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An Interview with David Hestenes: His life and achievements*

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The following interview was conducted with Professor David Hestenes in Gazimağusa, North Cyprus on March 23, 2009 during his visit for attending the Frontiers in Science Education Research Conference. He has served the physics education community since late 70s. He is most notably known by the project he led named the Modeling Instruction. Also, the Force Concept Inventory (FCI for short) is a very well-known tool for diagnosing student misconceptions in introductory mechanics. He alone and within his research groups, throughout the many decades, drew attention to the ways of conducting rigorous physics education research and contributing to the improvement of physics teaching and learning. Thanks to his leadership combined with the finest of the scholarship in the field, we now know much more than we did in the past. At the end of this paper, a list of Professor Hestenes' related publications is given for the readers' convenience.

Keywords: David Orlin Hestenes, physicist, physics education researcher, FCI, Modeling Instruction

T: Professor Hestenes, Thank you for giving this interview for the EURASIA Journal of Mathematics, Science and Technology Education and I'm sure many of our readers would be delighted to read about your ideas and hear about your stories from the past and I want to begin with your childhood, in what kind of an atmosphere did you have your childhood?

H: Well, I would say I had quite a fortunate atmosphere because my father was a mathematician, and a very good mathematician at that. He got his PhD from the University of Chicago, and I was born in Chicago shortly thereafter. He went to Harvard on what would be called a postdoc nowadays, but in those days he had one of a handful of fellowships in the whole United States. He worked with G.D. Birkhoff at Harvard and then he came back to a faculty position at Chicago where I grew up, except for periods during World War II when he went to Columbia University to help with military R&D. He was assigned the job of

organizing all the mathematicians in the United States for the war effort. That would be about 1941-42-43, and then he did this at Columbia University so he moved from Chicago and he stayed there (in New York City) for a while.

T: What year you were born?

H: I was born May 21, 1933.

T: Yes, mine is May 11.

H: (Laughs).

T: Also Feynman and Salvador Dali May 11, on my birthday, yes.

H: To continue, my father didn't try to teach me mathematics directly, but he created an atmosphere. I often saw my father sitting down and working with a piece of paper. In fact, he had amazing powers of concentration. My brother and I could run around making all sort of noise, and he would just sit there working away. Later on I asked him how he got such powers of concentration, because I myself am easily distracted by noise or anything. And he said it is because when he went to college he studied at a desk in the hallway of the dormitory, so he had to learn how to concentrate in the midst of all the noise of the other dorm students.

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Figure 1. David Orlin Hestenes

T: So, can you say he just sort of set a role model for you?

H: I can say he set for me the strongest role model among all people I had ever known. I admired him most as a very kind and wise man with high standards. He was always feeding me wise proverbs as I grew up. For example when I screwed up, he would just shake his head. Rather than punish me, he would say something like “David you must always check! Usually you screw up because you neglected to do something.” That was one of his favorites.

T: Apart from your father, what other things inspired you...?

H: ...well.

T: In terms of whatever you wanted to become?

H: Actually, I think I was somewhat unusual in being eager for a consistent worldview even when I was very young. Although my father had grown up in a religious atmosphere, he was non-practicing. However for some reason I felt the need to go to church and Sunday school all by myself without my parents sending me there. So Christianity became my first worldview. That lasted through high school. I remember my history teacher asking why is it that I'm the only one in my class who knows what he is going to be when he grows up, namely, a missionary doctor. Why a missionary doctor? Well, because there were ministers and missionaries in my heritage. And I was good at science. So I thought I should be a doctor, because you should use the talents God has given you. When I got to college I was a pre-med major to become a missionary doctor. But when I got to zoology classes where I had to memorize so much stuff, and everything stunk of formaldehyde, I decided to omit the doctor part. I changed my major almost every semester while I was in college. I felt very comfortable in college, given the academic atmosphere of my family and my position as the oldest child. So I looked upon college as a place to explore and find out new things. I was always taking classes in subjects outside my major, so I ended up with something close

to a year's worth of extra credits while I finished in the usual 4 years. To be a pre-seminary student, I became a speech major. I liked that, and it was forensics competition in speech and debate, which was extra-curricular, where I developed the most important skills I learned in college. That was how to present an argument in front of skeptical opponents who were looking to pick your argument apart, and so how do you defend your point of view? I have actually used that in the design of instruction for students, because I think it is important to promote scientific argumentation. How do you make — how do you formulate a claim and defend it with argument and evidence? That's one of the foremost skills I aim to teach.

T: Argumentation is becoming more popular topic in terms of science education research these days.

H: It's not popular enough actually (laughs). They should think more about it. But there has been some good work that hasn't been as recognized as it should be. They could learn a lot from what goes on in debate programs in forensics. So I was a speech major, but then I started taking some courses in philosophy. In the summer I returned to UCLA where my father was chairman of the math department and I took some philosophy courses. When I went back to Pacific Lutheran University in my senior year, I petitioned to be their first student to major in philosophy. They had only one philosophy professor, but he sponsored me in enough independent study for a degree. So I ended up graduating with degrees in both speech and philosophy. It was only in the last summer of my senior year that I started reading philosophy of science, in particular, a book by Hans Reichenbach. I noticed that the philosophers would argue that Heisenberg says this and Schrödinger says that, and Bohr says such and such! I reasoned, “This can't be real philosophy. They are just quoting physicists. This must be the Revealed Word! The physicists must be the real philosophers!” So I decided to change my major to physics, just as I graduated from college. And I took my first semester of freshman of physics in the last semester of my senior year. Well, in that last semester I got married, I had an overload of 19 units, I was on the golf team, and I did debate as extracurricular activities. Most difficult of all for someone with the habit of staying up late, physics was an early morning class. Besides, I took physics and introductory calculus at the same time and with the same professor because the school was small. He barely gave me a passing grade in both. He had heard about my plan to become a physicist and said as he handed out my poor grades at the end of the semester. “You'll never be a physicist.” However, that did not bother me because I knew that my father was a very good mathematician, and I did not think he was so smart!

T: And so how did he become a good mathematician you think?

H: Well, that's a very interesting story. You want to hear the story about how my father became a mathematician?

T: Shortly please.

H: Yes, well some years later when I was in graduate school, I came across an article in the intellectual journal *Daedalus*, a wonderful journal of science and the arts. It's been going on for a long time. It would be around 1960 when I read this article. The article was a profile of the typical professor in a United States University at that time. When I read it, I was amazed to see that it was almost a perfect description of my father: Scandinavian descent and educated in a Midwestern university. His parents were Norwegian living in Minnesota. And that was a time of migration by young people from farms to cities. They had wonderful universities and schools in the region: the University of Minnesota, the University of Wisconsin and St. Olaf College, which my father attended first. It is a liberal arts college with perhaps the best math program of any small college in the United States. Anyway, students like him who came from the farm had two obvious career options: either go back to the farm after graduation or continue on at the university. My father had gone to college with the intention of being the best hog farmer in the state of Minnesota, but he kept getting straight A's in mathematics. And they offered him a series of scholarships and fellowships that ultimately brought him to graduate school at the University of Chicago, one of the leading universities for mathematics in United States at the time. So that's how a farmer's boy became a mathematician.

Then, when I was growing up, I guess my father thought that, since he found his path to mathematics by himself, I could figure out what I want to do by myself, because he never pressed me to learn any mathematics or physics. I only remember one hint when he gave me a book on electronics and said, "You should take physics" in high school. So I took physics and I couldn't understand why he thought it was something I would like. The author of the physics textbook was aptly named Charles E. Dull. That was one the most widely used textbooks in the United States for 20-30 years, but it didn't inspire me in the least. So, I just brushed off physics and didn't think about it until my senior year in college when I started reading philosophy of science. I was actively looking for a coherent world view then, because religion didn't satisfy my intellectual needs. So I went into physics for that reason, I wanted to do real philosophy. That is how I got started late in physics. I was age 20 at the time – I graduated at 20. I was already married then, my wife was pregnant and the Korean War was going on, so I volunteered to be drafted into the army and ended up getting the G.I. Bill to support

me in graduate school when my army service was completed. The G.I. Bill was a wonderful Program.

So, I did not start studying physics seriously until age 23 when I got out of the army. I was accepted to graduate school at UCLA because I had good grades, but it was only a provisional acceptance because I only had one semester of physics. I was assigned an advisor who was an experimentalist, and he was appalled by my lack of physics background. He told me, "Well, you can make up your deficiencies in 4 years." I was shocked! Fortunately my father was chairman of the math department at UCLA. I complained to him: "That's ridiculous I shouldn't have to take 4 years to make up my deficiencies in undergraduate physics!". He said, "You go talk to Dave Saxon." Now, David Saxon, it turns out, was an associate professor at time, but he was a very clever man, and ultimately became president of the entire California University system. One of his talents was that he thought outside the box. So, he went through my program of study in detail with me. At one point he said, "Now, these two courses, if you take this course first, you don't have to take the other one because that is prerequisite for this course." And he said –he was a theoretical physicist– he said, "Well, you are already in the graduate program so you don't have to satisfy undergraduate requirements for labs, so you don't have to take any physics labs." So, I didn't. With his advice I was able to rush through the entire undergraduate physics curriculum in one year. And the next year after that, I completed a master's degree in physics. A total of two years for that, starting from practically zero! One thing I learned from that experience was that universities are too strict in requiring people to fulfill all the background requirements in their major field. You can always fill in missing pieces later on when you need them. There were, of course, big holes in my background in physics. And one of the holes came up when I had my PhD. oral exam. But, that's a separate story.

When I started doing my graduate research, because of my background in philosophy I was very much interested in the mathematical languages for physics and their history. I had read Bertrand Russell on the foundations of mathematics. Since I was too naïve to think otherwise, I was convinced by his proposal that mathematics is derivative from logic. So I tried to convince my father to collaborate on writing a calculus textbook with explicit foundations in logic. My father was so wise that he didn't try to convince me otherwise. He just let me talk about it, think about it and discover, of course, that I didn't know to how do it. And it wasn't very long before I decided that's the wrong way to approach mathematics anyway.

After I had finished all the basic academic requirements and was ready to start my doctoral dissertation, I took the year off from the physics

department, and my father got me an office in math department where I concentrated on studying mathematical languages. In particular, I studied quantum electrodynamics and learned about the work of Richard Feynman. And I took one of the very first courses in the United States on differential forms in differential geometry –taught by Professor Barret O’Neil. One day in the mathematics– engineering library I looked at a shelf of incoming new books and pulled down and some lecture notes entitled “Clifford Numbers and Spinors” by Marcel Riesz. It was about Clifford algebra as a mathematical system. I read, I think, for about 15 minutes and all of a sudden I had an epiphany. I exclaimed “Gee, differential forms and the Dirac algebra have a common algebraic structure!”... do you know the Dirac algebra?

T: I do.

H: Yeah well, you know Feynman’s trace calculus?

T: Not that much.

H: Well, I was impressed by the fact that with trace Calculus all the computations in quantum electrodynamics can be done without picking out a matrix representation. One thing I concluded from studying “Marcel Riesz” was that, in fact, matrix representations are irrelevant even in the Dirac algebra, and so in quantum mechanics generally. I saw that space-time geometry was encoded in the algebraic structure of the Dirac algebra, and I recognized that the algebraic structure of differential forms was included as well. That got me started on the long path of developing geometric algebra into a powerful unified mathematical language for physics. That development has been my main work as a physicist. I’ve written many books and published many papers on the subject, but I got started right then at graduate school. I also had Feynman for quantum electrodynamics during that period. I remember him saying that every idea that he ever had in physics, had its beginning when he was a graduate student.

T: So you took a course from him?

H: I took a course from him, but it wasn’t UCLA, it was at Hughes Research Institute in Malibu California. I went there with a group of graduate students from UCLA to hear him lecture for two hour once a week for two years. He lectured on quantum electrodynamics for a year and then on solid state physics. I ended up doing my thesis on geometric algebra in physics, even though my doctoral advisor didn’t really appreciate what I was doing. He wanted me to work on his own unified field theory, and he would tell me what he wanted me to do. I would go work on that for a while. Then I would return and say “your idea doesn’t work so I’m going back to doing my own thing.” This went on for about two years until I came to his office one day and told him what I had been doing on my own. Then I asked, “Well, do you think I have enough for a thesis.” He said,

“No.” So I went back to my office and I wrote up a 165 page thesis. When I handed to him and asked again, “Do you think I have enough for thesis?” He said, “Yes.”

T: What department he was in?

H: He was in the physics department, though I spent as much time in mathematics and had two prominent mathematicians on my dissertation committee. Well, another interesting thing happened at the time that hardly anyone in the world knows except me, and now you. My father had arranged for John von Neumann to be hired as a distinguished university professor at UCLA. At that time John von Neumann was the nation’s leading mathematician. He is regarded as the father of the modern computer. I was just starting my thesis work then and might well have had von Neumann as my mentor. Tragically, though, von Neumann contracted an aggressive cancer and died within a year before getting to UCLA. That is why his appointment at UCLA remains unknown.

One reason that my father was able to attract von Neumann, I suppose, was because he was a leader in connecting mathematics with computing. Besides chairing the math department, my father directed the Institute for Numerical Analysis, which housed the first electronic computer in western United States. And that’s where studied when I first started graduate school. That was good for me too, not because I did much computing, but because I was at the ground floor of computer science and artificial intelligence and got acquainted with the first generation of computer scientists.

T: And about what year?

H: That was... I started graduate school in 1956, so I spent 1956 to 1958 at the Institute while I was taking graduate courses and exams. After completing my exams, I spent that year in the math department where I got the idea for Geometric Algebra. Then I went back to the physics department and finished up my thesis, including in my thesis some things I thought my professor would like but I didn’t like. It turned out that 3 months after I finished my thesis I came across the idea that really made the whole approach work beautifully. That consolidated my ideas. To explain since you know Dirac algebra. you know that the whole algebra is generated by the Dirac matrices, so you can understand the significance when I reinterpreted the Dirac gammas them as vectors. These vectors then generate an associative algebra, mathematically speaking, a Clifford algebra. But I developed this algebra as an encoding of geometric properties for space-time in algebraic form. I call that system *space-time algebra* (STA). From that viewpoint, the Pauli algebra sheds its representation by 2×2 matrices to emerge as a sub-algebra of the STA. That was my second significant discovery about the Pauli algebra.

The first discovery is one of the highlights of my life. And it gave me strong motivation and direction for my research. That discovery was recognition that the Pauli matrices could be reinterpreted as vectors, and their products had a geometric interpretation. I was so excited that I went and gave a little lecture about it to my father. Among other things, I said, “Look at this identity $\sigma_1 \sigma_2 \sigma_3 = i$, which appears in all the quantum mechanics books that discuss spin. All the great quantum physicists, Pauli, Schroedinger, Heisenberg and even Dirac as well as mathematicians Weyl and von Neumann, failed to recognize its geometric meaning and the fact that it has nothing to do with spin. When you see the Pauli sigmas as vectors, then you can see the identity as expressing the simple geometric fact that three orthogonal vectors determine a unit volume. Thus there is geometric reason for the Pauli algebra, and it has nothing whatsoever to do with a spin. When I completed my little lecture on geometry of the Pauli algebra, my father gave me the greatest compliment of my life, which I remember to this day. He said, “You have learned the difference between a mathematical idea and its representation by symbols. Many mathematicians never learn that!” That really made me proud, because, actually, my father was very sparing with his compliments. So when he praised me, I knew I knew I had really done something good.

After completing my thesis, I was awarded a two-year National Science Foundation postdoctoral fellowship with John Wheeler at Princeton University. When I got there I found that Princeton was a kind of clearing house for postdocs from all over the United States. After the typical two-year tenure as a postdoc, they would be distributed to faculty positions at various colleges and universities across the country. I was invited to consider Arizona State University (ASU), one of several new universities that were created around the 1960's. It had been a teachers college since Arizona became a state, and it was elevated to a university in 1958. The physics department chair at ASU had his doctorate from Princeton, so he knew John Wheeler and asked him to recommend candidates for a new faculty position at ASU. That's how I got invited. Before accepting the position, I talked to my father. He had recently been on a committee of the National Research Council to evaluate new graduate programs that were starting up all over the country, so he had an ideal perspective on new faculty positions. He said that ASU is an excellent choice, because it is a new university that is sure to grow rapidly, given its location in a rapidly growing urban center. That makes it easy to write your own ticket on the academic train. He was exactly right. In fact, I was able to negotiate an agreement with the department chair to teach only graduate courses in my research field for my first several years on the faculty. That was indeed a first class ticket! Moreover, ASU has

been the most rapidly growing university in the United States during the 40 years since I was hired. It is now the largest university in the U.S., with more than 70,000 students. It has continued to move up quite rapidly in academic status. My father sure knew the landscape of the academic world!

T: You got the inside dope.

H: Another reason that I went to ASU and stayed there is because I was married when I was in college. I had my first child while I was in army and my second child was born on my first day in graduate school. By the time I finished my PhD I had four children. Then I went to Princeton. I have never heard of another postdoc with four children.

T: That's quite a success. Every child is like another dissertation. I can tell you.

H: So, I'm not impressed by the problems of having extra children. Though it does restrict your academic mobility, because moving is quite pain if you have a big family. But all of that worked out quite nicely for me. At Arizona State University I was able to write my ticket right away, because from my training in debate I knew how to negotiate. So, before I accepted the faculty position, I said to the department chair, “Well, if I come here I want to teach only graduate courses at first. I want to teach graduate electrodynamics and relativity using geometric algebra.” . . . because I wanted maximal opportunity to develop geometric algebra as a unified mathematical language for physics. The chair agreed to that and more. I also had become very impressed with the Maximum Entropy approach to statistical mechanics by Edwin T. Jaynes. It's the most coherent information theory approach to statistical mechanics and I want to teach it. In fact, I had also applied for a faculty position at Washington University where Jaynes's was located. However, they had such a small department of eminent people only; I didn't get an offer from them. Otherwise, I would probably have gone there, because Jaynes was one of the physicists in the generation ahead of me that I most admire. Thirty years later I was gratified to hear from Jaynes himself that he had lobbied hard to hire me. In retrospect, though, ASU was probably the better choice for me, if only because I got to develop my own graduate course in statistical mechanics, which I certainly couldn't have done in competition with Jaynes. Anyway, for most of my first 8 years at ASU I got to teach graduate courses that helped me develop curriculum materials based on geometric algebra. Development has continued to this day. I've written books and many articles on Geometric Algebra (GA) and its applications, so that, in recent years, it has become recognized as a discipline in its own right. There is now a steady stream of books by other authors on GA and its applications.. For example, computer scientist Leo Dorst and colleagues published “Geometric Algebra for Computer Science.” Chris

Doran and Anthony Lasenby at Cambridge University published “Geometric Algebra for Physicists.” That book arose from more than a decade of GA research at Cambridge that produced many important results, most notably, “Gauge Theory Gravity,” which improves on General Relativity. Now GA is being applied to robotics and there are conferences on GA every year around the world. It is clear now that the whole field will keep growing without my help. My ultimate goal has always been to see GA become a standard, unified language for physics and engineering as well as mathematics. GA is arguably the optimal mathematical language for physics. For example, you can do introductory physics using geometric algebra without using any coordinates. Actually, my Oersted Medal lecture, published in the American Journal of Physics, is an introduction to geometric algebra at an elementary level. So, I’m willing to bet that GA will eventually become the standard language, even in high school. There is a need to integrate high school algebra, geometry, and trigonometry into one coherent system that is also applicable to physics. GA puts it all together in a remarkable way.

T: So is it easy to make sense for . . . ?

H: Well, you see, if you’ve already learned a different language, right? A new language looks hard.

T: Yes.

H: No matter what language! However, if you analyze GA in terms of its structure, it can’t be harder than conventional mathematics, because its assumptions are simpler. The geometric interpretation it gives to algebraic operation is more direct and richer than ordinary vector algebra. It includes all the features of ordinary vector algebra, but it’s not limited to three dimensions. It works in space-time, and so you have a vector algebra for space-time, which, as I have noted already, improves on the Dirac algebra. Indeed, it turns out that I discovered something amazing when I reformulated the Dirac equation in terms of space-time algebra, where Dirac’s gammas –the gamma matrices– are now vectors, okay? The gammas become an orthonormal frame of vectors in space-time. But, what about the imaginary unit i in quantum mechanics? Well, it turns out that you don’t need it.

T: You don’t need it?

H: You don’t need it! You don’t need an extra imaginary unit because the frame of orthonormal vectors suffices when multiplication of vectors is defined by the rules of geometric algebra. Of the four vectors in a frame, one is a timelike vector and three are spacelike vectors, right? If you take the product of two spacelike vectors you get a new quantity called a bivector, which generates rotations in a plane of the two vectors, and its square is minus one. As I proved in 1967 (in the Journal of Mathematical Physics) the generator of phase in the Dirac wave function is just

such a bivector. And what is the physical significance of the plane specified by that bivector? Well, that plane determines the direction of the spin. Thus, spin and complex numbers are intimately, indeed, inseparably related in the Dirac equation. You cannot see that in the ordinary matrix formulation, because the geometry is suppressed. Because matrix algebra is not a geometric algebra; it was developed as a purely formal approach to handle systems of linear equations. In contrast, geometric algebra gives the Dirac equation geometric meaning. So, there is a meaning to the imaginary unit i that appears in the Dirac equation. We have seen that it represents the plane of spin. Eventually, I also proved that this property remains when you do the non-relativistic approximation to the Dirac equation, going to the Pauli equation, and then to the Schrödinger equation. Now, it is usually said that the Schrödinger equation describes a particle without spin. But, the fact is, when you do the approximation correctly this i , which generates rotation in a plane in the Dirac equation, remains precisely as the i in the Schrödinger equation. Thus, the i in the Schrödinger equation is generator of rotations in a plane, and the normal to that plane is a spin direction. In other words, the Schrödinger equation is not describing a particle without spin; it describes a particle in an eigenstate of spin, that is, with a fixed spin direction. Studying the implications of these facts has been a major theme of my research to this day. And more results will be published soon.

T: Great

H: Yeah, so, that keeps me going.

T: And you’re still excited after forty years?

H: Yeah, that’s right. So, if you are interested I tell you a little about what it has all lead to. Have you heard of zitterbewegung?

T: I’m not familiar.

H: That’s a German word meaning “trembling motion.” The term was coined by Schrödinger. He noticed that if you try to make a wave packet with the free particle solutions of the Dirac equation something funny happens. You can’t make a wave packet using only the positive energy solutions. The Dirac equation has troubles because there are both positive and negative energy solutions, and everybody believes that for a free particle the energy has to be positive. And, you need both positive and negative energy solutions to make wave packets, otherwise you don’t have a complete set. When you make a wave packet it has oscillations between positive and negative states that Schrödinger called zitterbewegung. The frequency of these oscillations is twice the de Broglie frequency. Do you know the de Broglie frequency?

T: Hmm!

H: It is mc^2 over h . The zitterbewegung frequency is twice that, okay? Schrödinger suggested

that this oscillation was actually a circulation of charge that generated the magnetic moment of the electron. And Dirac agreed. You can find it in his famous book on Quantum Mechanics, and it still pops up in books on quantum electrodynamics, but serves as nothing more than a colorful metaphor. My own research addresses the question: “Is zitterbewegung really a mere metaphor? Or is it a window to particle substructure in quantum mechanics?” After decades of trying, I have arrived at a particle model of the electron that moves at the speed of light along a helical path in space-time. The diameter of the helix is a Compton wavelength. The zitterbewegung is oscillation across the diameter and that generates the electron’s magnetic moment. I have found well-defined differential equations for the electron’s motion with quite a simple form and interesting new predictions. This model allows you to picture the zitterbewegung as a rotating dipole moment. The time average of this rotation is the well-known magnetic moment of the electron. The frequency of this rotation is the zitterbewegung frequency, which is about 10 to the 21st hertz, okay? That’s too high for anyone to detect, except in a resonance. It turns out that such a resonance experiment has already been done. Do you want me to tell you about it?

T: Please.

H: Okay. So this is science in the making. Going back to de Broglie, he’s credited with proposing wave particle duality. But, that wasn’t where he started. His original idea was that the electron has an internal oscillation, that is, an internal clock. This internal clock is attached to a wave that oscillates with the same frequency. When Schrodinger took de Broglie’s idea and made his equation, he discarded the clock part, and just had wave-particle duality. Most everybody else forgot about the clock as well, except for a few French followers of de Broglie. One French experimentalist who didn’t know about quantum electrodynamics (though he did high energy experiments) decided to look for de Broglie’s clock. He reasoned that if there really is such a clock, it should be observable. Well, since the clock has such a high frequency, it will take a resonance to observe it. So he looked around to find some way to create such a resonance. He found it in the field of electron channeling. Do you know what that is?

T: No, I don’t.

H: Okay, briefly. You can aim a narrow beam of electrons at a crystal so the electrons are moving parallel to a crystal axis and they get trapped in a spiral that spirals along a crystal axis. Then you have a greatly increased transmission through the crystal because scattering off atoms is minimized. Amazingly, you can focus the beams so that something like 60% of the electrons are trapped in spirals around a single crystal axis. Then you can tune the speed of incident electrons so the frequency of passage past atomic centers on the

crystal axis is the de Broglie frequency. This Frenchman reasoned if there really is an electron clock, then it should have a resonant interaction with the crystal in this situation, though he had no idea about a mechanism that could produce it. He simply said, “there should be an interaction there, if there is a clock.” However, nobody would support doing such a crazy experiment. So, he wrote a proposal to do a more conventional experiment on crystal channeling. After he got the grant and assembled his team to do the experiment, he told them the real reason why he wrote this proposal – to do the clock experiment. So they did the job they were paid for, and then they took one day off to do his experiment, and they got a positive result. They found a resonance near the de Broglie frequency, as predicted. Evidently, they had measured the period of de Broglie’s electron clock! The Frenchman took a long time write it up, mainly because he had to sharpen up the beam analysis and things like that, but then he couldn’t get it published in a regular physics journal. Finally, he got it published in an obscure journal called “Annals of the de Broglie Institute,” which hardly anyone reads. A couple of years later, he decided that he was confident that his experimental effect is real, though it may be inconsistent with conventional quantum mechanics, including the Dirac equation. A lot of work has been done on channeling experiments and nobody had even considered anything like the clock experiment. Conventional quantum mechanics doesn’t have any mechanism to predict or explain such a result. So the Frenchman rewrote the paper and submitted it to Physical Review Letters. There were five reviewers, I think, and the typical review amounted to saying, “Well, there’s no theoretical reason to why this should exist so it can’t be right.” Okay? That reminds me of Eddington’s remark: “I won’t believe the experiment until it is confirmed by theory.”

T: (Laughs). So, theory precedes experiment.

H: Well, you don’t want to do anything that contradicts quantum mechanics, do you?

T: No.

H: No, that couldn’t be right! I mean Bohr said quantum mechanics is a complete description of nature. Of course he didn’t use the Dirac equation, he used the Schrödinger equation. Hence, Bohr can’t be right, because the Dirac equation shows that the Schrödinger equation isn’t a complete description. But is the Dirac equation complete? Anyway, the reviewers didn’t believe in de Broglie’s clock so the paper was rejected once more. However, one of the reviewers said, “Maybe this effect you’re talking about can be explained by zitterbewegung.”, “Zitterbewegung?” The French experimentalist had never heard of it, because it is mentioned only in theoretical textbooks. So he googled it on the web and found a paper entitled “The zitterbewegung interpretation of quantum mechanics”

by a guy named David Hestenes. Then he contacted me and told me about his experiment. It happens that I was writing another paper on zitterbewegung, which I now call “Zitter” because my model for it differs somewhat from Schrödinger’s and zitterbewegung is too much of a mouthful to say all the time. I was looking for new experimental tests of my Zitter model, So, I used my Zitter model of the electron to analyze and explain the clock experiment. The analysis was not altogether straightforward, however, because the experimental resonance was found near the de Broglie frequency, which is exactly half the Zitter frequency. It turns out that there’s a mathematical reason for the difference that explains it beautifully! Besides accounting for that factor of 2, my model explains the small difference between the measured frequency and the de Broglie frequency as due to a small splitting into resonant peaks that are not separately resolved in the experiment. My analysis will be published in the journal Foundations of Physics in 2010. In the meantime, we are looking to have the experiment repeated with greater resolution to confirm the result and look for more details. It is an uphill battle, because the physics is outside the main stream, and the competition for accelerator time is heavy. So, we shall see what happens!

T: Good luck.

H: Yeah, yeah. So that’s the scientific research that I’ve been doing most recently. But, geometric algebra also has implications for general relativity, and gravity, and black holes, and things like that. Most of the work in that direction has been done by a group in Cambridge that has adopted geometric algebra completely and used it very beautifully.

T: I never knew about this kind of work that you have done. I know you mostly from your physics education.

H: Yeah, except for my Oersted lecture, okay? So, I gave my Oersted lecture on elementary applications of geometric algebra instead of my educational R&D, for which the award was intended. But I related that to science education by emphasizing that what you understand about science depends critically on your facility with conceptual tools, representational tools, and mathematical tools. For example, you had to do all of your calculations with roman numerals, you wouldn’t do very well, okay? Indeed, the whole history of mathematics can be understood as invention and application of better and better conceptual and mathematical tools. That history is still ongoing, and I submit that the publication record of geometric algebra marks it as the leading candidate to unify mathematics in the 21st century. Invention of an efficient grammar for geometric algebra seems to be pretty much complete, and the next phase of developing better tools for a broad range of applications is well underway. I have published the first advanced book on classical mechanics worked out exclusively with geometric

algebra. All equations are formulated and calculations are done without resorting to coordinates or matrices, including rotational dynamics, precessing tops, and all that. The introductory chapter is a kind of annotated history of geometric algebra. So, I am confident that geometric algebra will eventually become the standard language for physics and probably for engineering. On the other hand, I have done some education research.

T: And how did you get involved in education research? What were the reasons in the first place? Because as I can see you are way too busy with other scientific work.

H: When you have four kids that have trouble learning physics you wonder about what the problem is, and then you see that most students have similar difficulties. Well, I got seriously interested in investigating the problem of learning physics in 1976. I was a full professor at that time and I had finished my decade of teaching all graduate courses exclusively, so I started my turn to teach introductory physics courses. And I began lengthy talks about it with an experienced colleague – actually the chair who hired me in the first place. He was one of the most dedicated teachers I have ever known.

T: And his name?

H: His name was Richard Stoner, and his office right next to mine. He was so excited about teaching that he would regale me about it almost daily. He showed me all his examinations and data on student performance. This data was unique, because he believed that there was too much emphasis on quantitative problem solving in the usual physics course. So he designed very interesting qualitative questions for students to answer with qualitative arguments. He was frustrated because he could not write an examination on which the class average was better than 40%. So he would come to me and talk about the mistakes students made. I got very curious. I thought there must be something systematic going on. Now, because of my father I actually had the privilege to witness the early days of electronic computing and computer science. And because my background in philosophy, I have followed the development in artificial intelligence in my spare time from its beginning.

T: You have four kids and are a physics teacher in your spare time? (Laughs)

H: And so, I was always very much interested in cognitive aspects of physics and mathematics. Then, I heard about Robert Karplus while I was interacting with Stoner. You know about Karplus? Well, he was another theoretical physicist with many children (eight, I think) who got him interested in teaching science to children. Around 1976 he organized some famous workshops for the AAPT. Do you know about those workshops?

T: No, unfortunately.

H: Well these workshops were cleverly designed to involve professors in doing unfamiliar tasks under

conditions that simulate conditions faced by young students in science classes. For example, trying to do a manual task while looking at it through a mirror that inverted the image, so it was difficult to coordinate the hands. Karplus had been very successful in applying Piaget's research on cognitive development to the design of a science curriculum for grade school students. He organized instruction in a "learning cycle" with stages of "exploration, invention and discovery." And he introduced professors to his theory of instructional design in the workshops. Unfortunately, not long after that, I think about 1978, he had a terrible stroke and was permanently disabled. Otherwise the history of science education might have been much different. He was such a great intellect and motivator! Anyway, his workshops got me interested in Piaget, and as I devoured Piaget I kept thinking about the problems that Richard Stoner had with the students. Why do they have such difficulty in learning physics? And there were couple of the incidents, which I won't bother to tell you about, which gave me a clue that the student conceptions about physics are much different than the professor's. Then in 1979 I started a graduate seminar to read and discuss the literature in science education with graduate physics students. And I published my first paper on physics education in 1979, which was a review of Piaget and the relevance of psychology to physics teaching. It was called, "Wherefore a science of teaching?"

T: I read that paper.

H: Oh, you read that paper? Okay, well, I am proud to report that the paper had some influence on the emergence of Physics Education Research as a viable scientific discipline. It happened that, some 15 years later, two people independently told me that it got them going in the field. And both of them went on to make important contributions of their own. Anyway, I had done this literature review in science education and cognitive psychology for my seminar. That stimulated me to write the paper and contact the editor of "The Physics Teacher," Clifford Swartz, about publishing it. Do you know him?

T: No.

H: Well, Clifford Schwartz created "The Physics Teacher" himself and continued as its autocratic editor for several decades thereafter, so he had wide influence on physics education. He accepted my paper on the day that it arrived and moved it into publication immediately. That was the fastest publication in a science journal that I have ever seen. One week after I submitted it, I got back the page proofs – even before the notice of acceptance for publication. And shortly after publication, the Director of Science Education at the National Science Foundation made it required reading for all personnel in the Directorate. And, he

invited me to Washington D.C. be a reviewer of new research proposals; this was still in 1979.

T: That was a long time ago I read that paper. It was talking about how the computers work and would like to...

H: It talked about artificial intelligence models of human thinking, yes. They are fundamentally wrong. But that was first generation cognitive science, which was based on modeling the brain as a computer.

T: Long term memories, short term memories.

H: Exactly, that's right. All that is valuable, but has been thoroughly revised by developments in neural network theory. A revolution in cognitive science began about 1983, when I was privileged to help organize the very first conference on neural network modeling of the brain. I spent a decade doing that stuff too and published a few papers. But then, I had to stop because I was involved in too many things, and I didn't have really good colleagues at my university to work with.

T: You also developed with your colleagues, "the force concept inventory," the "mechanics baseline test" and modeling theory".

H: Okay. I was going to tell you about that. Since I had conducted a seminar, published a paper and visited the NSF, my department chair knew I was involved in physics education. This newly appointed chair unilaterally accepted into our graduate physics program a young man from Lebanon, named Ibrahim Halloun, who came with scholarship money to pay his way to get a doctorate in physics education. Then the chair told me that I would be Halloun's thesis advisor. That is how I got started on serious physics education research. Shortly thereafter an veteran high school physics teacher, Malcolm Wells, came to me and said that he had taken every university course in science and education that was at all relevant to teaching, and he had studied Piaget on his own. He said he was unsatisfied with the doctoral program in the School of Education and came to me because he wanted to write a dissertation that would be a substantial contribution to physics education.

T: We are saying in Turkish that the lip and the pop method, I mean just to throw around and hope something works out.

H: These two guys appeared at the same time, and also at the same time I was assigned to teach introductory physics. I was already convinced from my philosophy studies and scientific work that modeling is the essential core of scientific method. So I started to teach Introductory Physics with a modeling approach, and I formulated principles for a Modeling Theory of instruction. Halloun and Wells quickly picked up the idea of modeling instruction. I told Halloun about my ideas on why students are having so much trouble learning physics. That gave him a research theme: To develop an instrument for systematically assessing the difference between student preconception about the physical world and the ideas of physics that we want to teach them. The end result of that research (after nearly

ten years of development) is the “Force Concept Inventory,” which is essentially a discrimination test. It asks students to discriminate between scientific and nonscientific statements. It is a multiple-choice test wherein the nonscientific alternatives had been chosen from research to express popular nonscientific beliefs. If you don’t know science, the distracters look much more plausible than the scientific statements. So Halloun got started on developing the test as part of his dissertation. He did all the grunt work of designing specific questions and testing them with students. At the same time he joined me as a teaching assistant in teaching Introductory physics with a modeling approach. The first version of our concept inventory was called the “Mechanics Diagnostic” and it was published along with spectacular data documenting the ineffectiveness of conventional physics instruction. The results have since been repeatedly confirmed by others. Did you read the paper?

T: Most probably... I read almost every paper of yours in physics education.

H: Okay. First I should say that about fifty percent of the questions on “The Force Concept Inventory” (FCI) are about the same as on the “Mechanics Diagnostic,” so it is not surprising that student scores on the two tests are quite comparable. The “Mechanics Diagnostic” was administered to the physics classes of 4 different professors with a total of some 1500 students. Each professor had a different style of teaching. Two of them had awards for being an outstanding teacher.

T: Can you put a parenthesis there? How do you think the physics people learn to teach? Are they physics professors? You said that they had their own ways of teaching.

H: Well I think they just model their teaching on the teaching they had.

T: They teach the way they were taught?

H: They teach the way they were taught –which is the lecturer method, Okay? But the question here is: what is the effect of students’ prior knowledge on learning? How does it influence what students learn in the classroom? This was the first systematic study of that issue. There had been individual studies, some of which are referenced in our papers. They had noted various student “alternative conceptions” about physics, which are dismissed as mere misconceptions or “naïve beliefs” by the typical professor. It is not generally known that these “naïve beliefs” were held by intellectual greats of the past, including Newton and Galileo, as well as Aristotle. In fact, all of these beliefs were proposed at one time or another in the past as reasonable descriptions of the way the world works. It took the systematic scrutiny of experimental science to change them. However, when students take the typical introductory physics course, the “naïve beliefs” they bring with them are not scrutinized. Rather students use these beliefs to interpret what goes on in the course.

Consequently, students systematically misunderstand what they hear, read and do in the course. Student understanding is usually evaluated by problem solving performance. Problems are graded by assigning partial credit with the tacit assumption that mistakes are random. But, they are not random. They are systematic errors, due mainly to systematic student misunderstanding of the underlying concepts.

T: From the beginning?

H: From the beginning! And conventional instruction is not designed to address that fact. Okay? So, we got this first dramatic evidence for this failure from 4 professors all teaching in a different style. One was an instructor of problem solving drill. He believed you learn physics by practicing problem solving. In his lecture he demonstrated solving one problem after another; he had the students emulate that; then sent them home to solve more problems. The next guy was an experimentalist; he believed in developing students’ physical intuition by giving them enticing demonstrations of surprising physical effects – tacitly assuming that seeing is believing! Okay? Another guy was a theoretician who concentrated on explaining the logical structure of the subject. He believed that you can’t understand conservation of energy without deriving the work–energy theorem from Newton’s Laws, and things like that. The fourth guy was a new teacher and he just followed the textbook without deviation or elaboration. Okay? First we gave the Mechanics Diagnostic as a pretest to all the students, which showed the students to have appallingly low scores, even though almost all of them had taken high school physics. Then we repeated the Diagnostic as a posttest and found that there was only a 15% improvement. The average score was only about 60%, whereas the questions look so trivial to a professor that shouldn’t worth asking. Furthermore, the mean scores of all the classes were the same within 1% for all the professors. So, student scores were independent of the professor’s experience, teaching technique, or whatever. I must confess that I was one of those professors. It is sobering to be confronted with evidence that what you do in lecture has no net impact on the students at all! Okay? So, the paper had immediate impact. For example, after it was submitted to the American Journal of Physics, I gave a talk on the results at an AAPT meeting, and the editor came running down from the audience after my talk and exclaimed, “So that is what your paper was about!” Then he accelerated its publication.

Now, while Halloun was working on his thesis, I had many discussions with the high school teacher Malcolm Wells about what he should do for his thesis. He had still not decided when he saw the results just described, and he declared, “My students can do better than that!” So, he gave it to his students, and they didn’t do better!

Well, that got Malcolm Wells fired up. He knew that he was an excellent teacher, though we had argued for 2 years about what he could contribute to his profession that would be uniquely valuable. Finally, he had a clear goal for his dissertation: To design and conduct instruction that is more effective at overcoming student preconceptions and developing Newtonian thinkers. At that time I was writing up results from Halloun's dissertation for publication. It was then that I constructed a systematic classification, a taxonomy of alternative beliefs about force and motion by comparison with the Newtonian system. The taxonomy is key to success of the FCI. The classification demonstrates a complete covering of the Newtonian force concept. It is based on an analysis of the Newtonian force concept along five dimensions. Even physicists disagree on what constitutes the concept of force. Some say that "force" is defined by $F = ma$. I claim that it takes all five of Newton's laws to define the concept of force. Why do I say five? Don't they usually say three?

T: I was thinking.

H: That is right. It's because Newton didn't articulate all of his laws, okay? For example, one essential "law" that he took for granted is that space is Euclidean. Right? Geometry is essential to the concept of length and measurement. And you need the concept of a clock to define time and its measurement.

T: As a Zeroth Law?

H: And how did Newton handle that? With one sentence: "I presume everyone knows geometry? An explicit specification of the Euclidean geometry of space is what I call the Zeroth Law. Have you read my paper called, "Modeling Games in the Newtonian World?"

T: I did.

H: That is my favorite paper about "modeling." That's where I published a complete analysis of Newtonian theory, though it had been worked out ten years before and used to develop the taxonomy of force concepts. The complete force concept is thus to be regarded as a five dimensional concept, because you do not understand how to apply the concept in any given situation until you have mastered all 5 dimensions. It's a kind of conceptual engine that does not run unless all its parts are working. Of course, every physicist can run that engine, though most of them are unaware of all the working parts. Indeed, many physicists and physics textbooks do not understand the function of Newton's First Law? It says that a particle moves at a constant speed in a straight line when there is no net force on the particle. Well, that looks like a special case of Newton's Second Law, except, "what defines constant speed?" Well, that is the speed of a free particle! And a free particle is defined by the condition that there is no net force on it. So, a free particle can be used as a standard reference motion to define a clock. Thus, the First Law

serves to define clocks implicitly in Newtonian mechanics. That's not generally recognized. There are more people that understand the First Law as defining an inertial reference frame. Okay? But that is equivalent to identifying a system of free particles. To ascertain if a given reference frame is inertial, you set a few free particles in motion and see if they move in a straight lines, or not.

T: Or not.

H: Or not., Well, I was very pleased when I came to this understanding of the First Law while I was writing my Mechanics book, which, by the way, is the first place I articulated a systematic modeling approach to physics. That was background for my subsequent papers on Modeling Theory in instruction and cognition as well as scientific practice. All these ideas fit together into one coherent program. The modeling program is concerned with development and use of conceptual tools to enhance human cognition. This includes evolution of the natural languages, which developed informally in response to environmental pressures. The languages of science and mathematics developed more formally. Each science creates its own conceptual tools, its own specialized language to achieve its specific goals.

Anyway, I was inadvertently sucked into applying my philosophical ideas about modeling to physics education, because I had two graduate students to mentor. Then, Malcolm Wells got more spectacular results with his thesis. When he came to me he was already using Robert Karplus' "learning cycle" in his teaching. It has 3 phases called "exploration, invention and discovery." We discussed how it works at length. Then I asked him, "How do you explore? How do you invent? And how you discover knowledge?" The first phase is commonly called "discovery learning" Okay? In the exploration phase you bring students into contact with phenomena that you want them to understand and they play around with it. The hope is that they will identify significant properties and finally change something to discover how it changes or behaves. Then the students will invent or are introduced to a concept to describe or explain the behavior. The idea is that in this way the students will learn concepts that are grounded in experience. The trouble with discovery learning is that students tend to mess about with the phenomena without progressing to insight. They need a more systematic approach to sense making. So I suggested to Malcolm that he meld the learning cycle with the stages of model development and deployment that I spelled out in my paper "Toward a Modeling Theory of Physics Instruction," a preprint of which was available at the time. Malcolm ran with the idea and applied it brilliantly in his teaching, enhancing it with many subtle details. The method can be regarded as a way to teach systematic scientific inquiry.

The central idea in the modeling approach is that you understand a phenomenon by creating or adapting a model to describe it. The subtlety in teaching is in how to get students to do that for themselves, so they become autonomous learners. Scientific inquiry begins with specifying the system of interest and the variables involved. Representation of the system and its variables is the first step in constructing a model. The next step is specifying relations among the variables and how they change. Finally, validity of the model is established by comparing it with empirical data on how the system behaves. This is an outline of ideas about modeling that I presented to Malcolm. We coined the term “modeling cycle” for the integration of systematic modeling into the learning cycle. To guide students through the modeling cycle and develop their insight into the process, Malcolm developed a technique that we call “Modeling Discourse.” It has two major components: first, engaging students in explicit descriptions of what they are doing and thinking in terms of models and modeling. Second, sensitivity to student preconceptions about physics and getting them to articulate their beliefs clearly so they can be evaluated with evidence and argument. Malcolm became very skillful at managing modeling discourse. Halloun also worked on modeling instruction as part of his thesis, but he focused on a modeling approach to problem solving. We published a paper on that too. It worked fairly well, but it’s not as deep an innovation as what Malcolm Wells did.

Anyway, Malcolm evaluated his approach to modeling instruction in his thesis, with the most well-controlled and significant educational experiment that I know of. As a control for his experiment, Malcolm engaged another high school physics teacher who was perfectly matched with Malcolm in education and long teaching experience. That fellow used a conventional problem solving approach to teaching mechanics, assigning the students one problem after another throughout the course. Malcolm Wells synchronized his course to spend the same amount of time on mechanics, but he didn’t teach any explicit problem-solving at all.

T: Swackhamer?

H: No, Swackhammer entered the picture several years later. This guy’s name was Wayne Williams. To compare the effectiveness of the two approaches, Malcolm designed a conceptual problem-solving test that we refined and published later as “the Mechanics Baseline Test.” This test is non-routine in the sense that none of the questions can be solved by simple substitution in formulas; all questions require conceptual analysis with a quantitative aspect. Furthermore, all the questions are basic in the sense that they concern material that should be covered in any introductory physics course. To evaluate impact on student preconceptions Malcolm joined with me in revising the Mechanics Diagnostic to create a more comprehensive

coverage of force and motion concepts. The result is the famous Force Concept Inventory. With these two excellent instruments, the Baseline test and the FCI, to evaluate outcomes, Malcolm was able to validate the remarkable result of his experiment. Not surprising but still impressive, Malcolm’s students had much higher gains than the control group on the FCI, and even did much better than the university physics students in Halloun’s study. The big surprise was that Malcolm’s students did 20% better than the control group on the problem-solving test, even though Malcolm didn’t even try to teach problem-solving. Rather, he was teaching the modeling concepts that are needed for effective problem-solving. Problem-solving is a lot easier if you understand on the problem! The teacher of the control group, Wayne Williams, was so impressed by the result that he postponed his retirement. He asked Malcolm, “How did you do that?” Later on when we started Modeling Workshops, Williams attended and he kept on teaching five years past his retirement age.

T: Larry? Because I attended one of his workshops.

H: No, Larry Dukerich came in later. What happened next was, because I was convinced that Malcolm’s work had produced an innovation in instruction of great promise, I contacted the NSF program manager that I had met a decade earlier, and he agreed on the spot to fund a Pilot Project to see if Malcolm’s success is transferable to other teachers. Malcolm and I organized the first workshop for teachers together, and I learned that it was better to let Malcolm do it without me. Larry attended that first workshop, as did Gregg Swackhamer, I think. Swackhamer was a teacher from Chicago who had read my papers on modeling and was interested enough to visit me for his Sabbatical leave. I told him to spend the time in Malcolm Wells’ classroom. He contributed his observations to our joint paper on Malcolm’s teaching. He also helped clean up the FCI for publication. He has since served as a stalwart leader in the Modeling Program.

In our pilot workshop the teachers were very enthusiastic about everything. So we were surprised and disappointed that the FCI showed no improvement in their teaching for the following school year. We asked, “How could that be?” The teachers thought their teaching had improved considerably, but that was not reflected in measurable results. If you were doing an ordinary education experiment without the FCI to evaluate what the students actually learned, you would have thought from teacher reactions that it was a tremendous success. Then we identified the problem: while we were teaching modeling techniques we were also teaching use of new MBL (micro-computer based laboratory) tools, probes, and so on. The teachers were so focused on the mechanics of teaching with these new laboratory tools that they neglected to cultivate

modeling discourse to draw out the student ideas and correct their misconceptions. Fortunately, the pilot project brought the teachers back for a second year. This time we made sure that the teachers understood how crucial modeling discourse is to the success of modeling instruction. Subsequent evaluation with the FCI showed the significant gains we had hoped for. This experience serves as an example showing that inquiry based instruction isn't enough. How you do the inquiry makes all the difference.

That got me inextricably involved in physics education! Success of the pilot workshop set us off and running: The FCI got published and Eric Mazur at Harvard endorsed it. We got big grants for more and better teacher workshops and Modeling Instruction grew to a nationwide program that has remained vibrant for 20 years.. To date more than 2,000 physics teachers have taken Modeling Workshops, close to 10% of all physics teachers in the United States. The workshops have continued to evolve and improve through the contributions of many committed teachers. My own physics education research during the last decade has been what you'd call "informal." I have kept up with the literature and monitored what was going on with teachers in the program, offering occasional recommendations for improvement. I have suggested some very nice improvements that haven't been adopted by all the teachers because they haven't been fully worked into the modeling curriculum. Our government funding ran out, and I have wasted a lot of time trying to get it renewed or replaced to no avail. Here we have this great program, –you heard in my talk– we have this graduate program for teacher professional development that the teachers can't afford to attend. So, the big problem is financing; I won't go into details of everything though, as Malcolm Wells liked to say, the devil is in the details. One major thing we learned during the first decade of running Modeling Workshops is that for teachers to gain sufficient proficiency with the modeling approach to use it in teaching their own classes, the optimal length of a workshop is four weeks. Five weeks is more than you need, and three weeks is barely adequate, though that is most common for practical reasons.

T: How many hours in a week?

H: A typical workshop is scheduled for five hours a day for 3 weeks, okay? The total is the number of hours required for a three credit course at an American university, so teachers can be awarded university credit for attending. Actually, the workshop is most effective when the teachers have shared living quarters, so the teachers continue to talk about the experience for most of the day. This sort of immersive experience is a very important social component of the modeling program. Teachers describe the experience as "transformative!" Indeed, most of the teachers have remained in contact

through the "modeling listserv," which has attracted daily postings from teachers for a decade. And when funding for Modeling Instruction dried up, the teachers created their own organization, "the American Modeling Teachers Association" to continue the work. Thus, the Modeling Program has spawned a cohesive "community of practice," a nationwide community of dedicated teachers with a shared vision of models and modeling in science teaching. I believe that cultivating and supporting such a community of science teachers is the most promising possibility for broad, rapid, and sustained high-quality science education reform – perhaps the only possibility. Time will tell!

Having passed on the Modeling Program to a younger generation of teachers, I can spend more time on Zitterbewegung research. That's beginning to look very exciting, promising new answers to questions about quantum mechanics. For example, where do quantized states come from? Well, I have so far found exact Zitterbewegung solutions only for an electron in a constant magnetic field. Solutions of the Schrodinger equation for this case are called Landau levels. The states are essentially the same as for a 2D harmonic oscillator, describing uniform circular motion. Well, in my Zitter model the electron is circulating around a guiding center. So, the electron has an internal motion, and it is resonance of this internal motion with the orbital motion of the guided center that determines quantized states. This suggests that all quantization can be explained as Zitter resonances. In fact, Schrodinger said that when he first developed his equation, he was expecting to explain stationary atomic states as resonances. But he couldn't see what resonated with what. I see electron Zitter as the missing piece needed to explain quantized stationary states as resonances. I have long known that geometric interpretation of the unit imaginary i suggests an internal spatial motion of the electron. So now, in one case I can account for the stability of radiationless states by resonance. Of course, I have not yet proved that these quantized states are radiationless. That's the next thing I want to work on this summer, and I think I know how to do it.

T: I would like to do . . . I have studied superconductivity in my Masters. I am really curious to know your answer to that. Do you think we know all that is to know about the electron?

H: No. No, I don't.

T: Because it just occurs to me that we need a somewhat new theory of electron probably to solve the questions of superconductivity. Do you feel a need of that sort?

H: I feel more than a need. I think I know at least one way that the theory should be changed. The standard theory of superconductivity is not as successful as people make it out to be. When you get near the critical point they have a renormalization theory to explain what happens. But renormalization theory doesn't get the correct result for the critical point. And

not just at the critical point! The deviations of theory from the experimental data increase as you get closer and closer to the critical point. So, what is going on there? Here is my hypothesis: the electrons have this internal Zitter motion, and as you approach the critical point there is an increase in Zitter correlations, that is, in resonances between Zitter motions of different electrons. As temperatures increase correlations are destroyed by thermal fluctuations. I submit this as a general explanation for all critical phenomena in condensed matter systems.

T: *Is there coupling again going on there?*

H: Well, aside from Coulomb repulsion, the force between two electrons is not just magnetic coupling. It is generated by a magnetic moment plus a rotating dipole moment due to Zitter. If the rotating dipole moments are not in sync, not resonant, then their average force is zero. But, as you go to lower temperatures the opportunity for correlation increases, so electrons start circulating in sync; that changes the strength of the interaction. Thermal fluctuations destroy such correlations, but the thermal fluctuations become less and less significant as temperature decreases. Then chains of correlated electrons appear with longer and longer correlation lengths as temperature decreases. I submit that Bose Einstein condensation can be explained in the same way, by reducing it to condensation of Fermions. All the atoms in a Bose condensate have Fermion structure, and I suggest that the correlations among atoms are due to resonances among the electrons hidden in the atoms. That is a topic for research.

T: *More than a hunch?*

H: More than a hunch! I have well-defined equations of motion for the electron, so it is a hunch that can be investigated.

T: *So, we call it hypothesis.*

H: (Laughs) We call it a hypothesis. Let us define hypothesis as a hunch that can be investigated, a testable hunch.

T: *And my last question is about what is yet to be done in physics education. For the future generation of researchers, what do you suggest to them? In what ways to proceed? What is left to inquire in physics education research?*

H: Well, I just published a couple of papers in which I laid out a modeling theory of cognition. They say a lot more about what I think physics education researchers should be reading. They should be reading the literature in cognitive linguistics. The field of "Cognitive Linguistics" has emerged only in the last few decades. Have you heard about George Lakoff?

T: *No.*

H: You should learn about Lakoff because he is famous, and famous for good reason. I say quite a lot about him, actually, in my paper entitled "Notes for a Modeling Theory of Science, Cognition and

Instruction." It was published in the GIREP proceedings two years ago.

T: *In Amsterdam?*

H: In Amsterdam. That is the paper for my invited talk at the GIREP conference on Modeling in Physics and Physics Education. A copy of it can be downloaded from the modeling website. Then, I have more recent paper that follows it up. There is more and more work in cognitive science of great relevance to science education, especially in Cognitive Linguistics. They have learned, for example, that the notion of "force" in everyday language is the source of many metaphors in everyday life. I already mentioned that it differs drastically from the Newtonian concept of "force." I may write something about this in the future. There is an issue of finding sufficient time. Anyway, I see a few physics education researchers, such as Joe Redish, reading the cognitive science literature. Actually, his son is a neuroscientist, and I spend a few years studying neuroscience myself. There is some of that already in my paper "Toward a Modeling Theory of Physics Instruction" published in 1987. That paper was actually written at the same time as the "Mechanic Diagnostic," but publication was delayed for 2 years because of uncompromising objections by a referee. Ultimately, the enlightened editor of the AJP overrode his objections and published it anyway. Along with that, we published the positive results of Halloun's experiment with modeling instruction in problem solving. Of course, Halloun is now busy doing his own thing. He is very, very active in elevating physics education in the Arab world.

T: *Okay, it has already been one hour and 50 minutes. I very much thank you for spending this much of time with us. And I am sure—I am delighted that this has been the most wonderful conversation I have ever had in my entire life and I thank you for that. I hope the readers and the listeners of this conversation, you also enjoyed. And I thank you very much again and I hope to read much of your work again in the future.*

H: I was going to say that the main way you led me on was by saying that you actually read my papers. It's always pleasure to talk to somebody who takes your work seriously.

T: *For the last fifteen years I have been following your works.*

H: Ok. Thank you very much.

T: *And again thank you for all the contributions in physics and physics education and also giving this conversation. Thank you.*

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The audio recording of this conversation/interview is available from the journal web site.



* The first author conducted the interview and the other two authors transcribed it. Afterwards the text was checked by Professor Hestenes for accuracy.