Hans Bethe, My Teacher[a]

Roman Jackiw[b]

I sketch my experiences with Hans Bethe (1906-2005) as a teacher at Cornell University, beginning with my doctoral studies in 1961 and continuing with my work with him on an intermediate textbook on quantum mechanics.

Key Words: Hans A. Bethe; Cornell University; physics teaching; nuclear physics; quantum mechanics.

In recent times we have seen many appreciations and testimonials about Hans Bethe's outstanding achievements: extraordinary breadth of physics research, significant involvement with social, political, and policy issues, important activity in the industrial and technological arenas. I am sure Hans would be pleased with these encomiums. But perhaps he would wonder why there is little mention of his qualities as pedagogue. After all, Professor Bethe was fundamentally a teacher, imparting his knowledge not only to physics colleagues, not only to people in society, government, and industry, but most importantly also to students, many of whom became distinguished physicists. Even though he received the Oersted Medal of the American Association of Physics Teachers in 1983 for his teaching, I think that this aspect of his

activity is underrepresented in today's appreciations. So I decided to tell you about my experiences of Hans (figure 1) as teacher.

In fact, as I shall explain, Hans was not my research supervisor; nevertheless, I benefited from and enjoyed a pedagogical relation with him.

I came to Cornell in 1961 to study theoretical particle physics. When it was time to attach oneself to a professor, I did not find Hans — perhaps he was away — and I began work with the particle theorist Steve Frautschi. However, Frautschi soon announced that he was leaving for the west coast, so I had to get a new advisor. This time I did find Hans, but he explained that he was at a professional crossroads. Just at that time, fundamental physics was changing. Nuclear physics, which had dominated fundamental physics, was evolving into particle physics, in a manner similar to the evolution of atomic physics into nuclear physics a generation earlier. Hans told me that he needed to decide whether to follow the new direction, or to remain within nuclear physics. Of course, he had already accomplished foundational work for the new field with his Lamb-shift calculation and with the Bethe-Salpeter equation. But he also felt, as he put it, that within nuclear physics he could use his wisdom and experience to achieve more useful results. He decided he would research a problem in particle physics and then make up his mind. He offered to take me on, with the caution that I may have to search again for an advisor if he chose to remain with nuclear physics.

I accepted without hesitation. Why did I want to work with Hans, even in this uncertain arrangement? One reason, of course, was my desire to be attached to a powerful and successful physicist, and not only just to him, but also to the entire family of his professional forebears. And that is some family! Recently, with the help of the internet I reconstructed Hans's academic genealogy, as shown in figure 2. Note that the lineage comprises only German mathematicians,

except at the last step occupied by the mathematical physicist Arnold Sommerfeld, Hans's teacher. This vividly records one strand in the historical development of theoretical physics.

The problem that Hans selected concerned the behavior of physical amplitudes at complex angular momentum. The idea that something physically useful is hiding in the mathematical formulas at unphysical angular momentum was due to Tullio Regge, and it captured the attention of an overwhelming number of theorists, in a way similar to today's preoccupation with string theory. Hans's idea was to pursue this speculation in the context of equations for nuclear physics, and thereby assess its validity on the basis of his great familiarity with nuclear physics. Specifically, we would study the Schrödinger equation,

$$-\frac{1}{2}\frac{d^2}{dr^2}\Psi_\ell(r)+\frac{\ell(\ell+1)}{2r^2}\Psi_\ell(r)+g\frac{e^{-r/r_0}}{r}\Psi_\ell(r)=E\Psi_\ell(r),$$

where the Yukawa potential $g\dfrac{e^{-r/r_0}}{r}$ governs nuclear forces.

Physical processes occur at real ℓ — real angular momentum. The project was to determine the behavior of the solution — of $\Psi_\ell(r)$ — at complex values of ℓ.

As a starter, Hans told me to solve the equation for real ℓ, and then we would generalize to complex ℓ. He said that precise analytic solution was impossible; I should solve the equation numerically, with the help of a computer. And I went to work. My 1960s' physics education was not very advanced. I had little experience with Schrödinger's equation and none with numerical computer calculation. I learned both, and produced numerical tables for $\Psi_\ell(r)$. But to my delight, in the course of various manipulations, I realized that I could solve the equation exactly and analytically. This I did, and verified agreement with the numerical solution.

Now it came time to report my results to Hans. I prepared in my mind the order of presentation. I would first give him the numerics that he requested, and then casually observe

that analytic solution was also possible and that I had found it. But as Hans looked over the numbers, he voiced dissatisfaction with them – something is wrong, he said. I wasn't worried, because I also had the exact formulas, which validated the numerics. But while he was frowning over the numbers, I was reviewing in my mind my calculations, and to my dismay I realized that I performed a great blunder. Right at the beginning of my work I wrote the Schrödinger equation incorrectly: I omitted the wave function $\Psi_\ell(r)$ multiplying the Yukawa potential $g\dfrac{e^{-r/r_0}}{r}$! The equation that I solved was

$$-\frac{1}{2}\frac{d^2}{dr^2}\,\Psi_\ell(r)+\frac{\ell(\ell+1)}{2r^2}\,\Psi_\ell(r)+g\frac{e^{-r/r_0}}{r}=E\,\Psi_\ell(r).$$

I was mortified and told Hans of my blunder. He was kind. I asked whether he thought I should be a theorist. He reassured me that I should keep trying, but he also informed me that he didn't make mistakes of that kind. He left me in awe that he could spot an error from a table of numbers.

This story ends here, because Hans decided to remain in nuclear physics. I had to find a new advisor, and this was Frautschi's replacement, Ken Wilson, with whom I enjoyed an excellent course of study and research.

Evidently Hans forgave my blunder because he asked me to assist him in writing a textbook on quantum mechanics based on his lectures. I was thrilled to accept. Even though he would not direct my doctoral studies, I could continue my association with him.

His quantum-mechanics course lasted one year and dealt with the quantum-mechanical analysis of atomic and molecular systems. I attended the lectures, wrote a draft of the material, which was then typed by a secretary. Finally Hans and I would edit the text into its

final form. It was a pleasure to follow Hans's lectures. Obviously he liked the material very much — some of which was created by him. His presentation was calm and lucid. Frequently he smiled when coming to a particularly fine point, or answering a student's question. Hans's approach to the material was straightforward and conventional, but he also touched subtle points that I have not seen discussed elsewhere: relevance of locality to the statistics of particles, an argument against hidden variables. He preferred reasoning that was elementary and directly physical, over general mathematical proofs. Years later Julian Schwinger told me that Bethe always got ahead of him. He was referring to effective-range theory, which Schwinger derived from an elegant variational principle, but all texts present Hans's derivation based on the Schrödinger equation. Or he could have been speaking about the Bethe-Salpeter equation, which Schwinger also obtained by a method more involved than Ed Salpeter's and Hans's Feynman-diagram approach. The course and Hans's lecture style infused students with the confident feeling that intricate calculations were doable, provided one proceeded calmly and systematically.

I asked Hans what was his favorite topic in the course. He chose his theory of charged-particle energy loss, which he constructed with the help of the Bethe sum rule — an elegant application of quantum mechanics together with ingenious approximations.

My write-ups were not typed by secretary Velma Ray — known to all members of the laboratory — but by Hans's secure secretary, Mrs. Stout. She dealt with Hans's secret work. Her office was in an obscure corner of the Cornell campus, and it was extraterritorial — that is, it was U.S. government property. So I felt the thrill of being connected — distantly to be sure — with important matters of state. Hans's occasional absences for consulting work also thrilled us students. It was rumored that he was whisked by noncommercial aircraft from

Ithaca's sleepy airport. One time, after his return from Boston, where he consulted for AVCO Everett, I cheekily asked him what he had accomplished. He smiled and said that he had just saved the government a vast amount of money by developing the heat shield for rocket reentry.

In summary, working with Hans on the quantum-mechanics book allowed me to master that material superbly, and at the same time to enjoy contact with a remarkable person.

The book, *Intermediate Quantum Mechanics*,[1] was published. It went through three editions and numerous printings, most recently as an "Advanced Book Classic."

When our project ended, I wanted to give Hans a gift. I remembered that he was very fond of a German book with tables of special functions, put out by Jahnke and Emde.[2] But in the 1960s there appeared an American successor by Abramowitz and Stegun.[3] It contained tables with many more significant figures and for many more functions. So I presented a copy to Hans. He seemed to like it. He opened it to the Bessel function listings with nine significant figures. He ran his finger down the last column, looked at me smilingly, and said that there appear to be rounding errors. I do not know whether this was a humorous reprise of my Schrödinger equation blunder, or whether he actually recognized an error in the numbers.

The last time I saw Hans was in 1999 when he gave a colloquium at MIT. Usually colloquia by scientists of Hans's seniority are not technical, mostly historical. Not Hans Bethe! He gave a thoroughly technical description of his current research on supernovae. The presentation was calm and systematic. In the discussion period people asked complicated, technical questions. Hans smiled and calmly answered in an equally complicated, technical manner, citing precise numbers. It was not much different from his lecture course 35 years earlier, except that I did not take notes.

References

[1] Hans A. Bethe, *Intermediate Quantum Mechanics*. Notes by R.W. Jackiw (New York and Amsterdam: W.A. Benjamin, 1964); Hans A. Bethe and Roman W. Jackiw, *Intermediate Quantum Mechanics*. Second Edition (Reading, MA: Benjamin/Cummings, 1968); *idem*, Third Edition (Menlo Park, CA: Benjamin/Cummings, 1986).

[2] Eugen Jahnke and Fritz Emde, *Funktionentafeln mit Formeln und Kurven* (Leipzig and Berlin: B.G. Teubner, 1909); translated as *Tables of Functions* (New York: Dover, 1945).

[3] Milton Abramowitz and Irene A. Stegun, ed., *Handbook of Mathematical Functions With Formulas, Graphs, and Mathematical Tables* (Washington, DC: National Bureau of Standards, 1964).

Department of Physics

Massachusetts Institute of Technology

Cambridge, MA 02139 USA

e-mail: jackiw@mit.edu