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 middle period

David Pegg in 1978 was right in seeing a transition period - if not already decline - in the general field of beam-foil spectroscopy. Fifteen years after the start, many of the people who had joined in early largely had by then lost their initial drive. Many bright ideas had been tried, and by far not all of them worked out. Many loud claims, promises and expectations had not been fulfilled, which caused funding agencies to look at new money requests twice - and then to deny. So instead of heading for flashy new things, it was perhaps needed to do some things with more care.

Jürgen Andrä´s Berlin (and later Münster) laboratory was then a hotbed of groundbreaking developments: Quantum beat measurements, beam-laser excitation for precision lifetime measurements (Ba, Li, Na), ion scattering on surfaces (IBSIGI, later perfected by Helmut Winter after Andrä´s move to Münster - a place much worse for windsurfing, but with a full professorial position (to which HJA added a full position at Grenoble for a while (windsurfing on Lac d'Annecy?) – some people can fulfill their job duties in two far-apart places at once, or work twice as efficiently, or twice as long per week, or any combination thereof; maybe this was a precursor to entanglement physics experiments that even included the experimenter? HJA later also added photon-photon coincidences and the like. HJA pushed his students, very bright and capable guys like W. Wittmann, Andreas Gaupp and Peter Kuske, to strive for the ultimate limits, and it took them many years of graduate work to reach their since famous results.

Some time in this period, Stanley visited Berlin. Gaupp was preparing a publication on hyperfine structure measurements, and Stan was asked to brush up the language, which he delightedly did. The journal's referees then lauded the scientific content of the paper, but asked the authors that next time they should have the text checked by a native speaker ...

HJA was held in high scientific esteem by the colleagues, but was also feared for his influence which was perceived as possibly devastating, and for his open criticism of anybody´s work he did not cherish. His clashes with Josh Silver, another strong-minded excellent physicist (based at Oxford), are legendary: There is the story of the two ostensibly using the telephone, but shouting at each other at a volume level which would not have necessitated to use a phone - between Berlin and Oxford! And there were many things those two fought about, like who had the command in the joint enterprise at Oxford, using Josh´s beam line and laser set-up (and building on precursor experiments at both places), but involving doctoral students from both Oxford (Ed Myers) and Berlin (Peter Kuske), not counting the other members of the Oxford group. As a compromise, Ed would concentrate on the laser part of the experiment and Peter on the high-count rate proportional counter detector part. Both students excelled in their work and got along with each other well, but the bosses fought bitterly.

Eric Pinnington was one of the (extended) visitors to Oxford at that period. He joined in with a nice experiment on He-like fluorine, trying a differential decay curve measurement on the two long-lived n=2 triplet levels by using two slits and two detectors in a simultaneous recording of both signals. This involved the refocusing of a normal-incidence spectrometer by using a VUV-grade lens. Because of a shortage of beam-time and so many other worthwhile projects (like the above beam-laser experiment), the experiment was run in earnest only once. It worked as prescribed but failed at the same time (in the sense of yielding showable results). Eric recalls that this was due to the lens being of calcium fluoride (which would have been disastrous because of the wavelength cut-off, whereas Elmar thinks that magnesium fluoride was used which might have worked); Elmar is certain that the experiment failed because the data could not be evaluated: He had written programs to evaluate the ratio of the two signals directly, as would be sensible. The data, however, proved jumpy and made no sense: When converting the computer program for the computer-controlled experiment from the previous ALGOL version into FORTRAN (necessitated by a change of the lab computer), he had overlooked *one* of several integer/floating-point conversions in one of the subroutines which governed the signal normalization...).

When the afoementioned fancy beam-laser experiment on fluorine came to preparation for publication, Josh asked Eric whether he wanted to be coauthor. Eric was decent; he thought that he had not contributed enough to merit coauthorship on such a meritful paper. Anyway, he said, HJA would not accept him on the list of authors (see other story below). Josh said, he would like him on the list and would talk it over with HJA. The result was *No*. In another case, Stanley Bashkin was asked the same for a spectroscopy paper at Oxford. He had not really contributed anything to the actual work, but he had been to Oxford at the time and entertained people by discussing the daily Times crossword. The draft of the paper named him in the acknowledgement only. Stanley happily accepted the honour of coauthorship and was duly put on; however, it was overlooked that he was mentioned in the acknowledgement, and thus we have a case in which a coauthor is also thanked for his contributions by the authors (including himself) in the acknowledgement...

Eric next recalls the EGAS conference at Amsterdam (1972):

> Andrä has told the audience about quantum beat measurements and stressed the importance of measuring the ion velocity to near perfection. Eric Pinnington dares to comment that there are cases where this is not necessary, and Andrä is starting to crunch this impostor, but then asks him to prove this conjecture. Eric has a talk later on the same day and thus promises to tell then. After the Andrä session, Eric hurries to his files and tries to locate the reference he thought he remembered - in vain. Feeling the pressure mounting, he phones around, finally finds Michel Gaillard and his files. He asks whether he might borrow them, flips through and to his great relief indeed finds a paper which explains how to do certain least-squares fits of certain parameters concerning the fine structure intervals and the like. Eric feels he narrowly escaped being squashed in public.
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> When his talk comes up, HJA and his students sit very attentively, the latter obviously expecting the tearing-apart of the speaker by their great master. Eric explains his conjecture and then gleefully writes down the reference: A paper by HJA and coauthors ... (K. Tillmann, H.J. Andrä, W. Wittmann, Phys. Rev. Lett. 30, 155 (1973)).

HJA had influence in other places, too. Subtil at Lyon had done some work involving foreign visitors (Gordon Berry, Eric Pinnington), and when writing up for publication, he included those two on the name list as well as HJA. HJA interfered, claiming that one of the visitors had not contributed to the physics of the project and thus was unsuitable as a coauthor. When the locals denied that claim and said, the visitors did have a considerable intellectual share in the study, HJA forced two unrelated names (Gaupp and Wittmann) from his Berlin group onto the authors' list. Gaupp on his own account reported later that he had this paper without ever having done anything for it ...

At that time, the Berlin group studied hyperfine structure (hfs) via the quantum beat phenomenon (which later helped Andrä to swing the odds in his favour when applying for a nuclear physics chair at Münster: hfs (studied optically on fast ion beams) relates to nuclear structure!). Those quantum beats, while predicted and theoretically described in many variations by many experts, were often quite feeble and took pains to chase and nail down.

Both Berliners were indeed quite good guys and physicists, no doubt about that. At the Lund EGAS meeting (in 1973) Gerald Brown asked Indrek Martinson to write a review on beam-foil spectroscopy. Indrek accepted, but wanted Andreas Gaupp as a coauthor. The boss of the latter (HJA was not a full professor then), Eckart Matthias, intervened:" No, that is not possible, he does not have the doctor's degree yet!" Gerry Brown contradicted:"That is the sort of people we need." (meaning A.G.). Right he was. The article served well as a lucid introduction to the field, all over the world.

By the way, Josh Silver's ways with others have a resounding wall of gossip, too: Josh did his doctoral work with Derek Stacey at Oxford, measuring isotope shifts, as was the long-standing fashion there. To Josh it became apparent that this was all well, but as there was no theoretical quantitative understanding of these shifts, the experiments seemed to him a waste of effort. May be to compensate for his frustration, he got angry at Derek and shouted at him, calling him a crook and whatever else came to his mind. Fortunately this did not prevent them speaking to each other lateron, although some of those angry undertones were often present. Well, Derek recalls that Josh went to Lyon after his thesis and spent half a year or so there. When he came back, he seemed quite friendly. Derek thus asked him: "Half a year ago you called me a crook and worse, now you you are so civilized to me - what happened?" - "Hah, I met the *real* crooks!" and smiled. Yes, everybody who has experienced Josh can well believe this story, as he had many clashes with people lateron, who purportedly stole his ideas, "These crooks! Thieves!", but in several cases this did not hinder a fruitful and lasting collaboration with exactly the same people afterwards, sometimes interrupted by a few more of such clashes. Derek, undeterred, still finds everybody to agree that "Josh is a very good physicist, indeed".

Improvements to the basic technique:

Refocusing > higher resolution

Refocusing

Light from different positions along the ion beam reaches the entrance slit of the spectrometer under different observation angles and thus with different Doppler shifts. This results in a spectrally broadened image of the entrance slit in the exit focal plane. The procedure of *refocusing* can turn this problem, Doppler line broadening, into a useful entity, by reducing the observed line width and improving the light gathering power of a spectrometer simultaneously.

Stoner-Leavitt refocusing. In a regular normal-incidence spectrometer, Doppler shifted light from a moving light source is imaged in a location which is different from that for unshifted light. The imaging can be treated as if the spectrometer had a different focal length for Doppler-dispersed light (19), and by moving the exit slit (or the diffraction grating) appropriately inward or outward, a rather good (displaced) focus can be found in which (as a bonus) the light from a longer section of the ion beam (and thus more light than from a spot source) is being collected. The displacement of this new focus from that for a stationary light source is a function of wavelength. Many normal-incidence spectrometers can be slightly modified (for example, by an adjustment to the cam which anyway provides for a wavelength-dependent displacement of the grating during the scanning motion of the spectrometer, or by displacing the exit slit under servo control), so that the refocusing condition can be met for extended regions of the spectrum.

Bergkvist refocusing. Another type of refocusing first invented for magnetic spectrometers (in the quest for the end point of the b spectrum of tritium, where the finite neutrino mass might cause a nonlinear contribution) was later on adapted to the fast ion beam (20). A lens in front of the spectrometer collects parallel light from a section of the ion beam and focuses it into the entrance plane of the spectrometer. This Doppler-dispersed light is then diffracted and imaged by the spectrometer onto the exit plane. If the two dispersions are equal in magnitude (adjusted by the lens and the ion velocity), but of opposite sign (achieved by setting the spectrometer on either the left or the right side of the fast ion beam, or operating the spectrometer backwards), the result is a narrow spectral line.

The signal yield achieved with both methods can be one to two orders of magnitude compared to a non-refocused spectrometer (21), in addition to the gain of information which results from the improved spectral resolution. The technique of refocusing has, for example, been essential in achieving sufficiently high resolution for the observation of lifetime-broadened lines in few-electron spectra, from radiative transitions between an upper level of relatively long lifetime and a lower, very short-lived, autoionizing level (22,14).

Beam laser spectroscopy
Gaillard, Andrä, Wittmann, Gaupp, Kuske, Schmoranzer, Hartmetz, Harde, Guthörlein, Volz, Baudinet-Robinet, Dumont, Garnir

Laser techniques

Gas - Laser. The matter density encountered by an ion beam in crossing a dilute gas target is rather low; therefore the effects of energy loss and straggling are not severe. The collisions of the ions with the target gas atoms, however, can excite the projectile ions to metastable levels much higher in excitation energy than any practical laser could reach from the ground state. The interaction with the laser light can then be used to reach further excited levels. This technique has been pioneered and pursued at Kaiserslautern (31).

If the ions capture an electron from the gas, they neutralize and form a fast atomic beam. On such beams, selective laser excitation has been used to provide extremely precise lifetime data (uncertainties of much better than 1%) of Li, Na (30) and Cs (32). The experimental lifetime data on Li and Na invalidated many early calculations, and for more than 15 years they were at variance with advanced and involved theoretical calculations on these seemingly simple atoms having only a single electron outside a closed shell. At long last, improved measurements at Kaiserslautern (32a) superseded those earlier results and vindicated the best calculations. The Cs lifetime data are being used to test atomic structure calculations employed for interpreting experiments on parity non-conservation.

Foil - Laser. Interaction of the ion beam with even a thin foil causes a more massive energy loss and straggling than a gas target and reduces the fraction of ions which might interact with a narrowband laser. Undeterred of this prospect, the Liège group has managed to do laser spectroscopy on *multiply charged* foil-excited ion beams (in contrast to spectroscopy on neutral or singly charged atoms done elsewhere) (33): Laser light is used to selectively influence the population of a given level after foil excitation. Neither the decay curve with "laser off" nor a curve with "laser on" can be evaluated easily, due to the non-selective population mechanism of ion-foil interaction. The difference signal, however, can be a rather clean single exponential.

ANDC > reliable lifetimes beyond the range accessible by lasers

There is one measurement and evaluation strategy which avoids most of the above ambiguities and delivers reliable lifetime data. It requires the measurement not only of the decay of the level of interest, but also of the decay properties of all levels which decay directly towards this level. These cascade decay curves contain all the information on contributing higher lying levels, and thus the knowledge on the cascade repopulation is complete. The fit procedure then consists of correlating all decay curves of direct cascades with the decay curve of interest (ANDC (73)), by adjusting the relative amplitudes of the cascades. (As the lifetime of a level relates to the sum of all transition probabilities, one does not even have to measure the direct cascade itself, but may choose a decay branch of the same level which might be better amenable to measurement. Optimum conditions for ANDC exist when all lines of interest can be measured under comparable conditions; at Lund there is a facility with several spectrometers viewing the same ion beam section.) This, then, is a linear fit problem with is solvable with much more mathematical stability and reliability than the non-linear fitting problem of multi-exponential analysis. Treating the integro-differential problem of cascade replenishment, the various ANDC codes proceed via numerically integrating or differentiating the primary decay and the cascade data curves and checking the consistency of the results. In order to reduce the influence of individual data point statistical fluctuations, the most widely used code, CANDY (74), represents the data curves by multi-exponential smoothing functions.

Higher ion energies > higher charge states, isoelectronic sequences

Absolute and precise term energies of multiply-excited states (Mannervik)

Fighting the poor reputation of the field, not getting decently funded, people leave the field

Assorted people

S.B. and the Times Crossword Puzzles

Date: Fri, 10 Nov 89
From: "Indrek Martinson, Atomic Spectroscopy, Lund"
Subject: RE: Money, logarithms and Agnew

Dear Elmar,

Here is a fine letter from Bill Bickel to Richard Crossley, from about 10 years ago. Stan was then on sabbatical in Oxford [1979] and he spent his time writing letters to the editors of The Times and Daily Telegraph. Some of these were published and RJC sent them to Bill Bickel in Tucson. The answer was "Dear Richard, Thanks for your short note and the newspaper clipping. We all read it with great interest but wondered why it was sent to us - since we didnt recognize the name who wrote it. I showed it to several secretaries and then to a few old timers who are usually up on past graduate students - old retired -to the-pastime (long after their time) faculty and some visiting dignitaries off and on. One fellow said Bashkin worked with Van de Graaff. another suggested looking into old Tucson rape cases - but there his name never came up. Then one graduate student recalled an ancient research - bean-foil, bum-fool, barn-foul or something-like-spectroscopy - where he once was associated with polishing ions, studying light and dark elements and bending ions with magnets. Lo and behold we found out who he is - ol S. Bashkin (middle name Otto) left here last year - gone but yet not forgotten believe it or not. We all wondered about him - where he went - what he is doing. so now we are all happy that he found a nice job as newspaper writer, strange words and small words spelt a different way. Well thank you Richard for the little tidbit - another few weeks and I believe we would have had to enlist outside help to track down ole SB. Regards Bill" . I spent a few days in Oxford and one morning when we were having tea (Stan, Nick [Jelley], Josh [Silver], myself and some other people) a letter from RJS was brought to me. I opened it at once and began reading this letter from WSB to RJS which was in the envelope. After a few seconds I burst into a laughter and everybody looked puzzled. Then Stan grabbed the letter from me and started reading it... He did not look 100 % pleased. It was an embarrassing moment.
Regards, Indrek

S.B. at the Lund Pelletron controls

On one of the visits to Lund, there was no space near the experimental set-up for Stanley to sit (and doze). He therefore got the padded chair of the Pelletron accelerator operator. A group of visitors was shown in, and lo! and behold, the duly impressed visitors were told that a professor from America had come to personally control the Lund machine for an international experiment. They were not told that Stanley had not been trained at this machine (though he did the job at his home machine) and that he was not supposed to touch anything so as not to endanger the experiment.

In the late 1980es, S.B. had so little funding at Tucson that he had to buy the laboratory PC (for data analysis and e-mail) from his private money.

S.B. the poet

Stanley is an avid composer of cartoons, few-word situations with a kick / hook / pointe. He is happy to have gotten several of his cartoons and comments into the Wall Street Journal. However, he also tends to longer forms of prose, like stories about the Abominable Snowman, and to children's stories. He also can be quite funny in his lectures, and maybe some of the statements he made in conference lectures and not appreciated for their physics were perhaps intended (and should at least be appreciated) as poetry of the more surrealistic kind? Who knows, who could tell? It is certainly not up to the poet himself to reveal the deeper meaning of the things left unsaid ...

Funny formulations

In a 1993 report (Abstracts ICPEAC '93, p. 502) the Giessen group explained the limits of the experiment they presented. Among the problems they list the *low luminosity of grazing incidence spectrometers*. Sure enough, most of these spectrometers will not emit light at room temperature, but what if you would properly heat it ?

Indefatigable Indrek, of course, was not even puzzled by this:

"... it occurs to me that nuclear physics people use the luminosity for beta spectrometers as a parameter. E.g., our friend Karl Erik Bergkvist who got a 55 eV limit to the neutrino mass, was credited for improving the luminosity of the Kai Siegbahn double focusing beta spectrometer by a factor of 1000, and that was without even heating the whole thing. "

Literary Awards (as of the early 1990es)

No field of literature nowadays can be without awards. Literary awards in spectroscopy (not limited to beam-foil spectroscopy) have been suggested for the following people, along with appropriate eulogies:

Stan B.:

Poeta Laureatus of the FBS community, avid composer of witty remarks and illustrator-cum-verbolizer of spirited puns (he calls them *cartoons*). Man of letters to the Wall Street Journal long before Pons and Fleischmann. Solves the riddles of the modern age by way of the Times Crosswords. Joiner of tales for any age. Never tiring of reciting the latest of his opera (magna and minima). Dedicated transcriber of poetic works (like "Einführung in die Spektroskopie der Atomhülle") from dead languages into creative, flamboyant, living ones ones, giving them a new and much deeper meaning in the act. Stuff of legends already during his lifetime.

Gabriel Garcia "Indrek" M.:

An Estonian by heart, living in the Swedish diaspora if not touring the world. Provides the world with a plethora of stories of the Old Times. The treasures of his memory are brought forward in Marquesian patterns of intertwined persons, facts, allusions, historical remarks (see recurring notes on Uncle Joe / Stalin or Nixon), satirical spouts and empathy for colleagues' sufferings, evidencing his sharp eye and tongue on the doings and undoings of bipeds in his interaction zone. Soviet colchos chess master (by having beaten that person on a visit abroad ...). Poets and non-so-poets from all over the world flock to his home and workplace to exchange ideas, listening to and indulging in Great Tales. Thus his old goal of becoming a teacher has come true in a variety of meanings, not the least of which is in keeping the tradition of the funny pun alive and passing it on to the next generation.

Sam B.:

Another contender for a prize, but it is undecided yet whether Pulitzer for poetry or one of the French ones (Prix Argot) would fit best. Citation: "For ceaseless efforts to introduce French syntax into British journals, thus highlighting cultural diversity in the scientific embrace, at the same time expanding on scientific contents into poetic clouds of a truly grand design only the pure at heart can ever dream to understand, and even then not by mind, but only by sympathetic vibes. He relentlessly strives for maintaining the clarity and rigidity of La Langue Française by not allowing them to dissipate into his written English or that of his co-authors. The uniqueness of his approach and the concern for his art show in the schism of his effortless oral English verbality and verbosity on one side and his scribendous adventures on the other. The steadfastness of his artistic pursuit is witnessed by his utter refusal to rethink, reword, retract anything he in his godlike creative power has brought forward, to stand unwitheringly the storms of time."

Elmar T.:

Compulsory writer, indulging in new media (bitnet). Has been called the "McDonald of BFS" (by EH Pinnington) for consistently fast bitnet replies when in the U.S. (and for the junk conveyed?).
Tries to write faster than the referees can read. Succeeds sometimes. Alas, the awkward bitnet system at Bochum (till about 1992) slows him down. This might be a blessing in disguise.

The next two candidates are suggested to share the prize:
Roger H.:

A James Joycean character let loose on a bitnet terminal. His spoken English (?) is famous in the community for his pronuncial crypticity; his bitnet English is a delight for pastime decipherers of Ulysses' second/third meanings.

J. Holger B.:

Esteemed at home for introducing the melting pot lingual flavour of his region into his entertaining and witty style of oration, he goes on and tries to blend the unique (and never to be doubled) syntax of grass-roots and oral-history ellipses into written scientific English, to the amazement, joy and pleasure of those of his readers who are familiar with the origings of his jerky way of communication, but also to the utter puzzlement of the less initiated. He is one of the forerunners (he is a runner, too!) of the literary "quantum jump in word and thought movement", and he excels in moving either way even inside a given phrase.

Christer J.:

Has been called "Alberich" for hoarding a Rheingold of spectroscopic data, and for never letting go of them (by publication). He was cited "for his neo-surrealistic prose which by defying the established laws of Nature reverberates the conditions of Mankind in a chaotic Cosmos."

A.E. "Gene" L.:

Famous for his extended periods of non-communication during which he probably works on extremely compressed, hyperlevel lyrics. An illustrative example which purportedly relates to receiving a letter from NSF (the National Science Foundation in the U.S.): "Oh, shit!"