JESSE GREENSTEIN ON OTTO STRUVE

Cassette Tape Monologue
[July 1988]

This belt is dictated by Jesse Greenstein of Pasadena after several readings of the biographical memoir about Otto Struve. I hope that some of what I say might still be useful, since the biographical memoir is interesting, but in my opinion slightly underestimates how much Otto Struve did to bring about the present situation in astrophysics. (Kevin, you may use as much or as little of this as you wish, with or without attribution.) Let me first say that there are a few facts about Struve's contribution to the organization of astronomy that might have been worth including. This you mentioned on page 14. In my opinion one of the great inventions that profoundly affected American astrophysics was that Struve, possibly because of his escape from Europe, was more willing than any other person to bring foreign scientists to the Yerkes Observatory. The West Coast Observatories in general had little opportunity for European visitors and rather small respect for theoretical considerations. Harvard had summer schools at which some Europeans were invited. But Struve somehow found it possible to bring to Yerkes for short or long visits, an enormous number of foreigners. In fact there was considerable criticism of doing so. Shapley was also internationally minded but not sufficiently interested in the workings of atoms and nuclei and stars to take advantage of the possibility of bringing the best brains of Europe to the United States. Struve did this continuously, and many of the visitors stayed for a long time. You quite properly mention Pol Swings, Gerard Kuiper, S. Chandrasekhar and Bengt Strömgren. But if one merely looks at the group photographs at Yerkes it becomes apparent that there was a steady stream. As soon as the World War II ended, for example, Minnaert, van de Hulst came to Yerkes. Just before the war began Unsöld and

Wurm were at Yerkes and McDonald, and similarly Gerhard Herzberg, a refugee who had a position in Canada, was brought and a spectroscopic lab to study molecular absorptions that was set up at Yerkes. We are now used to the brain drain, but it was Struve who found it possible to put the best minds of European astrophysics into an observing observatory set up. This was done nowhere else.

Another topic you mentioned on page 20 is the beginning of radio astronomy. You note that he took the position as the first director of the National Radio Astronomy Observatory, which as you say was a mistake. Struve was not as good at engineering as many of the great observers of the West Coast Observatories. He had done only modestly well in supervising the construction at McDonald and the same can be said of Greenbank. On the other hand, intellectually, he was one of the first, except for myself and Fred Whipple, who saw the enormous importance of radio astronomy as an extension of optical astronomy. In the Caltech archives containing my papers several historians have found interesting correspondence between myself at Yerkes or McDonald and Struve on a visit to Europe during the time when I was trying very hard with Struve and the University of Chicago Administration to interest the Office of Naval Research in supporting the move of Grote Reber and his equipment to the McDonald Observatory location. I believe that I may have pushed at first but it was Struve who got ONR interested and I who did some of the negotiations. The project foundered on the minor problems of carpentry costs and relocation allowances, but we were already thinking of what of those days was fairly big science in the form of construction expenditures for radio observations in a quiet location remote from man made interference which had plagued Reber in his work near Chicago.

This brings me then to one of the central characteristics of much of Struve's complex interpretations of very complicated stellar systems. He did know some physics, perhaps not as much as if he had been born 20 years later. On the other hand he had found that it was possible to understand the conditions in stellar atmospheres from spectroscopic observation. He had also found in numerous different ways that the simple expectations of atomic spectroscopy were not always encountered in the stars. Your memoir mentions the discovery of the Stark effect. In my opinion this was only one example of how he realized that the peculiarities of the stars were not merely quirks of their own evolution or perhaps roughly you might say their biography, but rather that there were really interesting ways of studying what was going on. In an early letter I think you mentioned that you had not found many traces of Struve's legacy in current work. This is true in form of lack of references to that work. On the other hand it is very definitely not true when one considers what he worked on and what are now current problems. The odd swirls of gas surrounding close interacting binaries were all strange and mysterious when he studied their details. He soon realized that the massive B stars and their companions had strong interactions, but gases streamed from one source to another, and this is essentially the situation that is faced in very many astronomical applications where accretion discs are formed. I don't believe that a regular disc was in his mind in much of that work. Further, the stars he worked on had essentially the same surface gravity as the Sun in general so that the velocity of infall was only moderate, i.e. 100 kilometers a second or thereabouts, so that the gravitational energy release was not an important source of heating. It did produce the emission lines however often seen in

interacting binaries. Nevertheless, the fact is that mass transfer in interacting binaries is behind almost all the work now going on in such objects as novae, recurrent novae, and by generalization to more massive objects to the infall of material into white dwarfs, into black holes, etc. Of course relativistic infall doesn't follow logically from the work on ordinary stars, but it is merely the increase of the gravitational potential when one goes to neutron stars and black holes that results in the profuse emission of X- and gamma rays. In a sense it is somewhat glib to merely say that you could recognize any star by the peculiarities of its spectrum, implying that these are merely peculiarities. For Struve I am sure and for everyone who began as pioneers in this field in the 1950's, the infall process took on more and more meaning. It was probably Struve's classical education that resulted in his particular interest in binaries, and the masses obtained from ordinary binaries are basic observational data. But because so many stars were "peculiar" it gave information that was not merely gravitational but also entered the region of atomic physics.

Another whole field that this touches on therefore is the nature of the spectroscopic peculiarities of matter in rapid motion at relatively low densities travelling from one star to another. Furthermore, in cases where mass is lost to an individual star alone or in a binary expanding shells occur which are recognizable by spectroscopic peculiarities due to say the dilution of radiation. Note that Struve and Wurm were among the first to compute the effect of dilution of the radiation field on the spectroscopic evidence. The idea of non-local thermodynamic equilibrium, non-LTE, now dominates solar envelope astrophysics, theory and observation, and became a useful tool for the recognition of stellar winds before observations from space and the

ultraviolet made them directly observable. Similarly, the phenomenon he calls "large scale turbulence" which affects the curves of growth of disturbed stars is connected with the heating of the outermost envelope or the wind itself, so that the characteristic temperature inversion, with the chromosphere hotter than the reversing layer and the corona hotter than either seems to be a general characteristic of dilute gases in rapid motion. Another such example you mentioned on pages 17-18 are the beta Canis Majoris objects with multi-periodic oscillations so that their rotation couples with other spectroscopic phenomena to give different periodicities to different observables, for example velocities, line intensities, or light variations.

Side 2

As you know now the Sun shows thousands of periodicities and the innermost structure of its interior may be probed by model fitting to the observations of the various periodicities. The same is true in my favorite objects, the white dwarfs, where non-radial high mode oscillations appear in the light variability.

Let me add a few words on the personality rather than the scientific side. As is obvious to you Chandrasekhar and Struve thought in different ways, and since only Chandra survives, it is his story that you mostly tell. It is about your concluding remarks, pages 21 onward. I'm glad you've tried to emphasize that there was some warmth in him. I worked with him for 11 years when I was quite young beginning when I was only 28. The incredible devotion to science that you emphasize as a real characteristic was an important lesson for me as well as for everyone at Yerkes and in the wider astronomical world. When he could relax and merely talk about science he was charming and human. He had a great fund of personal anecdotes about famous

men which as a young person I enjoyed enormously. He had so much knowledge of the astronomical literature that many of the ideas which I eventually worked on for a long time arose in late night conversations about what were hot topics in astronomy. I came to Yerkes interested in interstellar matter and I left interested in stellar spectroscopy. He was extremely generous with his storehouse of information so that my education was continuous. He remained fairly formal with me till World War II after which in a touching moment he asked me to call him Otto. Much later when his health was only poor and he visited Pasadena before and after observing at Mt. Wilson, we became very close warm personal friends and discussed everything from the organization of astronomy in the United States to the details of the spectra of peculiar stars. Since he was a great man he lacked reverence for other great men and from him I learned about the fallibility of greatness. On one theme alone he was completely inflexible, that a scientist should think first of science and only third or fourth of himself. His conscience drove him to do things like writing the numerous articles for Sky and Telescope since he felt that both other scientists and amateur astronomers and the public should learn about the latest discoveries of astronomy in the country and the world. He had a personal devotion to the cause of internationalism in astronomy, as for example in the stories you tell about the International Astronomical Union meetings and the politics of the union, but he was also one of the first to bring Germans after World War II to the United States when this was not a popular thing to do. He was sympathetic not only to such people as Minnaert (old), van de Hulst (young) from The Netherlands but also to bringing such a person as von Weizsäcker who had become interested in aerodynamic phenomena in astronomy to University of Chicago when that place was buzzing with antipathy

to bringing Germans in. One can say it is consciously repaying the benefits yet obtained in his personal life for being brought from being a refugee to Yerkes but I think it was very much more. It was a genuine feeling of the international character of science and of life.

Let me thank you for listening to this somewhat wide ranging set of undocumented facts. As far as I know what I say is true, but I am not a historian of science, merely a person who has been fortunate to live through some of the best years that one can hope for. I believe that if you look at the book called Spectroscopic Astrophysics, edited by Herbig, University of California, 1970, you can see why I feel that he was a very broad gauged working scientist and that his mind was quick to adapt to new possible techniques of studying atoms in stars and therefore stars and how they behaved. His papers are almost always pioneering, and the breadth of his knowledge permitted him to place the new phenomena studied in the broader context of what was known. You might say I liked the ending of your article, page 23.

PREFACE

GREENSTEIN,

This manuscript is based on a tape-recorded interview conducted for the Center for History of Physics of the American Institute of Physics, the tape and the manuscript being the property of the Institute. I have read the transcript and made only minor corrections and emendations. The reader is therefore asked to bear in mind that this is a transcript of the spoken word rather than a literary product.

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