# 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.

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How to get started with a new technique and survive the mobbing by the colleagues :
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Beam-foil spectroscopy was invented as an ingenious method for atomic physics experiments using nuclear physics ion accelerators. It has been described in many physics journals, conference contributions and books, and it would be idle to add another technical treatment to the ones available - beyond a brief overview for non-specialist readers, in order to show the playing field. Instead, the present text aims at something quite different, in that inception, development, highlights and decline are described as a case study for the life cycle of a scientific method, from the fight for recognition to the eventual demise because of shifting and fading interests. The people who made it, the people who developed and spread it, notes of science and gossip, recollections and remarks from the sidelines are meant to illuminate a field and its interactions with people, science policies and the winds of change.

To a considerable part, 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 their implication clearly is that the stories are there! Maybe time will make those oral history documents available after more than 30 years ... and shed some colourful light on those special characters as well.

Unavoidably, the presentation is biased by my personal recollections as the compiler, by my particular niche in the scientific business and my temporary involvement and employment in the field, the laboratories I worked in or visited, and the selection of people I met and kept in touch with. My conclusions are not authorized by anyone, and there will be people quite annoyed about many things said (or left out) and the way this is done. As a compensation, I hope for some people to chuckle. Some of the recollections contributed by others have been edited for suspected typing errors (e-mail on-line typing into poor terminals, keyboards of different design and layout with different computers and in different countries, plus doing things in a hurry before the next lecture or committee meeting, all this is highly error-prone). Many anecdotes are instantaneous recollections which refer to events that took place decades ago and thus are likely to suffer from shifts and slips of memory and colourings in the perception of the original facts. However, some recollections sharpened by emotion and hindsight may be "truer" than the bare facts.

The reader is advised to take some of the personal stories as such, since a verification of details will be impossible in most cases. However, I have tried to preserve (or conjure?) some of the flavour of the bygone days. The persons named do exist, but I cannot guarantee that all the stories about them are beyond doubt. Similarly, there are atrocities, conflicts, personal feelings or ridiculous experiences exposed in some anecdotes. These may be close to factual events, but ought to be consumed *cum grano salis*. After all, even fierce clashes among individuals which are talked about in the community do not hinder the same people to get along considerately at other occasions, or to cooperate successfully in scientific projects. Nevertheless, such clashes and antagonisms also are part of scientific life - as run by humans. I hope that none of the people mentioned (or left out) feels overly hurt by my stories, that colleagues reading this might come forward with more memories or details to add, and that everybody has some fun of it.

E. Träbert

## Introduction

From 1988 to 1990 I spent two years at the Harvard-Smithsonian Center for Astrophysics, a visiting scholar supported by Max Kade (New York, USA) and Feodor Lynen (Humboldt Foundation, Germany) scholarships. I wanted to get experience with radiofrequency ion traps, because I had worked for more than 10 years, since my graduate time, in atomic physics using heavy ion accelerators. Is that a bad thing, doing atomic physics with accelerators? I certainly don't think so. However, financial support for such work was dwindling in this field, for reasons inherent in the science funding mechanisms, one of them being a general feeling among elder peers (who are involved in reviews and recommendations for funding) that this field did not quite deliver on its earlier promises. Such views, once emerging, are self-fulfilling: funding decreases, and this limits further research. Notable remarks like that by Lady Anne Thorne at the 1989 ASOSALP (Atomic Spectra and Oscillator Strengths for Astrophysics and Laboratory Plasmas) meeting in Amsterdam, telling the specialist audience that "finally" beam-foil spectroscopy has delivered some remarkably useful data, reach the eager ears and wounded souls of a few people who anyway know (like mine, as I had contributed to obtaining the data mentioned, and a few friends), but they do not reach funding bodies.

This experimental technique of beam-foil spectroscopy permits the measurement of dynamic aspects of multiply charged ions, that is atoms which have lost several of their electrons. Such ions are present in plasmas from welding torches to tokamak fusion test reactors, from lamps to the stars. However, the method has limits which seem to exclude measurements on some specific ions which are of great interest in astrophysics. One of the limits may be overcome by not working with a beam of fast ions, but by using ions confined in an ion trap. One of the previously successful radiofrequency ion traps existed at Harvard-Smithsonian, and it seemed a good way to present my interest in research there by claiming that it would be rather a logical extension of the astrophysical angle of my previous work to study lifetimes of some particular importance there. Also, if you are working in the same field for some time (without a permanent position, that is), irrespective of your success, you are considered as somewhat *narrow-gauge*, and that means you have no chance for a professorship in Germany. (If you switch fields until the right people get to know you, that might be different. If you should get to such a position and then return to what you did before for some time, and continue to do it until you reach retirement age, that might be considered *scientific depth*.) Well, I got my scholarships, moved to Cambridge (Mass.) (quite fitting a location, by the way, as I spent two postdoc years at Oxford (England)), and found the ion trap in shambles. Thanks to two eager postdocs (in succession) and in spite of my occasional involvement and advice, the trap in tedious course was revived *and* replaced during my time there. By careful nudging, the trap eventually (more than a year after I left) even yielded the data I had wanted to obtain in first place. I believe I helped then getting the postdoc in charge, Tony Calamai, in sufficient contact with capable theoreticians and made some of them aware of the interesting data. Finally, the theoreticians managed to support the isoelectronic trend of the experimental data, with the ion trap results at one end of the range (ions with few missing electrons) covered and my previous results from beam-foil spectroscopy at the other (half of the electrons taken away from the ions). In the end, some of this was intellectually pleasing, even as the experiment took more people and years to succeed than everybody thought.

While frustrated by the failures of the mistreated old ion trap and the slow rate at which funds and equipment for improvements and replacements were forthcoming, I worked off my frenzy by using the electronic mail system, tutoring my graduate student Gerhard Möller and assisting (by a kind of "remote control"?) the beam-foil experiments of the Bochum group ("at home"). And then, in the exchange of notes on strange experiences in various laboratories and with people we both knew, between Indrek Martinson (Lund) and me the idea was born to collect stories and gossip from people in the field as long as they could still be reached. This was indeed a concern - the medium of electronic mail was coming into wide-spread (and free) use, and Stanley Bashkin at Tucson, one of the inventors of beam-foil spectroscopy, was happily advertising it. However, at a 1984 conference at Oxford, Beam-Foil Spectroscopy had for the last time appeared in the (sub-)title of an international conference, and both Kay (the other inventor) and Bashkin had delivered notes on the early days - 20 years before. This was a sure sign of things nearing an end and wanting speed to catch them in time.

As the booklet will show, Bashkin is not just one of the inventors and successful promoters of the field of beam-foil spectroscopy. He housed and trained "the next generation" in his laboratory, started a

conference series in which he then starred, traveled the world in the quest of spreading his idea and gave rise to fond memories and funny stories everywhere. I am glad there is so much of him and by him in this collection - and that he takes it in his stride to see some fun made of him - occasionally.

With an ear on Stephen Hawking's "A Brief History of Time" my working title for the collection of beam-foil related stories became

"Fast-beam spectroscopy - A history of its brief time"

## How the project started out

Indrek Martinson headed the physics department at Lund University as the successor to Bengt Edlén. Where Edlén had driven classical spectroscopy to extreme precision, Martinson added the new-fangled itemry of beam-foil spectroscopy by acquiring a Pelletron accelerator, as well as (with Ulf Litzén) a powerful laser to produce plasmas for spectroscopic studies. Sweden being politically neutral and close to the Eastern European states, many scientists from the Soviet Union and the Baltic states were permitted scientific visits there, even as most were barred from going to NATO countries. Martinson, born in Estonia and loath of the Stalin era and the ensuing bad living conditions in the East, welcomed the colleagues and helped many of them to get some money on top of their meagre funds from home. As some compensatory measure, afficionado Indrek beat lots of those people at chess, including kolchos champions and the likes. At the same time he kept an open house, scientifically and socially, for people from the West and anywhere in the world. Following the Edlén tradition, he often entertained physics celebrities and their entourage as well as less prominent colleagues on their way to the Nobel prize ceremonies in Stockholm.

Having met Martinson at the 1977 EGAS (the European Group on Atomic Spectroscopy, then strongly motivated by Derek Stacey from Oxford) conference at Cracow (Poland), I first got to Lund immediately after my postdoc years at Oxford. Martinson then (1980) staged a conference on "Atomic Physics at Very Large Accelerators". Now - for the benefit of the funding agencies - this sounds a bit more exclusive than it was. The really big machines just running were doing nuclear physics, whereas the poor relatives, atomic physicists, everywhere in the world seemed to be second-hand users of those accelerators, when the nuclear guys' interests were shifting to the next larger size machine. Consequently there were very few people from laboratories with medium size machines, and most had just a machine capable of a couple of hundred kilovolt at their perusal, "small" machines even then. The meeting was very informal, Lund in late summer was a treat, there were many friendly exchanges of ideas, memories, thoughts and gossip. Martinson and his Lund colleagues obviously had in good hand in organizing such get-togethers.

With such pleasant experiences in my mind, I kept the contact to Lund, went there for more meetings, passed through for holidays, and, together with Martinson, got a rather productive collaboration Bochum-Lund going for many years. With Martinson in such a central, but well-earned position in the web of physics relations, I sought his advice and we exchanged views when we met. When the BitNet electronic mail system developed and became available, the exchange became more frequent and also rather asynchronous. (What now is called Asynchronous Transfer Mode, ATM, is rather what bitnet users did all the way. Send a message now, get a reply when the adressee can reply. On the phone, the money clicks away even if you are not speaking; with e-mail, you get free time for thought before you reply. ) Martinson seemed a bottomless supply of neat anecdotes and old time stories. When I began to hear the same stories again, I realized that both of us would be getting older, and that some of the stories might be worth preserving, not just for insiders, but as a background to field in which I by then had grown deep roots. Martinson was too busy to write down his stories at leisure, so I began to collect some when he told them over the (bit)net. Martinson had particular memories, but other people would have seen different angles. Electronic mail provided a cheap way of telling people that somebody would be interested to receive their recollections. The following message was sent to those people in the bfs community that I had e-mail addresses of (note the good old days of BITNET and of addresses that would not have more than 8 characters!):

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November 1989

Dear Fast-beam Spectroscopy Colleagues,

Next year there should be the next of the tri-annual fast-beam spectroscopy conferences, but there won't, unless somebody declares the Giessen conference on Highly Ionized Atoms to be a legal successor. This loss of a traditional meeting perhaps underlines the notable decline of the field; a decline, of course, not of the quality of the results (which rather tend to get better with the growing experience of the scientists), but of the number of people and laboratories involved, and, last, but not least, of the response of the funding agencies.

Before now the first generation of BFSsts (beam-foil spectroscopists) retires and the crowds disperse, it might be worthwhile to recapture some of the fun and excitement of the early days. Following an idea of Indrek Martinson's, I suggest to contribute recollections of funny episodes, anecdotes and gossips about people, events and experiments/theories in the early days of the field (and some later developments). Indrek and I would try to sort them out and to re-arrange them for future access and perhaps try to find some way of printing them.

In order to keep the effort on our side to a minimum, we would prefer to get contributions by electronic mail. Other written forms of communication would, however, not be discouraged. Everybody who contributes will be notified about the collection afterwards and will have access to it.

Would you join us in this effort, please?
With kind regards and greetings,

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In due course (that might mean quite some time!) responses from principal players like Ivan Sellin (Knoxville, TN) and Stanley Bashkin (Tucson, AZ) arrived:

From: <SELLIN@UTKVX>

I did not ignore your message -- I just have not had an inspiration about an amusing anecdote suitable for inclusion yet. Indrek was always much better at this sort of thing than I. If I think of something suitable, I'll be in touch. In the meantime, good luck with your effort, and happy holidays to you and your family. Regards, Ivan.

(Ivan was one of the early movers in the field of ion beam based atomic physics, with more than 30 PRLs and PLs to his credit. Nowadays authors are fighting to have a single PRL to their credit, which often is mostly hype and little information, instead of writing a meaningful paper rightaway. In the olden days, people had worthy news that they reported in two- or three-page Letters!)

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Tucson, November 13, 1989

Elmar -
I have your message about reminiscences of BFS. Actually, the whole subject is just a big joke! I'll think about it, but I can't off hand recall anything particularly memorable. Stan.
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Date: Thu, 28 Dec 89 11:47 MST

From: <BASHKIN@ARIZRVAX>

Elmar -
I have been thinking about an entry for the funny-story paper on BFS, and I think I have one. It will be somewhat long-winded, but, if I ever manage to send it to you, you'll be free to edit it as you like. Soon. Stan.

[It is the *Old Tucson* conference report somewhere below.]

Stanley Bashkin has an air for poetic writing. He got a poem reprinted along with conference proceedings once, and a particular sport of his are so-called cartoons (words only), and letters to the Wall Street Journal.

Emile Knystautas recalls:
I always remember when Charlie Moak from Oak Ridge stood up at the July '70 BF conference in Sweden and said "Nothing resembles a new effect so much as a mistake!", then sat down, to the consternation of the speaker (I think it was Bill Bickel from Arizona) and the audience. EJK also recalls "his Hungarian friend" to have told that "there are true stories, and there are good stories" ... I think that some of these stories fortunately fit into both categories.

Indrek Martinson remembers:
Oh, Stanley Bashkin was very much impressed by the new things this light source produced. One day in the early days they ran a beam of nitrogen, and Stanley undertook to check the observed spectra against the literature data [most likely in Striganov/Sventitskii, the book he still cherishes]. He could not find any match, and he proudly concluded that this magnificent light source again demonstrated an overwhelming capability of producing lines not excited in any other known source. Bill Bickel came along and checked, too: He found that Stan had looked up the tables for Ni (assuming them to relate to nitrogen).

Several other major players unfortunately refrained from releasing their stories, being afraid of possible retaliation by other players whom they considered to be (too) powerful, or they simply could not be reached via bitnet (an early version of the electronic mail system), like the Lyon group. Yes, it proved difficult to get any electronic mail contact with many of the dear French fast-beam colleagues (though the astrophysicists quickly used those means). Cultural diversity, I guess, and a reluctance to be exposed to non-French data which not always coincided with the findings made at home. Then (some) people in France would rather maintain the French version - even after being told and proven why it was probably wrong or superseded. Some French colleagues, speaking English very well, indeed, would not be moved to render a manuscript (prepared by less fluent collaborators) better readable by the non-French speaking world by revising the language (massively). Still, there are nice colleagues over there, and many splendid ideas have been thought up and excellent data obtained. And those enchanting and heartwarming French even offered a full professorial position to a German, Horst Jürgen Andrä, for which he did not have to give up his German professorial post at Münster. Yes, that reflects their incredible kindness; probably they had heard that he had enjoyed windsurfing on the Berlin Havel and then despised the local and dirty small lake at Münster. Grenoble, however, lured with surfing ranges on the Lac d'Annecy and with skiing in the Alps ... what a choice!
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**Bashkin's follacious spectroscopy (BFS)**
or
Collected gossip from a field that thrived prematurely and declined steadily since (like a decay curve with a growing-in cascade)

*Une comedie humaine* - maybe it is not funny, but at least it is about human errors
Players (not complete):

Horst Jürgen Andrä (Berlin (Germany), Münster (Germany), Grenoble (France)) / Stanley Bashkin (Tucson (Arizona)) (1923 – 2008) / Yvette Baudinet-Robinet (Liège (Belgium)) / Henry Gordon Berry (Madison (Wisconsin), Stockholm (Sweden), Lyon (France), Chicago (Ill.), Argonne (Ill.), Notre Dame

(Ind.)) / William ("Bill") Bickel (Tucson) / Jean P. Buchet / Marie C. Buchet-Poulizac (Lyon) / Hans Heinrich Bukow (Bochum (Germany)) / Haro v. Buttlar (Bochum) (1926 - 2000) / Lewis "Lew" Cocke (Manhattan (Kansas)) / Richard J. Crossley (York (England)) / Lorenzo J. "Larry" Curtis (Toledo (Ohio), Stockholm) / A. Denis (Lyon) / Jean Désesquelles (Lyon) / Michel Druetta (Lyon, St. Etienne) / Paul D. Dumont (Liège) / Lars Engström (Lund (Sweden)) / Finn Folkmann (Aarhus) / Michel Gaillard (Lyon) / Henri P. Garnir (Liège) / Andreas Gaupp (Berlin) / Harvey Gould (Berkeley (Cal.)) / Reinhold Hallin (Uppsala (Sweden)) / Paul Henrich Heckmann (Bochum) (1930 - 2005) / Sven Huldt (Lund) / Roger Hutton (Lund, Berkeley (Cal.)) / Brant M. Johnson (Brookhaven (N.Y.)) / Keith W. Jones (Brookhaven) / L. Kay (Manchester (England)) / Hugh Klein (Oxford (England)), Harvard, NPL) / Emile Knystautas (Laval (Québec)) / Peter Kuske (Berlin) / Jack Leavitt (Tucson) / A. Eugene "Gene" Livingston (Notre Dame (Ind.)) / Larry McIntyre (Tucson) / Sven Mannervik (Stockholm) / Richard Marrus (Berkeley) / Indrek Martinson (Stockholm, Lund) (1937 – 2008) / James Macdonald (Manhattan (Kansas)) / Edmund G. ("Ed") Myers (Oxford (England), Tallahassee (Florida)) / David H. Pegg (Knoxville (Tenn.)) / Eric Henry Pinnington (Edmonton (Alberta)) / Nina Reistad (Lund) / Patrick "Pat" Richard (Manhattan (Ks.)) / Dick Schectman (Toledo) / Werner Schlagheck (Bochum) / Ivan Sellin (Oak Ridge, Knoxville) / Joshua David ("Josh") Silver (Oxford (England)) / John O. Stoner, jr. (Tucson (Arizona)) / Reinhold Tielert (Bochum) / Elmar Träbert (Bochum, Oxford (England), Cambridge (Mass.)) / S. Varghese (Manhattan (Kansas)) / Erling Veje (København (Denmark)) / William H. Wing (Tucson) / Helmut Winter (Bochum, Lyon, Berlin, Münster, Berlin)

and a cast of hundreds

## HISTORICAL NOTE

In the 1920's, W. Wien observed the optical emission from canal rays and tried to study both the spectra and the intensity distribution along the flight path. Poor vacuum was a limiting factor, and thus the method of spectroscopy on fast ion beams fell dormant for another four decades. Since the 1930es, accelerators were developed with the aim of giving atoms sufficient energy so that in close collisions the Coulomb repulsion between the nuclei might be overcome and nuclear reactions be initiated. In such studies often a fast ion beam strikes a thin target, and the effect of energy loss of the projectiles in the target as well as a modification of the projectile ion charge state by the interaction with a material target was realized and measured. Because of the small geometric cross sections of the nuclei, most projectiles do not interact with the target nuclei, but rather with the screened Coulomb field of the nuclei (Rutherford scattering) and with the ubiquitous target electrons. Consequently most of the projectile ions continue their trajectories with little change in velocity vector, and are considered as wasted from a nuclear physics view. It is exactly these projectile ions that are of use for the atomic physics experiments to be discussed here, whereas those projectiles which undergo collisions resulting in a large angle of deflection or a considerable energy loss cause systematic errors and spoil otherwise clean atomic physics measurements.

Normally detectors for particles or g-rays would be used to see whether fast ions (guided inside a vacuum vessel from the accelerator to the experimental site) had hit a nucleus in a thin foil so that it would break up or otherwise tell of nuclear reactions. In 1962/63, independently of each other, two nuclear physicists working at accelerators dared to look with their own eyes. These two people, L. Kay at Manchester (England) and S. Bashkin at Iowa (then moving to Tucson (Arizona)), realized that the incoming ions have electronic shells outside the nucleus, and that after the violent passage of the ions through the foil target these shells must rearrange and possibly emit light in the process (Fig. 2). This was the birth of beam-foil spectroscopy (bfs) which later developed into fast-beam spectroscopy (fbs) (for references on some more details, see Physica Scripta **78**, 038103 (2008)).

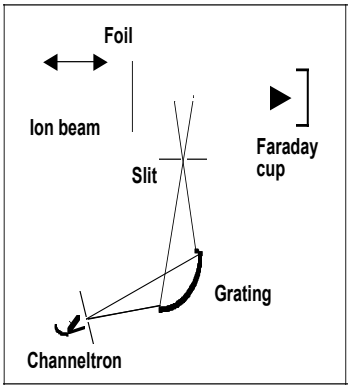


**FIGURE 2.** Schematics of beam-foil spectroscopy. The ion beam from the accelerator passes through a thin foil and is afterwards caught in a Faraday cup. If the ion-foil interaction shook the electron cloud of the fast ion, some light will be emitted which can be observed and analyzed by means of a detection system (Shown is a grating spectrometer with a channeltron detector for the vacuum-ultraviolet spectral range). The double-ended arrow indicates that (most easily) the foil is moved to adjust the distance between the position where excitation takes place and the viewing region.

Among the features of the new light source was the light intensity that dropped along the excited ion beam. This was immediately recognized as a signature of the exponential decay of excited atomic levels. Some of the early lifetime measurements gave good results. Many others, however, failed in the attempt of determining meaningful radiative lifetimes of specific atomic levels, for lack of linear detectors, poor spectral resolution and under-appreciation of the complex decay chains activated by the non-selective ion-foil interaction. These points need to be adressed in order to obtain reliable lifetime data.