

## **A Manual for Reviewers of Beam-foil Spectroscopic Studies submitted to Learned Journals**

Beam-foil spectroscopy has been conceived by two nuclear physicists, L. Kay at the University of Manchester and Stanley Bashkin at the University of Arizona at Tucson [1], more than a quarter century ago. Kay's colleagues on his faculty made it impossible for him to pursue the new ideas in practice [2], but Bashkin's colleagues were less successful in depriving him of the means of doing what he wanted [3], and that gave many people a chance to learn at his laboratory. The new field seemed exciting and promising in the beginning, but it failed to fulfill all the expectations within the first few years. Some people still drag on with it, although funding and numbers are declining steadily. However, recently even the Chinese seem to have fielded some interest in starting a research group of their own.

Before this folly continues for ever, it is about time to call for a stop, and I suggest to effect this by a collaborative effort on the side of the referees. The task needs to be better coordinated than the chaotic manners prevalent till now: Although this chaos led to a pattern in the right way, it has proven not to be sufficiently effective. Since some really awful BFS papers slipped into the recent literature in spite of many a referee's honest and careful (but failing) attempts at improving the manuscripts before letting them pass eventually, there is a fair (non-negligible) chance that your decision to refuse any such paper would be statistically justified by any standard. This should be balm enough for any mind with regretful afterthoughts. However, in the referee report you want to pretend that you applied much better reasoning than just gambling on statistical probability, and the following guide might help you to select the proper arguments towards the common goal, the defeat of an already weakened, widely disliked field of research.

General misconceptions you ought to have as a referee (Millions of people with prejudices can't be wrong ...):

Beam-foil papers (well, papers-to-be, educate those fools first, before their manuscripts might become published) tend to report on wavelengths, lifetimes or oscillator strengths, term values, x-ray energies, multiply-excited states, quantum beats, alignment, lines not seen anywhere else and similar irreproducible phenomena. And never bother to distinguish between beam-foil spectroscopy (BFS) and fast-beam spectroscopy (FBS), the guys with the latter label may not all do the same, but with an acronym so close to the former, they might provide a camouflage cover for the straight BFS guys, and we can't have that. Therefore, advise the authors that the interaction in the atom, the Coulomb force, is known since quite some time, that the Schrödinger equation and the Dirac formula contain all the physics anyway, and that there is no need any longer for atomic physics in general and experimental atomic physics in particular. If they mention the words "cascade" or "blend" or "spectral resolution" and you know the word "laser", ask them why they did not use a laser for their studies, since anybody should know by now that with a (one?) laser you can measure cascade-free lifetimes to perfection and obtain arbitrarily narrow line widths.

Don't be afraid of letting show that you know that there are no proper lifetime data ever from beam-foil spectroscopy, that the spectra are a mess, the lines too wide and too many, and that people are known to have picked the wrong lines when they wanted to measure lifetimes of particular states (and that the results measured and published on the wrong decays were still closer to true numbers wanted than the widely scattered theoretical predictions at the time [4], but don't remind authors of that). In short, remind the authors that their field is dirt(y), that the results are not to be trusted and probably (most certainly) are completely meaningless.

If you are a nuclear physicist, you might ask them how they make certain which spectral line is which, and whether never there were two spectral lines that exactly coincided? Of course you would not need to know that it took the NRL and NBS/NIST guys [5] years to find a single such coincidence for different ionic spectra of one single element, and you would forget to remember that gamma-ray spectra of nuclear reactions are much worse and never isotopically clean. You would remind them, however, that the literature is full of silly BFS lifetime data, even if for a proof you would have to dig to get down to the first decade of the business, and of course that

situation cannot by any means have improved since, due to bright enough people who could have bothered to find ways out, no, people so bright would have chosen different realms to graze but BFS.

After these preliminary remarks showing that as a referee you are far ahead of any FBS authors (nevermind if they did not work with foils after all, they just might have) in terms of knowledge, insight, clairvoyance, status, qualification, righteousness and sobriety, you can enlighten the authors of the latest, if not ultimate techniques of obtaining clean decay curves and narrow-line spectra, and if the data show sizable error bars, tell them to return to the laboratory and don't come back before the experiment yields error bars smaller than the small print we can't read without glasses. If they say that this can't presently or ever be done, the worse for the method they applied.

Dealing with Atomic guys is always a good occasion to show the value of a background in Nuclear physics. Remember Rutherford ? (Nobel prize, right! For finding that most particles don't care that there is a nucleus in the target: they just go on straight.) Well, some particles are deflected in the Coulomb field of the nuclei of the target foil (Remember, that is the interaction we all know perfectly well!), and their trajectories then change so much that they will leave the ensemble of the others sideways. Their decays will increasingly be absent from the main beam which is still in the field of view of the detection system, and the result is a decay curve with a kink and then a significantly increased slope downwards. This means, that the unaware experimenter will derive a lifetime result which is too low, and we all know that BFS does give wrong lifetimes.

On the other hand, the beam-foil interaction populates (too many!) states non-selectively. Whatever you consider, the cascades will make your decay appear more long-lived than it is, and we all know that BFS does give wrong lifetimes. And this effect is worse, the farther away from the foil you measure, because the yrast cascade is so incredibly persistent. And because of the cascade problem, we all know that all BFS lifetimes always are too long. This also is reflected in the data on the M1 decay in Helium-like Ar as obtained in the early 1970es at Berkeley by Richard Marrus and Robert Schmieder [6] : The first results fell 20 percent short of the predicted lifetimes. But then, the farther away from the foil the measurement started, the closer it got to accepted trends [7,8]. Hm, maybe this is not a good example, but it shows that the rule must be right, because the exception from the rule exists.

The referee should now proceed to recommend that the authors go back to the laboratory and try again, this time with the purposeful application of the "Rutherford-Scattering Cascade-Tail Cancellation Method" which has been invented by a former nuclear physicist (who unfortunately turned to BFS), Indrek Martinson [9]. By careful/clever arrangement, the slow cascade tail and the downward kink caused by Rutherford scattering can be matched to cancel ("lifetime refocusing"), and the result is a neat decay curve which satisfies Dirac's requirement of beauty (if not truth).

Elmar Träbert [10] must have done the right thing intuitively/cunningly when he encountered the down-kink in long-lived decays: After introducing a suitable collimator to restrict the range of possible beam trajectories, the criterion for success was that the curves then looked straight on a logarithmic plot. Mechanical obstacles, however, are a crude way to collimate ion beams. A much more elegant, though not yet widely known way is the following preparation of a parallel beam by angular straggling: Collimation is used to filter out those trajectories in a beam that are parallel to each other and a given axis. Usually this implies that one loses most of the beam intensity, since divergent beams from a source will never voluntarily become parallel ones, as they seem to know about Liouville's theorem. However, if one starts out with a collimated ion beam and passes it through a foil, the scattering of the ions at the atoms in the foil will lead to a notable divergence in the beam afterwards, as stated above. Anybody can visualize this by advanced graphics, that is on the back of an envelope.

From a similarly simple idea (in hindsight as simple as many great ideas, but time had to be ripe for it to be born) one derives the next step. Start out with a focused (convergent) beam, with the waist at the foil. The ion-foil imposed divergence will then evidently lead to a less

convergent beam. If things are done cleverly enough, one should obtain a more or less perfectly parallel ion beam - with no loss of intensity!

Now to some more sound advice from wise and thoughtful people like us referees to the naive and perhaps even well-meaning beam-foil authors whose work will inevitably show some shortcomings that can be dealt with using the hints below, on how to get data without fault: In case traditional techniques of spectral refocusing [11,12] have not been applied, were not applicable or insufficient, try to close the slits even further. The signal will be smaller, but what good is a high signal level at no physical significance of the results anyway?

Roger Hutton and Ulf Litzén [13] at the University of Lund (Sweden) (and certainly many other people elsewhere, too), set a fine example. They closed the spectrometer slits completely and, during a single night, obtained perfectly narrow lines. Admittedly, such narrow lines tend to fall in between the channels of a recording device used in scanning mode, but position-sensitive detectors are expected to do better: thanks to their intrinsic noise they often show a signal even if the spectrometer slits are closed.

In order to avoid high (or even any) background counts, a proven technique is to lower the high voltage on photoelectric detectors. When switched off, these detectors are even completely free of dark counts. This advice takes care of any author who claims to have corrected for background and dark counts - why such half-baked and dubious treatment, why not go for the perfect experiment immediately?

In some laboratories no electrostatic energy analyzer is available to measure the energy loss of a given ion species at a given energy in a given foil. The exact magnitude of the energy loss is a limiting factor in many lifetime measurements aiming at Andrä [14] precision (that is the level just about unattainable in any other fast beam experiment), because foil thicknesses are often uncertain by considerable percentages, if not integer factors - beam-foil never gives reliable lifetimes!). There is a simple and cheap way out: Don't replace broken foils; foil thickness and energy loss then are perfectly well known.

If you encounter a particularly stubborn BFS author who repeatedly refuses to change fields and possibly even insists on wanting to publish the junk he/she has submitted to a scholarly or even archival journal which boasts a select team of most learned and unbiased referees (like yourself) who are willing to exert utmost scrutiny, please spread the word among the refereeing colleagues how you finally dealt with the author to get rid of such follies; the colleagues might benefit from your ingenious schemes the next time they have to referee/judge/condemn a manuscript from that field. And that should happen before, eventually, those guys might produce data which somebody of proper conduct might be tempted to concede he/she needed/wanted, but could not get anywhere else, and which might even be right and reliable and meaningful. Preserve the high standards of our professional prejudices!

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