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 early days

The idea of atomic physics (optical) observations on ion beams which had interacted with thin foils came independently to two nuclear physicists, many thousand kilometers apart, L. Kay (England) and S. Bashkin (Arizona, USA). Both had great ideas of what to do with such a light source, and both saw and described many of the opening options. Both were nuclear pysicists by training and worked in nuclear physics accelerator laboratories. Suggesting non-nuclear work to be possibly interesting, they got into problems when encountering the inertia of their nuclear physics faculty colleagues who apparently actively disliked this upshoot technique which did deflect one's mind from the right (nuclear) path. However, both scientists fared differently: Kay's colleagues at Manchester were more effective: He was in a junior position, and they blocked his attempts at pursuing the new idea and possibilities. Bashkin's colleagues at Tucson tried hard not to let him do his new sort of research, but they did not quite manage. Thus Tucson became the breeding place of BFS.

However, these things don't work smoothly. As Bashkin put in in his recollections (Nucl. Instrum. Meth. B **9**, 546 (1985):

> "... I happened to look [still at Iowa] into the target chamber, and the solution to the oscillator-strength problem [of determining elemental abundances in astrophysics] was there before my very eyes, for I saw the glowing [lithium] beam. That was pretty exciting - but only to me. Nobody else was interested, and my requests for money to study this phenomenon elicited three kinds of response: there was suspicion, for the funding agencies were anxious to support imaginative research, but only if it were along traditional lines. There was amusement that anyone could think atomic structure could be worth investigating, and there was disdain, which was succinctly expressed by one agency representative who said, "You know, Bashkin, if these experiments of yours were practical, they'd have been done 30 years ago by somebody who was smart."

In 1962, Bashkin moved to Tucson, also hoping for interest from the growing number of astronomers there (and indeed he got some help, every 25 years or so ...). Somebody somewhere must have heard of this new use of fast ion beams, and an accelerator construction company, High Voltage Engineering Corporation, asked Bashkin to speak about it at the Third Accelerator Conference they sponsored at Boston (MA) in 1963. In this talk called "Optical Spectroscopy with Van de Graaff Accelerators" (always keep the sponsor in mind!), Bashkin outlined his ideas to the mostly nuclear physics and accelerator engineering oriented audience. The printed report (Nucl. Instrum. Meth. **28**, 88 (1964)) has lots of tables compiled from other sources (ionization energies and levels of helium, ionized lithium (Li^+), triply ionized xenon (Xe^{3+}) from Charlotte E. Moore's 1958 Tables (NBS Circular **467**, Vol. III, 1958)) and charge state distributions of fast ions after being passed through a thin carbon foil), but not a single atomic spectrum to show. One one hand this reflects Bashkin's lasting eagerness of compiling reference data - which lateron led to the Bashkin-Stoner Tables on "Atomic Energy Levels and Grotrian Diagrams", on the other it told plainly that the ideas were there, but had not been put into any spectroscopic practice - with the one exception of a concurrent paper by Kay (Phys. Lett. **5**, 36 (1963)) who had indeed achieved a first spectroscopic observation of foil-excited fast ion beams of nitrogen and neon and attempted to identify the dozen or so lines seen in the spectrum.

Spectrum of a Nova

This conference presentation of not yet existing data, however, spurred quite some interest in different quarters. One was essential: The conference sponsor, HVEC, within a week after the conference offered a week of accelerator time to Bashkin, with a single week left to prepare an experiment. Back to Bashkin's recollections:

"On a Monday, I learned that there was not any target chamber available. On Tuesday, during lunch with Carl Perceny, the head of our machine shop, I sketched a target chamber on a paper napkin, which is still in my desk. That chamber was finished by Friday morning.

Aden Meinel ... had agreed to come along to do the spectroscopy with a portable nebular spectrograph he had built. We left on Friday, November 23, 1963. We had to make an unscheduled landing at Pittsburgh, were we learned that John F. Kennedy had been murdered that morning. We continued on to Boston and,

for the next several days ... Aden and I, and Kevin Melia of HVEC, ... were working from early morning to late every night.
... The experiments were highly successful, although in a rather unexpected way, as you can see from the title of our paper, *Laboratory Excitation of the Emission Spectrum of a Nova* (Astrophys. J. **139**, 229 (1964)).
Aden [Meinel] preceded me back to Tucson and promptly persuaded Richard Harvill, then President of the University of Arizona, to buy me a small Van de Graaff accelerator. After that, matters moved a bit faster."

So HVEC's move to sponsor Bashkin's talk and giving him a chance to demonstrate his ideas in practice paid off - for both sides. Soon after, Bashkin got another offer of accelerator time and chose Leon Heroux at the Air Force Cambridge Laboratory. Again there was a single week of preparation time, to let Heroux build a target chamber suitable for attaching a grazing-incidence spectrometer [which works in the vacuum-uv spectral region] and attach the spectrometer which had just returned from rocket flight experiments overseas. Of the week of accelerator time, six days were utterly frustrating, but then everything worked on the seventh, from the first ever vacuum-uv spectra of foil-excited ion beams [of multiply ionized neon atoms] to the first bfs lifetime measurements (Phys. Lett. **13**, 229 (1964)).

Stanley´s first experiments with the beam-foil light source set the stage for much of the later work. They presented spectral features not seen in the laboratory before (at least not so), but often resembling observations in astrophysics: Nothing less than the spectrum of a Nova, most befitting the glorious prospects (and the short duration?) of beam-foil spectroscopy. Of course there were massive obstacles to be overcome, impossible deadlines required makeshift solutions, the journal hesitated to accept such a paper: quite a fitting overture. Bashkin and Kay in parallel conceived methods on how to identify the charge states of the ions responsible for individual lines, and Bashkin quickly added quantum interference effects to the agenda, by recognizing oscillations of the light intensity along the flight path when exposing hydrogen atoms to external fields.

Stanley's outlook papers: wide perspectives

Popular science journals (as in contrast to learned journals) were more eager than fellow scientists to get and spread the news on the new method. As early as in 1965, Bashkin wrote an article for Science (Vol. **148**, 1047 (1965)) on "A New Method for Studying the Atom". You know, there is the story of a physicist who was contacted by one of those journals and asked whether he would write a paper for them. The physicist replied that he would not, because he was too busy with the physics. Maybe years later, when his frenzy about his own work ebbed, he may have realized what a chance at self-promotion he missed - popular science journals are read by many more people than the dreary specialists' journals, and they are more likely read by people not already familiar with what one does. And their articles are often much more comprehensible, so they are sought for as references and for teaching purposes by the colleagues, much more so than the original papers!

Well, Stanley Bashkin was quick to exploit and advertise his new gimmick: He went to conferences (like the Flagstaff meeting of the American Astronomical Society) and spread the word on a light source that did not have most of the troubles (physics problems) of the others, and he wrote superb papers announcing the capabilities of his new technique. He was thorough in the presentation and very farsighted, indeed. Of course, he could not do all the follow-up work on his many ideas on his own, but he made himself famous, organized conferences on the young field and incited a new generation of workers in the new field.

(First) Conference on Beam-Foil Spectroscopy, Tucson, November 20-22, 1967

101 Dalmatines, sorry, Conferees convene at Tucson (Arizona)
Proceedings published in : "Beam-Foil Spectroscopy", edited by Stanley Bashkin, published by Gordon and Breach, New York 1968 (2 Volumes). Particularly notabel are two color photographs in the front of Vol. I, showing the light from foil-excited ion beams.

Bashkin himself delivers the introductory and survey talk: Photographic recording is still standard at the time, though the need for linear (photoelectric) detection is recognized; Tucson work shows how electric fields lead to quantum beats (Stark effect). There is a paper (L. Brown *et al.*) from the Department of Terrestrial Magnetism (of the Carnegie Institution, where a giant size old Van de Graaff accelerator is available); the authors report on using an image tube detector for decay curve measurements. This device is a precursor to modern multichannel photoelectric detectors, but here it is used only as an amplifier before a photographic plate records the signal. R.H. Hughes from Fayetteville (Arkansas) talks on spectroscopy with gas targets, and his work will be taken up again by Bashkin some twenty years later. J.A. Jordan points out tandem accelerators for BFS and names the study of high-nuclear charge one-electron ions (Lamb shift and such quantum electrodynamical effects) as one particular field of interest. C.D. Moak (Oak Ridge) presents charge state distributions of energetic heavy ions after interaction with the exciter foil, E.L. Haines *et al*. (JPL at CalTech) study electron yield from such processes. L. Heroux (Bedford, Mass.) presents lifetime measurements on extreme ultraviolet (EUV) spectral lines. The questions after this talk are amusing: the multi-exponential analysis is something the nuclear physicists know from radioactive decay chains, and *least-squares techniques* versus *plotting and visual analysis* continue traditional battles - until the advent of cheap computing makes the use of logarithmic maths paper obsolete. The cutest question is whether one can get at *shorter* lifetimes by slowing down the incident particles, that is by working at lower ion beam energies. Physics would suggest the opposite, but apparently nobody noticed at the session or when editing ... The rest of the discussion is on *longer* lifetimes, and that quest took almost three decades and the introduction of heavy-ion storage rings to achieve precise results far beyond the nanosecond range treated in the early work. Next, W.S. Bickel (Tucson) elaborates on the massive and nasty problems of intensity measurements - very recommendable reading. And there are experiences with the never-steady foil properties. J.Z. Klose from the National Bureau of Standards (NBS) explains lifetime measurements by pulsed electron beams, W. Happer those exploiting the Hanle effect. J.R. Peterson *et al*. (Stanford) study the production of fast atomic beams by letting fast ions capture an electron - this technique will provide very precise lifetimes on neutral alkali atoms more than a quarter century later, when such numbers are of value for testing theories on atoms which are studied for parity nonconservation. M. Dufay, the head of the very active group at Lyon (France), presents their lifetime data on various nitrogen ions. The subsequent paper reports on work by D.J. Pegg's group on neutral hydrogen, concluding the volume on fast beam experiments. Vol. II claims to be mostly on theory and astrophysics (a client community), but the actual contents contradict the announcement. W.L. Wiese (NBS) shows many graphical representations of his analyses of data along isoelectronic sequences, R.H. Garstang (JILA Boulder) outlines the steps in obtaining theoretical data on transition probabilities, as well as a catalogue of data needs in astrophysics. Glen W. Erickson (UCD) discusses the present status of the Lamb shift and his expectation that future precision experiments might tell about nuclear structure - right he is. Papers by Y.F. Fan (Tucson) and by I.A. Sellin (Oak Ridge) deal with one-electron ions, including their joint insightful experiments on those. The astrophysicists show spectra over spectra - so many lines! such a marvellous spectral resolution! - even elegantly curved spectra obtained on quasars - and still they ask for even more data: They often would like to know the identities of some interesting lines, need particular transition probabilities for the determination of stellar and interstellar conditions from observed lines. Many lines of their interest, however, do not appear in the usual terrestrial light sources, because there their initial levels would be quenched by collisions and external fields before radiation can be emitted and observed. Beam-foil spectroscopy might, partially and eventually, help with some of those quests. Not with precise wavelengths in general, though, that is a lasting and severe problem with the fast-beam light source. However, in the early days a factor-of-two lifetime precision often seemed useful; lateron these demands approached the 10%-mark (if not better) - and the experimenters eventually delivered. On the other hand, astrophysicists develop and use spectrometers and detectors which are the envy of the ion beam spectroscopists - such neat gimmicks! ... so much good work needed!

Well, this (incomplete) review of the contents of the first BFS meeting shows that it truly set the stage for quite some time: Some of the invited speakers would organize later BFS conferences (Dufay at Lyon 1971, Bashkin at Tucson 1972, Sellin and Pegg at Gatlinburg 1975), move on to advisory boards (and thus their presence at future meetings be subsidized), but some would simply remain a standing item on the program. So would most of the topics of this first meeting, reflecting the tedious development work on ideas which where there early, but needed many man years (very few women in the field, alas, like in most of physics) to bear the fruit of reliable data. The recently invented laser has not yet made it to the field - that will be an event 12 years later, when precise laser spectroscopy on fast beams stirs the community.

Young, eager scientists to take up the tasks:

like Indrek Martinson (born in Estonia, a World War II refugee child grown up in Sweden, studying physics at Stockholm and being detached to Tucson in 1968 by Ingmar Bergström, his boss, who had attended the first BFS conference there), Gordon Berry (from the Northern Dales of England, moving to the US (Madison, Wisconsin) after his Oxford doctorate, eventually becoming a convinced American with an operating base in the Chicago / Argonne National Laboratory / Notre Dame area), Horst Jürgen Andrä (after his Munich (Germany) doctorate, heading for Berlin, Münster and Grenoble), who came to Tucson for their first exposure to Stanley and to BFS. As it turned out, they installed much of the apparatus still in use (or at least on the premises) 25 years later. What can hardly be understood is that they enjoyed working even when Stanley was absent, on his then frequent trips to tell the world about the splendours of his pet physics technique. Even stranger seems the thought that these people claimed it being easier to get on with the work while the Great man out was of the lab ... they must have been joking!

Dick Schectman of Toledo got hooked by Stanley's talk at the Third Accelerator Conference at Cambridge (Mass.) in 1963 (Nucl. Instr. Meth. **28**, 88 (1964)). He then lured Lorenzo "Larry" Curtis to Toledo. Larry had been into oceanography at Woods Hole, sometimes going out in ships that seemed not particularly seaworthy. In those wild days Bill Bickel proposed (it is said) to study tritium in a pool, with himself scuba diving in there, whilst the rest of Tucson might be evacuated for safety reasons ... so it seemed about time to start something less dangerous, like Stanley's laboratory studies of Nova spectra!

Experimental spectroscopists run certain risks doing their job. In the old days, when shock tubes were used at the Harvard-Smithsonian Center for Astrophysics, Cambridge, Mass., Hans Palenius from Lund, Sweden spent time there as a postdoc (and tells the following stories himself). Together with John Kohl he constructed a unique shock tube making measurements possible even below the transmission limit for LiF windows. They used this shock tube to make the first measurements ever made of the photo ionization cross sections of atomic hydrogen and atomic oxygen on an absolute scale. The shock tube always worked fine, but at one occasion they needed a better capacitor for a flash lamp. Without Palenius' knowing, the capacitor they found in the lab also had some inductance and therefore did not discharge completely, so after the first time in use he got a spark from the remaining charge over to his nose, when looking close to the flash lamp in order to see if its alignment with the shock tube was correct. Luckily he was wearing shoes with rubber soles, so he was insulated from earth, but he felt the electrical discharge with the whole of his head and he got a conspicuous red scar at the tip of his nose. This scar stayed for a couple of weeks and he had to explain its origin to everybody he met. Palenius had been a frogman in the Swedish Navy and in those days he had handled mines. Maybe this explains his preferential procedure for getting rid of the hydrogen in the shock tube. High pressure hydrogen gas was used as a driver gas bursting a diaphragm and initiating the shock wave and building up the useful shock front in local thermodynamic equilibrium. It was important to get rid of the hydrogen leftovers, so it did not constitute a major danger under the roof of the laboratory after many tube experiments. A hydrogen explosion had occurred in another laboratory a few years earlier with disastrous result. Therefore, Palenius used a match to let the hydrogen puff off in a controlled way with a big flame after each experiment, when the amount of hydrogen was limited.

A few years before Hans Palenius came to Harvard, Indrek Martinson was in Tuscon frustrated at having observed spectral lines with a beam of fluorine ions that he and the Tuscon group could not identify. Indrek wrote a letter to professor Bengt Edlén in Lund about it, and Bengt forwarded it to Palenius, who worked with fluorine atomic spectra at that time. Hans could identify all the lines directly as belonging to transitions in different stages of ionization and sent the complete identifications back to Indrek. This correspondence resulted immediately in a lasting friendship between Hans and the beam foil people and as well in a publication in JOSA (1970) with the authors Berry, Martinson, Schectman, Bickel and

Palenius. Later in Stockholm, Palenius (1976) himself also performed a beam-foil experiment, this time with a scandium beam, resulting in a publication with co-authors Curtis and Lundin.

In 1980 Palenius was invited by Stanley Bashkin to visit Tuscon. The temperature was 45 degrees Celsius, when Bashkin welcomed Palenius at the air port, and telling him that already 15 people were reported having died from the heat. Bashkin also told Palenius that the air condition was out of order, but he had a convertible car. So the drive in to town without roof heated Palenius' both from direct sunshine and by convection from the flow of hot air. Well in Bashkin's department, Palenius was surprised finding no students and a laboratory with instruments looking like museum equipment. Two things were more important to Bashkin than anything else. First he wanted Palenius to write his name in his guest book, already filled with names of almost every other living physicist and second he wanted Palenius to play chess with him.

Who met / visited whom in the early days, at The early centers of growth:

Extended visits to Tucson:
Indrek Martinson, Gordon Berry and Horst Jürgen Andrä spent extended visits to Tucson in 1968/1969. They particularly enjoyed the time when Stanley Bashkin was traveling and Bill Bickel was the acting chief. They also started a network of influences, and the strings they pulled (or left dangling) shaped the field for decades to come.

> In 1968 Indrek Martinson came to Tucson to learn the beam-foil business from the grand master, Stanley Bashkin. There he met Gordon Berry and Horst Jürgen Andrä on the same mission. Thus some of the later stars were born in the same mould - surprisingly perhaps, when seeing the later developments.
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> In the beginning, Stanley had expanded on the possibilities of the new field and foretold almost everything that would be done for quite a few years. The gold rush, however, produced rather many weedy data everywhere, but not all of the early findings stood up to later scrutiny. Already in the early days, patterns of work and ways of people emerged which could only be reproduced later. Indrek Martinson remembers:
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> "Shortly after I arrived at Tucson, Stanley [Bashkin] told Bill Bickel that somebody had called and wanted to know whether the lifetime of the ^{1}D level in C III had already been measured by beam-foil. Would Bill please check the literature? Bill did. It had been measured and published. By Bashkin, Bickel and Curnutte."
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> In the glorious old Tucson days, there suddenly appeared a problem of charge state assignments. For this case Stanley wanted to use his techniqe of charge state splitting, by deflecting an ion beam towards the spectrometer in an electric field. The differently charged ions would be deflected differently and thus their emission lines would show different amounts of the Doppler shift. Normally one would think of this as requiring many kV of DC voltage, but Stanley had thought up a scheme which would work with 50 kV AC at 60 Hz. All he needed (on a Friday afternoon) was a proper transformer, and he called *Tucson Gas & Electric*'s boss for this. They had such transformers. The envoys to collect the transformer were Indrek Martinson, Jack Leavitt and Jan Bromander. They found the place and the warehouse, found the worker ordered to help them. When asked about the secondary voltage of the hefty transformer they had in mind, he kept answering "120 Volts", whilst the three visitors wanted 50 kV. The company used these transformers to downtransform high voltage to mains voltage, whereas for Stanley's scheme the transformation went the other way and needed the gadget to be run backwards. So this caused some discussion and misunderstanding. After some quarreling the guy got a phone call fom his buddies who wanted to go for a beer, and the visitors could clearly hear him answering: "Three assholes from the university are here and keep me up!"

When the noise on a photomultiplier tube went up during a measurement, Stanley ordered Indrek to change the oil in the diffusion pump. Stan left. Indrek postponed the chore which he could not quite see as being correlated with the detector problem. He met Jack Leavitt, who commented: "That's nonsense. But I guess we have to."

Jack later went to Berkeley. When asked about the former and the present boss, he replied: "Dick Marrus is the better boss. He doesn't come to the lab and spoil the experiment."

Ingmar Bergström at Stockholm's Research Institute for Physics had enthused about the options offered by the new field of BFS. After attending the first conference at Tucson, he suggested that his former graduate student (in nuclear physics), Indrek Martinson, spend some time at Tucson to learn the trade. In hindsight, Indrek at times was not so sure whether that turned out good for him. Had he followed his own earlier paths of thought, he would have become a schoolteacher and spend a quiet (?), modest life there, and sometimes he felt he missed that option. As it turned out instead, he became a centerpiece and stronghold of international relations, rose to the highest ranks of Swedish physics (yes, Nobel committee and all that), furthered international relations and the recovery of Estonian physics after the collapse of the Soviet Union, Editor and Honorary Editor, multiple scientific board member and whatever else, leaving little time for his favorite pastimes of masterly playing chess or listening to jazz music. Unfortunately, societal duties (which he performed extremely well) lateron took their heavy toll on his health.

At Tucson he met many of the key characters that were to form the field and to shape the politics in the field.

Gordon (Berry) was exceptional in those days, both in Tucson and Stockholm. When we had finished measurements and went home, tired - and sometimes happy with the results - Gordon came in next morning with a hand-written rough draft, the authors being typically H.G. Berry, W.S. Bickel, I. Martinson and S. Bashkin. So it was quite obvious that he would be the first author.
When Jürgen (Andrä) appeared in Tucson, in May 1969, he joined an experiment on Stark beats (Wasserstoff) that had been started by Gordon, Bill and Hank Oona (grad student in those days). A few weeks later, when Gordon was going to leave Tucson, for England and from there to Stockholm, a manuscript appeared, Berry - Bickel - Andrä - Oona. And then followed shouting in the corridor, not far from my office "Why am I the third author?" "Because we had already done much work when you came" "But all the good and important ideas came from me!" The I went out for a donut and dont know what followed. But the result was that Gordon and Bill gave up and later a new paper appeared where HJA was the first as well as the last author. (Indrek)

Date: Wed, 8 Nov 89
From: "Indrek Martinson, Atomic Spectroscopy, Lund"
To: EAMP9@CFA4.BITNET (that is to Elmar Träbert)

Early history of BFS.

Somebody should write a book on this, in the way Haro [von Buttlar] wrote a book with anecdotes about his teacher. (Will there be a similar book about Haro one day ???) [no! But there are many books by another von Buttlar (Johannes) who is disliked for them by the traditional branches of the family - time travel gossip and esoteric fantasy sells well, but is not quite up to the social standing and repute of the (formerly) landed gentry]. Just some tokens:

In 1970 (Lysekil) Veje gave a talk on excitation measurements for He I (beam-gas). After the talk someone asked "Did you correct for cascades", A: "There is no cascading", Q: "????", A: "Because there are no higher He I levels in Moore's tables". [E.T.: After the editing by the referees, the questions and answers after Veje's talk look rather bleak and noncommittal in the conference proceedings. However, with Indrek's memorized version as a reference, the oral history version sounds more real and tells about the true issues at hand.]

In another talk he was explaining his data with the Landau-Zener model. After the talk Weiss pointed out that the model was not applicable for the energy range used in the experiment, A: "Yes I know, but this is the only theory that there is".

In another paper (this time with Mannervik) Veje measured the lifetime of the Mg I resonance line (2851 Å) and obtained the value 2 ns, in excellent agreement with theory. The authors also determined the decay of a Mg I transition at 5706 Å obtaining 2 ns which differed wildly from the theoretically calculated 55 ns. That paper also includes discussions why theory is wrong for higher states, etc, but the authors did not realize that lifetimes tend to be grating-order (diffraction order) independent. [and their measurement obviously apparently was dominated the second diffraction order image of the first line]

Well, now I have been nasty only to Veje, lets hope that he has similar stories about me. I still remember my own first talk at the [research] institute [for physics, Stockholm], in 1964 or 1965.

It was a 30 minute report on an isomeric level in ^{113}In. In addition to gamma energies, lifetimes etc. I also reported on the intensity ratio of the K and L conversion peaks. Ingmar (Bergström) then interrupted me saying "why did you measure this ratio? It can be computed easily from the tables of Rose? For an M4 transition at this energy everybody knows that it should be 5 ± 20 %". I didn't know the answer, but my thesis advisor then interfered saying "Well, it is better than doing nothing, I guess". This probably holds for very many of our subsequent measurements.
Regards, Indrek

Yes, Indrek, we are right into that very book you suggested. And we are approaching the 1970 BFS conference at Lysekil (a former fishing and now resort village in the rocky islands off Uddevalla, on the Swedish west coast). There has been a smaller "European" BFS meeting at Aarhus (Denmark) in the year before, that is across the Kattegat from Lysekil. Torkild Andersen and Ove Poulsen lead the atomic physics efforts at Aarhus. There would be a "Second European BFS Conference", hosted by M. Dufay at Lyon in 1971, but then this side track faded out. Anyway, attendants would originate from all over the world, since they really moved around to work at each other's place. Bashkin visited Australia, Martinson returned to Stockholm to set up facilities on a small isotope separator there, Berry went to Stockholm (1970) and Lyon (1971), and many others travelled extensively, too.

European Conference on Beam-Foil Spectroscopy, Aarhus (Denmark), April 1969

No records found so far

Second International Conference on Beam-Foil Spectroscopy in Sweden, at Lysekil (Sweden, June 7-12, 1970.

Well, I suppose Ingmar Bergström backed the young generation in stageing the Second International Conference on Beam-Foil Spectroscopy in Sweden, at Lysekil in June of 1970. The Proceedings (Nucl. Instr. Meth. **90** (1970) - publication date in the same year as the meeting!) were edited by Indrek Martinson, Jan Bromander and Henry Gordon Berry, all at Stockholm. We also see Larry Curtis mentioned in the Editorial; Curtis had just forged a life-long friendship with Martinson. Curtis got a position in Sweden and married a Swedish wife, but then he found that he would not want to spend his life there (rumors say, he felt unnerved by the Iron Curtain across Europe and was afraid of someday seeing Russian tanks in his front yard at breakfast) and moved back to Toledo (Ohio), an industrial city near Detroit, but for years in such provincial backwaters that Richard Crossley (from York in England, also a Sweden fan with a summer house near Kalmar) called it the "Eslöv of the United States" - deriding Eslöv (a small town not too far from Lund) as a proverbial backwater place. Well Curtis' wife, Maj, then had to adapt to life in the US, working as an artisan weaver (even earning once the honor of contributing a small item to the White

House Christmas tree ornaments). She managed, but the couple settled for regular extended visits to the Swedish parts of the family heritage.

Back to Lysekil and the 1970 meeting. Again, Bashkin delivered the keynote introductory survey paper, cramming dozens of effects into his talk, all claimed to be interesting. Some were, some would disappear under scrutiny by people with a different phyics background. However, an enthusiastic speaker and poet as Stanley is entitled to see things differently from the rest of us. As a good salesman, Bashkin opened his 1970 talk by announcing the growth of the BFS community to more than 20 laboratories worldwide - and his own next BFS meeting, to take place at Tucson in 1972, and even telling about other meetings in the area where papers which might not fit into the agenda (by sheer numbers?) might be diverted to. Also, he showed his eagerness in obtaining records: With the mounting "flood" of BFS papers, he suggested to send a copy each to him, so that he would maintain a bibliography and send out quarterly updates to everybody interested. I am not aware that this scheme took off, because most people would be interested less in the method than in particular applications. The atomic lifetime data collection then was incorporated into Wiese's efforts at NBS (where also wavelength data were hoarded and analysed), and Baskin began his own collection of spectroscopic data which led to the Bashkin / Stoner Tables (Atomic Energy Levels and partial Grotrian Diagrams, several volumes published by Elsevier's). So Bashkin notes many serious and/or curious effects. Among these are the sad news that the valiant efforts to boost detection efficiency by using tricky collection optics (axicon and tubicon) did not nearly work out as well as expected. There still is the problem of odd (and rather wide) spectral line shapes (which will take interesting turns later), there is a peculiarity about lifetimes in rare gas ions which Bashkin claims runs contrary to all expectation: Apparently the lifetimes increase with nuclear charge, whereas isoelectronic sequences predict that they ought to go down. The slight oversight in the discussion is that (apart from experimental problems at the time) the data do not pertain to isoelectronic sequences, but to similar degrees of ionization, therefore dealing with levels of increasing principal quantum number, not just nuclear charge. There is no straightforward scaling rule for this case, and thus no need to worry. A very interesting point affects a strong line in sulphur spectra recorded at Tucson: One of the strongest lines in the spectrum is unidentified (this recurring experience, with a different explanation, will haunt Träbert's studies of delayed spectra two decades later). Bashkin claims this as something particular to BFS. "What place do these lines have in the scheme of things? We do not know. Neither do we know why these lines fail to appear in other light sources." This point shows the value of interdisciplinary conference attendance: Alan Gabriel, a plasma spectroscopist with interest in space research indicates that Bashkin's strong sulphur line coincides with hydrogenic transitions between levels of principal quantum numbers 6 and 7 in 5-times-ionized atoms. Sounds complicated? No, very simple: Electrons in such high levels see the nuclear electric field and the inner electrons averaged out and resembling a simple charge. Hence the level structure and spectra of such elevated electrons are (almost) as simple as that of the simplest atom, hydrogen. Gabriel has pointed out something very valuable, and the beam foil spectra everywhere abound of such lines. They can be calculated rather well, and this makes them suitable for the calibration of unknown spectra. An idea from astrophysical spectra fertilized the field of BFS - and BFS eventually would pay back by helping with line identifications and oscillator strengths for astrophysics.

But we are still in Bashkin's talk. The foil excited ions are fast by definition, as slow ions do not travel well through matter (and remain in low charge states and destroy foils more rapidly). This means that the ions pass quickly through any measuring chamber, limiting the observable lifetimes to the range of a couple of hundred nanoseconds at best (and even that with severe problems). For even longer lifetimes, Bashkin suggests using a pulsed ion beam (0.1 ns) sent into a vapor; nanosecond lifetimes would then be studied by delayed coincidences. A grand scheme, accepted to be difficult. In one aspect it is obviously silly, as the projectile ions would pass through and be gone anyway, so they could not help with delayed coincidences. The only way would be atoms of the vapor becoming ionized and excited and detected. This is something new which will lateron be used as recoil ion spectroscopy (and even with timing features), but I hesitate to acknowledge that Bashkin at the time saw this particular avenue. The gist of the talk then turns to the general benefit of BFS to the physics world. Lifetime measurements on iron have just been performed at CalTech and helped to remove inconsistencies in the data on abundancies. Bashkin spins this real yarn a bit further and links it to the purported neutrino deficit of the sun. A funny typo slipped into the Proceedings, too: Once in Bashkin's talk the Lamb shift (after W.E. Lamb) is contorted into something certainly useful for Applied physics, the Lamp shift.

Lots of useful talks followed Bashkin's speech. For example, M. Dufay from Lyon detailed the various systematic approaches tried to help with line identifications: While the element is obvious (an advantage over classical light sources), there are more charge states produced and higher lying levels (including those with several simultaneously excited electrons) excited than seen in other sources. In this context, Dufay discussed observation geometries for spectroscopy at fast ion beams, including peculiar designs like end-on observations to minimize Doppler spread of the light - at the cost of maximum Doppler shift - from behind an ion beam bending magnet (to avoid the ions striking the detection system). This particular design has been taken up, for example, by J.D. Silver (Oxford) more than 15 years later, still hunting for narrow spectral lines from the fast ion beam light source.

Wiese demonstrated updates of his isoelectronic analyses and struck a chord that was reactivated many times and reverberated in many people's memories for decades to come: Many BFS lifetimes came out too long, which was correctly identified with the problem of cascade analysis. Any distortion by cascades (short-lived or long-lived) would flatten the shape of the decay curve of interest and lead to a systematic error - if not countered by careful analysis. Unfortunately, many people who moved up status ladders and away from BFS (but into influential administrative or referee positions) only remembered that "BFS lifetimes are always wrong / too long". This persisted long into those times when some people had developed insight and algorithms to deal with the cascade problem (where possible) so that data without notable systematic error (because it could be quantified and corrected for or even avoided straightaway) became available - still from BFS, as nothing else was available. However, bad labels tend to stick.

Later comments on the early claims that bfs would yield proper lifetimes (which it eventually did, but only later, after people had learned what to do and what to watch out for):

> "BFS is a Random Number Generator" (George Victor, at the Harvard Smithsonian Center for Astrophysics)
>
> "Beam-foil lifetimes are invariably too long" (?? and Dalgarno, Ref.:)

(Well, there is experimental proof against this theorem, see the Berkeley work (R.W. Schmieder and R. Marrus, PRL **25**, 1245 (1970) on He-like Ar, 1s2s $^3S_1$, which made it to Phys. Rev. Letters, Physics Letters and Physical Review A at the time.)
Still, George Victor admits that there were cases in which a beam-foil lifetime measured on a wrongly-assigned line still was much closer to truth than the calculations which might have been off by several orders of magnitude.

> Date: Fri, 10 Nov 89
> From: "Indrek Martinson, Atomic Spectroscopy, Lund"
> Dear Elmar,
> As to George Victors point on lifetimes, similar things have been said about the papers by Chaghtai *et al*. I have been a keen defender of the Aligarh work by pointing out that it is also quite valuable to know where the transitions and energy levels aren't. In these days this support is more difficult to provide because now one cannot even be sure that all their data are wrong, this is partly because they now use the Cowan code.
> Regards, Indrek

Among the laboratories new on the BFS map were Aarhus and Stockholm in Scandinavia, Edmonton and Québec in Canada, Kansas State University at the Small Apple, Manhattan, Kansas, in the USA. Aarhus ran a relatively small 600 kV machine but used it on many elements all over the periodic table. Lyon went ahead with (then high) energies of 1 MeV per nucleon. Eric Pinnington (Edmonton) devised an optical system to scan the excited ion beam instead of moving the foil or the detector, and the Toledo (Ohio) group under Dick Schectman (who had been trained in BFS by tutorials at Tucson) devised an alternative geometry for similar aims. The main conference papers on competing techniques were on the electron photon coincidence method as developed by Frank Read at Manchester (England) and on electron beam phase-shift techniques pursued at Princeton. On theory, Andy Weiss from NBS showed a wealth of theoretical developments and their applications. He had a charming way of knowing how complicated many things in proper calculations are, but of nevertheless providing experimenters with the guidance of information from doable (simplified) calculations. He kept his output of formal publications relatively low, because he was aware of the imperfections of the days algorithms, but experimenters found an open ear when asking for advice or test computations. A second theory presentation was dealing with a new, very successful approach of treating partly filled electron shells, authored by Nicolaides and Sinanoglu from Yale. Nicolaides is a Greek, and Sinanoglu is a Turk - and a conference later they would be at arms against each other (replaying national history?).

The already mentioned Alan Gabriel presented the physics of plasma light sources in general, and then J.P. Connerade compared x-ray data from highly charged iron ions in a plasma focus device with solar observations made by satellite. X-ray astronomy and x-ray solar physics were still in their infancy and looking ahead to new venues and developments. So did some accelerator guys. For example, H. Krupp from Heidelberg reported on ion sources for multicharged ions which were planned for one of todays large heavy ion accelerators, UNILAC at GSI Darmstadt. That (later) made possible a twentyfold increase in energy over the Lyon work of the day (and so would the big French machine, GANIL), and then it was cut down to serve as the injector for a synchrotron (SIS) that would boost the energy by another such step and more. So this was a report from a group at Heidelberg preparing for the future big machine at Darmstadt. Other people who later made it to GSI Darmstadt worked at Jülich at the time, as a report (on inner shell vacancy production) shows, with Paul Mokler and Peter Armbruster among the authors - as well as Hans Lutz, who later specialized in lower energy collision studies at Bielefeld (Germany) university.

Let us return from the future for a while. The Lysekil conference had talks which could almost serve as tutorials, by L. Heroux on photoelectric detectors, by Bakken and Jordan on optical detection system designs, or by Bridwell et al. on computer control for BFS data acquisition systems. Carriveau (Australia) and his visitor Bashkin presented data showing the purported simplicity and usefulness of Bashkin's technique for identifying charge states. Well, the method fell in disrelict afterwards, as more and more charge states were reached which could not be resolved so easily ... The techniques presented by Larry Curtis and his Toledo colleagues fared better. This was the lead paper on cascade analysis techniques, explaining the basics in graphics and formulae, presenting simulations to demonstrate the principal limits of naive analyses, and then laying the groud work for a technique that (in principle) would perfectly cure the cascade nuisance for any lifetime measurement, ANDC. Curtis et al. suggested to measure not only the decay curve of a level of interest, but also the decay curves of the transitions that replenish this particular level. Any cascade "intensity" of a transition to level "A" must, sooner or later, contribute to the decay "intensity" of that very level. Thus rigorous mathematical connections can be formulated and implemented in various ways which can be checked against each other for consistency. This way the analysis moves from systematic-error prone evaluations of individual decays to a correlated analysis of connected decays, which may yield exact (with statistics limimits) results for the lowest decaying level in the system studied. In a later step, it would be realized that one does not need to measure the actual cascades, but only the shape of the decay curves of the levels involved - which might happen on other decay branches of the same levels, possibly easier to study for spectral range or absence of spectral blends. Furthermore, there is no need for absolute intensity measurements, and the relative intensities are the fit coefficients in a linear computer fit. Thus the Arbitrarily Normalized Direct Cascade (ANDC) scheme was born for which several laboratories developed suitable computer codes. Afterwards many atomic level lifetimes could be measured essentially *without* any systematic error. However, the aforementioned statement on (most) BFS lifetimes being wrong stuck in people's minds. Of course, Curtis and colleagues were by far not the only ones investigating the cascade problem. Even at the same meeting, other, less systematic approaches were presented, and for cases not amenable to a complete ANDC measurement (for lack of equipment etc.) other techniques would be needed and tried.

Technicalities?

A presentation by Hank Oona and Bill Bickel, on systematic problems with the beam-foil technique, sparked a particularly long and fiery discussion. Oona and Bickel stressed the problems of statistics and (naive) analysis by exponential functions, mass contaminations (insufficient resolution of the analysing magnet and the beam transport system) and an apparent effect of the ion beam current on the measured mean lives. The discussion relates to so many things of interest that I expand on it (a little). Indrek Martinson refutes Oona's suggestion that their boron spectra might have contained oxygen lines, reasoning that then other, reliably strong oxygen lines must then have shown as well . In hindsight, this comment does not necessarily hold: Some ten years later, Reinhard Bruch visited the group of Paul

Henrich Heckmann at Bochum for EUV spectroscopy of boron. He was delighted to find some previously unknown strong lines - until a check showed them to originate from oxygen. ^{10}B and ^{16}O have ions of different charge states which under certain chance circumstances follow almost the same trajectories in the analyzing magnet. When the high voltage system in the accelerator tank suffered a spark, the feedback system accidentally locked on the wrong mass peak (for a while), transmitting an oxygen ion beam (of a different energy and charge state) instead of the wanted boron ion beam for the last leg to the experimental site. Thus there were sections of boron and oxygen lines alternating in the recorded spectrum - a pity for Bruch's splendid discovery, but realized before publication. Once such a possibility is realized, one can take precautions, like running the isotope ^{11}B instead of ^{10}B. However, the Bochum error was more modern than the Tucson one: Incompletely resolved ion beam fractions - as suspected at Tucson - would mean an *analog* effect, with varying fractions of each beam species reaching the experiment At Bochum the system was jumping back and forth between two slightly separated ion beams - which may be considered a *digital* effect ...

Next T. Hadeishi and Horst Jürgen Andrä bring up the problem of polarization and quantum beats. W.H. Smith suggests listing cascade component results along with the lifetime results of primary interest, so that the reader can judge on problems and systematic errors. P.L. Smith mentions cascade population effects (which are found to show with ion energy variation). L.M. Beyer mentions scattered ions missing the Faraday cup, a suggestion that is discounted by Bickel, but stays with the community over the decades, resurfacing again and again and not always going away by discussion. The top comment of the session is by Charly Moak after mentioning their own odd experiences at Oak Ridge: " ...nothing resembles more a new effect than a mistake". (Thanks to I.M. for pointing to this remark from Memory, decades later!) Sellin mentions metastable states which - he claims - survive all cascades. Well, most truly metastable states (not decaying via electric dipole (E1) transitions) are too long-lived for beam-foil spectroscopy - proper measurements on these will have to wait 20 or 25 years for the development of recoil ion beams, storage rings and electron beam ion traps. Long-lived levels, which Ivan Sellin must have meant, not only survive cascades, but thrive on them, as their initial level is filled faster (for a while) than it is emptied by the wanted decay. Alan Gabriel mentions an angle of observation (in electron-excitation measurements) at which the observation is independent of polarization effects. Bashkin replies that "in practice" there is no such well-defined angle in BFS. Well, in spite of the master's verdict, lateron several spectroscopy groups work at the Magic Angle (which is a matter of geometry and thus pertains to all beam experiments). Moak and Curtis clarify details of polarisation effects on observed lifetimes. Wolfgang Wiese tries to summarize the discussion. Among other points, he repeats his observation of too-long BFS-lifetimes and sees it confirmed by Curtis' mathematical analyses. What he does not see (at the time) is that Curtis' analyses also point out how to correct data or, even better, how to obtain data which incorporate the necessary correction by a suitable measurement, so that the result is quite accurate. Michel Gaillard rightly stresses the need for careful spectral analysis - an everlasting problem. L.M. Beyer suggests coincidence (and anti-coincidence) measurements. Andrä, who had coincidence experiment experience from his nuclear physics graduate student days, replies that upon consideration (of course long since done by him) such experiments are forbidding because of hopelessly poor coincidence counting statistics. Well, the Tucson group (Larry McIntyre) does manage such an experiment (not very conclusive beyond the proof of principle), and, lo! and behold, Andrä himself initiates such a photon-photon coincidence experiment on a fast ion beam slightly more than a decade later, when he is moving from Berlin to Münster. [The experiment will turn out much better than the old Tucson attempt, but still not good enough: By that time, Andrä (at Berlin) has obtained a very precise lifetime from beam-laser experiments on Ba^+ ions, and the coincidence measurement does not come quite close to that in precision and accuracy.] Finally Wiese suggests a test case for which calculations are thought to be good and BFS data are systematically poor, the 3d-4f transitions in Li-like ions. Unfortunately, this line is part of the chain of decays connecting the levels of maximum angular momentum *l* for a given principal quantum number *n*. These transitions make up the most prominent hydrogenic transitions (mention above), because each one feeds the next exclusively and fully: The lifetimes are very well predictable in a hydrogenic approximation, but consecutive levels differ in lifetime by amounts not easily separable by data analysis, the spectral ranges and exclusive decay channels involved largely forbid the application of the ANDC scheme, and with increasing charge state the lifetimes get very brief, indeed. In short, this is a simple case in which nobody seriously doubts that theory is right and is far more precise than any perceivable experiment can do - and thus the case is not suited for the wanted test. After all, not even

BFS afficionados would claim that this is the technique to do all and everything - but it can do some things (right) no other technique can! Still, biased by the right prejudices one may say good things of achievements or blame under-achievements without realizing the reference frame. Uff. So much physics and mutual misunderstanding in less than a printed page (p. 228) of the Proceedings ...

The conference program continued with another fruitful venue of BFS, multiply excited states of atoms. In the bulk of the foil, the passing projectile ions experience such rapid collisions that an excited electron often does not have time to de-excite before another electron is shaken to a different orbit, and the result is an ion with several electrons not in their energetically lowest possible place (Well, things are more complicated, but this simple picture will do.). Such ions are rarely seen in other light sources, and thus new physics become accessible. In multiply excited ions, many levels are at higher excitation energies than is necessary to remove a single electron from a regular ion in its ground state. Therefore spontaneous ionization (electron loss) is possible; this autoionization may be partly suppressed because of atomic symmetries and selection rules which thus can be studied. Competing processes take place, like transitions as in a regular ion (with little energy transfer, associated with visible or UV light), spin-changing x-ray transitions, or autoionization induced via higher-order effects like spin-spin or spin-other orbit interactions - a real playground for specialists. Thus observations can be done by people with spectrometers for the visible or the x-ray range or for electrons - and eventually these observations need to be fit together. Although Chinese physicists, Wu and Shen, had dealt with triple excitation in a three-electron system as early as 1944, the field had lain largely dormant since. Now E. Holøien from Oslo introduces the community to the theoretical aspects, and J.C.Y. Chen demonstrates how these states relate to the problem of resonances in atomic collisions.

Over the years, many people tried their hands and minds at disentangling the actual details of the interaction of fast ions with thin foils. Prolonged skirmishes were fought over the role and importance of individual processes, a combination of which would lead to various observable phenomena. An early attempt at systematization was presented by Berry, Bromander and Buchta at the Lysekil meeting, classifying parameters, types of spectroscopic experiments and studies of the ion beam itself. Several of Gordon Berry's later graduate students would work within this experimental program, like Gerald Gabrielse (who moved on to trap particles with Dehmelt at Seattle and then antiparticles at CERN from a Harvard base) or Tim Gay. Line broadening because of the straggling (slight energy loss and change of direction caused by the collision of the fast ions with the atoms in the solid foil) was studied by John Stoner, jr., and Leon Radziemski, jr., from Tucson. The discussion shows questions by Jens Lindhard, one of the experts on theoretical descriptions of the straggling process (Bohr-Lindhard model, Lindhard-Scharff-Schiøtt). For John Stoner, this work on line widths lead to fame when he was involved in finding a method to reduce the (Doppler) line broadening in fast-beam spectroscopy, together with Jack Leavitt and Robson.

Another (silently!) trailblasting paper was contributed by L. Kay and B. Lightfoot. They explained how to combine light gathering power and relatively narrow spectral lines by suitable optics, two aims that had seemed to contradict each other but did not necessarily do so. Although much more from the sidelines than from inside the field (as Stanley Bashkin), Kay over the years thought up several very clever - though technically (basically) simple - ways to maximize signal and information at the same time. In this case it was a combination of two (crossed) cylindric lenses which by basic optics principles showed a much more desirable performance than the usual spherically symmetric lenses.

Part of the interaction of ions with the foil is the excitation of the projectile ions in the process. Tucson's theoretician J.D. Garcia descended from the eerie heights of calculating very precisely the structure of one-electron ions to the dirty problems of ion beams and foils. Since many excited ions will loose the outer electron quickly, the rear surface of the foil is important for what can be observed afterwards. Comments Hadeishi: "The last layer of the foil is dirt. It does not matter what foil you use." Quite right, in most situations. Lateron people try to heat the foils to get rid of contaminants, use sputtering, ultrahigh vacuum, whatever else. Foil surface effects remain tricky, to say the least, and many results contradict each other. Maybe one has to add to Hadeishi's comment that the properties of dirt seem to vary from place to place.

Ion-foil interactions without a surface - well, that would be impossible, but people tried whether they could understand the processes of ion-atom collisions, with the fast ion beam passing through a differentially pumped gas cell. One does see the excitation of both collision partners, and one hopes that the various effects are easier to understand than the mess happening in and around foils. After two talks by Erling Veje, representing a group at the H.C. Ørsted Institute at Copenhagen, a discussion is quoted in the proceedings which seems bland in comparison to personal memories by Indrek Martinson of the same event.

In print:
Gabriel: How do you rule out cascades?
Veje: We measure only on levels for which cascading has never been observed in other works. In the case of helium all lines observed are transitions to levels with n=1,2, or 3.

Contrast this with the personal recollections:

> In 1970 (Lysekil) Veje gave a talk on excitation measurements for He I (beam-gas). After the talk someone asked "Did you correct for cascades", A: "There is no cascading", Q: "????", A: "Because there are no higher He I levels in Moore's tables".

In print:
Weiss: The potential curves you showed may become meaningless since you don't have adiabatic conditions.
Veje: Yes, but that is the only theory available.

Personal recollection in contrast:

> In another talk he was explaining his data with the Landau-Zener model [no, molecular orbital theory. E.T.]. After the talk Weiss pointed out that the model was not applicable for the energy range used in the experiment, Answer: "Yes I know, but this is the only theory that there is".

Such a precise personal memory after more than 20 years demonstrates the value of oral history and the deep impressions those early meetings left with the principal actors in the field.
The second of the above items obviously is a beautiful physics equivalent for that old story about the drunkard who late in the evening digs in the snow under a street lamp. A policeman inquires about what he is doing there. "Searching my key!" - "Are you certain you lost it here?" - " I lost it over there, but here is light!"
Purportedly this is a metaphor for Theoretical Physics in general, so I might as well recite a similarly apt metaphor for Experimental Physics (which I learned from D. Kamke in his introductory physics course):
A researcher puts a flea on his (clean) laboratory desk and orders: "Jump!" The flea jumps.
The researcher repeats the procedure a number of times in order to collect sufficient statistical evidence and then notes in his lab book, below a careful description of his experimental set-up (including a sketch of the flea): "Flea jumps at command".
Then the researcher tears off all the flea's legs (tssst, tssst, it is an old story, not an acceptable animal treatment nowadays) and starts a new series of observations. He/she tells the flea: "Jump!", but the flea doesn't do it. The researcher repeats this command for a suitable number of times to guarantee statistical reliability of the results, and then writes down the straightforward conclusion: "A flea without legs is deaf."

A section of the Lysekil conference is dedicated to *fine structure effects*. Nowadays the appropriate catch phrase would be "coherence", with the leading edge groups working on non-coherence aspects of coherence, while those in the audience who are not actively involved or interested possibly complain about the incoherent presentation. Well, it was fine structure effects then, and it mostly dealt with quantum interference effects resulting in quantum beats. Lost the track? I hope not. Let me try differently: If two pendulums of similar properties swing without knowing from each other (without coupling), they simply do, fully indepently of each other. However, if one permits or introduces a slight flow of information, a mechanical disturbance, a signal, vibrations from one pendulum to reach the other, the movement of that one will tell. The effect is best studied with a weak coupling that permits some slight energy flow in either direction. One then finds that the previous repetition frequency of the individual pendulums has changed a little, depending on the kind and strength of the coupling, and that the amplitude of the swinging motion (the maximum "width" of the movement) now varies in time. If only one pendulum is pushed from rest, it will successively transfer its energy to the other and might itself come to rest again, and vice versa. The frequency of this energy exchange is lower than the frequency of the individual pendulums (or oscillators) and is called the beat frequency. Transpose the example into the quantum world, we find an effect called quantum beats. How does it come about? Imagine (simplistically) an excited atom as a system which would oscillate if struck, and the oscillation frequency would be different for different excitation states. If two of these are close together in energy (see fine structure), the atom might as easily oscillate with one or the other frequency and does, in fact, show both and the beat frequency from a coupling between the atomic states. The original frequency may be very high (in atomic transitions, that means perhaps visible light), as the energy steps in the atom are as big as eV (electron volts). In contrast, the beat frequency may be relatively low, if the original individual frequencies are very close together. How does it show? Like a switching on and off of the light intensity as the ions travel along their path after leaving the foil. Well, it is a lot trickier than that. Bashkin's early photographs show such drastic bright-dark variations with hydrogen atoms, but in most cases one will need to introduce polarizers in the light path and still have problems observing the wanted effect, because coherence is not perfectly achieved. Coherence here means that all ions receive a similar excitation or kick at the same position (rear side of the foil), so that as a function of distance from the foil all show the same phase (status of oscillation). Also, not all fine structure levels (or rather the magnetic sublevels) must pe populated according to their statistical weight factor, or the phenomenon is being washed out, may be even completely. Some level splittings (and the couplings of the oscillators!) can be varied by external fields (electric, magnetic, combinations, various orientations), and this exactly is one of the fields of interest: to learn about the reactions of atoms to such fields. Knowing these, one proceeds to study the excitation process, by trying to find out from the quantum beat pattern under zero field or external field influences what the conditions where at the time of excitation.

Although the experimental data samples in Ivan Sellin's and Horst Jürgen Andrä's contributions to the Lysekil proceedings seem clean and clear, the actual experiments sometimes were rather frustrating. For example, Wittmann, one of Andrä's students, purportedly stated publicly in 1974 "Initially you have quantum beats, then they disappear for a very long time, but if you are lucky, they sometimes come back " (referring to the strong, initial ones ... and they fade out along the ion beam!).

Sandwiched between these two presentations we find T. Hadeishi's paper on fast ion beams which are passing through periodic potentials. This work in a way reverses the above process: The environment provides a periodic perturbation (for example, a magnet with comb-like pole shoes), and if the internal structure of the fast ion (which experiences the perturbation as an oscillation in time) fits to the frequency and gets into resonance, some notable effects may occur. At the time, this was used for atomic structure studies, but such periodic structures have nowadays been developed to make up wigglers and undulators at (electron or positron) synchrotron light sources.

Concluding the conference are astrophysically minded contributions. One is showing examples on the determination of elemental abundances in stellar objects from observed light intensities, the other combines beam-foil lifetime measurements with branching ratio determinations in other light sources to derive (absolute) individual oscillator strengths for spectral lines in neutral iron atoms. After the first talk, the reprinted discussion contains useful comments along with (substantiated) wishful thinking:

Edlén: The coronal abundances and identifications depend on two different kinds of spectra. One consists of the forbidden visible lines and the other of the resonance lines around 300 Å. Both gave an iron abundance about 10 times higher than in the photosphere until the photospheric value was revised last year.
Engvold: The results from the corona have come under some criticism.
Edlén: Now everything is in good shape, and I hope nobody will introduce these troubles again.

So much about the Lysekil meeting in June of 1970.

Social life of the early days was never surpassed lateron, as is recalled below. But first some hands-on experience of experimental exercises at Tucson. It was reported to Stockholm, where Indrek Martinson had set up a new experimental apparatus (using many old parts, like the magnet and power supplies from a scrapped isotope separator. Apparently Bill Bickel returned to Tucson after the Lysekil meeting, whereas other people stayed in Sweden for the summer.

Bill Bickel's sufferings

September 26, 1970

The situation here is as expected. I would rate it as rather fair. Much has been broken in the lab but what the lab lacked most during our absence was tender loving care, i.e. there are switches broken off from power supplies and couners, lose wires and "temporary" setups. Much of the electronics is out of adjustment and settling on top of things and a few PM tubes have 106 counts/sec noise etc. It will take a while to get things in shape but I and Hank [Oona] are picking away at it.
One typical experiment Bashkin asked us to do was to measure a lifetime in Ne I, λ=6400 Å. I said we could do it in a day since I was told all the equipment was ready. First the beam-foil automatic chamber leaked, $p > 10^{-4}$ mmHg, and no one knew where. Also it had been leaking since June.

We got a leak detector, three fans were burnt out and it wouldn't pump down. Then we found lucite plates had been exchanged for our metal plates and feedthroughs while the Faraday cup feedthroughs were cracked. It turned out the O-rings were set too deep in the lucite windows so we faced them soft and then found they were too large and wouldn't seal, so we made them smaller. Bashkin wanted to clean the pump, but when he left we got $p < 3\cdot10^{-6}$ mm Hg! Then we got a Ne beam, after we fixed the stabilizer, and found arcing in the beam-foil chamber. It turns out the graduate student under Bashkin had removed both the beam-defining aperture and the grounding wire for the foil wheel, so we had a fat beam and arcing due to foil charging (and he was doing E=0 experiments on H for Bashkin)! So we fixed that (3 days now passed) and got a signal. It turns out that τ for $\lambda\lambda$6400 measured by Liu by the Hanle method is a factor of 2 different from Klose's value. So we did a wavelength scan and found that λ 6400 wasn't isolated, i.e. not resolved in Liu's experiment. Bashkin was surprised at this but neither he nor Liu took a wavelength scan. We narrowed the slits to .3 mm and saw A instead of B what Bashkin saw. [A: 3 almost resolved lines with a strong central component, B : a wide hump]
He said B was a single line since it was pure Gaussian etc. SO! with narrow slits we took a wavelength scan and tried to measure. While measuring the beam-foil track jammed since the graduate student has "adjusted" it, so we resorted to single channel experiments, the old way. All switches were broken off the scaler and you have to stick a pencil in the holes to turn on the START, STOP and RESET. The holes were small and you couldn't find them in the dark. (Light had to be out since no PM tube holders were used, just PM tubes covered with black tape and black cloth). By end of the fourth day I was totally annoyed, depressed and in a rotten mood along with Hank.

Finally we got counts. As we scanned the signal got weaker: after two hours I found that the tube cooling device, set up by Bashkin and Liu had unhooked - glyptol and monkey putty - and the tube wasn't only cooled but its surface was fogged with frost. It took two hours to warm tube, dry tube and get dark current to low count. Then the dark counts went from 500/sec to 10000/sec. That was the end of the tube. Liu told us that "someone" a few months ago had turned the high voltage on the tube from 1500 V (max allowed) to 3 500 V (by "MISTAKE") and the tube had gotten hot. He turned the high voltage off and the tube was erratic after that but he used it. Well, when we were using it, it gave up. It was a 9558, a damn good one, and we have no others good any longer! END OF EXPERIMENT.

What a fantastic fiasco. Now I am finished, I am repairing and making tube holders etc, and will refuse to put up with such crap again. That's why NOTHING got done this summer.

[Twenty years later Stanley Bashkin's own running commentary on his daily work confronting his own accelerator and data collection system doesn't sound very different from Bill Bickel's 1970 report to Indrek Martinson. And Elmar Träbert on visits in the late 1980es found mostly the old equipment still in place, and sometimes even used, for lack of any replacements since.]

International research connections

Extended visits to Stockholm:

The Stockholm Research Institute for Physics on Frescativägen (on the other side of the major road leading there one finds the auditorium where the Nobel lectures are given) lies in a wide, largely flat park with scattered other buildings, sections of woods, grassland and ditches. At the cyclotron laboratory established in the (Manne and Kai) Siegbahn days, where the Siegbahns had a magnificent villa as their home-cum-institute, Ingmar Bergström had Indrek Martinson and a few others set up beam-foil facilities. This was not done on the high-energy ion beam, but employing an old 30 keV isotope separator. That is basically a magnet with an attached ion source and an acceleration stage. Old, of course, is relative: More than 20 years later, the parts were still around in the laboratory ... At energies of only 30 keV, ions have a deleterious effect on target foils. The foils last for very short times only, and the angular straggling widens the beam very much, making it very difficult, indeed, to obtain meaningful decay curve data. Still, people managed, and the institute with its nice surroundings, the attractions of Stockholm and the skerries, and the flair of the Nobel institution (perceived only when far away from it?) lured visitors from far. Last to be mentioned, but certainly not least, there must have been a hospitable atmosphere and the local group's activity.

Bill Bickel (?)

Bill Bickel's importance to early Tucson fast beam work may be well have been underrated by many. An example from the 1990es: Carol Tanner (Notre Dame - where Gene Livingston already worked and Gordon Berry joined in later) and Elmar Träbert (Bochum) were both asked by Steve Shafroth (UNC) to contribute chapters to a book on accelerator based atomic physics []. Elmar wrote on lifetime measurements in highly charged ions and gave reference for the initial insights to Kay (Manchester) and Baskin (Tucson) and the ensuing trail of conferences, whereas Carol in her chapter on laser measurements on (lowly charged) fast beams chose to refer to developments of the fast-beam technique by Bickel (Tucson) only. As she had an expert nearby who had worked at Tucson in the old days, this may be very interesting ...

Among the visitors drawn to the emerging bfs group at the Manne Siegbahn Institute at Stockholm was young Ivan Sellin. When Sellin got near the open grounds after a long travel, nightfall or fog barred him from finding the right place, and nobody was around to help with directions. After quite some erring around he finally took to sleep in the partial shelter outside of what seemed a locked shed (Fortunately it was the warmer season). Awakening with morning light, Ivan hardly believed his eyes, finding himself beneath a full-size ski jump. Not a thing to expect in the flatlands near the coast! Well, it turned out that Sellin was in walking distance of the institute after all, and he soon got there, apparently none the worse for a night in the open.

When eventually Indrek Martinson became Professor of Physics at Lund University, a new hospitality center for the beam-foil community was set up there.

Extended visits to Lyon:
Gordon Berry, Hans Heinrich Bukow, Eric Pinnington, Helmut Winter, David Pegg, Josh Silver
(Not everybody was thoroughly happy with their experience, but some stories heard have not been authorized for a replay yet.)

M. Dufay at Lyon early on started a beam-foil laboratory and initiated a very successful group (Jean Désesquelles, J. Subtil, Michel Gaillard, Alain Denis, M.-C. Poulizac, J.P. Buchet, Michel Druetta). At Lyon, they worked mostly in the few-hundred keV energy range, but they also went to Orsay (1 MeV/nucleon) and later to Caen (France's Grand Accelerateur National des Ions Lourds - GANIL), a synchrotron at Grenoble (SARA) and occasionally to places abroad. They soon became busy with polarization effects measuring alignment, quantum beats and ion-beam - laser interactions. With such diverse activities, they attracted visitors and collaborators from Sweden, Germany (Horst Jürgen Andrä from Berlin, Hans Heinrich Bukow and Helmut Winter from Bochum) and Englishmen from the USA (Gordon Berry, David Pegg) and Canada (Eric Pinnington).

An example of the workings of such scientific visits: For Bukow (with a French wife), connections to Lyon were quite easy to establish. They culminated in an electrostatic ion-energy analyzer of Bukow's design to be manufactured at Bochum in two copies, one for Bukow's Bochum experiments and one for Lyon. Helmut Winter worked in Bukow's group towards his first thesis. He contemplated becoming either a school teacher or staying in university science. Bukow recommended a visit to Lyon, and that worked out so well that Winter went there again afterwards, collecting polarization data with his set of polarizers for Lyman alpha light (102.4 nm, vacuum ultraviolet) and joining Michel Gaillard when he developed velocity tuning as a technique for collinear laser spectroscopy. Winter lateron moved on to Berlin (joining Andrä's group, also following him to Münster) where he pursued the interaction of ion beams with surfaces - research which ultimately led to a professorial position at Humboldt university in Berlin.

With such good connections of Bukow's to Lyon, people wondered why Winter was the only one of his group to go there. Malevolent gossipers said that he only suggested such external work to people he had clashed with. However, he also suggested to Elmar Träbert (a student not in Bukow's group, but in the same physics chair) that he ought to contact Josh Silver at Oxford; he had formed the impression that Silver had plenty of research money. This impression was not quite correct, as Träbert found out when he secured funds for a research visit and later even obtained a postdoc position at Oxford. Nevertheless, Träbert still muses what earned him Bukow's suggestion to go abroad.

One beam-foil expert from the U.S., David Pegg from Knoxville, explicitly spent a sabbatical in Sweden and France (Lyon) in order to change fields away from beam-foil. He judged that (as early as in 1978) beam foil was a field "on the way out", and wanted to learn laser spectroscopy in time. How nice that this very combination was available at Lyon! And getting across Europe in a fairly new Volvo or Saab exported from Sweden (tax refunds!) was certainly an added boon for such a scientifically motivated (and supported) sabbatical. Well, they also had adventures on that trip to Lyon, as David can tell - intriguing memories for decades to come. And David made the best of the laser experience: He afterwards set up a laser spectroscopy group at Oak Ridge and took up electron detachment studies (on fast beams of negative ions), establishing himself among the world experts in this field.

Fast ion beam laboratories at / Extended visits to ?
Aarhus / Argonne / Australia: Melbourne / Berkeley / Berlin / Bochum / Bombay / Brookhaven / Bruxelles /
Caen (Ganil) / Chicago (Illinois) / Copenhagen / Daresbury / Darmstadt (GSI) / Edmonton (Alberta) / Giessen / Heidelberg / JAERI / Kyoto / Köln / Laval (Québec) / Liege / Lund / Lyon / Manchester / Manhattan (Kansas) /
München / Notre Dame (Indiana) / Oak Ridge (Tennessee) / Oxford / Pasadena (California) RIKEN / Stellenbosch / Strasbourg / Toledo (Ohio) / Uppsala / Windsor (Ontario) / Washington, D.C.

Foil excitation is very effective in producing ions in charge states beyond the reach of vacuum sparks and the like. This led to estimates of the work that needed to be done. Kay and Lightfoot (Nucl. Instrum. Meth. 92 (1972) 523) said: "If we take the number of single-electron ions to be of order 100, then the number of positive ions plus neutral atoms [that is the number of the atomic spectra] is 100!" [factorial]. Although this mathematical lapsus gives a "slight" overestimate of the order of magnitude of the task, almost two decades later still at most half of all spectra are represented in the charts of knowledge by at least a single experimental entry. And before the recent surge in high-power laser-produced plasma work, the ion-foil interaction provided a lot of the spectral data - and still is the only generally applicable one for lifetimes.

Second European Conference on Beam-Foil Spectroscopy (Lyon, July 20 - 21, 1971)
Proceedings edited by M. Dufay
According to the Editorial of the Proceedings, the conference "was limited to the European laboratories but benefited from the presence of some American physicists". 50 people are listed as participants, and of the 5 Americans/Canadians Berry (Lyon) and Curtis (Stockholm) gave European addresses, and Kernahan from Edmonton reported on work done at Lyon.

It is a small conference, but even the more or less abstract-like contributions in the Proceedings show some highlights of current progress:
Bill Bickel reports in a survey of all Tucson work that "The Doppler broadening of spectral lines caused by the high beam-particle velocity has virtually been eliminated. Stoner and Leavitt found that the line broadening could be reduced to residual instrumental broadening by refocussing the spectrometer." His subsequent brief technical description of the spectroscopic recipe is as concise and to the point as one can wish. Doppler broadening is interpreted as a defocussing effect in the spectrometer, and the great and successful idea is to realize that there is a rather good new focus at a different focal length of a given (normal incidence) spectrometer. Of course there are tricky details in practice, as researchers applying the technique will find out, but this is a large step forward for BFS. A number of other schemes have been thought up as well, but none is as simple and effective (for many problems) as this Stoner-Leavitt refocussing technique.

Uppsala and Edmonton introduce their new facilities. S. Feneuille from Orsay describes the calculation of transition probabilities, and where other people typed their manuscripts, he used handwriting - cleaner and better to read than many of the typed pages. Heaps of BFS data are presented, from Aarhus, Lyon, Stockholm. Jack Kernahan has an asterisk at his name on the abstract, and when searching for the meaning of that, two pages later the reader finds the footnote "* CASCADING PRESENT". Now we know that Jack Kernahan may be affected by cascades. As an alternative explanation, the next page after that lists his present (Edmonton) address, also with an asterisk. On the following abstract, by M.C. Poulizac and J.P. Buchet (later to marry each other), Poulizac bears an asterisk, as do a few of the wavelength data in their tables, but no explanation is revealed. Indrek Martinson includes Wiese-type isoelectronic diagrams among the many figures in his talk on results from Stockholm and Tucson. This is one of the first of very many survey talks he will give during his carreer, and his style already shows. L.J. Curtis, still at Stockholm, expands on cascade analyses. Imhof and Read continue on their electron-photon coincidence experiments, C. Camhy-Val and A.M. Dumont (Paris) explain their lifetime measurements using correlated photons in a cascade, for some of which they achieve error bars as low as 2%. L. Kay *et al*. have thought about line shapes, T. Andersen *et al*. at Aarhus and J. Remillieux and J.C. Poizat at Lyon have studied channeling effects (ion beams interacting with crystals along preferred directions). In almost each laboratory some people have studied alignment effects, that is an observable light polarization due to a non-statistical population of magnetic sublevels in ion-foil or ion-atom collisions.

Third International Conference on Beam-Foil Spectroscopy (Tucson, October 2-6, 1972)
Proceedings edited by Stanley Bashkin, published in Nucl. Instrum. Meth. **110** (1973)

> Date: Wed, 8 Nov 89
> From: "Indrek Martinson, Atomic Spectroscopy, Lund"

To: EAMP9@CFA4.BITNET (Elmar Träbert)

Dear Elmar,
In 1970 (Lysekil) Stanley noted that the lines in BFS were wide and he suggested that something should be done to solve this problem. A year later Stoner and Leavitt found refocussing. At the 1972 bfs meeting in Tucson, Bahcall gave a review of the solar neutrino problem (whatever that has to do with bfs except that neutrinos go through foils without causing damage or getting excited). Stanley's remark on p. 384, NIM **110** (1973) is another goodie "At the 1970 conference I made a plea for narrow spectral lines and that plea was answered. Perhaps by the time of the next conference my present pleas for some concerted attack on these problems of astronomy will also meet with an answer."

At the same time Nixon was trying to wipe out cancer as well as the Democrats. Bickel (p.254) gave a very clear picture of what a foil is like when you enlarge it, etc. And this is where I first met Haro [von Buttlar] who informed me about the *German accuracy*, etc.
Regards, Indrek

Yes, this 1972 Beam-Foil Spectroscopy conference must have been the highlight of the series. Nuclear Instruments and Methods Vol. 110 (conference proceedings Tucson 1972) is full of comments which relate to the original talks. However, the written text published has been edited by the authors to accommodate the comments experienced at the conference. Now some of the comments just sound silly, as if the commentator had slept through the talk.

There must be lots of nice stories floating around. Perhaps we should be a little careful and have some self-restraint (Finlandization is the word in politics), i.e. keep some of the nastier stories stored in the way the Pope and other VIPs in the Vatican store Lady Chatterley's lover and Fanny Hill's memoirs. What I mean is that the stories about Veje may be a bit nasty, for example, unless there are stories about everyone.

The 1972 conference proceedings are funny to read today. I remember that my own talk did not go too well. I had spent all the time on the manuscript and no time on preparing the presentation. So I would not find the words and was proceeding slowly and suddenly my time was out (I was only half-way through). It was followed by a talk by Jan Bromander which went much better. During the afternoon I was approached by Rolf Sinclair from NSF who told me that he had enjoyed my presentation very much and asked for copies of my transparencies. I was flattered, of course, and gave them to him. Next morning he returned the copies and said, "Im sorry, I was really thinking of the other talk" (by Bromander).

When you look at the paper by Buchet et al. NIM **110** (1973) 19, then you will find a very intelligent question by Martinson on p. 25. What actually happened is that in the oral presentation Jean Désesquelles only discussed spectra and said nothing about any lifetime measurements, whereas the printed text also includes lifetime data. So my question really refers to the oral presentation whereas it looks silly in the book.

After the talk of Dave Church, the great Kastler pointed out the "the Americans pronounce "dzhi-dzhei" whereas we in France tend to prefer "dzhe-dzhi"". This is unfortunately not in the proceedings. There is an earlier version, this time from chess. When an old German player Mieses had to leave Germany in 1933 (guess why), he ended up in England. A chessplayer there asked him if he was Mister Maises but he said, "No, I am Meister Mieses".

Regards, Indrek

However, the master of the Tucson place in his serendipity recollects other sides of the same business.

Date: Thu, 15 Feb 90
From: <BASHKIN@ARIZRVAX>
Subject: BFS amusements.

The Third BFS Conference was held at a resort - Westward Look - in Tucson. The date of the meeting was picked because the Conference Chairman was sure that the weather would be perfect. Needless to say, the day the excursion to Old Tucson was to take place opened with rain. Question - Did we dare the elements and go, or call it off? The responsibility for the answer fell on the bowed shoulders of the Chairman, who, in a fit of sanity, decided to take the risk, and go.

Thus, go we did, in buses loaded with conferees and their accompanying families, friends, what have you. Fortunately, the afternoon turned out to be fine, with a clear sky and an equable temperature. It was perfect for the day's performance of the gun fight between the forces of evil and the law. In accordance with the normal procedure, all the visitors lined up along one side of a dirt street, while the action occurred in the street and among the seedy buildings along the far side.

The action had to do with a bank hoist, perpetrated by three desperadoes, appropriately garbed with black neckerchiefs over their faces. They shot up everyone inside the bank, and made off with their swag. Alas for them, their escape was interrupted by the unexpected appearance of the Marshal, who found himself locked in deadly combat with the malefactors.
Well, the air was filled with the noise of gunfire, the cries of anguish from the criminals, who finally lay unmoving, one after the other, on the sun-baked ground. The marshal walked warily along the street, making sure that the robbers were indeed dead, and not merely trying to trick him into an ambush.

Ah. An ambush. Yes, indeed. For there, on the roof of one of the buildings, was a last, desperate outlaw, who carefully drew a bead on the unsuspecting representative of all that is good and decent in this western country. The Marshal's back was turned to the evil-doer, and he was sure to fall victim to a shot in the back.
As I said, the audience was lined up along one side of the street, whence the performance could be seen and enjoyed to the full, without any danger from the blanks that were fired. One of the onlookers was a small boy, perhaps six years old, who was absorbed in the scene that unfolded before his innocent eyes. He saw the careless Marshal. He saw the cruel enemy perched on top of the roof. He saw the thief take aim. Then he could restrain himself no longer. In the nick of time, he shouted at the top of his treble-noted voice, "Marshal, Marshal, look out!"

Who knows what the result might have been had it not been for that timely warning? Anyway, the crowd erupted with howls of laughter at the very moment that the Marshal wheeled about through 180 degrees, and fired at the villain on the roof. Needless to say, the latter, undoubtedly unnerved by the warning given to his foe, missed his target, and tumbled down to the clay, with which, no doubt, his bones were soon mixed. In this manner, the sanctity of the rule of law was upheld, another outlaw lost his bid for an easy fortune, and a wonderful time was had by all.

S.B.

Other rumours tell that the trip to Sabino Canyon was a wet one, too, although that is usually one of the very few spots around Tucson where one looks forward to see some natural water flows. Pretty and most enjoyable after any time in the dry lowlands of Arizona!

Living conditions must have been harsh, such a short time after the White (Non-Hispanic) Conquest of the American Southwest:

Hank Oona as a graduate student at Tucson was living in Bill Bickel's house. One day he felt sick and stayed home. He woke up from some noises when somebody tried to force his way in. Half asleep Hank shouted "What's going on?" and then only saw a black guy running away. He called the police to ask what he could do should such burglary attempt happen again. Police: "If he gets in, shoot him, then call the police. If you shoot him on the lawn, pull him inside, then call us." Yes, the good old American sense of Do-it-yourself procedures (crafts, physics, law, ...)!

At the 1972 beam-foil spectroscopy conference in Tucson, Haro von Buttlar (then a chairholder at Bochum, formerly a professor at Socorro, New Mexico) presented the very accurate lifetime results obtained by his Bochum student, R. Tielert, on the Lyman alpha transition of hydrogen ([illegible]=(1.592±0.025) ns). Tielert measured the yrast cascade separately and did a correlated fit, which is a method close to the lateron elaborated ANDC method for cascade corrections. Horst Jürgen Andrä seemed to have heard that not the expert student had gotten the money for the conference trip, but the supervisor. He thus asked v. Buttlar how he had obtained such a marvellous result where everybody else in the world was struggling to get any decent lifetime value at all. The purported answer was "That is German accuracy / Das ist deutsche Genauigkeit". This answer was quickly spread around, and the German element in precision physics duly noted.

A couple of years earlier there had been an experimental result from Tucson (Bickel) on the same transition that carried an even smaller error bar (W.S. Bickel, A.S. Goodman, Phys. Rev. 148 (1966) 1; [illegible][illegible]= 1.600±0.004 ns) and was in superb agreement with quantum mechanics as told by Condon and Shortley (1.60 ns). Two years later, David Pegg in his Masters thesis measured the same with a more decent error bar (1.60 ± 0.01 ns). Unfortunately Richard Crossley later found an error in the quantum mechanical treatment, and after the correction, Bickel's result agreed with the wrong computation. Tielert's result still holds (Theory: 1.5962 ns). He wiseley changed fields after his diploma work, went into electronics for his graduate time and now has a well-paid job in industry. However, old claims to precision may live to old age: In the early 1990es, Elmar Träbert dared writing to a well-reputed theoretician who - judging from a recent publication - apparently only knew of the Bickel and Goodman data of more than two decades ago.

After so much gossip, is there anything left to note on the science side of that conference? Maybe there is. Let us peek into the Proceedings.

The keynote introductory survey talk is up to Indrek Martinson this time, summarizing work from all over the world which had been done since the previous (1970) conference of the series. To do this in fairness is quite a task, and Martinson is up to that. A particular line of recent research is into highly charged one- and two-electron ions, with precise measurements by Marrus and Schmieder at the Berkeley HILAC leading the pack. Even as some of these measurements earn controversial responses and later turn out to have been underrated in their complexity, others remain standing out as important waymarks in the quest for the precise measurement of fundamental properties of seemingly simple atomic systems. In terms of atomic lifetimes and transition probabilities, there are many data, but Martinson quotes Feneuille from the Lyon Proceedings: "For theorists a single very precise measurement is often more useful than a hundred uncertain experimental values." [This is certainly so, but there are cases in which no experiment can produce a single very precise data point, but only a series of data of individually limited precision. If these data are in a sequence of predictable trend, like in an isoelectronic sequence, one often can apply isoelectronic smoothing or fitting proceedures and still obtain a marker theory may try to reproduce. Years later, when the data bases has grown, Martinson himself will partake in such procedures.] Martinson then reminds of the pessimism expressed about BFS lifetimes at Lysekil and the cascade analysis systematics hence developed by Curtis, which ought to permit more optimism. Astrophysical elemental abundances have been cured of some (but not all) problems by the aid of fast ion beams, and fine structure experiments have not only continued on quantum beat phenomena, but also obtained spectroscopic data on fine structure intervals in hydrogen-like (one-electron) ions, thus indicating the Lamb shift, a quantum electrodynamical (qed) effect. Martinson ends with another quotation that "A few years ago a sophist might have remarked, with a touch of irony, that beam-foil spectroscopy exists partly because there is not much else you can do with old, obsolete Van de Graaffs" [accelerators of the most ubiquituous type, as given to many American nuclear physicists after World War II)]. Martinson continues that "... we now all now that BFS is one important motivation for constructing new, powerful heavy ion accelerators". Well, that might have been the view of atomic spectroscopists, but it did not amount to any effect with the people in charge of accelerator laboratories (usually nuclear physicists, thinking about upgrades or replacements of their dated machines) or with funding agencies - nowhere in the world (except perhaps at KSU) a sufficiently heavyweight atomic physics community existed which would have been able to wring money for a dedicated atomic physics machine from any funding agency. Atomic physicists were accepted as guests on existant machines when they were not anymore of primary interest to nuclear physicists - which only underlines the above purported irony. Ten years later, J.D. Silver's atomic physics research grant lasted longer than the nuclear physics funding of one of the Oxford ion accelerators, to the dismay and lasting grudge of the local nuclear guys.

Fourth International Conference on Beam-Foil Spectroscopy (Gatlinburg1975)
Proceedings edited by Ivan A. Sellin and David J. Pegg, published by Plenum Press (New York) in 1976

Fifth International Conference on Beam-Foil Spectroscopy (Lyon 1978)
Proceedings edited by Jean Désesquelles, published in J. de Physique Colloque (Paris) C-1 (1979)

Sixth International Conference on Beam-Foil Spectroscopy (Laval 1981)
Proceedings edited by Emile J. Knystautas and R. Drouin, published in Nucl. Instrum. Meth. **202**, (1982)

International Conference on the Physics of Highly Charged Ions, incorporating the
Seventh International Conference on Beam-Foil Spectroscopy (Oxford 1984)
Proceedings edited by Joshua D. Silver and Nick J. Peacock, published in Nucl. Instrum. Meth. B **9**, (1985)

There were many graduate students and regular scientists from all over the world who went to Lund to work in spectroscopy, not only beam-foil. As they come from different cultural backgrounds, some made deep impressions even outside the laboratory. There is the story of a golden ring with a university emblem which, by tradition, Lund doctoral students are entitled to wear after they passed their exam. One such young doctor from Asia eagerly bought such a ring in a shop to wear it immediately after the ceremonies. Afterwards it was reported that at home he boasted that "The ring of the university" had been given to him. Well, that is about correct - it was given to him - by the shopkeeper on purchase, but not bestowed by the university as the bearer may have liked to hear implied. Since then the "Ring of the University" is particularly cherished by the Lund physicists - who would decidedly not wear it.

In 1988, Bashkin obtains faculty funds to contribute to a research visit of Elmar Träbert to Tucson. Stanley still has hopes that the faculty eventually might hire a young man to continue atomic physics with accelerators, the field he has opened and thus put Tucson on many physicists' maps. At the occasion of this visit, Träbert joins in with Bashkin's newest fad, the study of atmospheric gases under ion bombardment. Such things have been done 20 years earlier, by a number of groups, but Bashkin's spectroscopic equipment - though second hand and aged - permits the recording of *much* better data than reported in the old literature. Among the mountains of data collected, there are isolated, clean transition multiplets which Bashkin wants to tell the world about. Well, the multiplets are rather well known, but Bashkin perceives this as a chance to demonstrate that his experimental set-up can do measurements so cleanly that relative intensities can be measured (which translate into branching ratios which are often wanted). On one hand this connects to his earlier work (see below), on the other Träbert does not accept the value of the data: In the same data, Bashkin sees a massive contradiction to published tables, whereas Träbert sees no deviation whatsoever from basic principles put down some 60 years ago. Stanley's oversight is that in the classical spectroscopic literature, relative intensities are "visual intensities", that is modified to a logarithmic scale because of the logarithmic physiological response function of human senses. Such estimated intensity data from various sources, mixed without adjustment, are listed in the spectral tables by Striganov and Sventitskij. On the other hand, Wiese's tables give individual transition probabilities and statistical weight factors (resulting in exactly the predicted intensity pattern which the measured data confirm), but Bashkin - with his physics training in nuclear physics - has problems sorting this information into relative line intensities. After many futile attempts at sorting out this problem via e-mail messages to Tucson, Träbert confers with Martinson, and Indrek's living memory comes up with fascinating links to yesteryear:

Elmar Träbert to Stanley Bashkin, November 7, 1989

> Stanley,
>
> I sent a copy of the Chinese paper to Lund, but pointed out that I disagreed with the interpretation of the SS (Striganov/Sventitskii) intensities in that paper. [Bashkin had presented a paper on the above work at a conference on Taiwan, with Proceedings published in the (Taiwanese) Chin. J. Phys.. In that paper he stressed the relative intensities.] Indrek notifies me that the SS intensity miracles have seen earlier likewise clashes. He tells me about "a famous paper, submitted on 13 November 1968, by a number of distinguished scientists (Bickel, Berry, Désesquelles and Bashkin, JQSRT **9** (1969) 1145)," [would you know any of those guys?] "in which the authors provide experimental evidence of the fact that in most cases known to man the logarithm of a number may differ from the number itself. This also holds for ratios. If you read page 1148 you will find a discussion of bfs *vs*. theta pinch intensities. Hallin, in Arkiv Fysik **31** (1966) 511 " [that is the theta pinch data source] "gives logarithmic intensities with a base of about 2, so 20/19 and 2.00 are about the same thing."
>
> It looks like a recommendation to me to study more of the old texts to see what life was like when philosophy and physics had not yet been so cruelly separated. I looked up that paper and was truly amazed that anybody shot the configuration interaction cannon at the 2p $^2P^o$ levels of N V to reproduce the statistical weight factors. I admit that The Book [Heckmann/Träbert: "Einführung in die Spektroskopie der Atomhülle", later to be translated into English by Stanley] was not yet available by then and that at the time [1968] I had no idea that bfs existed or that I might end (?) up doing it or its off-springs.
>
> It should be a lesson to be kept in mind when you want to publish relative line intensities [this time from ion-beam excited gases] : JQSRT has done it before and may do it again [after all, it is the Journal for Questionable Spectroscopy and Ridiculous Tell-tale; they published (a.o.) the first few abominable papers of the Stellenbosch group], but the reasoning for doing it should be more convincing for posterity than the reasoning given with that old paper.

> The above discussion the Bickel, Berry, Désesquelles and Bashkin paper thus deals with an effect that is not present. Logarithms prove to cause problems to physicists well advanced in their carreer, and twenty years of further experience may have no notable learning effect helping to remedy this shortcoming. The lasting benefit of the paper is that the equation 4/2 = 2 has been verified with an uncertainty of only one percent. The paper concludes with the observation that perhaps in beam-foil spectroscopy "*l* [orbital angular momentum] may be a more significant quantum number than *j* [total angular momentum]".

This strange emphasis on the quantum number *l* surfaced again at the 1978 Lyon bfs conference. Stanley gave a talk on problems in beam-foil spectroscopy. He had problems with the curvature of most decay curves when displayed on a logarithmic scale. Most, but not all of the curves show such curvature. [which is commonly attributed to level repopulation by cascades. If the dominant cascades are faster than the primary decay, the case of a so-called growing-in cascade, the level of interest still cannot be deplenished faster than appropriate to its own decay constant. Also - but perhaps easier to grasp intuitively - slow cascades repopulating a level make the decay curve of this level appear flatter (less steep) than it would be in their absence. Kay even phrased this general observation into a theorem, the *steepest slope theorem* (Physica Scripta 5, 139 (1972)): "The steepest part of a decay curve gives an upper limit on the lifetime of the level of interest." Well, it may not have been mentioned quite as bluntly as that in the bfs literature before, but it had been obvious and known nonetheless. Now people not only knew (if they did), but they could give a reference, which is so much more scientific.]

Stanley's claim at the time was that the non-single exponential decay curves "must result from unresolved blends with the decays of other *l* states", like of states close to the yrast line. At the time he did not even accept evidence in the form of decay curves of spectrally resolved lines (by wide margins in many cases) from such states which still showed the multi-exponential behaviour - as expected near the main cascade chain in any atomic system. And Stanley did not see that the decay curve of a long-lived low-lying level which is fed by fast cascades must be an almost straight line even with the cascades - without the cascades it would be weak and hard to see at all (see Sellin's comment at the Lysekil meeting quoted above). If one does not believe in mathematics, one can do simulations, and they all show what experiment does as well.

People recall that Stanley for a long time claimed that one of the few things bfs could not accomplish was measurements on neutrals [although some of his earliest papers have photographs of radiating beams of neutralized hydrogen]. Fortunately the ions did not learn of this, at least not before being neutralized in the ion-foil interaction, and thus a number of data on neutrals could be obtained in spite of the Master's verdict. Also, Stanley was so disappointed at early lifetime measurements that he seems to have spread the message that bfs could not give proper lifetime results, even when it lateron did, after overcoming some severe birthing problems. With the well known one of the inventors so critical, the Nobel committe may have discarded the idea of sharing out the big prize to Kay and Bashkin at a time when there would have been a chance. When the techniques finally had matured, the field had already begun to shrink (in numbers of activists), and that certainly was detrimental to prizeworthyness then.

> Bill [Bickel] also used to tell us about a lecture that Stan gave many years ago. He was discussing how one might solve the problem of minimizing the distance between the beam and the entrance slit of the monochromator. Finally he announced that this could be achieved by getting a positive lens +f and placing it at a distance of 2f from the beam. [This knowledge got lost again: In 1988 it was not too clear to Stanley that a 1:1 imaging would require a beam-spectrometer distance of 4f, with the lens in the middle. But after a night's consideration, he conceded to Elmar that this was right. After all, this is a basic (school) physics problem, first solved only a few hundred years ago.]

The fact that there was anything being done at Bochum in the field of beam-foil spectroscopy was surprising. The chairholder of the other chair (D. Kamke) was disinterested from the beginning ("If they want to do it, let them. That is a fad which will go away soon"), and apparently there was no enthusiasm on v. Buttlar's side either. The first really expensive piece of equipment, a McPherson grazing-incidence spectrometer, was acquired by Paul Henrich Heckmann with a grant for work intended to be done at other accelerators, namely GSI Darmstadt's UNILAC, which was then being built. That instrument proved to be most valuable and helped to gather excellent data, and in due course the publishable output of the small fast-beam spectroscopy group at v. Buttlar's chair outgrew the published results of the rest of the two nuclear physics chairs by a wide margin. This, of course, related only to numbers of publications, not to quality, as some of the dear nuclear colleagues repeatedly felt compelled to mention.

Be that as it was, the very good thing the two nuclear physics chairholders had done was the purchase and installation of the Bochum 4 MV Dynamitron tandem accelerator (against the explicit advice by the DFG (the German research council) referees), and the choice of its boss, Klaus Brand. The accelerator was designed as a high-current proton beam machine and developed into the world's most powerful and reliable heavy-ion machine in its energy range. The ion source performance obtained by the care of Brand and his superb team of operators (in particular the most talented Schöngraf, Wylich, Reimöller, Noll) matched for days that proclaimed by the inventor, R. Middleton, whereas elsewhere people were often enough angry that they did not get even close, or not for periods exceeding a few minutes. Much of this success was achieved by installing stabilized power supplies for all components of the ion sources and by rebuilding sources from the ideas in the blue prints, but with more care for details and manufacture than sometimes found in the original commercial version.

Various Bochum people excelled in special ways: Heckmann's first graduate student, Friedbert H., wanted to overcome the problem of statistical fluctuations in the signal by guaranteeing that each signal channel should have the same chances: He planned to scan spectra not by normalizing for an equal number of projectile ions (or charge collected in the Faraday cup), but for an equal number of signal counts collected, with the time per channel to be measured and stored in the multichannel analyzer. With the notoriously low light level of ion beams and the vanishing signal rates in the gaps between the spectral lines (in particular when using a very-low noise channeltron), this might have saved statistics, but only by never getting to the end of the spectrum. For a presentation at a Spring Meeting of the German Physical Society, Heckmann and a photographer climbed on a crane to take an overhead view of the set-up. Friedbert then managed to talk all ten minutes of his talk about the screws, nots and bolts of the apparatus, visible or (mostly) not, showing this single bird-view picture. Heckmann suffered more during that talk than he had during the photo excursion climbs. Another talk Friedbert gave was obviously rendered by heart, but without any fervour or overview, just trying to repeat almost tonelessly a privately rehearsed text.

A later student, Helmut C., complained that the accelerator control units and indicators were not built as he declared that they should operate. He did not see any reason why an instrument should have switchable ranges if he expected it to be able to work without such complications. He had many questions so clever that nobody else could see the reasoning behind them anymore. After the first year with the group, however, it turned out that he had not yet understood even the working principle of the tandem accelerator. To make good on that and to show his self-perceived computer proficiency, he tried to concoct a driver's manual for the machine, a task the operators had given up on. He enquired about details like the Einzel lens, and dutifully reported in his diploma thesis that this electrostatic lens was named after the (German, of course) inventor Einzel. That was, he claimed, what Accelerator Technical Chief Klaus Brand had been telling him. (If so, Brand would have had a hard time keeping a straight face, and it would have happened only after technical arguments had failed to satisfy the listener.) The German root is Einzellinse, verbally for single element lens, which in principle is may be just a hole in a plate. The special name, was probably phrased in the days (long gone) when Germans did electron optics and lived long enough thereafter (half a century in this case) to collect a Nobel, and they survived into English.

This recalls a spoof played by Indrek and (?) on good old Stanley: When the first of the Bashkin/Stoner Tables on Atomic Energy Levels and Grotrian Diagrams were published, they secured an old letterhead of Professor Grotrian's and wrote to Stanley under his name, enquiring about Stanley's use of this type of diagrams. The reaction seems to have been some surprise at finding that the diagrams were named after somebody and that maybe a trademark violation had incurred.

Maybe these guys copied Grotrian's signature from a 1937 letter of Grotrian's (at Potsdam Observatory) to the then young Doctor Bengt Edlén at Uppsala. In this letter Grotrian points out that some (then unexplained) coronal lines agree in wavenumber with ground state fine structure intervals in Fe X and Fe XI (just measured reasonable well by Edlén). Edlén afterwards (1942) proved that this was not a spurious coincidence, but a real identity - that the solar coronal lines are indeed transitions between fine structure levels of highly charged ions. This superb work brought him into consideration for the Nobel price, but it is rumoured that the existence of Grotrian's letter (a copy of which purportedly was kept in a Stockholm institute director's desk) which hints at the proper idea was decisive for the committee's vote against this candidate. Some decades later, when the eagerness of getting such a prestigous prize had worn off, Edlén took it with humour when people asked him why he had not gotten that prize: "It is much better if people ask this than their asking 'why ever did that guy get it?' ".

Experiment and theory did not keep up with each other (a standard situation in physics):

In the 1980es, Edlén re-evaluated some (spectro-) photographic plates he had recorded in the 1930es. There had been many more lines on them than he could assign at the time. 50 years later the data were still unsurpassed ... Edlén published his new (expanded) analysis of the old spectra in Physica Scripta.

Well, Indrek Martinson and Dave Ellis published a paper on Be about 20 years after the initial (about 1973) measurements, the results of which had seemed too puzzling to publish at the time. After that period, other people had seen spin-dependent autoionization, and this then also served to explain those old data. The following note from Indrek Martinson to Elmar Träbert refers to this:

> "... don't be nasty to the Be I paper. I am quite happy that it came out during my lifetime. I always believed in this spin-orbit stuff, but only when Magnus W(esterlind) calculated the lifetimes with Cowan's code (and without knowing the experimental values came very close to them!!!) did I think that we should publish the stuff. I still remember when I did some of the first measurements, in 1973 (This was when Ingmar B(ergström) was on sabbatical at CERN, working with Herr Backenstoß on hadronic atoms, but he used to come back to the Institute once a month or so, just to check if all was ok). Ingmar came to me and asked what I was doing, and I told him in the ordinary way - measuring lifetimes and identifying transitions, etc. He got a bit upset and said that I had already been doing this bread-and-butter type of work for 5 years, so it was time for doing something new and exciting instead. He left and went to the channeling guys and wanted to know what they were doing. So a month later, when I was still doing the same thing, I was a bit better prepared and told Ingmar that "I was making quantitative studies of relativistic effects on off-diagonal matrix elements". Ingmar became extremely interested and wanted to know all the details. The next day the secretary showed me a copy of a letter that IB had sent to SB in Tucson where SB was told about the fantastic work going on. As usual with IB, he had big amplitudes and the truth was somewhere in between, I presume. ..."
