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The Kauzmann lab in the late 1940s

John A. Schellman*

Institute of Molecular Biology, University of Oregon, Eugene, OR 9705, USA

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1. The meeting

When I arrived at Princeton in the fall of 1948 I brought with me my first research idea. After presenting a talk as a senior at Temple University on the work of Ingold, it seemed to me that one could possibly facilitate and even direct reactions by orienting molecules and polarizing their electrons in strong external fields. I dimly realized that there would be some kind of electric free energy density involved but was far too ignorant of thermodynamics and dielectrics to go anywhere with the idea. I took the opportunity to talk with the teachers of the classes I attended but without enlightenment. One professor told me that 'We just take thermodynamics for granted these days'. Another said 'You should go and speak to Professor Kauzmann. He is a very smart man'. That sounded like a good idea. A person who was considered unusually smart even by a professor must be very smart indeed.

So I went to see Professor Kauzmann. He turned out to be the young man I had noticed running the slide projector at seminars and I had thought that that was his function in the department! He told me about his research on protein denaturation and other projects. After I was filled with the wonders that I could experience, if I were permitted to work in his laboratory, I ventured to speak to him about my one and only research idea. After listening very carefully, he leaned back in his chair and said 'Do you know? I went through that stage myself'. Readers who know Walter well will recognize this kind of response. Direct, honest, no pretences whatever. He then explained that you would get dielectric breakdown before reaction enhancement and indeed effects of the kind that I envisaged are only observable in the pulsed electric techniques later developed by Eigen and de Mayer and could not be converted to a practical method of inducing reactions.

2. The laboratory

I would estimate that Kauzmann's laboratory was less than 30 feet in length and less than 20 feet in breadth. It was a standard lab with a central bench flanked on both sides by two benches against the side walls and another in front of the windows at the far end of the lab. This small room was also WK's office: his desk was just to the right of the door with bookshelves above it and a blackboard alongside. The lab was dominated by the protein project with glassware, dialysis set ups, a pH meter, constant temperature bath for viscometry, etc. Optical rotations were measured on a loan basis in a laboratory of the organic chemists. Proteins had to be prepared from raw materials, mainly ovalbumin and β-lactoglobulin. Preparing proteins was apparently considered to be a good initiation

*Corresponding author. Tel.: +1-541-346-6093; fax: +1-541-346-5891.

E-mail address: john@molbio.uoregon.edu (J.A. Schellman).

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project. Though I played only a minor role in the denaturation studies, my first task was to prepare a batch of crystalline ovalbumin. Since then the art of separating yolks from whites has been useful in my periodic forays in the kitchen. Bovine serum albumin was purchased from Armour. WK was dismayed to learn later on just how contaminated with added reagents this crystalline protein was. He demonstrated by unfolding the protein and allowing one to smell the solution. It reeked of organics strongly bound to the protein and liberated by the unfolding. This protein was later demonstrated to bind a wide variety of materials and is nature's dispose-all and all-purpose carrier in the blood stream. At the time these were the only proteins available in quantity for physical studies.

There were no grants. In fact the only grants in the entire department were two Navy grants, one for dielectrics, the other, I believe, for kinetic and catalytic studies of gas reactions. Students were supported by teaching assistantships, a few with fellowships and a good number with the GI bill. There were no student desks. Experimental notes, graphs, reports, etc. were written up in the library just a few doors away or in some other remote place. Those with theoretical projects did most of their work outside the lab. Calculations were performed on Friden business calculators with their extraordinary number of keys and complete inappropriateness for scientific calculations. In spite of their size and complexity they could only perform the four arithmetic operations.

3. The mentor

WK believed that the graduate years were the time for developing independence. He was always ready to devote time with his students, but he generally expected them to come to him with results, problems, or questions. Weeks could go by without queries about your activities or achievements, if any. There are pros and cons to this approach since, in my experience, some students appear to be very dependent in the early stages of research, but most of us grew to like it very much. He never tried to take credit for a student's research and many of us were told to write our own papers about our work. Judging from my own experience he went over theses with the greatest care, providing voluminous notes and criticisms. My own writing was permanently influenced by his criticisms, suggestions and style. I learned never to point out that results are interesting or surprising. 'If it is interesting (or surprising) that is fine, but let your reader be interested (or surprised) on his own'. In other words judgments of your own work should not be part of a research report. Even after I had left graduate school I found him to be an excellent advisor on publications. This continued until about 1957, 6 years after I received my degree. He once looked over a paper written by William Harrington and myself and sent back about six or seven pages of detailed criticism. Bill was dismayed and stunned. I pointed out that it was a great compliment that a man of Kauzmann's caliber would spend so much time reading our paper and making suggestions. If you asked WK for criticism he took you quite literally at your word and supplied it.

His intuitive-level blackboard discussions were always a delight, and because his office was his lab, we grad students caught many of them. Molecules were always seeking happiness. They would stay in a phase because they liked it, and tilt the equilibrium in that direction. They would joyfully slide down the far side of kinetic barriers and remain there, if they liked the environment. The entire fields of heterogeneous phase equilibria and chemical reactivity were summed up in analogies of molecules seeking pleasure. Later, when intuitive factors were all clear, statistical mechanical concepts would take over and the blackboard would bristle with formulas. One lesson was driven into all of us: never confuse kinetic and equilibrium concepts.

On the other hand, with seminar speakers and other visitors, discussions started and continued at a high level that mostly went over our heads.

He was a fine teacher. He put a lot of thought into his lectures and he was an extremely fast thinker. This was obvious when he taught a course in Quantum Mechanics, while working on the first draft of his very successful book on the subject [1]. He was able to keep up with the class, distributing mimeographed notes of the text at every class. This version of the book had an introduction on the scientific method and the use of models, which had a deep influence on my own scientific attitude for the rest of my career. I do not know why it disappeared from the published edition. I have kept my copy for 51 years. In his later years he became totally enthused over the teaching of elementary courses and reached an enviable level of rapport with his students. At the time of his retirement in 1982, I noticed a large sign posted in the hall by his freshman students, 'We love you, Professor Kauzmann'.

All was not science. He returned from a 6 month stay in Linderstrøm-Lang's laboratory full of enthusiasm for the pressure–volume properties of proteins, an interest which remained for the remainder of his research career. He took up tennis, bought the new Studebaker model, the car that revolutionized automobile design, and it soon became clear that there was a lady in his life. He and Elizabeth were married before I left the laboratory in 1951.

4. Projects and personnel

Though the laboratory looked relatively quiet, there was in fact a great ferment of activity going on. The laboratory only got started after World War II, 2–3 years before my arrival, so my memories cover essentially the entire first 5 years of activity.

Howard Schachman was in the Biology department and not directly in the Kauzmann laboratory, but he and Walter collaborated on studies of viscosity and centrifugation of tobacco mosaic virus [2].

Bill Busing worked on the kinetics of electrode reactions. He later did very early work in neutron diffraction and wrote an early computer program for the refinement of X-ray crystal structures which was widely used [3].

Charles Tanford, who was in the Princeton Chemistry Department, did not work directly with WK, but acknowledges with enthusiasm that it was his contact with Walter that determined the course of his own research in the protein field.

Dick Simpson, finished his work on the use of optical rotation to study the kinetics of protein denaturation in 1948. His very extensive studies provided the foundation for the work which followed [4–8].

Walter Kauzmann published a paper on the glassy state in 1948 [9]. In the biological fields he may be known for his denaturation studies and the hydrophobic interaction, but in the larger world of science he is best known for his work on glasses. The Kauzmann temperature, the Kauzmann curve and the Kauzmann paradox are still lively topics of discussion in condensed matter physics.

Tom Watson was using viscometry to follow the denaturation of ovalbumin and serum albumin [7,8]. In Walter's 1959 review considerable emphasis is placed on the need to study both 'long and short length properties' of proteins. The initial work of Watson and Simpson indicates his awareness of this aspect of the problem from the beginning.

Larry M. Slifkin was involved in a study of creep in single crystals of zinc [10].

Frank Clough was working on additivity relations in optical activity [11].

Dick Attree was calculating the energy of hydration of cations in aqueous solution [12].

I was studying the orientational correlation of water molecules in ice and water via the Kirkwood theory of the dielectric constant and contributed in a small way to the protein project [13]. Walter suggested that an experimental problem would have been better. He was probably right, but there is something to be said for a theoretical thesis. It permits a student to spend his entire graduate career studying intensely, if he is so-inclined. He later told me to learn biophysics or biochemistry as a postdoc and this I did. He also gave me the best advice I have ever received, when he suggested going to Linderstrøm-Lang's laboratory in 1953.

At the time of my departure Karl Frensdorff had taken over the protein denaturation project which was published in a monumental series of five papers in 1953 [4].

Tanenbaum studied metallic creep, taking up the earlier work of Slifkin [14].

Blaine Levedahl, a post-doc, joined the group in the latter part of my stay to work on the protein project. I believe he was the first biochemist to participate and it was his advice that led me to the University of Utah to learn some biochemistry.

Somewhere in the midst of these contributions WK wrote a paper on the flow of ice in glaciers, inspired by his hiking in the Alps during his leave in Europe. His work on glasses, creep, and glaciers indicated a strong interest in the flow of solids. At the time of his retirement his major interest was in geology and in tectonic motion.

It is bemusing to consider that while each of us was carrying out our specific projects, Kauzmann's mind was occupied with all of the varied topics outlined above.

5. Only the beginning

The period we have covered is only a small part of Kauzmann's career. He had another 30 years of activity before his retirement in 1982. Later he became involved in administration and took over the leadership of the Chemistry Department for many years. He also became increasingly interested in teaching at all levels and wrote outstanding books for students. His graduate text, 'Quantum Chemistry', presents to students both the classical and quantum versions of atomic and molecular theory [1]. I have used it as a text a number of times and have recommended it to all of my students as a way to gain real intuition for the abstract relations of quantum mechanics. His two books for advanced undergraduates have played the same role in the fields of kinetics, thermodynamics and statistical mechanics [15,16]. He never wrote a book for his elementary chemistry students, but he had prepared an extensive set of handouts for his classes, which expressed his original approach to this phase of learning.

I cannot compare my period in the Kauzmann laboratory with other periods, but I do know it was an outstanding one. It supplied the background for the major series of experimental papers [4–8] and for the two reviews which had an extraordinary effect on future work in understanding protein structure and dynamics, as well as the subfields of denaturation and protein folding [17,18]. We all felt that we were a part of a group that was advancing science in a significant way. That is a wonderful feeling for a graduate student.

At Kauzmann's retirement celebration in 1982 a series of talks by Howard Schachman, Charles Tanford, David Eisenberg, myself and others summarized our experiences in his laboratory. Walter then stated humorously that all of us had gotten it all wrong. He will probably have the same opinion of the above.

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