fast beam spectroscopists thus become a travelling lot, spending time and money to get to the places where they may still do (often at higher cost and more effort) what they were used to do closer to home.

The additional obstacles are many: Competition for beam time at large machine needs longer-term preparation of proposals and funding applications, often involves long delays because of technical problems of such machines, gives rise to lobbying, clashes with the lobbies of other fields, extends project durations so much that the time scales of graduate students or fixed-term contracts of scientists are exceeded. Working away from home with limited technical support and the need for a different infrastructure is annoying, as is the time-consuming compliance with different safety rules and the like. Money saved by not supporting a machine close to home is not fully saved, as travel costs for extended preparations away from home can be enormous. Around 1978, Stanley Bashkin in a publication (Nucl. Instrum. Meth. 154, 169 (1978)?) noted the extreme benefit of multichannel detection using microchannelplates (MCP) in beam-foil spectroscopy: Data collection times might be reduced by orders of magnitude, and thus experiments exploiting the costly but feeble ion beams at high-energy accelerators would become feasible. In his subsequent experiments, Gene Livingston found out that for precision spectroscopy the microchannelplates available at the time had an appreciable dark count rate which limited the benefit much earlier than hoped for. Local nonlinearities of the MCP efficiency have since been largely cured and dark count rates reduced, but the typical read-out systems still suffer from severe distortions. In 1994, Finn Folkmann at Aarhus analyzed the time needed for preparation, calibration (before the accelerator run and afterwards), data reduction and error analysis when using an MCP-based detector in comparison to a single-channel detector on a mechanically scanning monochromator. While the actual data recording is much faster (easily a factor of ten - and that is decisive when facing the demand for scarce and costly machine time), the plethora of necessary off-line work reduces the time advantage to a factor of two at best. This is still something, but it comes at the cost (research funds!) of a high multiple of detector and auxiliary equipment prize and maintenance (soft- and hardware) efforts compared to, say, a simple channel electron multiplier (channeltron). Such complex detector systems need continuity in their use (there always has to be a capable graduate student around who is well-versed in recent PC-related electronics, as well as a workshop technician proficient in delicate electronics repair work), and this again forces small experimental groups out of this business.

Lack of money (plus other reasons) show even where "for the show" (to funding agencies and scientific boards) one has to demonstrate what one did even when nothing happened - the well-loathed Annual Reports:

The Swedish financial year runs from July to June. Quite regularly in the late 1980es and the 90es, the allocations for Lund were so much below the needs that the funds were practically exhausted before the end of summer (within a quarter after allocation!), leaving most of the year to gnaw fingernails. Not even the formerly regular annual reports could be kept:

> 30-Sep-92
> Hej Elmar. You and your buddy PLS [Peter Smith] have at least one thing in common, you ask about the Lund annual report. We did not issue anything in 1990, because we had a rough year economically and had to save as much money as possible. In retrospect this was silly, because in 1991 when things were looking better we were not alert enough to write one either. But this year we will definitely do it. Sven Huldt is the editor. [Note added in proof: No Lund

Annual Report on Atomic Spectroscopy in 1990, 1991, 1992, 1993, 1994, 1995, ... in later years similar declines in regular reporting happened at Bochum.]

[referring to a suggested correction of all calculated lifetimes in the Zn sequence:] Yes, I saw Alan's [Hibbert] paper, and he may be right. [Hibbert had remarked that if experiment and theory agree, neither is necessarily right - referring implicitly - kind Alan - to some joint Bochum-Lund work.] Shall we try to remeasure the Nb? [Indrek had been at Bochum for most of the data taking on Nb in 1990] Was it Holger Blanke who wrote a poem about Niob and the poor guy from the bible, Hiob? Is this the German spelling of his name? Job in Swedish. And I think it was the occasion we were speculating how it would have been if Goethe and Bettina v Brentano (or was her name later Arnim?) would have been sending e-mails to each other. There are indeed distinguished poets using e-mail, e.g. Stanley Bashkin. Indrek

Gene Livingston was turned down repeatedly by NSF. Some other people suspected they were being mistreated by individual referees, and in the uncertainty and frustration rumours spread of who might be the funding enemy of whom. Gossip like questions to somebody intending to go to Argonne with the tune "you are not going to do [Gordon] Berry-type physics, are you?" at least gave substance to some suspicions of adverse motifs. And people trying to leave Argonne were advised (even by Gordon Berry himself) that if they got a job, it was because people believed they would not do "his" type of physics. One such story is not quite beam-foil, but at least accelerator-related: Meyerhof at Stanford contemplated the regularly recurring grants discussion about setting aside funds for young physicists and drying up old ones. When he had submitted a proposal, he got three negative reports: Two referees said "Why do you still want research money at your age (barely 70 then)? Leave it to young people!". The third referee said "Meyerhof does not deserve to be funded. In an earlier project application, there were such severe faults, don't give him money now." Meyerhof later found out that the third referee had mixed him up - with Sheldon Datz.

When Elmar Träbert in his two years at the Harvard-Smithsonian Observatory struggled with very strange suggestions from Stanley Bashkin (on the interpretation of data from gases under ion irradiation), he met Ben Bederson. Over coffee, people chatted about such age problems in funding, blocking of positions and failing memory. Ben noted that an unnnamed colleague used to answer the question "What to do with old physicists?" by "They ought to be shot!" . In the winter thereafter, Elmar went to an informal conference with evening sessions, and when he told about this, the chairman and some others were quite upset. Meyerhof had been sitting in the front row, and they believed this to have been an insult for the old gentleman. (Yes, it is decidedly difficult to present some physics in 15 Minutes and to try to unstiffen the audience by making a few joking remarks at the same time! Träbert's impression was that he tried again and again, but never quite succeeded with physics professors in the audience - though some more down-to-earth people seemed to understand what he was trying to say.) Meyerhof himself was beyond immediate anger. For the next such meeting, though, in the year after, he phrased a long fun poem based on German author Wilhelm Busch's "Max and Moritz" illustrated story. In this poem he took up the notion of what to do with old physicists.

January of 1993 was one of the periods of hectic proposal-writing in Sweden. When Stanley Bashkin (who had just become double grandpa and now is entitled to the address "Stanley the Grand(pa)") enquired why Indrek Martinson had lessened the frequency of his letters to Tucson, Indrek replied about just having spent a frantic time with those proposals and meeting their deadline. This certainly left no time for other activities. It reminded him, however, of the fact that he had written his first beam-foil proposals 25 years earlier. The onerous task was not going to be more enjoyable by having to do the same over and over again, with diminishing returns. "The same holds with students' theses: In the old days, we tried to teach the students how to write a thesis or a paper. Nowadays we are trying to teach the students how to write - and do the paper writing ourselves, anyway." How true!

The number of people in the field dwindles away

Reaching limits: He-like uranium

Re-discovery of delayed spectra: Electron spectra (Aarhus/Bruch), X-ray spectra (KSU/Kauffman), EUV spectra (Bochum/Träbert)

Lifetimes good enough to challenge theorists on their home ground

Intercombination transitions

Storage ring work

Physics of multiply charged ions outgrows one of its roots, their conferences swallowing the fbs tri-annual meetings. After digestion little remains. Stanley revives a dormant branch of gas-excitation by ion beams.

Lasers

The long-standing competition of foil vs. laser excitation

The Wittmann/Gaupp/Kuske experiments in Andrä´s group and the Swedish answer from Gunnar Astner *et al*.
The Berlin group demonstrated lifetime measurements at the 0.25 % level of precision, using a laser and implying that this was the method so superior to anything without. Astner et al. found a case in He in which they could use quantum beats as a velocity marker; their result from plain foil-excitation was quoted with 0.26 % uncertainty. This was certainly a kind gesture and a wise move, though the difference is hardly real. However, Andrä acknowledged this respect ina conference contribution of his, and to this day beam-laser measurements are quoted as the top of the business and as being "much better than any beam-foil data possibly can". Well, much later beam-laser work at Kaiserslautern corrected some of the early Berlin superdata by more than the old error estimate. The new precision of 0.14% was almost perfectly matched by Heidelberg storage ring measurements on few-electron ions - with ion excitation achieved by ion collisions with free electrons (H.T. Schmidt *et al*.) or with the stripper gas in the injecting accelerator (Doerfert *et al*.) - almost a beam-foil experiment!

Lasers, too:

> Reinhard Bruch, after trouble with the Freiburg faculty, moved to Reno, Nevada. He inherited an assortment of accelerators which of course were claimed to be in good working order - meaning they needed a few years's work before anybody but their former masters could run them. He established contacts to many companies and thus could get in-time information on when they phased out fairly new equipment before it needed servicing or repairs. In this way he secured a wealth of impressive equipment: implanters, lasers, storage scopes, vacuum pumps, you name it, which needed little repair or service by an enterprising student to come to life. Thus he got around the low level funding and its problems for research and teaching. Of course it cost time and nerves and stamina, in particular for someone who has so many ideas and wishes to collaborate with so many people, with data arising from collaborations faster than the master at the controls could digest them into publications ...
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> Well, Reinhard got his machinery to work and began to get very nice data on ion-atom collisions, indeed. He then deserved a Sabbatical (1990/91), which he largely spent at Walther's institute in Munich (Max Planck Institute for Quantum Optics). As it happened, his first doctoral student, Stephan Fülling, after plenty of prompting, had finally written up and was due to be examined while Reinhard was in Munich. Reinhard took part in the examination via telephone, which was much cheaper and ecologically more sound than flying home or buying a video conference line.

> The time in Munich, however, was too short to see fruits of the newly started collaborations. Therefore Reinhard went to Stockholm for part of the summer (a recommendable move, because then the weather there is splendid and the institute (MSI) is fairly deserted. Sven Mannervik at Stockholm had then obtained quite involved laser systems, and he had put them to good use on ion beams already. After the warranty ran out, those lasers, though (or because ?) of German origin, needed service or repair. The engineer's time was to be paid at a rate of about SEK 800/hour (about $ 140/hour). However, the engineer was German, and Reinhard watched him working and then started to talk about the fancy lasers at Walther's institute, and the assortment of lasers at Reno, and so on. And poor Sven had to worry, because he was thinking of having to pay the engineer by the hour, for work, and not for keeping Reinhard company!

Side tracks and Black Holes : BFS data for Christer Jupén (Lund):

Christer is a superb analyst of spectra. Unfortunately he wants to be certain that he makes no mistakes (at least none that Bengt Edlén might spot, and that is hard). Therefore he solicits data, collects data, processes data, hoards data - but rarely releases them to the public. Someone aptly nicknamed him the "Alberich of spectroscopy" - sitting on the data treasures ... like the other Alberich on the Rheingold. This would be a good connection to Boel Denne. When she learned about her good salary at JET, she exclaimed that is was sufficient to buy a daily copy of *Der Ring des Nibelungen*, which before had seemed an elusive goal requiring long-term saving ...

Well, Christer repeatedly went to JET. He lateron enjoyed having a single job for a time when he was for a month or a few working at JET. His regular life at Lund consisted of a large percentage school teacher's position, and the variable minor fraction working in spectroscopy. However, the funds for the latter often were uncertain (and at times used by the boss to squeeze Christer in order to produce papers, not just analyze data and then let them acquire dust - hoping for later additional insights and fearing the need for a reanalysis), and it certainly is not too helpful for anybody interested in science not to know whether there will be any funds at all for the joy of late evening work on dreary data. On the bosses side, of course, one always feels short of appropriate means of reaping results of "subordinates'" purported work.

Lifeline of BFS: Foils

Arizona Foil Company (The Stoner-Bashkin War), Israel vs. home-made, ?

One of the backbones of BFs, needless to say, is THE FOIL needed to pass the ion beam through. This foil has to suffer a lot, even as usually not more than a single projectile ion is inside the foil at a given time. Standard material for availability, handling and sturdiness is carbon, although Be, Al and others have been tried in places, for specific effects or naïveté (Al foils melted in a second in Bochum ion beams ...) One clean way of producing such thin carbon foils is by evaporation of carbon which is heated by an electron gun. This procedure yields fairly uniform, smooth foils, but is very time consuming. And foils have a habit of breaking too early, anyway. Often they break already when one tries to float them off their production carrier and before mounting, others die when drying. Some survive hard shocks (accidental dropping to the floor), but break when first exposed to vacuum. They wrinkle and then straighten again when exposed to the ion beam. There are recipes on how to mount them with less strain, pre-slacken them, preheat them by laser irradiation, on how long to cure them before the experiment and whatever else. Foils remain an enigma.

Among others, Bukow at Bochum (and his students) undertook to find out what foils look like microscopically, in order to find out how they change under irradiation and then to improve their performance. Electron microscopy revealed their internal structure as that of turbostratical graphite (Now you know!). This particular diffraction pattern, however, was not among the already hundredthousand or so samples collected at the Jülich Nuclear Research Center where they study graphite for reactor purposes. The density of the carbon foils proved practically impossible to measure (Ulrich Sander): voids in the material probably captured gas, and floating in a liquid of approximately equal density, weighing, optical transmission (Matthias Holl), Tolansky thickness measurements (R.

Berger), a-particle attenuation, and whatever else led to discrepant results. Because it was found that the foil structure changes under ion irradiation, the same was tried with a home-built CO_2 laser, and those laser-preheated foils did last longer, indeed. Instead of minutes, Bukow's experiments in the few-hundred keV range could now run for 20, 30 minutes without a foil change. At the tandem accelerator, the Heckmann / Träbert group was relieved to have foils lasting up to two hours instead of 20 to 30 minutes. However, along came progress with the invention of producing foils from a discharge in which hydrocarbons were cracked. Theses foils were quickly produced, lasted much longer than the old ones, were found to contain lots of hydrogen. This latter might account for some of the reduced damage, by evaporation etc., but details remained unclear.

Bukow built an apparatus for cracking following the design of a Heidelberg device. Stanley Bashkin, co-owner of Arizona Foil Company, one of the world's leading suppliers of carbon foils, advised on some details when he stayed at Bochum for half a year in 1982 (and translated the Heckmann / Träbert book (based on Heckmann's lecture notes) "Einführung in die Spektroskopie der Atomhülle" into English. It took only 8 years to condense into print, because of transcription problems, changes of the word processing systems, and the like. This then brought an end to the happy years in which Indrek Martinson at Lund and Larry Curtis at Toledo had ben able to set problems based on this book, knowing that the students would not be able to track the solutions in a German book.). However, though these Bochum foils were generally sturdier than their predecessors, they were of limited reproducibility. Whilst all techniques showed that within a production batch the foils were quite uniform, the batches varied drastically in durability, thickness and eye-optical appearance (smoothness, transmission, reflectivity). Whereas Bukow repeatedly claimed that he knew what the problem was (type, freshness and thickness of the backing material deposited on the carrier onto which the foils was to be deposited and could be floated off afterwards, solvent, hydrocarbon gas composition, pressure, discharge conditions, you name it) and how to cure it, his students often had the opposite suggestion. Till today it remains unclear whose arguments were better, since foil production at Bochum has still not evolved into a reproducibly reliable process ... At times, the variability of foil durability brought back reminiscences and specimens of foils which broke before mounting, during mounting, when drying, during evacuation in the experimental chamber, at first beam contact, or, in a few lucky cases, after half a day. A sample of commercially acquired foils (obtained for free from Lund, but of either Jerusalem or Tucson origin) was cherished very much at Bochum, because it lasted markedly longer than the home-grown variety - which, however, at the time was available for the begging, citations in papers and lending of equipment.

But those foils had marvellous properties, too: Schlagheck, in the old days, had mistaken a reflection at the foil (and thus appearing at a Doppler-shifted and not quite reproducible wavelength) for a long-sought decay to an autoionizing lower level (and consequently measured a lifetime orders of magnitude away from prediction, whereas Gordon Berry saw the same line in a quite different position with a very different lifetime. (The conflict was noted in the literature (by Indrek Martinson, of course), but it took Sven Mannervik's precision work in settling the problem and measuring the lifetime by an evaluation of the line broadening. This led to new conflicts, with Nicolaides, on the interpretation of lifetimes of levels close to ionization limits, fought in the pages of J. Phys. B.). The second Bochum foil feature involved Sven Mannervik as a visitor to Bochum. When aiming at a maximum signal from the very short-lived $2p^3\ ^4S^o_{3/2}$ level in Lithium (near 14.9 nm), foill holders with a surface tilted toward the spectrometer were used. However, the calibration lines (1s - 2p in Li III and 1s2p - $2p^2$ in Li II) seemed to show new satellites of about 10 % of the diagram line intensity. The beam energy was varied, and the "satellite" wavelengths shifted: Another case of reflection at the surface of a carbon foil. The reflection efficiency at a wavelength of 13.5 nm (more than 90 eV) was of order 10 % for grazing angles of order 5o. Not bad for a purpose-built reflector before the age of multilayers! For a prospective application to obtain SDI money, one would have exploited the effect, probably dubbing it Single-sideband Tuning of Ion-beam generated EUV-Radiation.

Those foil quarrels mentioned above were minor compared to what happened at Tucson. John Stoner, jr., and Stanley Bashkin had jointly organized the Arizona Foil Company. Practical production had for a time been done by Stanley's sons earning money by watching and running the apparatus. At some stage, however, the Stoner-Bashkin war broke out and raged, with mutual suspicion of strange behaviour, unfriendly actions, even claims of sabotage. This led to a split: Stoner became even more inaccessible by anybody in the faculty, Stanley in a settlement accepted to stay out of the company and to refrain from doing business with foils for five years. Stan then wanted to come back to the industrial stage, and he already had ideas on how to market special applications of such foils in non-BFS fields (Like at Aarhus, Ove Poulsen and his guys devised multichannel spectral detectors with optically grey attenuators made from simple carbon coatings.). However, the necessary risc capital for the BFS company (what a name!) did not materialize, but Stanley may still have something to do after retirement (anticipated for 1992, at age 69, and postponed again to 1993 ...).

By the way, Bochum was not the only place where the effect of the foil composition was studied. Effects like the surface roughness and orientation were attempted to clear up at a number of laboratories. Contradicting evidence abounded, on surface fields, temperature vs. beam density effects, bulk vs. surface, etc. Many effects proved to be localized in single laboratories with interaction lengths much shorter than the journey to any other laboratory. In all this was heat, fighting, competition and amusement.

When the Bochum chairholder, von Buttlar, retired, he was replaced (1990) by Claus Rolfs who had high merits in nuclear astrophysics but was strongly asked by the faculty to concentrate on materials studies instead. Atomic physics - like fast-beam spectroscopy - was to be phased out, obviously. Luckily for him, Träbert had been put on a temporary docent position by von Buttlar, and this way he could continue his work for a few more years while Rolfs urgently hoped to get this position freed for his own people. Bukow was on a staff position and had little choice; he ended his beam-foil work, transferred his home-built 2m normal incidence spectrometer to Liége and took charge of the microbeam facility. This change made him ask for money for future foil preparation, which in turn let Heckmann and Träbert opt for commercial foils. Heidelberg and GSI Darmstadt accelerator labs tried their luck, but their foils were no better than Bukow's while requiring a lot of coaxing and effort for having them made. For example, GSI's target laboratory needed certificates of the radiation levels of foil holders shipped to them - because nasty things happen when ions at the typical GSI energies strike any material. At Bochum energies and conditions, nuclear activation normally is of little concern.

After using up donated foils from various sources and collaborators, Bochum turned to Yissum in Israel, the research company of the Hebrew university. The S-type carbon foils looked good and uniform, but did not last better than the competitors'. Then Yissum offered SS-type foils (super strong) for a surcharge. Träbert ordered a box in time. However, when almost a year later one of the students, Jutta Granzow, tried to float these foils off, she failed. The foils curled up on contact with water - if they let loose of the glass slide at all. Yissum then recommended the same as Stanly Bashkin had at an earlier occasion: Expose the foils to water vapour for a time, then try to float. Somewhat better yield of floating, though still most foils broke before floating properly. Super-strong obviously implied that the foils clung to the glass slide very tightly. Anyway, Yissum claimed that they had no problem reports from foils being floated soon after production. O.K., Bochum wants to time the next purchase so that the foils arrive just in time when sufficiently many foil holders are empty and the foils will be used soon after. Yissum offers a free sample. Granzow succeeds in floating and mounting about 30% of the foil material - not bad, if the foils are better. But are they? Many show pinholes even when still on their glass slides, and this is lethal for some experiments, in particular lifetime measurements. Yissum comments that they have noticed the problem and are working on it, offering a fresh free sample. Yes, thanks! The new foils look much better, 60% can be mounted within a day after arrival. Of these some 15% die of mechanical stress within a week. Exposed to the ion beam, the SS foils last much less (but uniformly so) than the ordinary S foils, some 15 minutes compared to about 1 hour during the same accelerator run. Bochum will return to buying S foils, obviously, if ever there is money to buy any foils at all again.

So this is the straight way of technical development and progress with carbon foils. Maybe scientific progress apart from these technicalities follows a less winding path?

--- xxx (Version A of a sad story) ---

Lifetime measurements on low-lying levels in the Ne isoelectronic sequence - progress which way?

This piece of history has several parts, separated by (almost) decades and oceans. Part I plays mostly at Bochum, Part II evolves from Part I and concerns a tripartite collaboration which at first concerns scientific concerns about Part I and then turns into a knife fight of sorts. At some stages the ways of real progress (and of its perception) may become visible.

In 1975 W. Schlagheck (Bochum) published a paper on lifetime measurements done on the resonance and intercombination line in neonlike Na^{+} ions. Because foils break so rapidly with slow ions, he presented only a short section of the spectrum and two decay curves. Of the latter, one had been evaluated using a single exponential, the other with two. Errors quoted were 5% and 10%, respectively. Due to a printers's error, one of the errors in print read 0.5%, making the experiment appear as one of the most precise lifetime measurements ever.

In 1989, postdoc Jim Babb at Harvard-Smithsonian (Cambridge, Mass., USA) wanted to present very precise calculations on Ne-like ions, in the context of Casimir forces and the like, and found only Schlagheck for an experimental reference point for certain properties of the wavefunctions. He happened to discuss this with Elmar Träbert, at the time a visting scholar at the same institute, who ventured to suggest caution about those early Schlagheck numbers. Still, Babb said that they seemed not so bad in comparison to his calculations.

In 1993, Larry Curtis (Toledo) came back to the problem when trying to do his mixing-angle systematics to the Ne sequence. He was coauthor on a nice pair of data points for S and Cl (done at Lund), there were some less precise numbers for P and S, and some widely disparate numbers for Ne, plus Schlagheck's for Na. Larry managed to remeasure the shorter-lived decay for Ne at Toledo. Elmar looked up Schlagheck's thesis, found the above transcription error on the error margins, and reconstructed data files from Schlagheck's ink trace drawings. Evaluating the very same data with a few more exponentials (as seemed warranted by visual inspection) and considering the spatial resolution to be important for measurements of short lifetimes, Elmar found new results of the old data which differed from the old ones by about 30%, a multiple of the old error estimate. When he mentioned this to von Buttlar, the reply was: "That is a long time ago, what is it, 20 years? There you see!" So he blamed it to progress in science over two decades. What he did not see was the point that Elmar's evaluation program (quite standard, with no tricks at all), except for updates and patches, was 19 years old. All its basic features had been taken from Bevington's book which already was a number of years old at the time.

However, the story of progress continues, and it develops into a partial scientific nightmare. A one-sided report on this is the following fairytale on the lifetimes of the resonance and intercombination level lifetimes in the Ne isoelectronic sequence. (This part of the story had to be anonymized of sorts, and to be declared a fairy tale with only fictitious characters, although you anyway would not believe a plot like this to happen with mature people dealing with science - or would you?)

---zzz---

A new unit of smallness:

In 1992, Westerlind, Engström, Bengtsson and Curtis did the then precisest lifetime measurements on the resonance and intercombination lines of Ne-like ions of S (Z=16) and Cl (Z=17). Three years later, Larry Curtis (also a coauthor) tried to use these data as the pivoting point for yet another of his famous mixing-angle analyses. In preparation for this task, Larry had solicited new data from Bochum, which were forthcoming for the singlet level in ions of Na (Z=11) through P (Z=15). Träbert, however, found that the triplet level decay curves he recorded could not be positively evaluated because a tricky cancellation of the primary decay and the cascade slopes distorted the curves too much. For higher Z, this would not be a problem, and the S and Cl triplet data from Lund matched a recent careful (and slightly fine-tuned) calculation from Belfast (Alan Hibbert). The singlet numbers did not agree, and this was suspicious, because the two values for each ion are connected via atomic structure. Well, Lars ascertained that those (their) numbers were very good and beyond any doubt. When Larry notified him of the draft of a paper largely based on these very data, Lars informed him that by extended measurements and re-evaluation there would be a change of the singlet lifetime result for S. His message started with a notion that they had reduced the lifetime "somewhat". In his proposed section to be added to the joint paper this was phrased as "a small, but significant reduction". In a discussion with Träbert this was recast as "the reanalysis had a very small effect" ... "An even better ANDC analysis is almost impossible to make ..." "...it is now up to theory to fix the discrepancies!!"

Well, the change made was by almost 20%, or six times the previously quoted error margin, and the new error margin was twice as large on an absolute scale and even mor on a relative one. So this is *a very small effect*, and I propose the

1 Engström = 6 standard deviations

as a new unit of smallness! By the way, the Biermann unit in astrophysics refers to an error by several units in the exponent, so there are blunders on quite different scales.

However, this was not even the end of the story: Träbert, by individually poorer, but systematic measurements for a sequence of ions obtained internally consistent results which did not meet the Lund data for S VII and Cl VIII, but happened to coincide with calculated numbers provided by one of the masters of the atom-theoretical universe, Alan Hibbert. These calculations had been done before Träbert's experiment, but it took his reading the references in an Indian doctoral thesis to find this study which suddenly matched his experimental results after more than a year's struggle of getting decent answers. Engström, however, had always claimed that Hibbert's numbers on the lifetimes in the Ne-sequence ions were not particularly good. Only after some fierce and prolonged resistance on various levels against Träbert's conclusion that something unrecognized might be wrong with the Lund data, Engström eventually did find a problem in the Lund measurement, the first with a new spectrometer, in a new spectral region. Then the Lund results for S VII jumped again, this time to the trend indicated by the Bochum data (and earlier measurements elesewhere) - matching Hibbert's prediction rather well. If the mountain does not come to the prophet, sometime the prophet has to move there ...

--- xxx (End of version A) ---

--- (Version B of the same sad story) ---

Knife fights by mail - a gothic short story from scienceland

Persons:
Enzo, a well-aged (not old yet) experimentalist-cum-theoretician from Granada (in Burgerland), takes pride in finding very useful straight line plots from trickily massaged data
Björn, an aspiring young docent in the Nordic country of Köttbullarlän, well-trained to appear twice as important, very much self-assured and a personification of precision and reliablity in the business. He works at Finnbü, where trolls have been shaping pebble stones with utmost precision since many generations
Jan, from Beecham in Wurstland (pronounced worstland), by age almost in the middle of the other two, but much less given to detail and elaboration, tends to type drafts of papers faster than he can think physics. He has access to an accelerator that can produce beams of more elements in a day than others in a month. What good is that for? Good question ...

Living in different parts of Middleearth (or was it Post-Pangaea?), the gnomes Enzo, Björn and Jan are eager to play the *Fine Balls and Straight lines* game (FBS). In this game, pebbles are placed on a cloth. Smaller, finer pebbles are considered more valuable than coarse, big ones. However, after many pebbles are in the game, one expects patterns to become visible. For a game to appeal to great minds, the patterns have to be simple. A special art is developed in which the cloth may be pulled to help make straight-line patterns, and Enzo is a world-renowned player of this variant. Obviously it is easier to see straight lines with thick pebbles, so master players want to show their proficiency with extremely small pebbles, like those produced (with troll help?) in the long northern winter nights at Finnbü. There are famous games and even moves which are stored in folklore and are retold at fireplaces all over the world, and people remember the actors' names out of reverence to the great masters.

One day, Enzo wants to take up an old game again which he hopes to improve on by replaying it. It calls for NeNe pebbles which all have ten black dots underneath (This game variant is called "iso-dotted" pebbles). He has two superb pebbles close to each other, ground finely and placed with exquisite care (he is told) at the Finnbü workshops. Björn even had bestowed on Enzo the honour of being part of the name of the move that put the pebbles there, because

Enzo had visited the workshops close to the time the pebbles had been polished. From magic spells of the Dirac Fock oracle, Enzo has determined where the straight lines must point to when seen from one way. However, there are two pebbles in another place which seem odd: One is big and ugly, and the other looks neat, but seems in a wrong place. Did one of the trolls spoil an emerging pattern?

The two suspicious pebbles seem ancient, but they bear the trade label of the Beecham shops, where Jan has been trained in the grinding of pebbles. Enzo asks Jan about these oddities, and whether he could do better? Jan looks into the story books and finds that the odd pebbles have been manufactured long time ago, by apprentice Nerwer who since left this business and joined a large candlemaker's shop. When digging up the old stories, he finds that the both of the original pebbles were large, so one of the exported pebbles must have been mixed up with the real one, and the procedure for the placing used much too simple magic spells. Jan's own spells, not much less ancient than those Nerwer used, would put the pebbles at quite other places.

This changes the prospects of Enzo's big design. He therefore asks Jan whether he could grind a few more pebbles which might be placed near the new positions of Nerwer's stones. Jan complies, but after many months in the workshop he has several new pebbles, but he finds that with his tools he cannot grind as finely as the Finnbü workshop does. The worse thing is that he has problems with the old spells (need a spell-checker?) - they give no good patterns any longer with ten-dot NeNe pebbles! Jan varies the spells a bit, and then his pebbles suddenly do make a pleasing pattern - but not the same as Björn's. Jan notifies Enzo and Björn (by electronic pigeon) of his pebbles and the new pattern. Enzo dislikes the news and wants to stick with the old pattern - two beautiful small pebbles in a nice place are better value than five of the new, bigger ones! Björn mentions that maybe he now would place one of his pebbles slightly differently, three diameters away from the old position, and the pebble has grown, by the way. He does not give away what new magic spell made this happen. When Jan enquires, Björn even says that this now is the perfect position, that the other pebble will stay in place forever, and that Jan ought to work on his own pebbles if he does not like the new pattern on the cloth.

Enzo brings his new pattern to the attention of the fellow artists, by publication in the *Pebbular Review*. Although he names Jan and Björn and some others as coworkers, he seems to have taken clues for his pattern-making spells mostly from Björn's data, even as they clearly look much less good than before. Jan feels that his new spells have been underrated and prepares for publication in *Flimsica*, the monthly yellow press paper often used by the Finnbü workers. He speculates that the topical druid - also at Finnbü - might read his notes and get Björn privately to attend to the problem which Jan sees with the placing of Björn's pebbles. Yes, Björn is indeed being chosen as one of the umpires, as he lets Jan know privately. He even reveals what Björn told him in private while he lets the world wait for his umpire statement: Jan's grinding and polishing is so much inferior to his own! Unless Jan uses Björn's techniques, there is no value in letting the world know of Jan's doings. The other umpire asked by the druid sees no value in the matter anyway, and he does not communicate any further. Jan does not give up. While he waits for the umpires' reports and their comments on his own replies, he continues grinding. His pebbles get smaller, the spells work better - and his pattern seems stable. In his mind the idea solidifies that there must be something wrong with Björn's pebbles!

After several seasons, winter and spring having passed, summer almost over, more letters have been exchanged with the topical druid, another umpire puts in that "maybe after all there is something" in Jan's pattern. For this pattern, Jan meanwhile sees support in a dream of Hibernius, a renowned dreamer in the Green Island, who, after taking the miraculous CIV3 ointment, often has dreamed of new patterns even before pebbles were placed by spells, and was so often right. Björn, by the way, claims Hibernius' dreams to be wrong - as demonstrated by comparison with his own, different patterns. Finally the topical druid overrules the Björn's delays and lets the report pass to the *Flimsica*'s head office near the Royal court of Köttbullarlän. Jan is notified that stone tablets with his report will be manufactured eventually.

Months later, Jan travels by eagle, boat and cart to Finnbü and talks to Björn himself about the strange break in the pattern, which looks as if either one or the other of them must be wrong -

and how ever came that so-called "slight" shift of one of Björn's pebbles about?. Björn discloses that an apprentice worked the spells and slipped - wasn't that obvious? Jan is shocked to learn that in the well-reputed shops of Finnbü some apprentices apparently are not properly supervised in their spell-casting, yet the negligent supervisor wants to earn the honours if a spell worked out? Because he anyway mistrusts Jan's spells, Björn tries one of his spells on one of Jan's pebbles - and gets the same placement as Jan had found. So this easy way of spoiling Jan's pattern did not work out! Hmmmh. Only then, at long last, Björn tells of the uneven ground the cloth at Finnbü may have been spread on, when they reorganized their workshop and got new tools. Indeed, on a more even ground Björn's own pebble might have belonged in a different place ...

Jan wants to add this insight to his publication, eager to add to the credibility of his own findings, by mentioning that Björn is working on a probable solution of the noted discrepancy. This, however, brings Björn up in arms, he forces Jan to have the stone tablet cutters at the headquarters of *Flimsica* be stopped. Jan complies and does not mention Björn's recent insight any longer, but he can now skip his own, weird speculations about the disturbing placement of the Finnbü pebbles. However, when he dares to mention (in a letter to the *Flimsica* head office) that the wide-spread high repute of the Finnbü shops might have contributed to the slow pace of his publication's progress (because the umpires are used to see Finnbü being right), the topical druid at Finnbü takes offence, has the cutters stopped again and needs to be affirmed that not he was suspected of unfair dealings. Well, *Flimsica* does publish Jan's report eventually. Half a year later they also publish Björn's report (with another apprentice leading the list of responsible authors): Björn and his colleagues now concede that their beautiful pebble needed to be moved again, that the other of their pebbles must be in a wrong place, too, and that Hibernius's dreamed pattern seems right after all. And they have managed not to mention Jan's work at all. Certainly, that must be the apprentice's fault - senior craftsman Björn does not make mistakes! It must be for such infallability that Björn got a permanent job at the Finnbü shops, whereas Jan at Beecham is being phased out (a modern spell) by his seniors.

After such experiences Jan remembered half forgotten tales of earlier mishaps with Björn of Finnbü: A lengthy earlier Finnbü paper stated that many of the pebbles were collected at Beecham - but not even mentioned the fact that in many cases the Beecham shop crew collected and ground pebbles explicitly for Finnbü, when Finnbü people did not find the time and/or the travel money to join in the Beecham production runs. Or he remembered the repeated failure of attempts to produce ground pebbles of Beecham interest at Finnbü, failing, for example, because the technical spells of earlier times had not been documented, proven Beecham recipes were disliked, shop equipment broke down and was not repaired a.s.a.p. while the guests were there - at least to Jan it seemed that there was a marked lack of enthusiasm of some people for involvement in practical work with Beecham guests.

This mean story, of course, is only a fairy tale. It ought to be rather uncomfortable for anybody in science to recognize that one's own doings might be compared to those of Björn.

However, this sad story also relates to the once excellent pebble shops of Finnbü. It certainly contributed to the general decline of pebbleology, when one of the outstanding druids in the field, a superb communicator and organizer with personal connections to everywhere in the world, began to suffer from depressions and their ill effects on physis and psyche (what the late Vinlanders call "substance dependence"). Thus his pebbly institute faded from the former status of being an interesting place (except for some activities in other business branches). Then the local coordination of efforts and a scientific moderation of some activities of young people still in the making began to fail, too. It is sad to see how some of the young people just make it through the institutional barriers and get permanent jobs before the next hiring freeze, with help from within and from outside, while others of at least equal capability and possibly better interpersonal skills are left in the cold because their boss druid is no longer able to supply helpful letters of reference in time (if at all), or to coordinate efforts in the right places. Maybe the place will recover its good reputation when young Sonsson, who places pebbles in the sky, takes over the responsibility for daily business. [Well, young Sonsson was not that young either, and the pebble shop has been closed for lack of customers not much later.]

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Idle musings about the ways of scientific life. Clashes of personalities with different ways of self-esteem. Fights for recognition in a time when the basic experimental technique is far enough developed that everybody in the field ought to be able to do things about right, and then somebody with a more oldfashioned and limited version of the technique collides with somebody who perceives and styles himself as a leader in the perfection game - and is caught by his claim and his own lack of attention to detail. Fights over a few leftover but notable problems with which to demonstrate excellence?

---- (End of version B) ------

Multiply excited states - what are they, besides being one of the graces/curses of beam-foil spectroscopy?

The beam-foil interaction leads to the population of many more excited states than are reached in classical light sources. Among these are displaced terms and multiply-excited levels, and confusion about which is which is widely spread, in the absence of accepted definitions. After digging in the (inconclusive) literature and pondering the issue, J.H. Blanke came up with the following definition:

> "Eine N-fach angeregte M-Elektronenkonfiguration ist eine Konfiguration, bei der in der Seriengrenze N-1 Elektronen andere Hauptquantenzahlen haben als in der Grundzustandskonfiguration der M-1 Elektronen."

For friends of Goethe and Schiller and Kant who prefer not to read the True Language of Thought but to understand what is meant, this might be translatable as

> "For an N-fold excited configuration of M electrons, N-1 electrons at their term series limits have principal quantum numbers that differ from those of the M-1 electrons in the ground state configuration." (of the next higher ionization stage)

This is so clear to me that I always abbreviated it to read as

> "Displaced terms have series limits in the same ion, doubly excited levels have series limits in the ion of the next higher ionization stage, and so on."

However, J.H. Blanke tells me that the two definitions are not equivalent (which is obvious), and his version might even stand the proof of time. It is certainly more self-consistent than counting electrons which are not in the possibly lowest orbital. Blanke assumes that anyway everybody knows what he/she means (I am less certain about that), he/she who uses those terms.
In case of such troublesome subjects, help may come from unexpected quarters:

> "To have one surprise was a nice way to start any day, but to be told he was going sailing a n d to have a new outfit at the same time was doubly exciting." (Michael Bond: "Paddington abroad", Houghton Mifflin Company, Boston 1972, page 93)

That should help settling matters, since we now have at least one clear-cut example.

Elmar

Holger Blanke intently pursued the measurements of multiply excited ions. For some measurements he ensured theoretical backups by Tomas Brage (then at Nashville) and his visitor Gregorz Miecznik, and one of the subjects, sextets in five-electron ions, was pursued in parallel papers. The synchronization, as obvious from relativity and in case of such far-apart partners, was troublesome. Finally things were written up, but encountered different delays in the submission and refereeing stages. A letter from the Editor at this stage:

> July 16, 1991
> ... Tomas and Gosh are not the worlds greatest administrators, but I noted in their letter a request to be published together with your paper which is still in the Dirac continuum, at present. We will make sure that this pair creation will appear in Physica Scripta, provided that the referees are beneficial. You are fast moving in the direction predicted by Minnhagen many years ago, i.e. observation *av den gula Mässingoktetten* (the yellow brass octet). What will Herr Dr. Blanke do in industry? Regards, Indrek .

[This firstly shows that editors can do their job creatively (sometimes), and secondly, that Minnhagen was farsighted. By the way, Blanke, an excellent mixture of a theoretician with an experimenter, went into industry to calculate the diffusion of toxic materials through waste disposal sites, in order to estimate whether it would be cheaper to put concrete lids on old waste dumps or to sanitize them. As soon as he had joined the company, however, his real job was software and systems analysis...]

Date: Fri, 19 Jan 90
From: "E.Biemont" <U2129EB@BLIULG11>
To: "E.Traebert" <eamp9@cfa4>
Dear Elmar
Thanks for your messages.
I was planning to send you a copy of my new results for Ag when I received your message! As you notice, it is good that the agreement is better. [and the considerable change is not mentioned in the paper!] My results are like the wine, they improve with age! To be more serious, they confirm that the HFR results for weak lines may be strongly dependent upon scaling factors,...(at least for some lines!)
Best regards. Emile

[Apparently it needed bfs to find out about those truths ... E.T.]

Stanley Bashkin took up gas excitation by hydrogen projectiles in the mid-1980es. A problem from hitting gases with fast ions and finding charged products with broadened emission lines:

Date: Wed, 10 May 89
From: <BASHKIN@ARIZRVAX>
Subject: doppler energy calculation
To: eamp9@cfa4

Elmar -
I have two problems. One is that I don't get the same Doppler shift energy that you get. According to me, V(source) =(delta lambda/lambda)times c. For ***Dl*** = 0.6 Å at 5000 Å, this gives V = 3.6 x 10(4) m/s, or an energy of 200 eV! Even going to 0.3 Å line width gives 50 eV. Have I made a mistake somewhere?
The second problem is that I have calculated the recoil energy of the N_2's. For a head-on elastic collision, I find that the recoil energy is [4m(one)m(two)/(mone + mtwo)squared] times E incident. This gives the recoil energy as 0.35 E incident!

Please check my algebra and arithmetic carefully, because much depends on the numbers.
I have received your hard copy of the paper, and will read it tomorrow - today is reserved for giving and grading my final exam, plus making out term grades. Ugh.

[ad 1: full line width instead of half of FWHM; ad 2: this would hold for a nuclear collision, not for a collision of projectile with the electronic shells only; Stanley, however, is a nuclear physicist by training - as were many of the people who took up fbs. Do they show a tendency towards violence - in their collisions?]

Date: Wed, 24 Jan 90
From: "Indrek Martinson, Atomic Spectroscopy, Lund"
To: EAMP9@CFA4.BITNET

Dear Elmar,
How is the BFS history book proceeding? I havent received any answers as yet.
The other day I happened to think of a visit to York University in Toronto, in 1981 after the Quebec bfs meeting. I gave a talk on Bfs, lifetimes etc, and afterwards the department head, R.W. Nicholls, a molecular physicist gave me two papers of his, published in Proc. Phys. Soc.

78 (1961) 588 and 76(1960) 217, in which he and some others had done some beam-gas work. Nicholls also told me that many years before my visit Stanley had given his usual flamboyant talk on bfs in York whereby Nicholls had informed him about those early papers. I wonder if these are of any relevance to the present U of A activities.
Regards Indrek

[Yes, Stanley quotes those papers faithfully.]

On seeing no Balmer lines when using 1 MeV H_3^+ projectiles to excite nitrogen gas, whereas people with H^+ projectiles have seen them clearly:

Date: Wed, 31 Jan 90
From: <BASHKIN@ARIZRVAX>
To: eamp9@cfa4
Elmar -
I have made a fascinating discovery about the VdeG - the current out of the ion source WITHOUT THE SOURCE MAGNET IN PLACE is BIGGER than with the magnet! I'm going to close up and try to run this way. It is a frustrating time, dear boy, but maybe I'll be able to take some data again.
I'm delighted that you agree with me about the Balmer lines. I think a brief comment would be in order - provided we ever get the paper back from the referee. Or do you think it would be worthwhile to write a separate little note about our failure to see those lines? Maybe N-H would publish it for $ 50/line in one of its journals. I wouldn't mind milking our results for all we could get. (That's the Dean effect.) Stan.

Highlights and outlook

There are many types of berries, among others there are blue-, black-, goose-, straw-, rasp-, boysen-, cranberries, myrtilles, hjortron (served as a dessert after the Nobel laureates' dinner), the lot. Thus in present physics with some relation to Atomic Physics there are S.D.B. (Chicago), H.G.B. (formerly also at Chicago, now at Argonne laboratory and in Chicago schools science education), and M.B. (Bristol, the man with the geometric phase). This is to give easier access to the following story, which came up after Indrek wanted to run Beryllium at Bochum, but the Bochum accelerator people did not want to, not even if the experimenters cleaned the ion source afterwards (never trust academic people to do the job right ...) :

24-Sep-92

I tried (together with the phaseless Berry) to run Be at ANL in 1984. But the spectrum looked crazy, and at first we didn't understand anything. But later we found out that it was largely K^+ (Nucleosynthesis in the ion source? I don't know.) ... Indrek

(Maybe the stuff was Berryllium instead of Beryllium. E.T.)

Upper limit of the sunspot-induced variations of the Sommerfeld fine-structure constant, from

A new beam-foil lifetime study of low-lying levels in Si IV

S. Maniak, L.J. Curtis,
Department of Physics and Astronomy, The University of Toledo,
Toledo-Eslöv, OH 43606, U.S.A.
E. Träbert *
Harvard College Observatory, Cambridge, MA 02138, U.S.A.

Abstract:
The previous measurements at a period of low solar activity of lifetimes of 3p, 3d, 4s, 4p and 4f levels of Na-like Si IV have been repeated at a period of almost maximum sunspot frequency. Problems and advantages of the evaluation methods are outlined. The results agree with recently established isoelectronic trends and confirm earlier studies employing the ANDC scheme. They also define an upper limit on the Dirac variation of fundamental physical constants like the Sommerfeld fine-structure constant.

* On leave from Ruhr-Universität Bochum, Fed. Rep. Germany

To be submitted to Nuovo Cimento Letters (as suggested by Honorary Editor I.M., see endorsement below)

(First publication on Si IV by Larry was in 1971, the measurements for the second were done in 1989, and the paper written up and submitted for publication in 1992 - not in NCL!)

Thanks, This stuff is excellent. I wonder whether this 3p ^2P liftime also could tell us what the mass of the Higgs particle is. Or whether there might be a fourth neutrino or not.

When Dave and myself were looking at the Si-sequence, we noted that some isoelectronic trends were very irregular in Si-like ions while there was but smooth variation in the case of the C I sequence. Having studied Heckmann-Träbert (both the Urtext and the translation) we knew that the explanation is provided by the 3d-electrons in the Si-like ions. My suggestion was to really examine the C sequence as carefully as possible so as to obtain "A rigorous upper limit to the existence of 2d-electrons" and send this manuscript to Nuovo Cimento Letters or Phys. Rev. Letters. There might even be some interesting experimental findings, e.g. by exposing diamonds to 3d-electrons the end product should be sand.
Indrek

[Indrek is mistaken: There are 1.4 times as many 3f electrons in Si as there are 2d electrons in C, so they must be more important. See also that 1.4 is exactly one tenth of the atomic number of Si. Furthermore, Si is used in semiconductors and therefore probably does not know about proper conduct. This is an interesting case of ethics in science, and it should go to either Nature or Phil. Mag. (E.T.)]

But don't forget about priorities:

Date 03/20/90
Holger Blanke
Tel. (+49) 0561/804-4571, Sekr. -4771, Telex 99572 ghkks d
GhK University of Kassel, Physics Department
Heinrich-Plett-Str. 40
D-3500 Kassel / Fed.Rep. Germany

Lieber Herr Träbert!
..... ceterum censeo: Schon 1983 konnte ich durch Extrapolation des Quanten-Effekts den Übergang F VII 2d - 3f der Linie bei 12.056 nm zuordnen. Diese Identifizierung musste zurückgezogen werden, weil dort eine Quintettlinie gebraucht wird. [As early as 1983, by extrapolation of the quantum effect, I could assign the transition F VII 2d-3f to the line at 12.056 nm. However, this identification later had to be withdrawn for need of a quintet transition in the same place.]
Mit freundlichen Grüßen
H.Blanke

Funding with the Founders went bad everywhere. Stanley finally was even afraid that his machine might be closed down before he wanted to retire, for lack of funds to replace broken parts - like a bellow on the machine which broke after the high-pressure insulating gas in the tank found a way into the vacuum system and blew it up. Fortunately, it took only a week to raise US$˜320.- and to restore faith in the own future:

Date: Tue, 3 Apr 90 13:57 MST
Elmar -
Yes, I have the money for the bellows. The order went out today, thanks to Carruthers [Dean] and Thews [Ass. Dean]. When I went in to see Carruthers, he said, "Omigod!" To which I responded, "Ah, you recognize me!"

Stan.

[Isn't this "Administration/Physics on a High Level"?]

Tapping sources

Whilst the money supply was dwindling, people (as expected in a market oriented economy) came up with new ideas. When Elmar in a GSI experiment (that is high energies compared to most beam-foil experiments) encountered a microchannelplate background that increased with the distance of the observation region from the foil (possibly d electrons), Roger and Indrek came up with the idea of cascades from the Dirac sea of electrons. This would have been an infinite and worthy source to exploit! A leak in the Dirac sea! The future of the field would have been secured - for another year ...

A trinational experience at Daresbury (Sept. 1991)

The participants:
From Tucson: Stan B.
From Daresbury: Dave W.L. T., John W., Derek E.
From Manchester: Jon B.
From Bochum: Elmar T.

Prelude

Stanley rediscovered that gases under ion bombardment may emit light. He measures H_3^+ >> N_2, O_2, CO_2 and CH_4 at Tucson. H_3^+ is the projectile of choice because it works so well. He intrigues E.T. to join in with the project, the latter accepts the invitation and obtains travel money from NATO for work at Tucson. H_3^+ is efficient, but messy. A lower ion energy would be preferable, as well as the use of H^+ at lower velocities. Since Stan is happy about the progress, he applies for beam time at Daresbury, for experiments with even faster primary ions (but at least they are multiply charged). For the test run, the participants convene in view of Runcorn Power Station and in sight of Liverpool.

The test match

Josh Silver has proffered a 1m normal incidence spectrometer. This one has been carefully adjusted at Oxford around 1979/80. Elmar has worked with it before and knew it to be in perfect working order then. Stan arrives a few days before the accelerator run, Elmar arrives the evening before. The spectrometer has not been set up completely. Real work starts in the morning of the day of truth. The discussion about which detector to use (an OMA system or a regular PM tube with a sodium salicylate wavelength shifter) is yet undecided. Both require different extensions of the exit slit mount. The spectrometer has ben set on the right hand side of the ion beam, whereas at Oxford (for refocussing/Doppler cancellation purposes) it had been set on the left hand side. Stan sees no reason why it should not have been put on the Oxford side anyway.

John decides that entrance and exit slit have to be exchanged, since the spectrometer must not work in minus first diffraction order (Physics certainly does not forbid such operation...). Because there are no spare parts, this turns out to be a tedious affair. Also there are claims that in the days before people wanted to adjust the spectrometer, but the refocusing drive (grating displacement) did not reach far enough. All this was judged from laser light shining in through one slit. Elmar asks whether a light bulb would not be more suitable, since the parallel laser light beam normally would be expected to mislead the eye and have different focusing properties. The local spectroscopy experts verbally agree, but continue with the laser. The refocusing drive needs repair. By 16h the allocated two accelerator shifts begin, but everything is still in shambles.

Decision to use Jon's multichannel detector. It does work in air, with the ambient light. The resolution is still poor. The spectrometer vessel is being closed and reopened a number of times, until by plenty of technical help the drive works. The vessel does not pump down. Since the old diffusion pump days days of 1980 the spectrometer has been re-equipped with a turbo pump, that is good, being backed by a tiny 2.5 cfs roughing pump. Stan already knows that the gauges won't show any change from atmospheric pressure before ten minutes of pumping, that is, if the system is leak tight. Needless to say, it is not. The late night vacuum crew member has to be called from home. The leak is huge, the leak is twofold: Both slit heads have been extended in that futile adjustment procedure so that the o-ring seals slipped into the clamp ring grooves and stopped sealing. This can be cured close to midnight.

The system is still leaking like a sieve. Now the OMA, designed for vaccum operation, turns out to be sealed insufficiently, and air is sucked through its housing and the front window seal. Change to John's PM tube. This is quickly done, the system pumps down (slowly ...). The usual problem in a nuclear physics laboratory arises: How to store single channel signals in a computer which serves ADCs and the like, but cannot provide constant time bins for mimicking a multiscaler? Jon thinks about the problem, remembers a DAC ramp generator, fixes a solution. The DAC is unexpectedly (frighteningly) non-linear, but the priciple of operation is valid. Second mode of operation: A chart recorder.

Procedure: Stop the ion beam. Send somebody into the target room (radiation interlocks!). Start the synchronous spectrometer drive. Run upstairs (pass the radiation doors, released via intercom), go to the control room. Admit the ion beam to the gas target. Start an x-y chart recorder. Estimate wavelengths (uncertainty factor of two because of unspecified diffraction grating) from time after start.

There is light at two positions, very wide features. Repeat procedure four times. Second shift of allocated accelerator time is over, with about 6 hours used out of 16. Drop in for breakfast. Sleep. Drop in for lunch. Sleep.

Think about the possible origin of the two lines. Test with a mercury lamp. Derive grating constant (conversion factor between drive indicator and wavelength) from the fact that zero order and the onset of the first Hg line are at the ends of the drive range, none of them being fully covered. Doesn't this seem like a time lag exercise, a time lag of more than 20 years since the early makeshift days of fast-beam spectroscopy?

Elmar flies home. At Bochum he gets about 20 times the primary ion beam current of ions of the same charge state and at a lower beam energy at Bochum. Determines the wavelengths of the mystery features (near 150 and 173 nm to fractions of a nm at a very low signal level, using a properly adjusted, but grossly unsuited grazing incidence spectrometer of abominably low efficiency. Checking the Kelly and Palumbo tables, the lines are close to the positions of lines from neutral nitrogen.

In December a work study weekend is held at Daresbury. Stanley reports on the beautiful beam-gas experiments and mentions the unidentified lines as well as the different appearance of ion- or electron-excited spectra. He shows a spectrum after electron excitation, but has overlooked that it features bright lines at the positions of the mystery lines, and that the authors assign them to neutral nitrogen. Neutral nitrogen did not show in the visible spectrum (as observed at Tucson), so Stanley declares that neutral nitrogen is not excited in his collision system of ions with nitrogen. In his talk, Stanley also includes spectra recorded at Bochum, declares them to be full of lines of whatsoever (implying: non-interesting) origin. The similarity of the Bochum spectra of air, excited by C^{6+} or Si^{8+} ions, with all notably strong lines easily identified with nitrogen (and, appropriately weaker, oxygen lines) from the target gas does not find its way into his mind. [After the conference, it becomes clear that he believes the weak mystery lines to result from the projectile, Si. His second choice was oxygen, of which he saw atomic lines in the Tucson spectra. The Daresbury spectra, however, had used pure nitrogen as a target, and there were those mystery lines. Yes, Stanley was fairly ill at the time, suffering from an outbreak of varicellae which had been dormant since his youth and now caused terrible pains.]

In the week after the meeting he gets 7 accelerator shifts at the Daresbury machine. One of Josh´s students, Will Hallett, who knows the spectrometer, joins the locals and Stanley for the run. However, until a day before the run, he is not told about what happened to the spoectrometer during the first run three months earlier. Nothing has been done to the spectrometer in terms of readjustment yet, only a marker circuit for the drive has been activated. Six of the seven shifts are spent with trouble-shooting. The data collection system fails as well as the computer link. Data are taken during the last shift, again with a strip chart recorder as the medium of (no) choice. However, the resolution has been improved and the formerly broad lines seem split into components now.

Elmar gets the data by mail. He has no trouble matching the few more prominent structures with fine structure intervals and relative intensities of N I transitions. The presumably coarse calibration is off by about 1 nm. The Daresbury locals confirm that there has been no check of the calibration after the run and that anyway there was no detailed calibration before.

Since in the visible spectra at Tucson no neutral nitrogen seemed to be present, Stanley does not like this idea. He claims that the lines are close to positions of Si II. He does not accept that this would involve observation of radiation of the projectiles; these would have to capture (practcally simultaneously) at least seven electrons and still would have to end up in a singly excited state. A Nobel prize-worthy effect, if true. He wants more measurements to be done with other target gases and other projectiles to disprove his idea. Grudgingly he reports that the British accept Elmar's interpretation.

Occam´s razor does not work in Arizona, apparently. This might explain all those unshaven cacti in the desert, too ...