



The failed battle of the nuclear scientist Hans Frauenfelder (HF) to win the Chemistry Nobel Prize in 2013

Comment by Wolfgang Doster at
www.bioneutron.de

How I became the most important enemy of Hans Frauenfelder

Prof. Hans Frauenfelder died in 2022 a few days short of his 100th birthday. He was active almost to the last minute of his life. He was a very influential scientist, both ideologically and in deciding about who is receiving positions in the field of protein dynamics. He saw himself as the spider sitting in the center of a web. I was his postdoc around 1980 at the University of

Illinois at Urbana Champaign. In his group I learned a lot about low temperature biophysics, dynamic heterogeneity in proteins and the kinetics of ligand binding to myoglobin. I had a great time. At the end of this period I got a position in the physics group of Edgar Lüscher at the Technical University of Munich, Germany due to his recommendation. Edgar was working hard to establish a new neutron source the FRM 2 in Munich. He was finally successful, although he did not live to see it. I got interested in this project applying neutrons to record fast hydrogen dynamics in proteins together with MD simulations. Hans Frauenfelder supported this idea and due to him as a referee we could publish our first significant results in Nature in 1989. Unfortunately my scientific interpretation of the new data did not support the rigid conformational substate model of Hans. He postulated that proteins should be viewed as a disordered semi-liquid solid. The neutrons should show that. When this discrepancy became obvious, it was not discussed, instead it initiated a battle.

In 2009, Gene Stanley, at the time Editor of PNAS, warned me at the Rom Meeting of complex dynamics, that Hans Frauenfelder was extremely determined to “move me (Wolfgang Doster) out of his way”. According to Gene, this battle was now the main focus of HF’s life. He was joking that this would prolong his life. He was right! In 2009, the “battle” was going on already for at least 10 years. At the first level, a series of un-reviewed PNAS papers with advertising titles were published, where the Doster papers were re-interpreted. Apart from “slaving” HF discovered the hydration shell and “dubbed” the term “ β -relaxation”. It was essential to introduce always the opposite: Doster’s α -relaxation in the hydration shell was turned into β -relaxation, no physical explanation or references were given. At the next level, HF forced the organizers of conferences to put him on the speech list. The retired neutron spin echo expert F. Mezei was hired by HF to back up the issue. During the European Biophysics Conference in Budapest 2008, they could eliminate the program committee thanks to the intervention of the former HF postdoc Pal Ormos: The key-note

speech on bio- neutron scattering was assigned to Mezei, although he had never done any work with biomolecules. HF himself, who also had never done such experiments, would talk in the special neutron scattering session, telling everybody his new message about the protein dynamical transition: After 20 years HF had realized: “There is no transition”. For this purpose the transition was redefined. This message would be distributed everywhere to the audience of workshops and conferences. In parallel, HF was extremely effective in undermining my job applications. In one case, a position for an experimental biophysicist was advertised by a university, where a new neutron source was built. Thanks to intervention of HF my application ranged behind the one of a theoretical liquid metal physicist. A low temperature optical glass physicist finally got the job, to stop the deluge of mode coupling theory against the energy landscape believers.

What was the main reason behind these unusual (?) and vigorous attacks on my person and my work? What did I do wrong? (“I was too independent”) Why am I so important? I came up with a striking answer, which was confirmed in the fall of 2013: HF’s quest for the Nobel prize. That’s why he wanted so desperately to play a leading role in bio-neutron scattering as an external scientist. The “battle” against my person had a real background: My data and my ideas appeared to be useful as a vehicle on the road to Stockholm. He just made room for himself:

Protein dynamics measured by elastic/inelastic neutron scattering, on a pico-second time scale together with theory, MD simulations and Martin Karplus!! That was the secret. For this purpose, my work was reshaped in a proper direction and blown up with catch words. Everybody in the field was urged (sorry slaved) to cite his PNAS papers and to down-grade my papers. For my colleagues this was an extremely useful opportunity to get rid of their high quality competitor, and to promote their own carrier. Some main positions at Oak Ridge can be understood only in this context.

Martin Karplus indeed won the Nobel prize for Chemistry in 2013 together with Michel Levitt and Arieh Warshel for their pioneer work with MD simulations of proteins. No prize went to the experimental side. A.Warshel has frequently criticized the simple HF concepts of a protein dynamics-function relationship like: “It is now widely accepted, that proteins dynamics is essential to function...” as unsupported by simulation of enzyme activity.

My involvement with the HF story probably started in 1999 with the publication of our independent Mössbauer paper by Lichtenegger et al (Biophys. J. Jan.1999, 76, 414), which demonstrated, that (1) the heme interacts directly with the solvent and not via the protein and (2) that the protein dynamical transition (interpreted incorrectly as anharmonic onset of displacements) depends on the viscosity of the solvent. This paper was submitted in 1996 and was finally accepted in August 1998! Not because it was wrong, but because the English of Fritz Parak was not publishable, and because he definitely wanted to kill this paper. The experiments and the analysis were done by Helga Lichtenegger in Vienna, F. Parak had provided a ⁵⁷Fe myoglobin sample submerged in sucrose. After several resubmissions without my intervention, I rewrote the paper and submitted it with G. Vogel (Vienna) directly to BJ, where it was accepted. Parak withdrew his name. Nevertheless, it is still one of the best papers in the field. For the first time it introduced a time scale effect in the context of the Lamb

Mössbauer factor of myoglobin. In the Parak papers, the non-harmonic onset was essentially a de-trapping feature reflecting the HF energy landscape. These obvious deficiencies seem to have threatened HF's final rush to Stockholm. Stockholm was particularly bad, since at the first EBSA conference in 1985, I had introduced the concept of the solvent glass transition also in protein crystals as the origin of the low temperature substate trapping. This turned out to be correct, especially with the crystals. Today this is an important method of crystallography.

In 2000, probably after Parak had complained about the Vienna paper, HF organized a meeting in Los Alamos, his eternal retirement center. Officially it was concerned with the LA neutron scattering facility. Ironically, I met for the first time P.W. Fenimore, who had spotted an error in my flash photolysis work. After 2002 he did no longer respond to my comments on their work. F. Mezei was one of the main players at the Los Alamos meeting, where a position was organized for him. His secret task was to retrace my steps in protein dynamics, redoing and analyzing everything, that I had done with neutrons and to talk about it. His results, published as small notes or conference papers, were very modest, far below the level that we had achieved 15 years ago. But it was important, that F. Mezei would show some results, giving the impression that corrections were in order, and to suggest the relevance of the HF "β-relaxation". His conversion to the landscape community was quite unexpected, since F. Mezei had previously published papers on Mode Coupling Theory (Götze) and glass-liquid dynamics. In the end, F. Mezei accomplished his job, since he claimed to have done it to those who would not understand the issue, but who would decide about jobs.

Also my new boss, Winfried Petry, was specially invited to the Los Alamos meeting. It is now clear, that Frauenfelder and Mezei pushed him hard, to no longer support my career. Someone else had to be found to represent bio-neutron scattering. Who that was, became clear in 2001 at the International Neutron Scattering Conference in Munich, organized by Winfried Petry. Everything went together: Petry's good friend, Giuseppe Zaccai was invited to give the keynote speech. Petry had rejected my proposal to organize a protein dynamics session. After Zaccai's talk, Dieter Richter remarked, that I had said similar things much better, ten years ago.

Beyond 2010, HF completely took control of the neutron scattering community with the assistance of Zaccai, Smith and Sokolov. It was getting close to 2013. In the spring, a workshop was organized by Magazu in Messina, where HF played a major role, critical people were not invited.

In spite of all the effort and lobbying, Hans Frauenfelder did not receive the Nobel Prize for Chemistry in 2013. Sometimes there is a small shining of justice in the world. Bio-neutron scattering however was turned into a virtually dead field. Good people left the field. It was no longer possible to perform serious science. One had to be afraid, to get killed, if one would publish the wrong kind of results.

2013 was probably the last chance of HF to win the Noble Prize. This time he came really close, but it had been much closer in 1956. HF, trained as a particle physicist, worked on the β-decay symmetry breaking. He was a few months too late. Instead, the Nobel prize in

physics went to the Chinese female scientist Wu. This may be the reason, why HF was so obsessed with β - relaxation in proteins, he wanted to win the prize that he did not receive in 1956.

His most recent papers (PNAS 2015) suggest a complete re-interpretation of quasi-elastic neutron scattering theory in terms of inhomogeneous spectral broadening. The hopping across an energy landscape is defined as the basic physical process in proteins. This is explained by a “wave-mechanical model” of neutron-proton interaction in proteins. The neutron, like the photon of the Mössbauer effect, is transiently absorbed by a proton and is reemitted with modified energy due to transitions across the energy landscape. Spatial displacements, the basic physical process of the “old interpretation” involving Van Hove space time correlation functions, are dismissed (see comment below). If this explanation turns out to be correct, the next Nobel prize in Physics, Chemistry or Medicine will go for sure to HF. There still is hope.